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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2019 February 8 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

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

The President. I take great pleasure in inviting Professor Martin Rees to tell us everything he knows about cosmology [laughter]. The title of the Gerald Whitrow talk is 'Progress and frustrations in cosmology'.

Professor Lord Rees. Gerald Whitrow was interested in the history of our subject. He was also a speculative thinker. So, in homage to his memory, I'll offer some history and some speculations too.

Back in 1960, cosmology was a subject with minimal empirical content. Astronomers couldn't probe far enough back in time to detect cosmic evolution even if it occurred. Indeed, there was little evidence that the Universe had the large-scale uniformity postulated in the models that were discussed by Hoyle, Lemaître, and others. But the subject was transformed in the 1960s, especially with the discovery of the cosmic microwave background (CMB). Today cosmology is still on a roll. We can with confidence trace cosmic history back 13·8-billion years, to a moment only a billionth of a second after the Big Bang. Our Universe's expansion rate, the mean density of its main constituents, and other key numbers, are pinned down to a precision of one or two percent; we can detect redshifts as large as 10. When the history of recent decades gets written, that amazing progress will be acclaimed as one of science's greatest triumphs — up there with the genome and plate tectonics.

Werner Israel divided cosmologists into two types: the chess players and the mud-wrestlers. The chess players are tackling fundamental questions, such as why does the Universe expand in the way it does? Why does it contain the mix of constituents we measure? But for the mud-wrestlers cosmology is the grandest environmental science — they're exploring the messy physics whereby clusters, galaxies, and their constituent stars and gas emerged and evolved during cosmic history. Computer simulations can now portray how the fluctuations in the early Universe, measured with exquisite precision by ESA's *Planck* spacecraft, evolved into galaxies and clusters with properties that match what's observed. Importantly, we find that all galaxies are embedded in a swarm of particles that are invisible, but which collectively contribute about five times as much mass as the ordinary atoms — the dark matter.

We can trace cosmic history back to a nanosecond. That is when each particle had about 50 GeV of energy — as much as can be achieved in the *LHC* — and the entire visible Universe was squeezed to the size of our Solar System. But questions like "where did the fluctuations come from?" and "why did the early Universe contain the actual mix we observe of protons, photons, and dark matter?" take us back to the even briefer instants when our Universe was hugely more compressed still.

According to the 'inflationary-Universe' theory, the entire volume we can see with our telescopes evolved from a hyper-dense blob no bigger than an apple, which itself inflated from a domain of microscopic size. The physics at that era is of course still conjectural, but nonetheless the generic idea of inflation has achieved success in predicting two features of the fluctuations in the microwave background: that they are Gaussian, and that their amplitude depends on scale in a distinctive way, consistent with what the *Planck* satellite data show.

What about the far future? In 1998 cosmologists had a big surprise. It was by then well known that the gravity of dark matter dominated that of ordinary stuff — but also that dark matter plus baryons contributed only about 30 percent of the critical density. This was thought to imply that we were in a Universe whose expansion was slowing down, albeit not enough to eventually halt. But gravitational attraction was seemingly overwhelmed by a mysterious new force latent in empty space which pushes galaxies away from each other. This could be Einstein's famous cosmological constant, Λ; or empty space must contain some form of 'dark energy' with negative pressure (that's because $\rho + 3p/c^2$ appears in the Friedmann equations and negative p gives acceleration.)

Galaxies accelerate away and disappear over an 'event horizon' — rather like an inside-out version of what happens when things fall into a black hole. All that's left will be the remnants of our Galaxy, Andromeda, and smaller neighbours. Protons may decay, dark-matter particles annihilate, occasional flashes when black holes evaporate — and then silence.

Here we must admit to frustrations. This vacuum energy won't be understood until we understand the graininess of space itself — and this is expected on the Planck scale, a trillion, trillion times smaller than an atom. We shouldn't hold our breath for a quick solution here. Indeed, string theory, even if true, might be too complicated for any human brain to work through. But I want to add a perhaps curmudgeonly remark. Popular presentations of cosmology customarily present a 'pie diagram' showing that about 5 per cent of the Universe is baryons, 25 per cent dark matter, and 70 per cent 'dark energy'. This is of course true of the present-day Universe, but it distorts the relative astrophysical importance of dark matter and dark energy. Dark matter is crucial both within and outside galaxies, and was crucial in controlling how galaxies formed. In contrast, the dark energy was everywhere swamped by dark matter in the first half of cosmic history, so it had no influence on how cosmic structures were laid down; moreover it is swamped even today within galaxies and clusters. Dark energy may be the number-one challenge for fundamental physics, but it is almost irrelevant to astrophysicists. So it's more frustrating that we don't know what the dark matter is, even though it was first suggested by Fritz Zwicky 80 years ago.

Here's another question. How much space is there altogether? We can only see a finite volume — a finite number of galaxies. That's essentially because there's a horizon: a shell around us, delineating the distance light can have travelled since the Big Bang. But if we were on a ship we don't think the sea ends just at the horizon. Even conservative astronomers are confident that the volume of space–time within range of our telescopes, what astronomers have traditionally called 'the Universe', is only a tiny fraction of the aftermath of our Big Bang. And this raises another query. The physical laws are the same everywhere — we can observe spectra of high-z objects that show no change in the microphysics of atoms or their nuclei or of the strength of gravity. But need that be true over this far vaster domain? And there's something else. 'Our' Big Bang could be just one island of space–time in a vast archipelago — one big bang among many.

Of course this 'multiverse' concept often triggers the response that unobservable domains aren't part of science. I want to contest this by aversion therapy — the psychological process whereby if you're scared of spiders you're at first presented with a small one a long way away, but end up at ease even with tarantulas crawling over you.

I mentioned that there were galaxies beyond our horizon: in a decelerating Universe they would come into view in the far future. But because they're accelerating away from us they'll never even in principle be observable. But does that make them any less 'real'? They're the aftermath of 'our' Big Bang. But since they would never even in principle be observable, why is their epistomological status higher than that of galaxies in the aftermaths of other big bangs (if there are other big bangs)? Of course, we'll only take other big bangs seriously if they're a prediction of a unified theory that gains credibility by being 'battle tested' in other ways.

Let me give an analogy. We can't observe the interior of black holes, but we believe what Einstein says about what happens there because his theory has gained credibility by agreeing with data in many contexts that we can observe. Likewise, if we had a theory that described physics at 10^{16} Gev that had been corroborated in other ways (by, for instance, deriving the unexplained parameters in the 'standard model' of particle physics), then if it fulfilled the requirements needed for Linde's 'eternal inflation' multiverse we should take that prediction seriously.

A challenge for 21st-Century physics is to answer two questions. First, are there many 'big bangs' rather than just one? Second, if there are many, are they all governed by the same physics or not? According to string theory, there could be a huge number of different vacuum states, different Λ, different microphysics. What we call 'laws of nature' may in this grander perspective be local by-laws governing our cosmic patch. If physical reality is like this, then there's a real motivation to explore these 'counterfactual' Universes, with different G, different Λ, and so forth, to explore what range of parameters would allow complexity to emerge. This is what's called anthropic selection. Some people foam at the mouth at mention of the A-word. However, quite apart from these motivations, I think it helps to develop our intuition to explore what would happen if the laws or parameters were different from their actual values. It's like 'counterfactual history', where scholars speculate on what might have happened if the British hadn't lost the American colonies in 1776, and biologists on how our biosphere might have evolved if dinosaurs hadn't been wiped out.

I want to mention three counterfactual Universes.

What if G were different but all the other numbers — baryon/photon ratio, the dark matter to baryon density, and lambda — were kept the same? If gravity were stronger, then stars — gravitationally-bound fusion reactors — would exist, but they'd be smaller and shorter lived. And gravity would be so strong that it would crush creatures our size on the miniature planets that could still exist. But if G were lower, then there would be a grander and slower-motion Universe. The masses of stars and galaxies would go up as $G^{-3/2}$, and stellar lifetimes would go as $1/G$. This Universe would offer more time for complex evolution, on bigger planets.

A second key number is called Q. The present 'texture' of our Universe — the scale of clustering — depends on how 'rough' the early Universe was, as measured by the number Q, the amplitude of fluctuations in the CMB. The actual value of Q is about 10^{-5}, measured by the fluctuations in the CMB. If the early Universe were completely smooth, then today, even with the same microphysics, there would be cold hydrogen and helium — no stars and no people. The value of Q determines the scale of structure in the Universe. The bigger it is, the earlier structure forms and the bigger the structures get. The 'anthropic range' is from 10^{-6} to 10^{-3}. If the Universe were too smooth, no galaxies would form; if it were too rough, conditions would be too violent.

But what if the nuclear force was much weaker? As a third 'counterfactual' let's suppose that the microphysics were changed so the nuclear force were too weak to allow a periodic table of elements. A counterfactual Universe where hydrogen was the only element would look much the same as ours. Galaxies would form. And, surprisingly, stars would shine, but releasing gravitational energy as they contracted to white dwarfs or black holes. There could even be Jupiter-like planets of solid hydrogen. But there'd be no chemistry, no complexity, certainly no life as we know it. Fred Hoyle's *Black Cloud* is the best you could expect. This 'nuclear-free universe' would resemble our actual Universe only to the extent that, for instance, a marble statue resembles a real human.

There's no time to mention other 'counterfactuals' — for instance, a different number of dimensions — a special interest of Gerald Whitrow. We can imagine a range of Universes with different combinations of the key numbers. Obviously some would be sterile or stillborn, but no especially fine tuning is needed. Some don't like the multiverse idea because it would nullify the hope for neat explanations of the fundamental physical numbers. But our preferences are irrelevant to the way physical reality actually is, so we should surely be open-minded to the possibility of a fourth and grandest Copernican revolution — we've had the Copernican revolution itself, the realization that there are billions of planetary systems in our Galaxy; and that there are billions of galaxies in our observable Universe. But we'd then realize that our observable domain is a tiny part of a far larger and possibly diverse ensemble. We'd live not in a typical part of the multiverse but a typical domain in the subset that allows complexity to develop. To address that question we have to know the probability distribution — and the correlations between them. That's a can of worms that we can't yet open and will have to await huge theoretical advances. What's needed is a well-corroborated theory that can describe physics at the inflationary era, trillions of times higher energies than the *LHC* can reach.

Edwin Hubble wrote "Only when empirical resources are exhausted should we enter the dreamy realm of speculation". So I'd like to redress the balance of this talk by emphasizing that progress in cosmology and high-energy astrophysics has owed 95 percent to advancing instruments and technology — less than five percent to armchair theory. I'd expect that balance to continue. But there is one big change, brought about by the fact that computer simulations have hugely advanced in power and expanded in range.

The 1960s were exhilarating for young astrophysicists. So much was new that those with long experience didn't have a big head-start over the youngsters. But I'm not here to be nostalgic. Indeed, since there are students and postdocs here I want to emphasize that today is an equally good time for young researchers to study phenomena all the way from Mars to the multiverse. The pace of advance has crescendoed rather than slackened; instrumentation and computer power have improved hugely; and still more will come on line in the 2020s. And on that encouraging note I'll conclude.

The President. Martin, thank you very much indeed. This talk is open for questions and comments.

Dr. G. Q. G. Stanley. It would be lovely to hear what Gerald would have said in response. On the idea of the multiverse, would there be a Big Bang to end all other Big Bangs, and if there is not, would that tell us something about the multiverse?

Professor Rees. In most scenarios there is an infinite future, but there is debate about whether there is an infinite past or not. Some people say you cannot go back into an infinite past, others say it would be a like a grand version of the old Steady State theory.

Reverend G. Barber. The infinite-inflation theory depends basically on the idea of spontaneous symmetry breaking, whereby the laws of physics fall out in one particular case in our Universe, and therefore they fell out in others. The question really is about the scientific method, in that science has always been based on observation, and hypothesis being verified by observation, experiment, and, possibly, falsifiable experiment. If you say that the laws of physics fall out by spontaneous symmetry breaking, you are automatically bringing in the idea that there's an ensemble of other possibilities which require an ensemble of Universes. In other words, it's a circular argument, bringing in the assumption that there are other Universes in order to confirm there are such entities. Now, if it is a case of explicit symmetry breaking, there would have been some reason why these physical constants would fall out in the way they did. But, the very act of saying it is spontaneous removes the question of what happened. In other words, it might actually be cutting off the scientific investigation about how these forces ended up having the values they do.

Professor Rees. We know of course about spontaneous symmetry breaking in the electroweak transitions, and how, extended to the strong interactions, this may account for why there is matter and not anti-matter in our Universe. As regards the more exotic issue of the different vacuum states, the experts (and I'm not one!) are very far from having any agreed theory. But some of the people who work on string theory think there would be very many different vacuum states, and in each of them the microphysics would be different (and there would be different constants). Now, as I said, we have no idea whether this is true. However, if such a theory had some empirical successes telling us why there are three species of neutrinos, what their masses are, and so forth, then we would take its further predictions seriously. And if it did predict the physics needed for Linde's eternal inflation, we would take that seriously, just as we take seriously what Einstein's theory says about the inside of black holes, which we can't observe, because other tests have vindicated it. That is the best we can hope for.

Reverend Barber. My worry is that the scientific questioning would be short-circuited by simply saying that the anthropic principle means that there is a multiverse.

Professor Rees. Oh no, I wasn't claiming that. There could be just one Universe with unique physical laws. I didn't say anything against that. And even if there were many big bangs, they could all have the same microphysics.

The President. Last question, unless anyone has a theory of the Universe they wish to air.

A Fellow. In your cartoon, showing the multiverse theory where each universe was expanding, it looked like they were joined together. So in that theory does that mean that each universe sets off the next one? Could the fluctuations occur such that they could spontaneously appear anywhere?

Professor Rees. That was just a cartoon. There are some versions like that, but in other versions there is an exponentially expanding substratum (like a mega-Steady State theory). 'Universes' condense out of it that would then be completely disjoint and not connected. And there's another quite different version which invokes a fourth spatial dimension. Imagine ants crawling around this sheet of paper (a two dimensional Universe) being unaware of ants on parallel sheet of paper. Likewise, there is a version of the multiverse where there is another space–time just a small distance away. But that distance is measured in a fourth spatial dimension and we are imprisoned in our three. So, there are lots of ideas. My key point in the lecture was that we have no reason to think the domain we can observe even in principle with 'perfect' telescopes, is more than a tiny part of physical reality.

The President. Let us thank Martin again for a superb lecture [applause]. I would now like to introduce our next speaker, Rebecca Bowler from the University of Oxford. Rebecca won the Winton 'A' Prize. She is going to talk on 'The search for luminous star-forming galaxies at ultra-high redshifts'.

Dr. Rebecca Bowler. [The speaker began by defining ultra-high redshift as being the redshift limit of galaxies currently observable with the biggest telescopes, in other words $z = 7$ and above. The first galaxies formed when structures appeared after the end of the Cosmic Dark Ages a few hundred million years after the Big Bang. We can now observe some of those primeval galaxies, the most distant having been observed with *HST* with a redshift of 11·1. The re-ionization era is a critical one — it is when the first stars and galaxies and black holes were forming, along with the first particles of dust. To investigate this epoch, quasars can be used as probes; they are extremely luminous and thus visible over great distances. In addition, 'sub-mm' galaxies, which are heavily dust-obscured and which are undergoing bursts of star formation and forming thousands of solar masses per year, can be observed. Another useful probe is the gamma-ray bursts (GRB) which highlight galaxies not ordinarily visible, and thus permitting observation of very faint galaxies at high z. What appears to the observer is the afterglow after the GRB goes off.

These three types of observation are very rare, extreme events; so today the speaker said she would focus on more normal galaxies, which are typical objects at this epoch. These objects can be detected using deep exposures with the *HST* such as the Ultra Deep Field images. We are interested in the faint, extremely red objects in these exposures but the *HST* has a very small field of view. So to uncover the galaxy population, these images need to be combined with data from different sources so as to probe larger volumes. This can be achieved by use of ground-based telescopes. To select the distant, massive galaxies a series of broad-band filters is used to map the Lyman break, the visibility of which gives an approximate redshift. The *HST* is limited to $z = 12$ as the Lyman break then lies beyond the sensitivity of the camera, and this limit will only be improved upon by use of the *James Webb Space Telescope*.

Ten objects have been found in wide-field surveys. These massive galaxies help us to understand the astrophysics of galaxy formation at early epochs. If you have a Universe consisting of dark matter and then insert 10% baryons what would the galaxies look like? The observations show deviations from the number of such galaxies at both the faint and bright ends, so the bright galaxies that are predicted in a purely dark-matter scenario are not seen. The reason for this may be various types of feedback: the star formation in the galaxy may be quenched and the galaxies then do not grow as large.

For faint galaxies the quenching process is thought to be due to supernova feedback. In a small, faint galaxy, a few supernova events would disperse the star-forming material and stop star formation. In the bright galaxies, supernovae do not have the same effect because the galaxy gravity can hold on to the material allowing star formation to take place. What is thought to be the actual mechanism is feedback from active black holes; the jets which are emitted stifle star formation. At very high redshifts the black holes may not be massive or efficient enough to do this, and what is observed actually relates to the dark-matter scenario.

The result of the search for massive galaxies in the wide-field data revealed a few more objects than expected. Using data from deeper exposures, the ten galaxies mentioned earlier were confirmed and also a few more found. At that epoch the decline in the number density of galaxies does not follow the decline found at later times, and instead is more consistent with a power law, similar to the underlying dark-matter distribution. As more were found than expected, the new data were double checked to look for contamination from low-z galaxies or Galactic stars. Are some of them QSOs? Although there is an excess of galaxies at $z = 3$ which is entirely due to the presence of QSOs, is this also the case for $z = 7$ galaxies? At that redshift there are less than ten QSOs known. Extrapolating the quasar luminosity function to $z = 7$ from the low-redshift results shows that QSOs are not numerous enough to explain the excess of galaxies observed, so QSOs can be excluded as contaminants. Another problem is gravitational lensing. If you look at the number density of distant galaxies in a field with a very massive foreground cluster, the background galaxies get lensed, making them appear more luminous than they actually are. There are no massive clusters in this sample but there is some moderate lensing produced by foreground galaxies; however, it is not significant.

Comparing these observations with the luminosity function reveals that there is good agreement with what is predicted by the simulations if there is no feedback from the active galaxies. The galaxies under investigation are extremely bright so they can be observed with relatively modest exposures. The wide-field observations have been followed up with *HST* observations which help to measure the morphology and size. *ALMA* maps of the dust in sub-mm wavelengths can be made. The *VLT*s in Chile have been used in the optical and IR to obtain spectroscopy of the targets. The *HST* images resolve features in some of the galaxies and it appears that the brightest galaxies are actually merging pairs. *ALMA* yields mm-wave spectra from the dust emission, and 10-minute exposures using this array reveal dust in one of the galaxies.]

The President. Rebecca, thank you very much indeed. That was a lovely talk. Open for question or comments.

Professor Monica Grady. The dust you are talking about in these early galaxies — is there enough to form planets?

Dr. Bowler. That's a great question. I don't know what "enough to form planets" is, I am very much a galaxies person. There's plenty of dust. The question is whether planets can form in such a turbulent environment, because these are very early galaxies, so they are not static discs with not much happening in them. They are possibly merging and gas is falling in. The stars that are forming in these galaxies are producing a lot of high-energy photons and light, and this can really impact the dust.

Professor Grady. You talked about supernovae going off. How many cycles have stars been through in these galaxies to produce all the heavier elements and stuff. Is it all there?

Dr. Bowler. It is thought that the first generation of stars, the pristine stars that formed after the dark ages, had very rapid lives. They explode as supernovae within a few million years. In those explosions they are then enriching their environment. It is thought that this pristine period is very short. Quite rapidly we form the chemical composition that we are more used to locally.

Reverend Barber. Following on, my original question follows neatly from that one. Any sign of these Population III stars?

Dr. Bowler. There have been several claims for Population III signatures in these high-redshift galaxies. One of the problems is that you can spend a long time staring at these things and not really get very much back in terms of information. There was a detection recently of He II emission. This is important because you have to have incredibly high-energy photons to excite this transition and only Population III stars can give you that transition. There was an observation of it, but another team reanalysed the data and the line disappeared, so this shows you how hard it is to do. So, as I say, not yet, but, especially with *James Webb*, we have high hopes.

Reverend Barber. Watch this space?

Dr. Bowler. Yes.

The President. Rebecca, thank you very much indeed. [Applause.] Our next speaker is Jennifer Jenkins from GFZ Potsdam, winner of the Keith Runcorn Prize, and she is going to talk about 'Understanding Iceland: imaging deep-Earth processes from mantle to crust'. And Jennifer, we are looking forward to your pronunciation of the names of all the Icelandic volcanoes [laughter].

Dr. Jennifer Jenkins. Iceland sits astride a mid-ocean ridge (MOR) where two of the Earth's tectonic plates move apart and magma upwells in-between to form new oceanic crust. In Iceland the normally submarine MOR is raised above sea level by an underlying mantle plume — a hot convective upwelling within the convecting system of the Earth's mantle. What kind of crustal structure forms when these two geological phenomena interact? Could a detailed understanding of the plume beneath Iceland teach us something about convection within the mantle? These questions motivated my PhD research where I used earthquakes as imaging tools to observe the deep structure of the Earth beneath Iceland.

Earthquakes are often thought of as destructive natural events that disrupt daily life; however, they can also teach us about the internal structure of our planet. When large earthquakes occur seismic waves spread outwards from the epicentre in all directions, both as damaging surface waves and downwards through the Earth as body waves. Seismic waves that travel within the Earth are P (compressional) and S (transverse) waves. My work looks at how these waves interact with sharp changes of seismic velocity called seismic discontinuities.

The most obvious example of a seismic discontinuity is the core–mantle boundary, where we transition from a solid-rock mantle to a liquid-metal core. The mantle also contains notable seismic discontinuities. Examples of relevance to my work are the Mohorovičič discontinuity or Moho (the crust–mantle boundary) and the transition zone (TZ) discontinuities (410–660 km depth). Seismic imaging of discontinuities measures presence (or lack thereof), depth, impedence contrast, and sharpness. These factors vary with temperature, pressure, and the chemical composition of the mantle. Thus by imaging discontinuities, we can indirectly image mantle plumes, based on the effect they have on surrounding material.

When P waves hit a seismic discontinuity, they can also be converted into S waves. Since P waves travel faster than S waves, the first arrival we see is a P wave, followed by later arrivals from P-to-S converted waves (called Pds phases where d is the depth of the causative discontinuity). By measuring the differential time between P and Pds arrivals we can estimate the depth of the discontinuity that caused them, either assuming we know the velocity structure and converting time delays to depths, or by inverting for a model of velocity structure that can fit the waveforms we observe.

The work I carried out during my PhD used Pds phases to investigate the Iceland mantle plume and its effects over a range of depths, from surface to mid-mantle (~1500 km). So let me take you on a tour of Earth structure beneath Iceland, and show you what we can learn from it. We'll start with the crust.

We have a reasonably good understanding of how the Earth's crust is formed under normal MOR settings. But what happens when we add in the influence of an underlying plume? Iceland is the perfect setting to investigate this question, thanks to the large number of seismometers (instruments that record earthquakes) there. The crustal structure of Iceland has been studied since the 1960s, and there has been an on-going debate on how thick it is, with estimates ranging from as thin as 15 km to as thick as 40 km. So which is correct? And more interestingly why have different studies come up with such varied measurements?

Using seismometers all over Iceland, we made measurements of Pds crustal phases, and inverted them for models of the velocity structure throughout Iceland. We expected to see one major discontinuity representing the Moho (the base of the crust). However, we found that in some places there were two discontinuities. We imaged a consistent layer of about 20-km thickness with a wedge of additional material stuck on the bottom, which was thickest in central Iceland (at 45-km depth) right above where the underlying plume is thought to sit.

So we finally understand why previous estimates of crustal thickness have been so variable: some studies imaged the shallower discontinuity, while others imaged the deeper. Working with geochemists we looked at the different chemical compositions of lavas erupted at the surface. Careful modelling revealed that where there are melt sources of two different compositions (shallow MOR melting and deeper plume-induced melting), the sequences of rocks that crystallize out of this melt combination can form a two-discontinuity structure similar to what we observe in central Iceland. So the presence of one or two discontinuities reflects where the influence of extra melt generated from the underlying plume feeds into building the crust.

The mantle is predominantly made up of a mineral called olivine (~ 60%), a magnesium silicate. Olivine undergoes polymorphic phase transitions: under certain pressure–temperature conditions its crystal structure rearranges into higher-pressure forms. At normal temperatures within the Earth this happens at 410-km depth (where olivine transforms into spinel) and at 660-km depth (where ringwoodite breaks down into perovskite). This causes sharp changes in seismic velocity as the mantle becomes suddenly denser, forming what is known as the transition-zone discontinuities. The 410-km and 660-km discontinuities react in opposite ways to temperature: in hotter conditions the 410-km one occurs at deeper depths, while the 660-km one occurs at shallower depths, thinning the transition-zone (TZ). Thus for a long time measuring TZ thickness has been used as a thermometer to measure temperatures in the upper mantle.

Taking data from Iceland and the surrounding North Atlantic region, I measured Pds phases from the TZ discontinuities, with the hope of mapping a thinning of the TZ that could locate the Iceland plume at depth. However, what I found was that under Iceland the 410 was deeper than normal (as expected), but the 660 was also deeper. Careful testing proved this was a valid result, leading us to question what was happening at 660 km, as the observed behaviour could not be explained by the olivine transition normally assumed to be the controlling factor.

Garnet, another major mineralogical component of the mantle, also undergoes a phase transition at around 660 km. Importantly the garnet phase change has the opposite effect to temperature as the olivine phase change — it gets deeper in hot scenarios. It seems in Iceland the behaviour of the 660 discontinuity is controlled by garnet, not the normally assumed olivine transition.

This has important implications. The garnet transition is predicted to become dominant only in very hot conditions, indicating that Iceland may be a particularly hot plume. A garnet transition would promote upwelling while an olivine transition impedes it. But most importantly the long-lived assumption that TZ thickness can be used as a mantle thermometer is no longer valid, since a garnet transition would not cause significant thinning but still represents high temperatures.

Not many people study the mid-mantle because it's generally assumed to be pretty boring. The Earth's mantle has been convecting for billions of years, so we expect the middle to be well mixed and homogeneous, with interesting heterogeneous regions restricted to the thermal boundary layers at the top and bottom of the system. However, while studying Iceland I noticed some unusual signals coming from 1000-km depth, indicating the presence of a mid-mantle discontinuity. This is an intriguing observation as we don't know of anything in this depth range that would cause such a feature. Working with collaborators, we extended the study area across Europe to look on a large scale for more of these strange signals.

We found several patchy observations of a 1000-km discontinuity stretching from Iceland south-eastwards into central Europe. Analysing the response of these seismic arrivals across different frequencies allowed us to determine that the causative feature was a sharp boundary occurring over < 8 km. Newly developed global seismic-tomography models mapping the large-scale velocity structure in the Earth had recently suggested that the Iceland mantle plume has a diverted branch at 1000-km depth, and we found that our small-scale discontinuities aligned along the top of this diverted material.

So what was causing a 1000-km-deep discontinuity? Were we imaging the velocity change moving into the slow velocities of the diverted plume? This couldn't be the case as this would be a decrease in velocity with depth, and our observations suggested an increase. We also expect thermal boundaries to be diffuse in nature, while our observations require a sharp boundary. Was it a phase change? The sharp nature of the boundary would suggest so, but there are no known phase changes in a normal mantle composition that we know of at that depth. This suggests we require a change in composition within the mantle to explain these observations.

This is an exciting conclusion, as it indicates that mantle plumes may not just be thermal in nature, but also of a different composition to the rest of the mantle. Where can we source this heterogeneous composition? When oceanic crust is subducted back into the mantle, maybe it isn't as well mixed in as we thought, and somehow becomes concentrated in plumes. We also know there are strange structures sitting on the core–mantle boundary, which we don't fully understand — could upwelling plumes be sampling this strange material? These are still open questions, but there is an obvious conclusion here: the boring mid-mantle is clearly not as homogeneous as we previously thought.

On this journey through the Earth beneath Iceland looking at the underlying mantle plume, I have told you about several exciting and unexpected observations, and the consequences they have on some key assumptions that no longer appear to be valid. I have told you about the unusual two-layered structure of the crust in Iceland, caused by additional melt generated by the underlying plume, interacting with the normal mid-ocean-ridge melting. This neatly explains why previous studies have come to such widely different estimates of crustal thickness. In the transition zone we saw that the 660-km discontinuity is controlled by garnet rather than the normally assumed olivine phase change, which indicates that Iceland may be a particularly hot plume. It also shows that measuring transition thickness cannot necessarily be assumed to represent directly temperature variation as previously thought. Finally, I showed evidence that the Iceland plume may be thermo-chemical in nature, though where this compositional difference comes from remains an open research question. This implies the mid-mantle is not so homogeneous and well mixed as has previously been assumed.

The President. Thank you very much indeed for a most interesting talk.

Professor I. Crawford. Is this the first time such a thing has been observed in the plume?

Dr. Jenkins. No, it is not the first time. When I first started seeing these signals I did a big search of the literature to see if anyone had found them before and they have been seen in other places. They are usually tagged on the end of a paper that said "we saw this strange thing". They are often found in subduction zones. That is not surprising because there you have got a source of heterogeneity down into the mantle, so that makes sense. But a couple have been seen in plume locations, so I think this is a new feature that we need to start specifically looking for, rather than saying it is there as well.

Professor Grady. If you have this wider feature in the mid-mantle, could this have implications for prediction of earthquakes that we haven't looked at in the past?

Dr. Jenkins. Probably not. The reason for that is that we think the mantle is mechanically decoupled from the crust, and earthquakes are only generated in the crust because it is brittle, whereas the mantle is ductile, so it (usually) can't host earthquakes. So, probably not. The real interesting question there is why is it diverted at that depth. There is nothing that we know of that should cause that to happen, so there is something else that we do not understand at the moment about mantle structure.

Mr. M. Hepburn. My background is in physical chemistry. As soon as you put up that particular compound then I think we know that olivine is not a good space-filler. It is not stable at the sort of depths that you are complaining ought to be part of transitions in magnesium silicate. It can't exist down there because at that depth only spinel would be present. It seems to me that you have come up with something that all the theorists have simply ignored because they don't pay attention to large-scale thermodynamics. We know from meteorite work that you will only find these beautiful crystals of Mg_2SiO_4 in rather small bodies. As soon as you get larger ones, then you get what they call the mesosiderites where you have that sort of meteorite that contains both iron and rock. And it is like that because at that depth we know from the Widmanstätten patterns that these are deeper and older things. It cannot exist as a separate compound, therefore it seems to me that everything you said is really a challenge to a whole lot of cosily accepted nonsense.

The President. He is being nice.

Mr. Hepburn. The mantle is supposed to be mainly magnesium silicate. It is a much more complex stucture than that.

The President. Let Jennifer answer, please.

Dr. Jenkins. I will start by saying I am an observational seismologist and not a geochemist and I am sure you know a lot more about the chemical structure than I do. But, as you said, this is the widely accepted explanation for the transition-zone discontinuities which are globally seen and the way that they react to temperature in normal circumstances really seems to fit with what we understand about the transitions in the olivine system. Maybe it is more complicated than we previously thought, but it is not something I know about enough to disagree with you.

The President. Let us thank Jennifer again for a super talk [applause]. May I remind you that our drinks reception will take place immediately after the meeting in the library of the RAS. And finally, I give notice that the next monthly Open Meeting will be on Friday the 8th of March.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2019 March 8 at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

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

The President. One of the nice things about standing here is that I get to give people some very well-deserved prizes. You may have seen in the library outside some posters for our GCSE competition. I will first call Natalie Shapiro of JFS School whose poster was entitled 'Active galactic nuclei and the Unified Model' [applause]. The second prize goes to Benjamin Andrew of St. Johns College Cardiff for 'Ice volcanoes in the Solar System' [applause]. The first-prize winner was Jack Forrest from Torquay Boys Grammar School and his title was 'Breakthrough Starshot — approaching light speed' [applause]. If the posters are still up there at the end of the meeting I hope that you'll look at them on the way out.

It now gives me great pleasure to introduce our Eddington Lecture speaker, Professor Amanda Karakas of Monash University, who is going to talk about 'Heavy elements in red giant stars'.

Professor Amanda Karakas. [It is expected that a summary of this talk will appear in a future issue of *Astronomy & Geophysics*.]

The President. Thank you very much indeed. That was very interesting. Open for questions.

Dr. P. Allen. I enjoyed that. I was struck by the mass-fraction curve — the way it just drops off a cliff above lead. Previous magic elements went down but not so rapidly, so what is going on?

Professor Karakas. These elements up to here are radioactive. There is a bit of a stable point around thorium and uranium. Everything in between lead and thorium has a much shorter half-life.

Dr. Allen. So are they being made?

Professor Karakas. They would be made. Even though it is called the solar abundance distribution, in reality it is the Solar System distribution and those elements are found in old meteorites. We determine the composition from those old meteorites in the lab. You can then extrapolate backwards and say that is what the composition must have been like at $t = 0$, the epoch of Solar System formation. There is a little bit of wriggle room there.

Professor N. W. Evans. The Large Magellanic Cloud is clearly an unusual environment because it is interacting with the Small Magellanic Cloud and the Milky Way, and material is being exchanged between these systems, so how certain are you that the conclusion you drew for the post-asymptotic-giant branch stars of the LMC is actually a more general conclusion for post-AGB stars in the Galaxy?

Professor Karakas. That's a really good question. People have done a study and when they found one or two with low lead, they thought that it was unusual. They had a sample of quite a few post-AGB stars that vary as a function of metallicity and they also had some in our Galaxy as well. So, including the Galactic sample, in terms of [Fe/H], they go from –0·3 to about –1·5. They weren't worried about the Galactic environment — they were just looking at what the initial iron abundance was. That low lead level seems to drop off below –0·7 so it seems to be more a function of the star's metallicity than its environment and regardless of where it comes from.

The President. Time for one last question.

Professor D. W. Kurtz. You had a note on one of your slides stating that technetium was observed in stars. Could you tell me what the evidence is and how robust do you think that is?

Professor Karakas. It's very robust. The first measures of Tc were made in 1952 by Merrill. He did not follow up his *ApJ* paper with one in *Nature*, as would be done today. Since then we have routinely measured technetium lines in stellar atmospheres. We measure the low-mass evolved red giants and long-period variable stars such as Mira. These have long lifetimes, longer than the longest half-life of any isotope of technetium. I forgot to mention that the slow-neutron-capture path moves through Tc^{99} as well, so there is a nice convergence there between the theory and calculations and what we observe in stars. When they were first measured, there were a lot of explanations raised. Maybe it was an atmospheric thing but now we're seeing model atmospheres so we think it's quite robust.

Professor Kurtz. Do you see prometheum?

Professor Karakas. We don't see prometheum. It has a very short half-life.

The President. Let's thank Amanda again [applause]. The next speaker this evening is Dr. Emma Chapman from Imperial College London, who is a Research Fellow and who is going to talk about 'Observing the first stars'.

Dr. Emma Chapman. [No summary had been received at the time of going to press.]

The President. Thank you very much. Open for questions.

Reverend G. Barber. What happens to the Population III stars when they end their lives? Is there a kind of hyper-nova? Might it be possible to observe such transients?

Dr. Chapman. They are called pair-instability supernovae, which I'm not an expert in, but they are incredibly bright objects and the hope is that if you get one of those supernovae behind a very large gravitational lens with a large galaxy in front of it then it's possible that the *James Webb Space Telescope* might be able to observe them. I believe that the rate is still very low -– possibly of the order of tens per observing season. It is not an optimistic outlook for observing such objects.

Professor Lyndsay Fletcher. What is it that underlies the original assumption that your foreground should be skewed?

Dr. Chapman. Galactic synchrotron radiation is being observed a lot, and the physical processes behind it do indeed follow a power law very nicely — the same applies to Galactic free–free emission. When you look at extragalactic sources, even though they have lots of different power laws, when you get a sum of such power laws together, you still get a power law. The underlying assumption is sound, based on those foregrounds that we know, but we always have unknown unknowns. The big one we missed, early on in this field, is how the instrument would deal with the foregrounds as the instrument changes sensitivity over frequency: we did not realize quite how it would interact with these very brightest point sources which throw up the power into the window. The hope is that, if you know your instrument really well, then you would be able to model that effect very well and still be able to use your polynomial removal methods, for example. What these blind methods do is that they provide a confirmation where, when you are talking about low-signal-to-noise experiments, I would not personally believe any experiment based on one method. You have to use other complementary ones.

The President. Thank you very much. The next speaker is Professor Wyn Evans from the Institute of Astronomy in Cambridge. His talk is entitled 'The *Gaia* sausage — the head-on merger that swallowed our galaxy'.

Professor N. W. Evans. [The speaker began by briefly describing the mission of the *Gaia* satellite which was launched in 2013. It repeatedly scans areas of the sky recording the positions of stars. In 2018 April the second data release (DR2) was issued and it consists of measurements of position, proper motion, parallax, and brightness for more than one billion stars and, in addition, includes the radial velocities of about 7 million stars (a smaller number due to the brighter limiting magnitude for spectroscopy). This data can be cross-matched with spectroscopic surveys to yield full velocity-space data.

Two results of DR2 were briefly mentioned. By selecting objects from this catalogue with no proper motion and parallax, and cross-matching these data with those from the infrared *WISE* satellite, a significant number of quasars were discovered, and this has revealed new gravitational lenses — these have been found at the rate of about one every few days, greatly adding to our knowledge of those objects. Secondly, using data that is publicly available, a feature called the '*Gaia* snail' was found. Looking at nearby disc stars by plotting the height above or below the Galactic plane *versus* vertical velocity a 'snail' is seen in the distribution. This is produced because there is a correlation between the amplitude of motion of these stars, the vertical frequency, and their angular momentum. When something passes through the Galactic disc the stars respond and the resulting pattern that we see is a *Gaia* snail.

The *Gaia* sausage, on the other hand, is a structure in this part of the halo of our Galaxy. Here interest lies in the mass, shape, and clumpiness which has been built-up from many smaller objects. The aim is to know the accretion history of our Galaxy, because it tells us about the dark-matter particles and the coupling of baryons to dark matter, and therefore it is concerned with the first stars, the first galaxies, and the nature of gravity itself.

Stars in the halo have a velocity relative to the centre of the Galaxy in the x, y, and z directions. The anisotropy parameter β, which is related to the velocity dispersions in the radial, polar, and azimuthal directions, is defined as being zero if the velocities of stars are mostly isotropic. If the stars are mainly moving on eccentric orbits then $\beta > 0$, and if the orbits are mainly tangential or circular then $\beta < 0$. Suppose that there is an isotropic distribution of stars, then we see a sphere in velocity space. If the distribution is tangentially isotropic then the shape in velocity space is a burger. To get a sausage we require a highly radially-anisotropic distribution of stars moving on radial orbits. The *Gaia* sausage is a sausage in velocity space. If we cross-match *Gaia* data with those from the *SDSS* spectroscopic survey we have not only proper motions but also radial velocities and we look at main-sequence stars centred on the Sun.

One may plot stellar metallicity *versus* Galactic height for metal-poor stars in the stellar halo and the velocity distribution is round and isotropic. It is built up from lots of small galaxies merging a long time ago and the velocities of those stars have been isotropized. Looking at slightly more metal-rich stars in the stellar halo, the distribution in velocity space appears like a sausage. If β is plotted against metallicity as we move from metal-poor ($\beta \sim 0$) to metal-rich stars in the halo then there is a strong jump in β. How can such an extreme distribution of velocities, *i.e.*, moving on exactly radial orbits, be produced? The answer is that it is due to the impact of a large object, which came in on a radial orbit, in other words a head-on collision between a sausage-shaped galaxy and the Milky Way about eight to ten billion years ago. This calculation has only been possible due to the precise proper motions given in DR2.

The speaker has been doing a suite of simulations of the formation of the Milky Way galaxy using a computer program called AURIGA which has been developed by the University of Durham. We can decompose stars in the stellar halo into different velocity distributions and see if we can match what we see in the data, *i.e.*, strong radial anisotropy ($\beta = 0{\cdot}9 - 1$) and the contribution of these stars to the stellar halo is between 60 and 100%, so we are looking for simulation of signs in the build-up of structure in the suite of Milky Ways, and indeed we do find some. These suggest a collision with an object of about 10^9 solar masses about six to eight billion years ago. The collider was a big galaxy (larger than the Sagittarius galaxy) so we can look for globular clusters associated with that object. They should be on extremely eccentric orbits and indeed we do see a bunch of globular clusters on highly eccentric orbits which we believe belonged to the sausage galaxy. They also form a track in the age–metallicity plane distinct from the bulk of the Milky Way globulars, to suggest that they had indeed been treated. We can also look at the RR Lyrae distribution from the *Gaia* data. We see a tri-axial structure, *i.e.*, a sausage both in configuration space and velocity space. If the stellar mode is bi-modal then the dark matter is also bi-modal. There must have been a slew of dark matter brought in by the sausage galaxy. If the dark matter is a neutralino it will self-annihilate into gamma rays and there will be a caustic in the galaxy corresponding to the location of pericentres of the dark-matter particles that were brought in by the sausage.

There are still lots of outstanding questions. Was this merger responsible for the formation of the Galactic disc? If there was a pre-existing disc, what happened when this big object came in? How much dark matter has this event contributed? Did the progenitor bring in an entourage of smaller satellites? If the galaxy was as big as this work indicates then it too would have had a black hole in the centre, which would have merged with that at the centre of the Milky Way; if so, when did that happen? The speaker concluded by saying that the first *Gaia* data release was relatively small, but the second one was the revolutionary moment.]

The President. Open for questions on sausages [laughter].

Professor J. Zarnecki. That was a fantastic advert for *Gaia*. Looking ahead, I believe that Data Release 3, whilst not imminent, is at least in the calendar. I wonder what we might be looking forward to in that. Is it a matter of expanding on these already incredible numbers, or will there be a further qualitative quantum leap?

Professor Evans. That's a good question. For single stars, I think the revolutionary moment was 2018 April 25. For the variable stars, doubles, and binaries, that revolutionary moment is yet to come. DR3 will provide us with those riches, so I agree that the revolution eats itself, and there will be another revolution to come.

The President. Any other questions or comments? You've satisfied people's appetites about sausages! [Laughter.] Wyn, thank you very much indeed [applause]. May I remind you that our drinks party is in the RAS Library immediately after the end of this meeting and I give notice that the next monthly A and G Open Meeting of the Society will be on Friday, the 12th of April.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2019 April 12 at 16^{h} 00^{m}
in the Geological Society Lecture Theatre, Burlington House

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

The President. Good afternoon ladies and gentlemen, and welcome to our monthly A&G Open Meeting. I am going to begin by introducing Dr. Ciarán Beggan from the British Geological Survey, and his title is 'From raspberries to aurorae; building a schools' magnetometer network in the UK'.

Dr. C. Beggan. Most people are familiar with compasses as used for navigation when out hill walking or for boats and airplanes. Modern-day smartphone users will typically carry one around in their pocket, often without realizing that they are measuring the Earth's magnetic-field vector. Consumer compasses measure the Earth's magnetic field to a rather poor level of accuracy. With the advent of electronics in the 1970s, magnetometers became much more accurate, though in essence they are just sophisticated compasses.

The Earth's field is composed of the sum of a number of sources. The dominant source is the internal field from the liquid outer core (with a strength of 50 000 nanoTesla (nT) at the Earth's surface). This is generated by the geodynamo which converts motion of liquid iron/nickel into electrical currents and hence magnetic fields. At the Earth's surface, there are additional external magnetic fields generated by currents flowing in the upper atmosphere and near-Earth space. These ionospheric and magnetospheric fields are around typically 1% the magnitude of the main field, around 50 nT on quiet days, but can rise to over 1000 nT during a geomagnetic storm. In contrast to the internal field, they vary rapidly over seconds to days compared to the core field which changes over years to millennia. As an aside, we note that the geomagnetic field is actually extremely weak — a fridge magnet is several orders of magnitude stronger.

The aurora, or northern lights, always fascinate though they are commonly believed not to occur too often in the UK. However, they are typically visible once per month on average if you live in the Shetland Islands and perhaps once per year in southern Scotland/northern England. At the latitude of London, they appear only once per decade. Additionally, they are often obscured by cloud. The northern lights are formed by the flow of electrical currents in the ionosphere at heights of 100 km and thus generate a detectable magnetic-field signature. They are driven by the buffeting of the solar wind on the Earth's magnetic field, particularly during geomagnetic storms. In the past decade it has become possible to build a relatively low-cost instrument using a Raspberry Pi computer with small fluxgate magnetometers to detect the aurora.

At a scientific observatory the absolute strength and direction of the magnetic-field vector is measured. Great care is taken to ensure the magnetic cleanliness and stability of the site in order to maintain an accuracy of 0·01 nT or better; such high-quality instrumentation costs around £15 000 to purchase (at current prices) and is beyond the reach of non-professionals. In contrast, a Raspberry Pi magnetometer costs around one per cent of the price (around £150) but its accuracy is also two orders of magnitude lower (at 1 nT). However, this is sufficient to detect variations of the external magnetic field, particularly during geomagnetic storms when the aurorae are visible.

After winning some initial funding from the Natural Environmental Research Council in 2014, we built a prototype magnetometer using three small fluxgate magnetometers connected to an analogue-to-digital convertor which was controlled by the Raspberry Pi computer. This prototype was installed in Edinburgh and its data were compared with those from the scientific sensors at Eskdalemuir geomagnetic observatory, around 70 km to the south. By good fortune, a geomagnetic storm occurred which allowed us to demonstrate that the low-cost magnetometer measured the rapidly-varying external field to a similar level of accuracy as the scientific sensor. From this experiment we also noted that the sensors were very sensitive to temperature changes, which can masquerade as changes in the magnetic field.

Buoyed by this achievement, in 2015 we applied for an STFC Small Public Engagement grant to build ten systems for schools around the UK. The updated systems included a temperature sensor to measure change and allow us to account for the effect on the magnetic data through calibration. We tested the ten systems at Eskdalemuir and compared them to the scientific data to confirm our previous findings. Again, we found the accuracy to be around 1 nT and that the systems could reliably detect the magnetic signature of the aurora.

We attended Institute of Physics workshops for teachers and asked colleagues for local contacts in schools in order to spread the magnetometer systems around the UK. We managed to place them in schools from Benbecula to Eastbourne. Sophisticated back-end-collection software was written which pushed the magnetic-field data collected to Lancaster University's AuroraWatch website. There the data are stored and made publicly available for viewing in a web browser and/or download for research purposes.

One of the goals of the project was to test if the Raspberry Pi magnetometer data were useful for understanding the spatial and temporal variation of the magnetic field across the UK. The UK has only three permanent geomagnetic observatories (in the Shetland Islands, the Scottish Borders, and Devon) separated by around 500 km. Filling in the 'gaps' between the observatories would allow us to examine the smaller scale (< 500 km) variations of the field.

This opportunity arose on 2017 September 7–8 when the largest geomagnetic storm of the present solar cycle occurred. We collected freely available data captured by magnetic observatories in the UK, Ireland, and around the North Sea, which we augmented with data from the Raspberry Pi magnetometers in Benbecula, Glasgow, Peterborough, and Norwich. Using the magnetic-variation data we created maps of the spatial and temporal variation of the geomagnetic storm during its most active phase around midnight. This showed new detail in the motion of the aurora southward, including indentations in the leading edge and westward-travelling vortices. Such information is very useful in a scientific sense for understanding the potential impacts of severe space-weather events on grounded technology such as the high-voltage power network. The finer-resolution network of magnetometers allows us to model such effects better in future.

The Raspberry Pi magnetometer project is thus a combination of an outreach project and a citizen-science experiment and has been quite successful on both accounts. We wish to thank Dr. Steve Marple at Lancaster University and acknowledge the help of the BGS Geomagnetism engineers Ted Harris, Tony Swan, and Tim Taylor who helped to build the software and the systems.

The President. Thank you very much indeed. The talk is now open for questions or comments.

Mr. H. Regnart. It is a wonderfully brilliant project; bringing science to children and children to science, and I hope the principal people involved can be awarded a prize for it.

Dr. S. Mitton. I've got a history-of-science comment, which is that in 1692, Edmund Halley published a paper in the *Philosophical Transactions* about the magnetic variations in the north and south of the Atlantic and he did some modelling from that. A striking feature of the paper is right towards the end, where he says that he hopes master mariners will follow his instructions for measuring magnetic variations at sea and that they will send their observations in to the Royal Society when they return from their voyages. I don't think that ever happened, but as far as I can tell it was the first attempt to set up a magnetometer network.

Dr. Beggan. Very good. A lot of those measurements were collected by Andy Jackson in his 2000 paper looking at the historical magnetic field, so somebody else had to do the work.

Dr. P. Wheat. Is the current pole wandering? I know it doesn't make a lot of difference in terms of our variation here at the moment but is that causing any significant changes in the aurora that's happening?

Dr. Beggan. Yes, it is a good question. The pole has been moving very rapidly over the last 20 years or so; it has moved from Northern Canada across towards Siberia and, yes, that will have an effect on the location of the aurora. At the moment, I think it is dragging it towards Russia, away from, say, northern Canada and North America, but it's not really dragging it towards us if that is the question.

Dr. Wheat. Is it changing the magnitude as well or is it just where it's happening?

Dr. Beggan. I'd have to refer you to the literature for that one. There has been a paper out recently about it. And one of my students, Ashley Smith at the University of Edinburgh, did write a paper about this a couple of years ago. I think we don't know if it is changing the magnitude, but it is definitely changing the position. As to whether it is changing the magnitude, we don't have long enough records from, say, satellites to make a good estimate of that.

The President. Last question now.

Dr. Jena Goldsteine. It was quite interesting to see the graph of the temperature and also all the magnetic fields. What is interesting to know is whether the temperature variation has any effect on the magnetic field and if so, what is the effect on it?

Dr. Beggan. The temperature effect is actually on the sensor because they're quite small, and as they are made of metal — so that they might be shortened — that changes their response to the magnetic field and changes the calibration. You can account for temperature changes if you've measured it, which a lot of people do — you try to calibrate that out; but it's not perfect. It also varies between different sensors so you have to do it for all of them individually.

Dr. Goldsteine. There is a lot of gossip going round that the magnetic field on this planet is getting weaker. I do not know whether scientifically it is true or not. If it is true, is it related to the fact that this planet is getting warmer, day by day, or year by year?

Dr. Beggan. It is nothing to do with climate change because the main field comes from the core which is completely isolated from the surface of the Earth. The field is weakening but if you look historically at, say, the last 10 000 years, it just varies with strength, it goes up and down, so actually at the moment, it's about average for the last 10 000 years. The last 3000 years it's been stronger; prior to that it was a bit weaker. So the strength itself varies, and the position of the magnetic pole changes all the time. There is no reason to think we're going into a reversal actually, if that's the question, and no, there's no link between the surface temperature and the outer core. The outer core is at 3000 K, so it doesn't matter if Earth's surface varies by one or two degrees as far as it's concerned.

The President. Let's thank Dr. Beggan again. [Applause.]

The President. Our next speaker is Dr. Anne-Marie Broomhall from the University of Warwick, and her talk is entitled 'Is the Sun's magnetic-activity cycle in transition? Insights from helio- and astero-seismology'.

Dr. Anne-Marie Broomhall. Helioseismology uses the Sun's natural oscillations to observe beneath the visible surface of the Sun, down into its interior. The dominant oscillations in the Sun, which have periods of around five minutes, are sound waves and are referred to as *p* modes as their main restoring force is a pressure differential. Solar *p* modes can be observed either in Doppler velocity data, as the oscillations cause the surface and overlying atmosphere to move up and down, or in intensity data, since the oscillations cause small, but still highly significant variations in the Sun's brightness. At any one time there are thousands of acoustic oscillations propagating through the solar interior. These oscillations are trapped in different but overlapping regions inside the Sun, and so by examining their properties, such as their frequencies, and comparing to solar models, we can build up profiles of the solar interior. For example, using helioseismic observations we can learn about how the temperature and rotation of the solar interior vary with depth. The properties of *p* modes are also affected by magnetic fields inside the Sun, so following their variation with time allows us to track the Sun's magnetic-activity cycle.

The Sun's magnetic activity cycle varies on a time scale of around 11 years from minimum to maximum and back to minimum again. During the course of that approximately 11-year cycle the Sun's magnetic polarity reverses with the north pole becoming the south pole and *vice versa*. It therefore takes two 11-year 'Schwabe' cycles to make a complete 22-year 'Hale' cycle. It is worth noting here that that cycle length of 11 years is approximate: the length of the solar cycle varies from cycle to cycle between around 9 and 13 years. The strength of each cycle also fluctuates: the most recent cycle, solar cycle 24, was the weakest cycle seen in around 100 years and the upcoming cycle 25 is expected to be similar. The easiest way of observing the solar cycle is to track the number of sunspots (dark, strongly magnetized patches that appear on the Sun), and sunspot observations of the solar cycle go back centuries. In comparison helioseismology is a relatively new field, with coherent observations of the Sun now spanning around 40 years. The frequencies of helioseismic *p* modes increase systematically between cycle minimum and cycle maximum, tracking closely the increase in sunspot number and the 10·7-cm radio flux, another well-known proxy of solar activity. Since *p*-mode frequencies are probes of the solar interior, by comparing frequencies at different epochs we can learn about how the Sun's internal magnetic field varies. What's more is that since there are so many of them, *p* modes can be extremely sensitive probes of magnetic activity.

A good example of how their exquisite sensitivity can be useful comes when we compare the oscillation frequencies observed in the minima either side of cycle 23. The minimum following cycle 23 is often referred to as being 'unusually deep' and 'unusually long', and when saying this many refer to the abnormally large number of days where no sunspots at all were visible on the Sun. And while it is true that some measures of solar activity, such as the strength of the magnetic fields at the north and south poles, do show the minimum following cycle 23 was weaker than the minimum preceding cycle 23, other measures, such as the 10·7-cm flux, only show small differences. We asked how different the *p*-mode frequencies were between the two minima: the answer was significant, not in magnitude, but statistically speaking. Indeed, the difference in mode frequencies indicated that the global average magnetic field was 1 Gauss weaker during the minimum following cycle 23 than in the minimum preceding it. Perhaps more interesting was that the signature of the difference in magnetic-field strength in the *p*-mode frequencies was the same as is perceived when comparing mode frequencies observed at cycle minimum and maximum. This implies that even at cycle minimum, even when no sunspots are observed on the Sun, there is an underlying internal magnetic field that varies in strength from one cycle minimum to the next.

We now have enough helioseismic observations that we can also compare complete 11-year Schwabe cycles. For example, Rachel Howe looked at whether there is a cycle-to-cycle difference in the gradient of a plot of 10·7-cm flux against mode frequency. Again, a small but systematic and statistically significant difference was observed when comparing cycles 22, 23, and 24. Such a change implies that the atmosphere is responding differently to the internal magnetic field from one cycle to the next. However, the need for further data is clear. The data we have currently cover little more than one full Hale cycle and is not enough for us to determine whether this is a systematic long-term trend and evidence for the Sun's magnetic activity cycle transitioning. Neither can we answer the all-important question of what causes the different cycles to vary in length and strength. If we can answer that question, we will gain great insights into the workings of the Sun's magnetic dynamo, which is responsible for the Sun's magnetic field, and we will substantially improve our ability to predict long-term variability in space weather.

Of course, the Sun is just one star, and a fairly normal star at that. It has long been known that not only do other stars have magnetic fields, but that some also exhibit activity cycles like our own Sun's. Evidence from the Mount Wilson survey implies that the length of a star's activity cycle is proportional to the rotation period of the star, which in turn is known to increase as a star ages. The rate at which the length of a star's activity cycle increases with rotation does so along two main branches, one containing predominantly younger stars and the other containing older stars. Interestingly, the Sun lies right between the two main branches, leading to suggestions that the Sun could be transitioning from one branch to the other, although it is important to stress that measurements of cycle length (and to a lesser extent rotation rates) can be uncertain. As mentioned before, the length of our own Sun's activity cycle varies from cycle to cycle and so it is entirely possible that these two branches may not be as distinct as first impressions suggest.

With *CoROT*, *Kepler*, and now *TESS*, asteroseismic intensity data are available for literally thousands of stars. Furthermore, the nominal *Kepler* mission looked at the same field of view for around four years, opening up the possibility of observing variations in oscillation frequencies through stellar magnetic-activity cycles. Although no full cycles have been revealed, systematic variations in *p*-mode frequencies have been observed in a handful of main-sequence stars. On-going work can therefore, hopefully, reveal further details of the structure of the internal magnetic field in stars, and the impact it has on *p*-mode frequencies.

Studies of this kind have focussed on main-sequence stars since, as a star's rotation rate decreases with age, so too does its magnetic activity: old stars tend to be less active. Nevertheless, based on a number of assumptions and observations of main-sequence stars, René Kiefer and I made predictions over whether one might expect to be able to observe variations in the frequencies of older subgiants and red-giant stars. Not only did we predict that it may be possible to observe such changes in frequencies but that those frequency variations may even be larger than those observed in main-sequence stars. Based on those predictions we have, very recently, examined the *Kepler* catalogue of red-giant and subgiant stars and found over 1000 stars with statistically significant frequency variations, thus providing a whole new plethora of stars that we can use to study and understand the magnetic field of the Sun and stars.

The President. Thank you very much indeed. So, open for questions? Well, I have one. And that is, do these frequency shifts give you any idea of where the changes in the star are happening — in the core or in the surface?

Dr. Broomhall. Unfortunately, the acoustic modes are very much more sensitive to the near-surface region than the core because the sound speed in the core is very fast, so they don't spend very much time there. But that's why we're so excited about the possibility of red giants and subgiants because in those stars we can then start to see mixed modes, for example, which have much higher sensitivity to the core, and in fact it has been shown with some red giants and subgiants that there is a differential rotation between the core and the outer convection zones, so perhaps with these stars we might be able to get more information.

The President. Very good. Other questions or comments?

Miss Tishtrya Mehta. I was just wondering, in one of the graphs you showed that there was a relationship between the rotational period of the stars and age: you mentioned that it slows down over time and you mentioned very briefly some of the mechanisms. I was just hoping you could expand on what magnetic braking is?

Dr. Broomhall. Yes, if you think about what we know about the Sun and solar winds taking material away, as solar winds stream out they are associated with magnetic fields and that just adds a torque with time that slows the rotation.

The President. Any more questions? If not, then we thank you very much indeed. [Applause.]

The next speaker is Dr. Rebecca Smethurst from the University of Oxford who is going to be speaking on 'Quenching galaxies and the search for evidence of AGN feedback'.

Dr. Rebecca Smethurst. [No summary of this talk had been received at the time of going to press.]

The President. Thank you very much. We'd like, actually, some answers from the audience, but you can ask questions as well [laughter].

Professor C. Lintott. I wanted to ask about the status of quenching in simulations which you touched on. I think you were making the argument that the simulations predict AGN feedback as the cause of quenching. Is that actually true? I'd always assumed that the simulations needed something to do quenching to prevent getting too many massive galaxies, and AGN feedback was an easy way for simulators to do that, or are there things in the simulations that suggest it really is AGN feedback?

Dr. Smethurst. I think it's one of those things where if they don't have it, everything breaks down. I think if you considered each mechanism of the simulation one by one, for example, if you took AGN feedback out, that would be the one that had the most impact. People say that it's at the high-mass end — that's the one that's going to have the biggest effect. But I think they find that without the others, they also wouldn't get the same processes, and the other mechanisms come more naturally out of just gravitational effects and laws of physics rather than the AGN that have been put in empirically.

Mr. M. Hepburn. I was always taught that there are not two populations of red and blue galaxies but rather three, with about 1% of much bluer galaxies than the main sequence, and you haven't dealt with those at all. If you think of the analogy of how we worked out the development of stars, we started out with a reasonably satisfactory explanation as to why stars are the size they are. Now galaxies do form a very clear pattern of size and nobody seems to have any explanation whatsoever about that. And so, my question is here, that we know with stars that they start out, as it were in the middle, then they go to the red side and then they go to the much rarer and smaller blue sizes. There is no obvious reason why that should be the sequence. It seems to me that, before you can make realistic predictions about how galaxies evolve, you've got to start with a reasonable approach as to why they are the sizes and shapes that they are, and as far as I can see this question has simply has not been addressed.

Dr. Smethurst. The shapes of galaxies are important. It wasn't something I chose to focus on here but there are many people that are considering how these quenching methods tie in here. Your question is about starburst galaxies — which are the 1% that are much higher than the main sequence or the blue cloud. Obviously I am not someone who really cares about star formation. So for me the starbursting scenario comes in where they're going to use up their gas very quickly. In terms of the sizes of galaxies, or the sort of mass that they can reach, that's something that simulations are focussing on, and what they're finding is that AGN feedback is the limiting factor in how big they can get. Now obviously, in a galaxy you're looking on a global scale, whereas a star is something that is much more individual, so you can detect the individual pulsations of a star that move it around on the stellar colour–magnitude diagram. With a galaxy, it is a much more global scale and so when you observe a galaxy, at least in the Sloan Digital Sky Survey, it's one observation, so that very much averages what is going on across the whole galaxy and that's why you obviously don't see as many fluctuations and movement; that's why these Integral Field Unit studies with *MaNGA*, where you obtain spectra of the galaxy, are so crucial because they're really going to help us understand these things a lot better.

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

Well we've done the Earth, with its magnetic field, we've done stars, we've done galaxies, so now we're going to have a little bit of history from Professor Peter Coles from Maynooth University on 'Light all askew in the Heavens: the 1919 Eclipse Expeditions'.

Professor P. Coles. On 1919 May 29 there was a total eclipse of the Sun during which measurements were made that changed the course of science by verifying a key prediction of Einstein's theory of General Relativity. Many people think that 1905 — the year in which he published his theory of relativity — was the year that Einstein become a famous public figure, but this is not the case. It was the 1919 eclipse that did it: in the *New York Times* annual index of proper names there is no mention of Albert Einstein at all before 1919, and after 1919 there is no year in which his name does not appear.

The May meeting, being the AGM rather than an Ordinary Open Meeting, is the nearest occasion to the centenary on which it is possible to mark this momentous event, so in this short talk I want to describe briefly the experiments and why the results had such a profound effect on Einstein's theory and on Einstein the cultural icon.

For over two centuries before 1919, scientists had believed Sir Isaac Newton's view of the Universe. Then his ideas had been challenged by a young German-Swiss scientist, called Albert Einstein. The showdown — Newton *versus* Einstein — would be the total eclipse of 1919 May 29. Newton's position was set out in his monumental *Philosophiae Naturalis Principia Mathematica*, published in 1687. *The Principia*, as it's familiarly known, laid down a set of mathematical laws that described all forms of motion in the Universe. These rules applied as much to the motion of planets around the Sun as to more mundane objects like apples falling from trees.

At the heart of Newton's concept of the Universe were his ideas about space and time. Space was inflexible, laid out in a way that had been described by the ancient Greek mathematician Euclid in his laws of geometry. To Newton, space was the immovable and unyielding stage on which bodies acted out their motions. Time was also absolute, ticking away inexorably at the same rate for everyone in the Universe. Following that concept, scientists saw the cosmos as a vast clockwork machine, evolving by predetermined rules through regular space, against the beat of an absolute clock. This edifice totally dominated scientific thought, until it was challenged by Albert Einstein.

In 1905, Einstein had introduced new ideas that dispensed with Newton's absolute nature of space and time. Although born in Germany, during this period of his life he was working as a patent clerk in Berne, Switzerland. He encapsulated his new ideas on motion, space, and time in his special theory of relativity. But it took another ten years for Einstein to work out the full consequences of his ideas, including gravity. The general theory of relativity, first aired in 1915, was as complete a description of motion as Newton had prescribed in his *Principia*. But Einstein's description of gravity required space to be curved. Whereas for Newton space was an inflexible backdrop, for Einstein it had to bend and flex in response to the presence of massive bodies. This warping of space, in turn, would be responsible for guiding objects such as planets along their orbits.

By the time he developed his general theory, Einstein was back in Germany, working in Berlin, but a copy of his theory of General Relativity was soon smuggled through war-torn Europe to Cambridge. There it was read by Arthur Stanley Eddington, Britain's leading astrophysicist of the time, and the Astronomer Royal, Frank Watson Dyson. Eddington and Dyson realized that Einstein's theory could be tested. If space really was distorted by gravity, then light passing through it would not travel in a straight line, but would follow a curved path. The stronger the force of gravity, the more the light would be bent. The bending would be largest for light passing very close to a very massive body, such as the Sun.

Unfortunately, the most massive objects known to astronomers at the time were also very luminous. This was before black holes were seriously considered, and stars provided the strongest gravitational fields known. The Sun was particularly useful, being a star right on our doorstep. But it is impossible to see how the light from faint background stars might be bent by the Sun's gravity, because the Sun's light is so bright it completely swamps the light from objects beyond it.

Eddington and Dyson realized the solution: make observations during a total eclipse, when the Sun's light is blotted out for a few minutes, and you can see distant stars that appear close to the Sun in the sky. If Einstein was right, the Sun's gravity would shift these stars to slightly different positions, compared to where they are seen in the night sky at other times of the year when the Sun is far away from them. The closer the star appears to the Sun during totality, the bigger the shift would be. Much earlier, in 1804, using Newtonian ideas, Johann Georg von Soldner calculated the effect of light bending, but he had got a result a factor of two smaller than Einstein's theory. Here was a potentially crucial test of Einstein's theory.

Dyson realized that the 1919 eclipse would be ideal for undertaking this experiment. Not only was totality unusually long (around six minutes, compared with the two minutes we experienced in 1999) but during totality the Sun would be right in front of the Hyades, a cluster of bright stars in the constellation of Taurus. But Eddington was a Quaker and, as such, a pacifist. In 1916 the British government had introduced conscription to the armed forces. Eddington refused the draft and was threatened with imprisonment. In the end, Dyson's intervention was crucial in persuading the government to spare Eddington. His conscription was postponed under the condition that, if the war had finished by 1919, Eddington himself would lead an expedition to measure the bending of light by the Sun. The rest, as they say, is history.

The path of totality of the 1919 eclipse passed from northern Brazil, across the Atlantic Ocean to West Africa. In case of bad weather (amongst other reasons) two expeditions were organized: one to Sobral, in Brazil, and the other to the island of Príncipe, in the Gulf of Guinea close to the West African coast. Eddington himself went to Príncipe; the expedition to Sobral was led by Andrew Crommelin from the Royal Observatory at Greenwich.

Given my current affiliation I have to mention the strong Irish connection.

The two main telescopes (one to Sobral and one to Príncipe) were both equipped with astrographic object glasses made by the Grubb Telescope Company in Dublin, as was a smaller object glass taken to Sobral as a back-up. After the expeditions, both main lenses were returned to their original locations at the Royal Observatory and Oxford University Observatory, respectively. The smaller lens (and the coelostat used for the measurement) are normally on display at Dunsink Observatory, just outside Dublin, but are heading to Sobral for the centenary celebrations.

The expeditions did not go entirely according to plan. When the day of the eclipse (May 29) dawned on Príncipe, Eddington was greeted with a thunderstorm and torrential rain. By mid-afternoon the skies had partly cleared and he took some pictures through cloud.

Meanwhile, at Sobral, Crommelin had much better weather, but he had serious problems with the equipment. The focus of the main telescope was set up the night before the eclipse, but experienced distortions as the temperature climbed during the day. Luckily, he had taken the smaller back-up telescope along, and this in the end provided the best results of all, perhaps because it was installed in a wooden tube that was less prone to thermal expansion and contraction than the steel tube in which the main astrographic glass was mounted.

After the eclipse, Eddington himself carefully measured the positions of the stars that appeared near the Sun's eclipsed image, on the photographic plates exposed at both Sobral and Príncipe. He then compared them with reference positions taken when the Hyades were visible in the night sky. These comparison plates were taken on location in Brazil, after the eclipse, by Crommelin; the comparison plates for the Príncipe experiment had been taken in Oxford before the eclipse. The measurements had to be incredibly accurate, not only because the expected deflections were small; the images of the stars were also quite blurred, because of problems with the telescopes and because they were seen through the light of the Sun's glowing atmosphere, the solar corona.

Before long the results were ready. Britain's premier scientific body, the Royal Society, called a special meeting in London on November 6. Dyson, as Astronomer Royal, took the floor, and announced that the measurements did not support Newton's long-accepted theory of gravity. Instead, they agreed with the predictions of Einstein's new theory.

The press reaction was extraordinary. Einstein was immediately propelled onto the front pages of the world's media and, almost overnight, became a household name. There was more to this than purely the scientific content of his theory. After years of war, the public embraced a moment that moved mankind from the horrors of destruction to the sublimity of the human mind laying bare the secrets of the cosmos. The two pacifists in the limelight — the British Eddington and the German-born Einstein — were particularly pleased at the reconciliation between their nations brought about by the results.

But the popular perception of the eclipse results differed quite significantly from the way they were viewed in the scientific establishment. Physicists of the day were justifiably cautious. Eddington had needed to make significant corrections to some of the measurements, for various technical reasons, and in the end decided to leave some of the Sobral data out of the calculation entirely. Many scientists were suspicious that he had cooked the books. Although the suspicion lingered for years in some quarters, in the end the results were confirmed at eclipse after eclipse with higher and higher precision.

Einstein's theory of General Relativity remains the focus of intense research.

Only recently, the first ever picture of the 'shadow' of the event horizon of a black hole has been on the front page of newspapers around the world. That image is a consequence of the bending of light by gravity, but in a much stronger gravitational field than in the eclipse measurement. Einstein and Eddington would definitely both have approved.

The President. Sadly, I think we've run out of time and we won't take any questions, but that was a fascinating piece of history and it is a pity we couldn't have had this in the May meeting, but nonetheless you've done us proud.

Can I just remind you that there's a drinks reception in the RAS library immediately now and I give notice that the next monthly A&G Open Meeting of the Society will be on Friday the 10th of May following the Annual General Meeting.

STF1396 AC: A QUESTIONABLE BINARY SYSTEM

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Introduction

STF 1396 is a possible triple system in the constellation of Leo. Component A is the star HD 86059 (HIP 48731, $\alpha_{2000\cdot0} = 9^{h}\,56^{m}\,22\cdot2^{s}$ $\delta_{2000\cdot0} = 10^{\circ}\,39'\,55''$) and is listed in the *Henry Draper Catalogue* with a spectral type of A0 with no distinguishing remarks and a photographic magnitude of $8^{m}\cdot5$[1]. The *V* magnitude in the *Hipparcos* catalogue is $8^{m}\cdot58$[2,3]. The data reported in the *Washington Visual Double Star Catalogue*[4,5] (Mason *et al.*, 2001–2014) are given in the informal table below.

Identifier	*First*	*Last*	*N*	*First*	*Last*	*First*	*Last*	*Pri.*	*Sec.*	*Sp. Type*
	Year			*PA*		*sep. (″)*		*mag.*		
09564+1040 STF 1396 AB	1829	2018	28	129	130	3·5	3·9	8·79	10·42	A0
09564+1040 STF 1396 AC	1895	1895	1	276	276	3·8	3·8	8·79	11·1	A0

G.V. Schiaparelli observed the STF 1396 AC system only once, on 1895 April 10 (Bessellian epoch 1895·28) using the Merz–Repsold 0·49-m refractor of the Osservatorio Astronomico di Brera (Milan, Italy)[6]. The *WDS Catalogue* does not report other AC astrometric measurements after that date. The separation of 3″·778 is uncertain, as reported by the *USNO Double Star Catalogue*'s Notes[6]. While the reality of the B component has been confirmed on numerous occasions, we were curious as to the existence of the C component reported by such an eminent observer.

The purpose of this work is an investigation into the actual existence of the STF 1396 AC system.

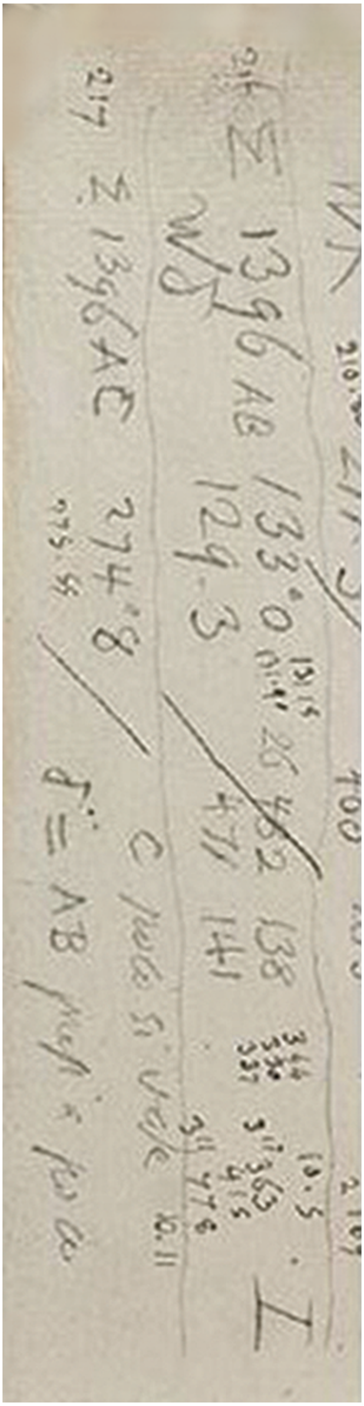

Fig. 1

Schiaparelli's original annotations taken at the eyepiece. (Credit : Historical Archive INAF-OAB Milan, Italy.)

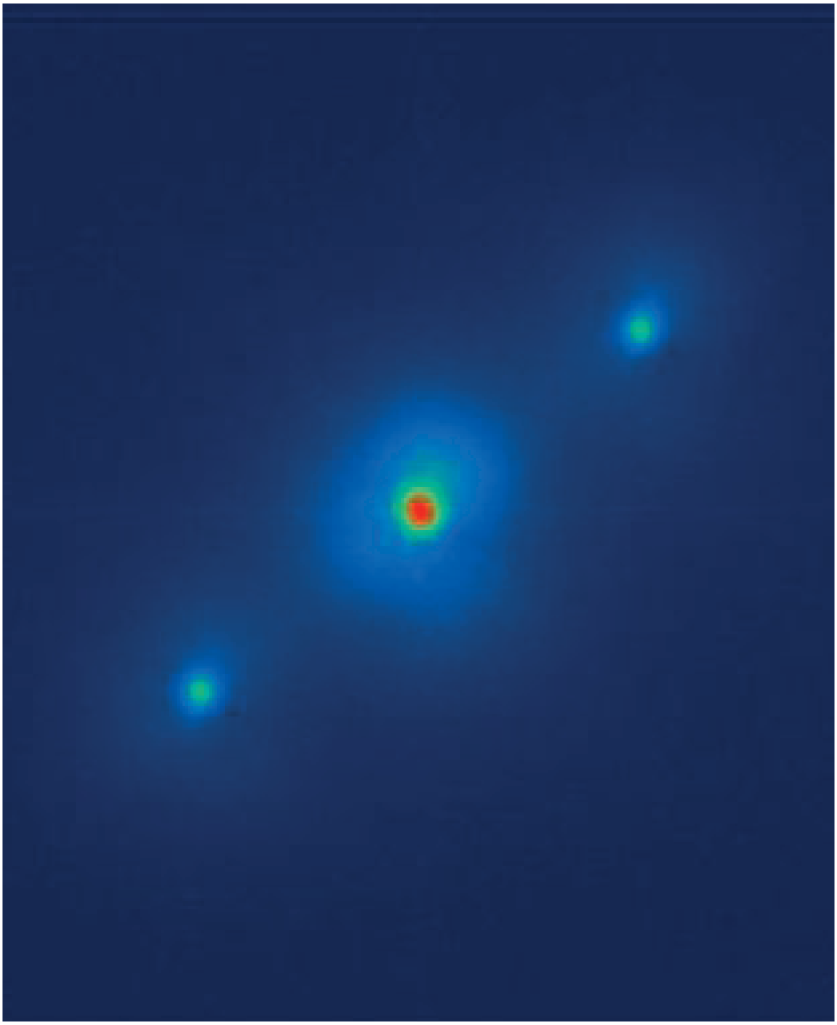

FIG. 2

Autocorrelations of the STF 1396 AB system observed on 2018 February 7 with the *PISCO* speckle camera at the *Epsilon* telescope (1·04-m) with the 24-mm eyepiece. Only the autocorrelations of the AB component are seen. (Credit : C2PU – Observatoire de la Côte d'Azur, Plateau de Calern, France.)

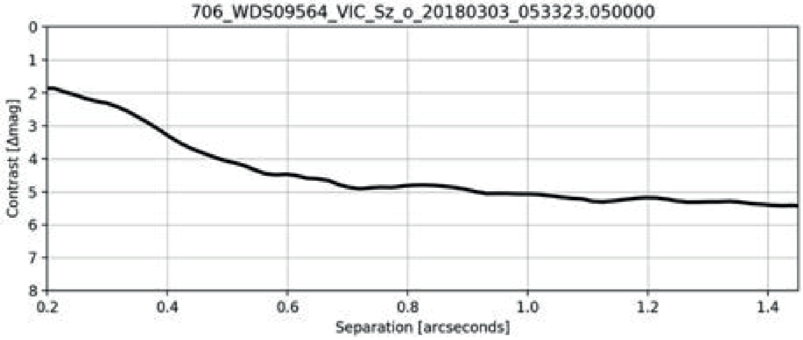

FIG. 3
Robo-AO 5-sigma detection sensitivity for STF 1396 for the SDSS z' filter (component A).

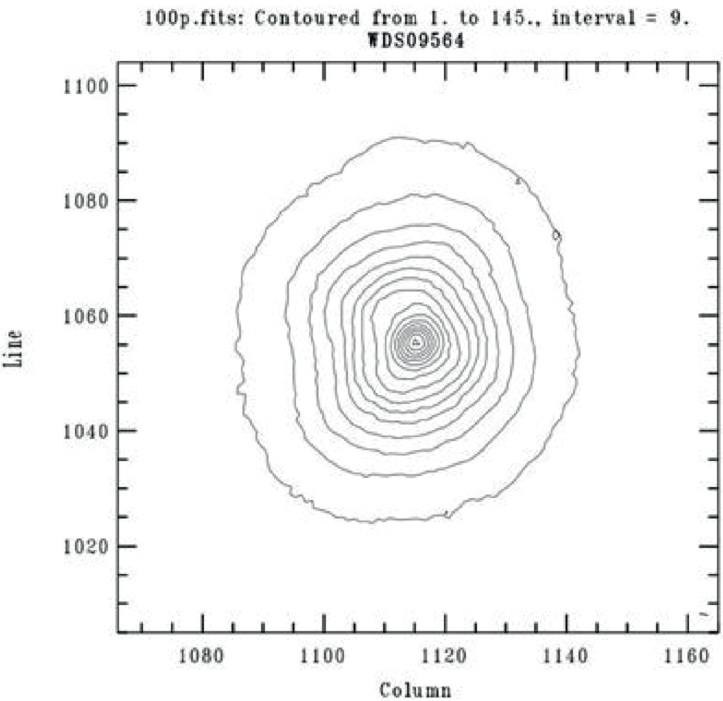

FIG. 4
Robo-AO isophotes of STF 1396 (component A).

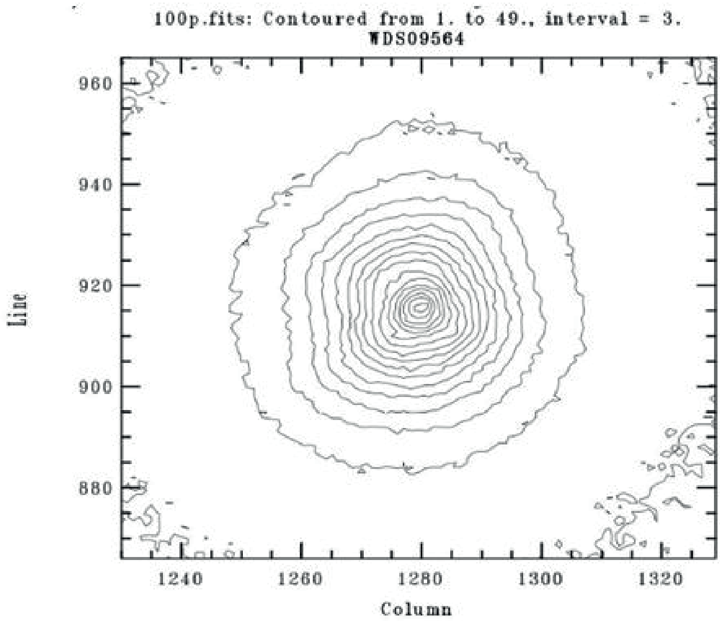

FIG. 5
Robo-AO isophotes of STF 1396 (component B).

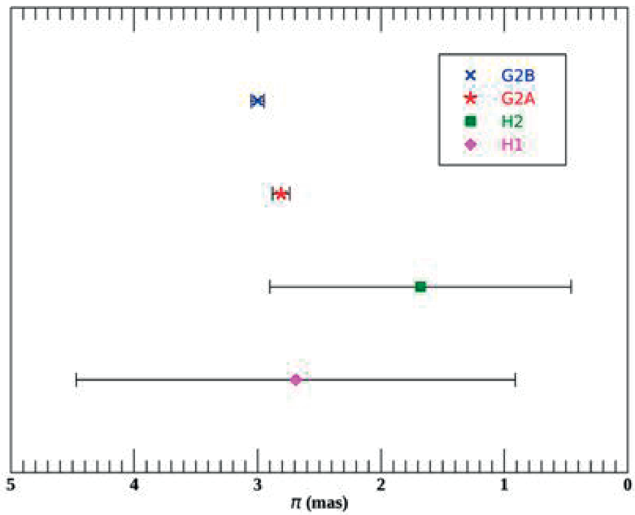

FIG. 6
The measured parallaxes of STF 1396 from the original *Hipparcos* reduction H1 (magenta diamond), the re-reduction by van Leeuwen H2 (green square), *Gaia* DR 2 for the A component G2A (red asterisk) and *Gaia* DR2 for the B component (blue x).

The diary of G. V. Schiaparelli

The original diary of the observations of the astronomer G.V. Schiaparelli is kept in the historical archive of the INAF–Osservatorio Astronomico di Brera (OAB Milan, Italy). For a brief biography of Schiaparelli see the obituary that appeared in *Monthly Notices*[7]. Finding a scanned copy of the manuscript was essential to trace and interpret any notes written by him during the observation of the C component of the STF 1396 system.

As shown in Fig. 1, the diary page shows for the AC system only an estimation of the separation, evaluated by the great Italian observer, roughly equal to the one he measured for the pair AB (δ = AB "*pres'a poco*" – δ = AB "roughly"), but stressing that the quality of the observation of the C component was uncertain ("C *poco si vede*" — "C little is seen"), while a value of the position angle (274°·8) is not corrected for the zero of the position angle of the micrometer.

Observations, past and present

F. G. W. Struve made micrometric measurements of 2714 double stars between 1824 and 1837 and published his results in the work *Stellarum duplicium et multiplicium mensurae micrometricae*[8]. From 1827 to 1832 (Bessellian epoch 1829·20) he observed STF 1396, but his report does not show any C component. Over the course of more than a century, after the first observation made by G. V. Schiaparelli, no other double-star observers, even of great experience, such as S.W. Burnham or W. H. van den Bos, equipped with telescopes comparable to or larger than the Merz–Repsold used by the Italian astronomer in Milan, have detected the presence of the C component near the position indicated by Schiaparelli. The magnitude of the C component should be, by the way, not very different from that of the B component of the system. More recently the astrometric satellite *Hipparcos*, as reported in the Double and Multiple Systems Annex[9], detected only the B component. This situation encouraged the authors to investigate further the existence of the C component.

A literature review published in the last twenty years showed that more recent speckle interferometry observations were made during 2001–2002 with the 26-inch (0·66-m) refractor of the US Naval Observatory[4] and later in 2007 with a 12-inch (0·30-m) reflector + CCD[10]. No detection of a third star (component C) of about magnitude $+11^{m}\cdot10$ within 3–4″ (arcsec) from the A component was made.

In 2015, the STF 1396 AB system was observed[11] with the technique of speckle interferometry using the *PISCO* camera at the 40-inch (1·02-m) Cassegrain Zeiss telescope of the INAF – Osservatorio Astronomico di Brera (Merate, Italy), but unfortunately the seeing did not allow for images of good quality. The observation was repeated in the early months of 2018 at the Observatoire de la Côte d'Azur (OCA), on the Calern plateau located a few kilometres north of the city of Grasse (France), using the same speckle camera at the 41-inch (1·04-m) Cassegrain *Epsilon* telescope, with the seeing definitely better than the previous observation. Both observations, although conditioned by the magnitude and separation limits of the instrumentation used, which would have allowed the detection of a star as bright as $11^{m}\cdot0$ whose distance from A was greater than 0″·3 (arcsec), did not detect the presence of the C component (0″·30 < sep < 4″).

In Fig. 2 is shown the autocorrelation for the STF 1396 system from the *Epsilon* telescope. Only the signal from the AB system is apparent.

At this point we have made some hypotheses on the possible reasons for which the component C was no longer detected after the first observation of Schiaparelli. (*i*) C could be an irregular type variable whose current magnitude is far below the observability limit; (*ii*) C has a very large proper motion so that it has now become a 'close companion' of component A or B, and is below the resolving power of the one-metre class telescopes used to date; (*iii*) an error in the evaluation by G. V. Schiaparelli due to the poor quality of the images, to his advanced age, and to the health problems that, a few years later, would have prevented him from continuing his observations[7].

In Table I the stellar objects are indicated within a radius of 10 arcmin from the coordinates of the STF 1396 system displayed through the *Aladin*[12] platform and the *VizieR*[13] for Astronomical Catalogue service (Centre de Données Stellaire, Strasbourg, France). No large-proper-motion stars are evident within 60 arcsecs of separation from the STF 1396 system excluding the star classified as LSPM J0956 + 1048 (source *Gaia* DR2 3879521740887706752) whose parallax (12·12 ± 0·09 mas) is not comparable to that of the components of the STF1396 binary system (respectively 2·81 ± 0·07 and 3·00 ± 0·05 mas)[14,15].

To continue the investigation, it was important to have an instrument with a higher angular resolution. At first it was decided to use the *NPOI* (*Navy Precision Optical Interferometer*) jointly managed by the Naval Research Laboratory (NRL) and the United States Naval Observatory Flagstaff Station (NOFS) in collaboration with the Lowell Observatory, which is located on Anderson Mesa (Flagstaff, Arizona, USA). *NPOI* operates at visual wavelengths using as many as six array elements simultaneously. It is a dual-purpose instrument, intended for both wide-angle astrometry and imaging. The array elements are siderostats that feed a 12-cm diameter vacuum-beam transport system conveying light to the optics laboratory, where access to the path-length-compensation system is provided and where the beam combiners are housed. The baseline lengths ranging from 19 m to 79 m produce '*fringe spacings*' (a measure of angular resolution) ranging from 13 milliarcsec (mas) at λ 850 nm on the 16-m baseline to 1·7 mas at λ 550 nm on the 79-m baseline. The *NPOI* is described by Armstrong *et al.*[16] and in Hutter *et al.*[17], with performance details specific to binary stars detailed in the latter work.

Interferometry is capable of angular resolutions that no other technique can match, simply because it is 'easier' to separate the baseline elements than to create a single large mirror. Unfortunately, one of the disadvantages in the case of optical interferometry is the need to detect a signal within the coherence time of the atmosphere that imposes a set of constraints connected to the magnitude of the observed star. In the case of the *NPOI* the system limiting magnitude is $V = +6^{m}{\cdot}0$, or roughly naked-eye brightness for a dark site. Thus, it is not possible to observe STF 1396 due to the present sensitivity limits of the *NPOI*.

To overcome that problem, in early 2018, R. T. Zavala proposed to observe the STF 1396 system with the *Robotic Adaptive Optics* (*Robo-AO*[18,19]) system operating on the 2·1-m[20,21] telescope of the Kitt Peak National Observatory in Tucson (Arizona, USA).

Robo-AO is an automated system (Robo) based on the technique of adaptive optics (AO) with a laser guide star (LGS). *Robo-AO* uses an automated laser system in ultraviolet light (355 nm) to create an artificial reference star that is projected to a distance of 10 km on the sky: the purpose is to measure the deformation effect of the wavefront caused by the Earth's atmosphere. Once this effect has been measured on the artificial guide star, 140 actuators deform a mirror by modifying the shape of its surface and correcting the aberrations.

TABLE I

Distance from STF 1396 within a radius of 10 arcmin (Gmag <15)
(astrometric data by VizieR[13] service and Gaia DR2 Catalog[14,15])

Source Gaia DR2	*dist* (*arcsec*)	*RA* (*J2000*)	*DEC* (*J2000*)	*Gmag* (*m*)	*Plx* (*mas*)	*e_Plx* (*mas*)	*pmRA* (*mas/y*)	*e_pmRA* (*mas/y*)	*pmDE* (*mas/y*)	*e_pmDE* (*mas/y*)
3879423682489757952	48	09 56 22·04	+10 39 07·28	13·93	1·0660	0·0324	8·010	0·057	−5·553	0·055
3879423781273492992	58	09 56 26·12	+10 39 54·03	13·58						
3879424262309675520	126	09 56 28·66	+10 41 19·06	12·84	1·9896	0·0433	−18·410	0·072	0·221	0·065
3879421895783363200	167	09 56 24·32	+10 37 10·92	13·57	0·8223	0·0294	−5·913	0·049	0·426	0·044
3879427255902544768	210	09 56 28·38	+10 43 04·43	13·77						
3879426946664889088	219	09 56 08·34	+10 41 13·71	13·21	1·2342	0·0269	1·887	0·041	1·457	0·038
3879427354686128384	256	09 56 33·09	+10 43 14·62	14·91	1·5143	0·0648	−23·943	0·087	4·269	0·080
3879422892215771008	349	09 56 44·71	+10 38 06·22	11·53	2·0192	0·0616	−6·821	0·090	1·262	0·083
3879424610202700928	375	09 56 00·70	+10 36 37·01	14·70	2·2570	0·0397	−19·116	0·067	−100·534	0·066
3879428252334946816	440	09 56 00·97	+10 45 04·48	13·97	2·1485	0·0372	−7·221	0·052	−8·874	0·047
3879521367226064256	452	09 56 41·12	+10 45 50·81	9·65	9·0423	0·0430	−46·420	0·072	−66·038	0·063
3879517140978300416	465	09 56 52·96	+10 41 39·97	14·71	3·0169	0·0419	14·633	0·067	−28·445	0·060
3879616371902489344	497	09 56 06·55	+10 47 16·14	9·70	4·6788	0·1216	−10·309	0·120	−27·775	0·130
3879614310318188928	497	09 55 52·63	+10 43 54·52	14·92	0·5992	0·0502	1·539	0·074	3·171	0·063
3879521740887706752	518	09 56 34·75	+10 47 59·86	15·33	12·1168	0·0856	−174·116	0·108	23·139	0·104
3879408426765927040	528	09 56 27·09	+10 31 13·21	13·98	8·5662	0·1413	−17·457	0·214	−54·331	0·063
3879522668600582016	539	09 56 17·84	+10 48 50·82	14·13	1·1115	0·0396	−3·609	0·053	−5·353	0·050
3879408358046452096	551	09 56 20·78	+10 30 44·88	14·11	0·7672	0·0577	−6·446	0·066	−4·896	0·059
3879615856506415232	554	09 55 51·51	+10 45 14·08	12·94	2·9400	0·0578	−35·921	0·065	−45·288	0·062
3879710345786852096	555	09 56 16·11	+10 49 02·74	14·47	1·4012	0·0423	−2·210	0·058	−11·872	0·054
3879410793292390656	560	09 56 56·21	+10 35 45·16	9·92	1·3839	0·0636	25·236	0·085	−15·452	0·081
3879418661672477056	561	09 55 48·22	+10 35 42·58	12·15	0·3646	0·0451	−4·020	0·076	−3·285	0·077

The science camera is a 1024×1024, 13-µm-pixel, electron-multiplying CCD with a pixel scale of 35·1 mas/pixel and a 36″ field of view.

The image acquisition process is completely automated and the data collected by the KPNO *Robo-AO* telescope are initially processed and archived, both at the Kitt Peak and Caltech observatories, in order to be available later for the analysis process. Following is the scheme of this process:

(*i*) Programming of the observing target, pointing the telescope, and sending images for mass storage.

(*ii*) Pretreatment of the frames (flat/dark calibration), frame alignment, and possible resampling (4×), cross-correlation of reference stars with diffraction-limited PSF (point spread function).

(*iii*) Frame-quality classification on bases of PSF core size.

(*iv*) Subtraction of the best-fit PSF based on the selection of 25 PSF references chosen near the observing target.

(*v*) Cross-correlate PSF subtracted image with diffraction-limited PSF. Generate a SNR map for convolved image.

(*vi*) Flag significant peaks in SNR map as a possible companion star.

On UT 2018 March 30 a series of frames were taken with SDSS photometric filters in the near infrared bands (λ_{eff} without extinction: i' — 767 nm, z' — 911 nm)[22] with a field of view that included both components A and B of the STF 1396 system. Neither image revealed the presence of an eventual component C with a separation of 3–4″ (arcsec) from the main component. The uncorrected seeing was 1″·2 and the observations were taken between 05:31:11 and 05:33:23 hours UT. Total exposure times of 120 seconds were made in both the i' and z' filters. The Strehl ratios of the AO corrected images were 2·99 for the i' filter and 5·16 for the z' filter.

Robo-AO results

In the theory of signal detection, the separation between the signal means (photons) and the noise distributions (thermal noise of the CCD, sky-background brightness, *etc.*), with respect to the standard deviation of the signal can provide indications on the possibility that it is consistent to consider the difference in light contrast as a hypothesis traceable to a source, too close to the signal to be detected optically. In Fig. 3, relative to the A component of the STF 1396 system, the brightness peak with a slope below the separation threshold of 0″·6 characterized by a contrast difference of just over 2^{m} does not allow a 'close companion' of component A to be considered statistically probable. The level of confidence at 5 sigma corresponds to a probability of discovery $p = 0{\cdot}0000003$ or about 1 in 3·5 million.

We present in Figs. 4 and 5 the isophotes, *i.e.*, the place of the points of the image of a star having the same superficial brilliance in the i' and z' bands. Each isophote is characterized by its own superficial brilliance, which corresponds to an isophotal ray. As regards component A of the STF 1396 system, the profile of the isophotes from the centre to the outside is circular and presents a uniform distribution; relative to component B the outermost profile is slightly irregular in shape. In Table II we provide the magnitude differences in the z' and i' bands and the relative astrometric results of the STF 1396AB system obtained from the *Robo-AO* images. For the astrometric solution the results of Appendix B of Jensen-Clem *et al.*[19] were used, including the slightly different pixel scales in the X and Y directions. The astrometric results are derived from the z' images due to the better Strehl ratio obtained in that filter.

TABLE II

Magnitude differences and astrometry derived from the Robo–AO images of STF 1396AB. Astrometric results refer to the z′ filter data as explained in the text.

Δmag i′ (*mag*)	*Δmag z′* (*mag*)	*rel.* α (*arcsec*)	*rel.* δ (*arcsec*)	ρ (*arcsec*)	θ (*deg*)
1·11 ± 0·01	1·003 ± 0·006	2·936 ± 0·005	2·449 ± 0·005	3·824 ± 0·013	130·42 ± 0·42

Hipparcos, and other evidence

To investigate further whether components A or B may harbour another companion that may be the star observed by Schiaparelli, we consider other archival data, as well as the physical nature of possible orbits.

First, considering the original *Hipparcos* reduction, STF 1396AB is resolved as a double star and reduced by both data-processing consortia with a Component 'C' solution. This solution is applicable to optical doubles and to multiple systems for which a linear solution (*e.g.*, a common-proper-motion pair) was appropriate over the length of the *Hipparcos* mission[2]. The solution type is fixed, meaning that the A and B components have identical parallaxes and proper motions, and the solution has the highest-quality grade possible[9]. The magnitude differences in the *Tycho* bandpasses are $\Delta B_{\mathrm{T}} = 1{\cdot}6$ and $\Delta V_{\mathrm{T}} = 1{\cdot}7$. Assuming both stars are on the main sequence, an estimate of the B-component spectral type is possible. Using tables in Chapter 15 of *Allen's Astrophysical Quantities*[23] we can estimate the properties of the B component. The magnitude differences suggest the B component could be a late-A to even an early-F star. As a rough estimate of the mass, we adopt 1.8 solar masses for the B component and 2·9 for A, for a total mass estimate of 4·7 solar masses.

Given reasonable assumptions for the two spectral types, and the *Gaia* and *Hipparcos* parallaxes, we can evaluate whether any significant orbital motion might be expected, if STF 1396AB is a visual binary. We display the measured parallaxes in Fig. 6 from the original and the re-reduction[24,25] of *Hipparcos* data and *Gaia* DR2 with their uncertainties. These parallaxes and their inferred distances are listed in Table III. We note the long 1-σ 'tail' of the *Hipparcos*

TABLE III

Parallaxes and inferred distances to the STF 1396AB stars.

Catalogue	π (*mas*)	σ_π (*mas*)	*Dist.* (*pc*)	*1σ min. dist.* (*pc*)	*1σ max. dist.* (*pc*)
H1	2·69	1·78	372	224	1099
H2	1·68	1·22	595	345	2174
G2A	2·81	0·07	356	347	365
G2B	3·00	0·05	333	328	339

Note. — Parallaxes and inferred distances for the STF 1396AB stars from the original *Hipparcos* reduction (H1[3]), the re–reduction (H2[24,25]), and *Gaia* DR2[14,15] for the A component (G2A) and the B component (G2B).

distances characteristic of parallaxes with large fractional uncertainties that are inherent in large surveys[26]. Considering the uncertainties, the parallaxes are in fairly good agreement, especially the original *Hipparcos* value and the DR2 results. While the *Gaia* DR2 uncertainties are less than 0·1 mas, there are a few caveats to consider. The *Gaia* DR2 processing treated all stars as single stars, unlike the component solutions in the *Hipparcos* reductions. Multiple star processing in *Gaia* will be provided in future releases[15]. The *Gaia* parallaxes do exhibit small angular-scale correlations, and systematics may ultimately limit parallax uncertainties on global scales to ~0·1 mas[27] in DR2.

Even considering these circumstances, a distance of 360 pc seems a reasonable estimate to assume at this stage for a bound orbital solution. At a distance of 360 pc a separation of 3·88″ of the *Hipparcos* component relative astrometry corresponds to a distance of 0·007 pc or 1397 astronomical units. This is approximately the inner boundary of our Solar System's Oort cloud[28]. Using Kepler's Third Law and an assumed total mass for the binary of 4·7 solar masses yields an estimated period of 24085 years for a bound orbit. The 188-year time span of the observations of the AB binary from the WDS listed in the *Introduction* is 0·008P where P is the orbital period. Thus, for a bound circular orbit, one would expect a motion of almost 3° under the most favourable orientations. This also suggests that if Schiaparelli did indeed detect a C component at 3–4 arcsec separation, orbital motion alone is unlikely to have accounted for the inability to detect component C at the present epoch. The *Hipparcos* component solution in turn suggests a common-proper-motion AB pair, or at least an orbit with no detectable motion during the span of the mission. Subsequent multiple-star solutions from *Gaia* will be needed to determine if there is a significant line-of-sight separation between the AB stars.

Conclusion

It is not yet possible to exclude the existence of the component C, but it is highly probable to assume with reasonable certainty that above the threshold of separation of 0″·30–0″·40 (arcsec) there is not in the vicinity of the AB system a star of comparable magnitude with that of the known component B.

The authors believe that, before being able to conclude the non-existence of the C component, and consequently accept the hypothesis of an observational error by G. V. Schiaparelli, it would be desirable to observe the STF 1396 system with telescopes with a greater diameter, at least in the class of 4 metres. Alternatively, the eventual *Gaia* multiple-star solutions could also settle the question. The B component is also a radial-velocity target of *Gaia*, and these data will further constrain the problem. At this early stage, with just three radial-velocity observations, deriving firm conclusions from the RV data for individual stars seems premature[29].

Acknowledgements

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R. T. Zavala is indebted to Dmitry Duev for his assistance with *Robo-AO* post-observing queries and advice, and Sian Prosser, Librarian and Archivist of the Royal Astronomical Society, for research assistance.

We would like to thank Agnese Mandrino, head of the historical archive of the INAF-Osservatorio Astronomico di Brera, and Raffaello Braga for the digital scanning of the original pages of the diary of G. V. Schiaparelli.

This research has made use of the *SIMBAD* astronomical database, *VizieR* catalogue service and *Aladin* interactive sky-atlas software, operated at Centre de Données astronomiques de Strasbourg (CDS), and NASA's Astrophysics Data System.

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CORRESPONDENCE AND CORRIGENDUM

To the Editors of 'The Observatory'

Kepler's Ideal Polygon Law

In Martin Beech's interesting article[1] 'Looking for Kepler's Ideal Polygon Law in *Kepler*-Mission Data', the figures do not seem to represent the texts. For example, Fig. 1 contains no triangle, although one is mentioned in the caption. Also, he makes frequent references to a pentagram. In the interests of accuracy, this should be amended to pentagon. The ratio of the circumradius to the inradius of a pentagon is $1/\cos(\pi/5) = \sqrt{5}-1$ or about 1·236, whereas the ratio for a pentagram[2] is $\sin(3\pi/10)/\sin(\pi/10) = (\sqrt{5}+3)/2$ or about 2·618.

Yours faithfully,
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2019 June 5

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(1) M. Beech, *The Observatory*, **139**, 105, 2019
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Martin Beech Replies

I concede that I was a little lax in using the word pentagram, and Michael's comments (as such) are not unreasonable given typical usage of the term — that is taking the word pentagram to correspond exclusively to a five-pointed-star configuration. However, I would argue that there is no confusion with respect to the textual usage and/or the numbers (with respect to inscribed and circumscribed circles) used. The words pentagon and pentagram are etymologically similar in that they are based upon the Greek for 'having five lines', the pentagon (as used in the diagrams) is the regular pentagon which has the extra conditional constraint that the five central angles are equal as well — so, perhaps one could say that 'legally' the term used in the text is adequate (mathematically we are essentially quibbling about how the five lines are arranged). Accordingly, there are no changes required with respect to the article's results and conclusions.

Michael Baxter's comment about Fig. 1 is correct: it seems that Fig. 2 has been printed twice.

Yours faithfully,
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Corrigendum

The Managing Editor confesses his gross oversight in allowing Fig. 2 to appear twice in the 2019 June issue (p. 107), and offers his sincere apologies. The correct Fig. 1 is given here.

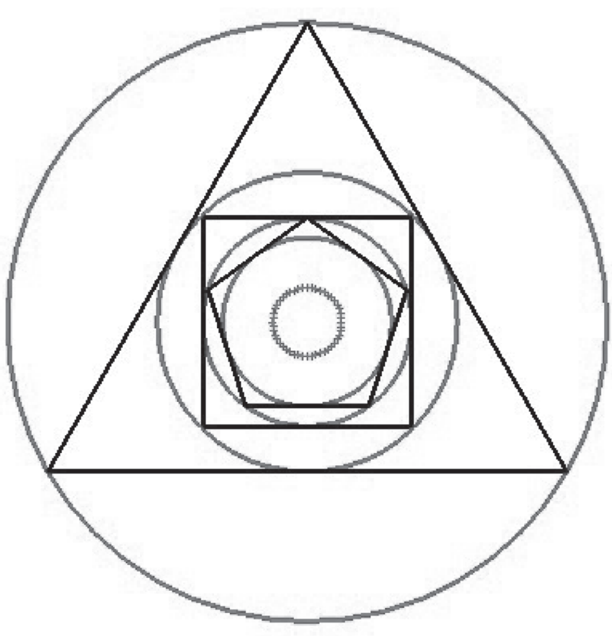

FIG. 1

The polygonal sequence for planetary spacing as originally proposed by Kepler in 1596. The outer two orbits are the circumscribed and inscribed circles to a triangle, and the subsequent orbits are deduced from the inscribed circles to a square, pentagram, hexagon, and so on. The central small circle has a radius corresponding to the Kepler–Bouwkamp limit — that is, it corresponds to the radius of the inscribed circle associated with the *infinitely* regressed polygon interior to the starting triangle.

REVIEWS

Interstellarum Deep Sky Guide, Desk Edition, by Ronald Stoyan & Uwe Glahn (Cambridge University Press and Oculum), 2018. Pp. 264, 28 × 28 cm. Price £74·99/$99·99 (spiral bound; ISBN 978 1 108 45313 4).

The *Interstellarum Deep Sky Guide* is a companion volume to the excellent *Interstellarum Deep Sky Atlas* which I have reviewed previously in *The Observatory*. Although designed to be used in conjunction with the *Atlas* it may still appeal as stand-alone. The *Guide* has been written, or maybe more correctly compiled, by the same people as the *Atlas* (Ronald Stoyan and Uwe Glahn).

There is a very brief introduction and overview to the *Guide*, which is followed by the meat of the book, which are images and sketches of selected deep-sky objects. Usefully, the page numbering of the *Guide* corresponds to the page numbering of the *Atlas* — so it is easy to navigate between the volumes. Where there are a large number of objects on an *Atlas* page the *Guide* uses an 'a' and 'b' suffix. Most of the object images have been derived from the Digitised Sky Survey (DSS) files of the Palomar Observatory Sky Survey (POSS-1) red and blue plates. The red and blue images have been combined and printed in red and blue which gives a useful direct indication of the emission sources. And for the visual observer using a red torch at night, the red component virtually disappears giving a more realistic impression of what can be seen through the eyepiece.

Drawings of objects are also presented to give a better idea of what an experienced observer might see through the eyepiece. These were made by the authors using telescopes from 4- to 48-inch apertures at various magnifications, with the aim to show the maximum detail. Each drawing has an annotation to indicate the observer, telescope aperture, magnification, and filter if used. Each page also includes a brief listing of the objects shown. These are divided into visibility classes; that is, objects that can be seen through 4-, 8-, or 12-inch aperture telescopes, or are challenges for big telescopes. The authors seem to observe from better dark-sky locations than are available from the UK, so it is likely that in many cases UK observers would need to use a larger-aperture telescope for a successful observation. Finally, on each page there is a small finder chart showing the positions of the objects on the corresponding *Atlas* page.

Overall, the *Guide* has been well thought out and implemented. The desk edition supplied is printed on fairly robust paper but there is also a field edition with waterproof pages. It is a really useful addition to the *Atlas*, and while mainly aimed at the visual observer, will also find friends among astrophotographers who wish to obtain a compendium of interesting-object images. — CALLUM POTTER.

Observer's Guide to Variable Stars, by Martin Griffiths (Springer), 2018. Pp. 316, 23·5 × 15·5 cm. Price £22·99/$29·99 (paperback; ISBN 978 3 030 00903 8).

I'm not sure about this book. It says it's an "Observer's Guide", and Chapter 1 is fine, but the second chapter is entitled 'The astrophysics of variable stars' and gets a bit 'heavy' in places for someone just looking to observe them. There are also many errors, although the book does have its good points!

Chapter 3 then covers 'Observing techniques' and is OK until page 57 where, in the table of limiting magnitudes for various telescopes, there appears an anomaly in that with a 40-cm reflector one can see, apparently, fainter than with a 45-cm one! Further on in this chapter under the sub-heading of 'Visual estimation of variable stars' the author has made a bit of a jumble trying to describe the estimation where the variable is fainter than either of the comparisons. However, before that error I was pleased to note that he states that the brightest star in any estimation is always written first, and also that you should try and use comparisons that match the variable in colour. The chapter ends with some examples of observing notes and records before finishing with an example of the AAVSO's excellent *Variable Star Plotter* and their *Visual Observation Form* for reporting said observations (but sadly no such example of the VSS reporting format).

Chapter 4 discusses the important issue of 'Instruments and equipment', including filters for photometry. It ends with the statement that whilst CCDs are the industry standard for photometry, there is now increasing interest in using DSLR cameras on driven mounts. However, a drive is not necessary if you limit the exposure time and look at relatively bright stars. See below for more on this.

Chapter 5 takes us into the realm of photometry and has an excellent comment that "you most likely will be spending more time working with your data at the computer than actually taking images at the telescope". How true that is! This chapter also introduces us to the photometric software package MAKALI'I, although this reviewer knows it by the name of SUBARU, but has never used it. The chapter ends with a more detailed look at DSLR photometry and mentions the excellent article by Des Loughney on the subject in the *Journal of the BAA*, but I wonder if the author has actually read it, given his comments noted above at the end of Chapter 4?

Chapter 6 returns us to the more humble practice of 'Observing variable stars with binoculars'. On pages 107–110 is a listing, straight from the AAVSO web site, which is wrong! For example, R Aqr is listed as being in the northern hemisphere with an RA of $23^h\ 43^m$ and a Dec of 49° 46′! It is, of course, south of the equator with coordinates of $23^h\ 44^m$ and –15° 17′. Indeed, none of the stars copied from this table and described as "South" are shown with a negative declination — not very helpful for beginners!

Chapter 7 covers 'Giant stars and their variability' and lists a number of rather obscure types and ends with a lengthy table of long-period variables, but it doesn't follow the same pattern as that for binocular stars mentioned above (perhaps a good idea, as the latter is wrong!). But this time there is no RA and Dec listed at all, although the stars I checked were not as up to date as the AAVSO VSX data even though that was updated several years ago in some cases.

The next five chapters follow a similar pattern, with a brief introduction and description of the various types of star in each category. This includes, Cepheids, rotating variables, eclipsing variables, explosive and eruptive stars, and unusual variables.

We then move on to Chapter 13 and variable-star-observer associations with particular emphasis on the BAA VSS and the AAVSO.

Chapter 14 is the longest in the book and suggests stars to observe at each season of the year and includes a brief description and a finder chart from the BAA VSS. Unfortunately, probably due to reasons of space, only one chart is included when, for example, that for omicron Ceti has three charts drawn, at 60^d, 18^d, and 5^d, the latter for when the star is faint, but the 60^d chart shown in the book will not be of much use.

There then follows a welcome section in the form of an Appendix on the subject of 'Spectroscopy of variable stars', something that a number of amateurs are getting into nowadays very successfully. The book is completed by a glossary, references for further reading (although at least two are out of print!), and an index.

So, given all of the aforementioned, I don't think I can recommend this book, as I'm not sure the author has even fully read many of the sources he quotes. If you're a visual observer wanting to 'get into' variable stars a good start would be Gerry A. Good's *Observing Variable Stars*. — ROGER PICKARD.

The Sun Today, by Claudio Vita-Finzi (Springer), 2018. Pp. 111, 23·5 × 15·5 cm. Price £9·99/$12·99 (paperback; ISBN 978 3 030 04078 9).

This new concise book by Claudio Vita-Finzi from the Natural History Museum, London, summarizes our present knowledge of the Sun and its interaction with the environment: interplanetary, the Earth and the other planets, and interstellar medium. The most important data obtained from ground and space-borne instruments are nicely presented in many carefully selected graphs and images. Each of seven short chapters is followed by an extensive list of references covering not only the most recent observations, but also historical ones. This provides the necessary context illustrating changing ideas and important steps in our understanding of the Sun as a star and ever-changing source of energy (electromagnetic & particles) for our Solar System. The book is eloquently written and wherever possible addresses existing problems in understanding basic processes driving solar radiation, particle and magnetic fluence. In Chapter 5, 'The Solar Furnace', the author speculates on the new (besides wave or magnetic) mechanisms of heating the outer solar plasma layers — the corona — to temperatures above a million degrees. This may give rise to discourse within the solar-physics community.

I would recommend the book to geo- and helio-physicists who are already somewhat familiar with the subject, as the book encapsulates our present knowledge on our closest star that creates the environment supporting our existence and driving dynamics on Earth. The book is tarnished only by a number of editorial mistakes. — JANUSZ SYLWESTER.

Finding our Place in the Solar System, by Todd Timberlake & Paul Wallace (Cambridge University Press), 2019. Pp. 378, 25 × 18 cm. Price £28·99/$39·99 (hardbound; ISBN 978 1 107 18229 5).

Unlike our scientific bed-fellows in physics, chemistry, and biology, we astronomers have had a real revolution. Just the one, and it started in 1543. Before that time, in the 'good old days', Earth was at the centre of the cosmos, did not spin, and everything orbited around us. Then along came the Polish cleric Nicolaus Copernicus. Earth was demoted to the role of a mere planet. Heliocentrism took over. Stars were suddenly outrageously far away, and the Earth rushed around the Sun every year, and rotated every day.

Our revolution was not swift. A moving Earth seemed contrary to common sense. It took the combined efforts of hardworking super-intelligent visionaries like Tycho Brahe, Johannes Kepler, Galileo Galilei, Isaac Newton, and Edmond Halley to ensure its success. But by the time of René Descartes' *Principia Philosophiae* in 1644, the revolutionary views were generally accepted, and complete acceptance came after the publication of Newton's *Principia* in 1687.

As to proof one then had to rely on the measurement of the Earth's oblateness, a precise estimation of the astronomical unit, Léon Foucault's pendulum, James Bradley's discovery of the aberration of star light, and the eventual improvement of telescopic astronomy by the mid-18th Century that enabled the distance to a few nearby stars to be measured.

The beauty of the heliocentric hypothesis was that it explained the complexities of planetary motion, helped introduce the physics of universal gravitation, and opened up the possibilities of extra-terrestrial planetary systems and extra-terrestrial life.

This book delves into the details and intricacies of our astronomical revolution, its precursors, and aftermath. It is accurately aimed at a general readership and is based on a course for non-science majors at Berry College, Rome, Georgia, USA. It is superbly produced, beautifully illustrated, well referenced, and has the clarity and precision engendered by the two authors having had to teach a difficult subject to typical first-year university students. It is an excellent introduction to scientific methodology, an absolute joy to read, and I recommend it wholeheartedly. — DAVID W. HUGHES.

Gemini 4: An Astronaut Steps into the Void, by David J. Shayler (Springer), 2018. Pp. 378, 24 × 17 cm. Price £24·99/$37·99 (paperback; ISBN 978 3 319 76674 4).

On 1965 June 3, US astronaut Edward White became the second human to leave his spacecraft and 'walk' in space, a feat that had previously been achieved only by Soviet cosmonaut Alexei Leonov. As the second in a series of volumes about NASA's two-man Gemini programme, this book tells the story of the 4-day mission that made the first tentative steps on the road toward future long-duration flights and, eventually, extravehicular activities (EVAs) on the Moon by Apollo astronauts. The first chapters give an in-depth overview of the activities leading up to the first American EVA, including the decision to progress from a stand-up EVA to a full excursion outside the spacecraft, following Leonov's sensational breakthrough.

The highlight of the *Gemini 4* mission was the 20-minute spacewalk by White, an achievement which is covered in considerable detail. Even today, the spectacular colour images of Ed White taken by command pilot James McDivitt remain some of the most iconic of the entire space age. The main activities of the crew during and after the flight are presented, including numerous quotations made by the astronauts during and after the mission. The volume ends with an analysis of the significance of *Gemini 4* in the long-term evolution of American human spaceflight.

This well-researched volume by David Shayler, the author of numerous books about human spaceflight, will be a valuable addition to the bookshelves of anyone interested in the events that took place more than 50 years ago and culminated in humans walking on the Moon. — PETER BOND.

Card Planispheres — A Collector's Guide, by Peter Grimwood (Orreries UK), 2018. Pp. 84, 30 × 21 cm. Price £27·50 (paperback; ISBN 978 0 9551 336 1 9).

When I first noticed this book listed in the section 'recent books missed' in Vol. 11, No. 1, of my hardcopy of The Society for the History of Astronomy e-News, I wondered why the price was so high for an 84-page paperback. After I received it, the opposite question was "why was it so low?" — in brilliant full colour, printed on high-quality art paper! The photographs were carefully matched to the colour of the real object. In the printing trade this is referred to as 'registered colour'. There are over 195 planispheres and related devices depicted. I have collected many astronomical items and publications including planispheres since I was very young. I purchased my first one at the Adler Planetarium of Chicago, Illinois, in 1954. Although I have several planispheres I was only a casual collector and acquired them when I came across them; the author is a dedicated collector and sought them out. When he could not find a guide book he wrote this one, the first collectors' guide to planispheres. We can all be grateful for his efforts that result in this wonderful publication.

The book begins with a brief introduction to the subject, an explanation of how they work, and a description of how they are constructed. It is then divided into four sections. Section 1 (pp. 10–61) is the longest, dating back to the 1800s, featuring planispheres from Germany, France, the UK, the US, and other nations. Each photograph is accompanied by a brief paragraph mentioning the date, size, brief history, and notable differences. Section 2 (pp. 64 and 65) has similar information of devices with non-roman alphabets from Ceylon (Sri Lanka), Russia, Israel, and Japan. Amazingly there are no Chinese-, Arabic-, or Indian-language planispheres addressed; perhaps future editions will rectify this. Section 3 (pp. 68–71) presents many interesting booklets and their matching planispheres. Section 4 (pp. 74–81) is most delightful as it pictures several unusual devices related to planispheres. All this is rounded out by an index on pages 83 and 84. The volume is a delight to thumb through, read the descriptions, and enjoy the crystal-clear photographs.

I hope that the path this book follows is that of a different guide from another hobby of mine — I collect plastic model kits: airplanes, ships, *etc*. The first guidebook issued many years ago was under 100 pages and featured a few-hundred items. After several years and seven editions it grew into a volume of several-hundred pages featuring thousands of items. The author, John Burns, has sadly passed away but his more than 20 years of hard work has left behind an enduring legacy that is sought out by every kit collector in the world. With each successive edition more information was unearthed and it grew phenomenally. I sincerely encourage Mr. Grimwood to pursue this pathway. I assure you I will purchase every new edition. One other added suggestion is that a narrative of several pages in length be included outlining the history of the planisphere in detail. — Leonard Matula.

The Race to the Moon Chronicled in Stamps, Postcards, and Postmarks: A Story of Puffery vs the Pragmatic, by Umberto Cavallaro (Springer), 2018. Pp. 338, 24 × 17 cm. Price £24·99/$34·99 (paperback; ISBN 978 3 319 92152 5).

Most of us enjoy a good race and the 'Moon Race' in the 1960s to our nearby satellite was a gem. The precursor was the USSR's launch of *Sputnik*, but the real starting gun was in 1961 April when Yuri Gagarin circled the Earth in *Vostok 1*. To say that the USA was somewhat shocked by the revealed abilities of their communist cold-war rivals was to put it mildly. The USA collectively opened the purse strings and rolled up their sleeves. President John F. Kennedy pledged to land a man on the Moon and bring him home safely to Earth by the end of the decade. For the next ten years it was the 'free world' *versus* the communists. And interestingly, for the majority of this period the Americans thought they were trailing.

There were radical differences between the two approaches. The Soviets were secretive. For many years the name of the man in charge, Sergei Korolev, was not revealed. Also the public was not told the location of the launch site (Tyuratam later renamed Baikonur in Kazakhstan). Missions were not announced in advance, only being revealed if they were deemed successful. The cosmonauts were not trusted to manoeuvre their space craft. The military and the politicians, who controlled the system, were worried that cosmonauts might try and land outside the USSR. A cosmonaut was regarded as just 'a man in the can'. The American approach was completely different. Their skilled test-pilot astronauts were active participants, peasant proletarian backgrounds being unimportant. The USA also publicised everything in advance. For example, millions watched the failure of the Vanguard launch in 1956 December on TV.

Umberto Cavallaro has done two things, and done them very well indeed. First his book provides us with an informed, detailed, and beautifully written insight into the workings of two completely different political, managerial, and engineering approaches to a similar problem. Much is made of the USSR's documentation revealed after *glasnost*. And I was also touched by the quoted transcript of Richard Nixon's speech, the one that was to be used in the event that *Apollo 11* astronauts had not managed to get back to Earth. Then we have the philatelic side of things. This provides the book with a fascinating and colourful array of illustrations. And again, we see two completely different approaches to the use of postage stamps. To the USSR they were items of propaganda, ways of bragging to the world about successes. By the end of the 1960s the Soviets had issued 160 different 'space' stamps, the USA only five. The approach to first-day covers was completely different too. All the Russian ones were 'fakes', only produced well after the event, not postmarked in a relevant post-office, and only manufactured if the mission had been successful. In America John Glenn's three orbits in the Mercury capsule resulted in 10 290 850 commemorative stamps being sold on one day, 1962 February 20, out of a total issue of 289 million. Not surprisingly Apollo astronauts could not get life insurance so stamps came to the rescue. The crews signed illustrated envelopes posted on the day of launch, and these were to be sold by the families if the worst came to the worst.

The Soviets won the stamp race, Yuri Gagarin appearing on 605 stamps world-wide as opposed to Neil Armstrong only making 536. But the USA won the race to the Moon and this year we are celebrating the fiftieth anniversary of that famous 1969 July foot-step. I can think of few better ways of celebrating this anniversary than reading this enthralling book. There is not a dull page in it. It might even tempt you to buy a few stamps. — DAVID W. HUGHES.

Gerard P. Kuiper and the Rise of Modern Planetary Science, by Derek W. G. Sears (University of Arizona Press), 2019. Pp. 368, 23·5 × 16 cm. Price $45 (about £35) (hardbound; ISBN 978 0 8165 3900 0).

Dutch-born Gerard Peter Kuiper (1905–1973) was an energetic astronomer who will be remembered as a first-rate observer, theoretician, editor, and administrator. As the founder of the Lunar & Planetary Institute in Arizona in 1960 (and its *Communications*) he injected new life into ground-based studies of the planets, and provided critical support for early space missions. The *Kuiper Airborne Observatory* and a prominent bright-rayed crater on Mercury are just two ways in which his name is perpetuated. His chance discovery of the methane component in the atmosphere of Titan in 1944 was to change the direction of his research and eventually to lead to the modern discipline of planetary science.

As discussed in this biography, it seems reasonable to assert (as H. C. Urey repeatedly did) that Kuiper did not always give due credit to prior discoveries, and this is particularly evident in the case of the Edgeworth–Kuiper belt. Kuiper appointed his staff as much for their individual skills as for their academic qualifications, and a number of his most successful co-workers did not possess university degrees. But they did not always get full credit or exposure. At Kuiper's request, A. P. Lenham wrote a detailed history of planetary observation for inclusion in the classic book *The Solar System* edited by Kuiper & Middlehurst, but that excellent essay (the typescript of which was shown to me by Lenham) was squeezed out of the book by pressure of space due to the inclusion of additional material on planetary satellites. We have to balance these negative points against Kuiper's other achievements that constitute such a remarkable record, from Solar System studies to white-dwarf stars.

This biography is very well written, looks really authoritative, and never fails to maintain an engaging narrative, covering the many incidents in Kuiper's private and public lives. We read of his bravery in scientific intelligence operations in the closing months of WWII. We hear of his excitement in accessing wavelengths beyond the visible spectrum. We learn how he stayed for long periods on the mountain during his years at Lick Observatory without descending, due to chronic car sickness.

The book's matt paper nicely suits the numerous and valuable portraits that constitute the majority of illustrations. However, with planetary photographs the result has been unhappy, and there are incorrect captions on page 108. There are over 50 pages of valuable notes and references to support the text.

In summary, this comprehensive biography is to be welcomed and can be warmly recommended. — RICHARD MCKIM.

Probes of Multimessenger Astrophysics: Charged Cosmic Rays, Neutrinos, γ-Rays and Gravitational Waves, 2nd Edition, by Maurizio Spurio (Springer), 2018. Pp. 591, 24 × 16 cm. Price £79·99/$109·99 (hardbound; ISBN 978 3 319 96853 7).

Maurizio Spurio's *Probes of Multimessenger Astrophysics* is, I have just discovered, a very useful book! It is aimed "(*i*) at those undertaking postgraduate courses, (*ii*) PhD students, (*iii*) post-doc researchers involved in high-energy physics or astrophysics research, and senior particle physicists eager to understand and appreciate the mechanisms of the largest accelerators in the Universe." This 2018 2nd edition is a prompt update of the 2015 original, driven by assorted recent gravitational-wave events reported by *LIGO* and *VIRGO* (the author is Italian), though each gets only two index entries. The references (mostly, deliberately, to review articles, which the author says are often easier to understand than original research papers) are given chapter-by-chapter. There are no end-of-chapter problems, let alone answers, so *Probes* is not quite designed as a stand-alone textbook.

But there are lots and lots of numbers on graphs and in tables and equations that start at the beginning (a VERY good place to start), with $F = q_1q_2/r^2$ and an explanation of why these Gaussian units make sense when one is trying to understand the Galactic magnetic field, cosmic-ray acceleration (to keep up the Milky Way's 8×10^{54} erg supply), *etc.* There are two beautiful spectra of the

Crab Nebula, a photo of a neutrino detector being lowered into Lake Baikal, and a periodic table colour-coded with the hot-off-the-presses view that all of the *r*-process nuclides in the Milky Way come from mergers of pairs of neutron stars, based on one event, for which the gamma-ray fluxes from individual elements are not resolved.

The index is very good, on both entities ('unobtainium' is there, and isospin, and the Larmor radius, just when I needed it) and people (Kaluza, Klein, Kelvin, Kepler, Kerr, Kohlhörster, Koshiba, and Kuzmin among the K's alone). To save your Google finger, I will confess that Kohlhörster belongs to the history of cosmic rays, Koshiba to neutrino detection, and Kuzmin is the K of the GZK cutoff in the cosmic-ray spectrum caused by interaction with the cosmic microwave background radiation. But also here (p. 216) are pulsar radiation mechanisms; there are the categories of TeV gamma-ray sources (the largest single set is still that favourite "unidentified", p. 324), and many other things you might want one of these days!

WIMPs creep in near the end, and we are assured that "Many important events probably occurred in the Universe's evolution at $t = 10^{-35}$ s", just after we have been invited to calculate how many protons are likely to decay in the Kamiokande detector per year. About one, it turns out, comparable with the number of serious books as good as this one I've reviewed this year. Target (*iv*) is clearly "folks trying to teach this stuff!" — VIRGINIA TRIMBLE.

The Oxford Handbook of the History of Modern Cosmology, edited by Helge Kragh & Malcolm S. Longair (Oxford University Press), 2019. Pp. 626, 25 × 17·5 cm. Price £95 (hardbound, ISBN 978 0 19 881766 6).

This book, edited by a leading historian of cosmology and a leading astrophysical cosmologist (or cosmological astrophysicist), is a survey of modern cosmology, though one could argue that almost all of the history of cosmology which is directly relevant to current cosmology falls within the timespan covered; in other words, the neglect of earlier works is mostly irrelevant for those interested in modern cosmology and its history. Kragh also wrote three chapters and Longair three-and-one-half. Five additional authors (slightly more historians of science than scientists, though of course most are both to some degree) wrote a chapter each, and one has a chapter of his own and one co-authored with Longair.

Although the preface mentions "approximately 1860" as the beginning of modern cosmology, the first chapter, on pre-Einsteinian ideas (Einstein's 1917 cosmology paper[1] is often deemed to mark the beginning of modern cosmology), actually starts considerably earlier, with Immanuel Kant and his *Theory of Heaven*, before moving on to Olbers' paradox, the rise of astrophysics, and ideas on the thermodynamics of the Universe (the last of which had little input from astronomy). It carries on to non-Einsteinian ideas developed and/or still current after 1917, such as Charlier's hierarchical universe and pre-relativity versions of curved space and the cosmological constant. The second chapter starts at around the same time and covers observational cosmology up until about 1940; principal topics are of course the establishment of the size of the Universe and the fact that that size is increasing. The third chapter covers in considerable detail the core of the history of modern cosmology, namely the development during the 1920s and 1930s of cosmological models based on General Relativity; the usual suspects Einstein, de Sitter, Friedmann, Lemaître, Eddington, Lanczos, Weyl, Milne, McCrea, McVittie, Robertson, Walker, and Tolman are all here.*

In cosmology, the history of the subject is perhaps more entwined with the subject itself than is the case with other fields. As such, it makes sense to include two chapters on dead ends, namely various alternative theories, with an entire chapter devoted to the Steady State theory. Chapter 6 brings the history of observational cosmology up to 1980: cosmological parameters, Big Bang nucleosynthesis, source counts (and hence evidence for cosmic evolution), discovery of the CMB, structure formation. Astrophysics and advances in (instead of applications of) GR played a larger role during that time than before 1940; so an additional chapter on relativistic astrophysics and cosmology is appropriate. Chapter 8, by Bruce Partridge, is an excellent review of the history of CMB research.

The ninth chapter, on technology, tests of GR, and the Cold War, seems somewhat out of place: too little cosmology and too much emphasis on the USSR. While not without its uses — it is a good capsule summary of Soviet astrophysics and cosmology during the Cold War — it is too imbalanced, too off-topic, and the language quality is noticeably worse than in the other chapters (except Chapter 12). (While one usually cannot expect perfection from a non-native speaker — or even from most native speakers — one should expect more from a major English publisher and two editors, one a native speaker, fluent in English.) It also concentrates on observational cosmology, whereas Soviet scientists were probably more important as theoreticians. Chapter 10 brings observational cosmology up to 2018; like Chapters 6 and 7, it was written by Longair, so there is good continuity. The main themes are the impressive refinement in the accuracy and precision of the measurement of cosmological parameters and the advances in the understanding of the formation of galaxies and large-scale structure. In both cases, larger telescopes and substantial improvements in detector and computing technology played a significant role. Chapter 11, co-authored by Longair and Smeenk, is a bridge between the previous and later chapters: inflation, dark matter, and dark energy are extensions to the simpler cosmological models of yore (though dark matter dates back to Zwicky or even Oort and the cosmological constant played a major role in the very first paper on relativistic cosmology[1]).

Chapter 12 is somewhat different in tone than the others, since it is very much a description of work in progress (the multiverse, string cosmology, and physical eschatology). Future historians will probably see the material covered in the previous chapters in much the same way as we do now; it is too early to tell which of the fashionable themes of today will have lasting value; perhaps one or more of them will suffer the fate of the Steady State theory: a dead end, but interesting in some respects, and important because of its influence on the field at the time. The somewhat more speculative topics mentioned above have been developed at the same time that traditional cosmology, which had long lived with orders of magnitude, holds entire conferences on differences of a few per cent in the measurement of cosmological parameters. In some sense, cosmology today is similar to particle physics of a century or so ago: we know the basic components and the characteristic numbers describing them, but it is not clear why those numbers and not others. Perhaps there will be progress in cosmology similar to that in QCD which led to an understanding of the mass of hadrons.

*More detail is given by Barrow[2], while an excellent synopsis is given by Harwit[3], both reviewed in these pages[4,5].

The final chapter, on philosophical aspects of cosmology (uniqueness, underdetermination of theory, origin, anthropic reasoning, the multiverse) is a good summary of the topic, though the author shows perhaps a bit too much personal bias. A criticism in my review[6] of Ryden's textook[7] also applies here: while most of the book is up to date, progress in the understanding of the flatness problem has been largely ignored. That is even more puzzling here, since the author thanks Marc Holman in the acknowledg(e)ments, but mentions the latter's recent thorough investigation of the flatness problem[8] only as an afterthought.

Of course, with such a broad scope, no topic can be considered in full detail. Nevertheless, some get too short a shrift. Apart from the flatness problem, I was a bit surprised that cosmological models with small-scale inhomogeneities and their effect on light propagation (thus potentially affecting the redshift dependence of observational quantities, the very basis of observational cosmology) get only a single paragraph. Moreover, while the summary is essentially correct though a bit confusing, it is also stated that the effect on the angular-size distance is greater than on the luminosity distance, but (at least in all cosmological models considered in the book, and more besides) the effects must be essentially the same due to Etherington's reciprocity theorem[9]. I recently counted about 500 papers on that topic, so it is not some small detail. (Perhaps I noticed those two topics because I had worked on them both[10,11], but I rather suspect that I chose to work on them because they had been neglected.)

There are no serious factual mistakes, but the old canard that John Wheeler coined the term 'black hole' is repeated (he didn't, but did popularize it[11]); there are two different ordering schemes for NASA's Great Observatories; a caption mentions colours in a black-and-white figure; while GR is notorious for differing notation schemes, I had never before seen '$G_{\mu\nu}$' instead of '$R_{\mu\nu}$' in the Einstein field equations (though of course '$G_{\mu\nu}$' is a common abbreviation for '$R_{\mu\nu} - \frac{1}{2}g_{\mu\nu}R$')*; Geoffrey Burbidge is wrongly listed as the first author of the famous B^2FH paper (in the text; it is correct in the bibliography). Apart from those goofs, and some sloppy editing, the book is excellent: thorough enough for a good introduction, but accessible to non-experts. More than 63 pages of references allow the reader to pursue any topic in detail. There are no footnotes (the better option for supplementary information) but each chapter has endnotes. Those for Longair's and Smeenk's chapters, though most also contain references, are typical supplementary-information notes. Partridge's are similar, but are sometimes only references. In other cases, the notes are often just references, *e.g.*, "Eddington (1923)", but then one has to consult the bibliography for the full reference; in-the-text citations would have been more convenient. While sometimes necessary to avoid ambiguity, *e.g.*, "addressed by Zeldovich and Dashevsky (Dashevsky and Zeldovich, 1964, Zeldovich, 1964b)", in other cases the references could have been more compact, *e.g.*, "George McVittie argued ... (McVittie, 1965)" instead of the standard "George McVittie (1965) ...".

There are a handful of black-and-white photos and diagrams throughout the book. The copious references are followed by a subject index and an author index (each seven pages of small print). Though somewhat too large for a literal handbook, it is still compact enough to be read almost anywhere, which I highly recommend. — PHILLIP HELBIG.

*To be sure, Einstein himself used '$G_{\mu\nu}$' instead of '$R_{\mu\nu}$', but with the equations formulated in terms of the Laue scalar rather than in the usual form.

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Einstein and Heisenberg: The Controversy over Quantum Physics, by Konrad Kleinknecht (Springer), 2019. Pp. 185, 24 × 16 cm. Price £24·99/$37·99 (hardbound; ISBN 978 3 030 05263 8).

Author Konrad Kleinknecht has written parallel biographies of Albert Einstein and Werner Heisenberg with some, but not exclusive, emphasis on the relatively brief period in the 1920s when they interacted about — and disagreed about — quantum mechanics. Heisenberg was the younger by more than 20 years, in contrast with Erwin Schrödinger, born only nine years after Einstein, leading to interactions more extended in time and perhaps more equal (see *Einstein's Dice and Schroedinger's Cat*, reviewed here: **135**, 356, 2015). Perhaps most interesting are the territories on which the author disagrees with other Einstein biographies.

He is of the opinion (*i*) that Einstein's daughter with Mileva Marič, Lieserl, survived her bout with scarlet fever, was adopted and died as Frau Marta Zolg in Biettingen, Germany, in 1960; (*ii*) that Einstein, despite having sworn "never again" when he left Germany in fact returned in 1952 June and visited the Castle Museum, Budingen (Hesse); and (*iii*) that Einstein fathered a second daughter and fourth child in 1942, "stemming from his relationship with a nightclub dancer in New York." I cannot know whether any of these is true. I wasn't there. But neither was Kleinknecht.

The saddest passage is perhaps a 1944 May letter Heisenberg wrote to Carl Friedrich Bonhoffer (a physical chemist) saying, "I greatly fear suffocating at work in the next few years. If I survive the war, I'll have perhaps ten years left in which I can hope to take an active part in science … . We have all lost so many years because of the war." I don't think I ever heard (or read) any of the young American Manhattan project physicists express a similar sentiment, but at least one US Navy Lt. Commander definitely wanted to resign his commission in 1948 to go to graduate school, while he still might be able to contribute productively to physics research. And he was born in 1919.

The number of 'misspeaks' is very large. It is hard to know where the author has been ill-served by his translator (clearly so for calling X-rays and gamma-rays "energy rich") and where he simply got something wrong (*e.g.*, the glossary definition of uranium, saying "the element with 146 neutrons in its nucleus (uranium 238) is stable").

The arrangement of topics is odd. The current state of quantum physics and relativity comes in the middle (up to gravitational waves and MRI) as "Impact of the Discoveries", followed by World War II; Heisenberg thinking that America had no class distinctions could perhaps be counted as an honest mistake; the author crediting fission exclusively to Hahn and Strassmann (where is Lise Meitner?) can claim no such excuse.

Nonsense is perhaps the kindest word that can be applied to an attempt to explain the Schwarzschild solution to Einstein's GR equations: "Schwarzschild found a solution to this problem in the context of gravitation theory. But the theory broke down for the innermost layers of the Sun; there was no solution within 3 kilometers from the center." If this structure had auditioned for the role of our Sun, it would have been told "Don't call us. We'll call you."

The lists headed 'oops', 'no', 'something missing', *etc.*, are all long. The 1919 eclipse expeditions were not equipped by the BAA; the Einsteins did not divorce in 1915; credit for orbits of stars around the Galactic central black hole should be shared by Reinhard Genzel and Andrea Ghez (surprise, only the German researcher is credited).

I am not sorry I read this book (it was an official *Observatory* review copy), but won't need to tell you that I didn't like it very much if I end with another quote, this from page 101: "The flowering of Leipzig physics … (was) then brought to an end as the Jewish members of the group turned their backs on Germany." Who turned whose backs on whom?? — VIRGINIA TRIMBLE.

Principles of Space–Time–Matter: Cosmology, Particles and Waves in Five Dimensions, by Paul S. Wesson & James M. Overduin (World Scientific), 2019. Pp. 274, 23·5 × 15 cm. Price £85 (hardbound; ISBN 978 981 3235 77 9).

Paul S. Wesson, whose obituary (written by the second author) appeared in these pages[1] a while back, is the posthumous co-author of a new book which summarizes the main topic of his life's work. The first eight chapters had been essentially completed by Wesson before his death; the second author wrote a concluding chapter, which is a short summary, and a biographical appendix. Wesson was certainly an interesting character: from a working-class Nottingham background, he became a doctoral student of Martin Rees at Cambridge (after Hoyle, Narlikar, and Wickramasinghe had left due to a dispute with Ryle; Wesson had approached all four as potential supervisors), and had published 50 papers and a book by the time he was 30. After research trips to Canada before and after obtaining his doctorate, he spent some time in Norway, where, together with Rolf Stabell, he concentrated on another important topic in his career, namely extragalactic background light (*i.e.*, a quantitative investigation of Olbers' paradox, arriving at the correct conclusion — still not appreciated by many — that the resolution is due mainly to the finite age of the Universe and not to its expansion), married a Norwegian woman, and became so proficient in Norwegian that he used it for his annotations of books and articles. In 1980, he became assistant professor at the University of Alberta; he was to stay in Canada for the rest of his life, moving to Waterloo in 1984. Until the end, he focussed on science while retaining a serious interest in a wide range of other topics.

Wesson's science is, for most, probably more difficult to understand than his life. Although he wrote on many topics, a five-dimensional extension to General Relativity (with a non-compact fourth spatial dimension) was the main topic of his career. The first eight chapters of the book, starting with an overview of standard GR, describe the extended theory and its applications to cosmology, astrophysics, electromagnetism, and so on. The main idea is that a 4-dimensional (three of space and one of time) universe with matter and energy can be explained as a consequence of a 5-dimensional universe with neither. Easier said than done: the description here, though thorough and well written, is very mathematical, and takes appreciable effort to digest. Rather than a geometrization of physics or a physicalization of geometry, the goals are a unification of fields with their sources and of gravitation with electromagnetism, similar to the goals of Einstein in his search for a unified field theory.

Space–Time–Matter theory hasn't taken the community by storm. Although there are some experimental tests, such as a predicted violation of Lorentz invariance*, the magnitude cannot be predicted from first principles, and observations indicate that any deviation must be very small, enhancing the feeling that a possible violation is probably not 'just around the corner'. (It also would not immediately follow from observed Lorentz invariance that Space–Time–Matter theory, rather than some other explanation, were correct.) It is also not clear whether, as usually assumed, the fifth dimension is a spatial dimension; it could also be a dimension of time, or associated with spin, or mass (*i.e.*, displacement along the fifth dimension could give rise to mass in a geometrical analogue of the Higgs mechanism). In addition, even enthusiasts admit that there are problems: the nature and role of a new scalar field is not clear; there is no theory of fermionic matter, particle spins, *etc.*; descriptions of induced 4-D matter sources are macroscopic. As such, Space–Time–Matter is perhaps similar to string theory, MOND[2], shape dynamics[3], and loop quantum gravity[4] in that, while there might be something to it, it is clear that it is not yet a complete theory. (Also, apart from MOND, those ideas all suffer from a lack of easily testable predictions.)

While the editing is above average, there are still a few typos and so on. The main text contains a handful of black-and-white diagrams while the biographical appendix contains many photos, most in colour. Each chapter (except the last, which is a summary) begins with an introduction and ends with a conclusion followed by references. The references in the appendix are to all of Wesson's work. There are neither footnotes nor endnotes and the book ends with a six-page index. The only factual mistake I noticed is a wrong parenthetical description of the Hubble law, a common mistake[5–8].

According to the second author[9], there are two target readerships. One is those who are already interested in Space–Time–Matter; for those, the book is a good and well-written overview of the work of Wesson and his collaborators. The other is those with a general interest in extensions of standard physics; for them, accessibility is strongly dependent on the reader's technical background, though the good structure of the book and copious references (including many to work by more-mainstream physicists on related topics) make that possible for those willing to invest some time. — PHILLIP HELBIG.

*No deviations are expected from Solar System tests or from inflation. In principle, the m–z relation might be able to distinguish a 5-D-inspired dark energy from other forms, and more accurate experiments in the style of *Gravity Probe-B* could perhaps detect a predicted anomalous precession. Also, at some level, there should be a violation of the equivalence principle. None of those tests is likely to be performed in the foreseeable future, so while the theory is testable in principle, it isn't in practice, at least not yet.

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Principles of Gravitational Lensing: Light Deflection as a Probe of Astrophysics and Cosmology, by Arthur B. Congdon & Charles R. Keeton (Springer), 2018. Pp. 287, 24 × 16 cm. Price £69·99/$99·99 (hardbound; ISBN 978 3 030 02121 4).

This book sets out the theory of gravitational lensing, the bending of light that is a consequence of Einstein's general theory of relativity. The book covers some of the underlying physical theory, taking some shortcuts in places to make it accessible to students without familiarity with General Relativity. The main focus of the book is on presenting analysis of systems with certain symmetries, which allows in many cases an analytic treatment. These are mostly but not exclusively concerned with the simple case of a source and a single-lens system somewhere along the line of sight. The reader will find many examples of increasing complexity that will help build insight into what can be a complex subject to navigate. Even relatively simple configurations can give rise to a rather rich set of phenomena, and these are covered very nicely. There is some discussion of cosmic shear and of lensing of the cosmic microwave background radiation, but these topics are not covered in great detail. The readership for this will probably be undergraduates who are undertaking project work in lensing, or graduates who are beginning doctorates in strong gravitational lensing or microlensing, for which this will be an excellent and readable resource. — ALAN HEAVENS.

Dispatches from Planet 3, by Marcia Bartusiak (Yale University Press), 2018. Pp. 304, 21 × 13 cm. Price $26 (about £20) (hardbound; ISBN 978 0 300 23574 6).

Many of us know Marcia Bartusiak as author of a number of outstanding non-technical books in astronomy and its history, including *The Day We Found the Universe*, *Einstein's Unfinished Symphony* (the pre-2015 search for gravitational radiation), and *Black Hole*. It now turns out that she was, at the same time, producing dozens of short articles for *Natural History* and other magazines on everything from comets to the multiverse. *Dispatches* is a collection of 32 of these articles. 'Added value' in this hardcover version includes a lot of references back to primary and secondary literature and a fine index with 194 people and a comparable number of other entries. Divide that 194 randomly into eight groups and you would expect eight pairs sharing birthdays. Nonetheless, it came as a surprise to find John Michell sharing December 25 with Isaac Newton.

A couple of other 'a ha!' moments describe Cecilia H. Payne (later Gaposchkin) having been pre-conditioned to look for the patterns she found in stellar spectra (leading to the realization that stars are made mostly of hydrogen and helium) by her early interest in systematic classification of plants, and Charles Ellis being the one who collected data on missing energy in beta decays that led

Wolfgang Pauli to postulate neutrinos. Another remarkable thing about Ellis (besides your reviewer never having heard of him before) is that he had his first physics lessons from James Chadwick in Germany while they were both prisoners of war (WWI) there.

Bartusiak does a great job of describing the origins of the astronomical use of the phrase 'black hole' (descending probably from Robert Dicke, though indeed *via* John Wheeler). She confirms one of your reviewer's prejudices by saying that "careful choice of one's parents and social class" are very important in giving women (but really everybody) a fighting chance of succeeding in science.

Of the 50 pictures, nearly half are not the ones you usually see of famous people, objects, and experiments. Strangest perhaps is a rendering of a black hole evaporating by Hawking radiation in the form of a dozen out-of-control bed-springs. But it comes from an apparently aged chapter that leaves the impression that evaporating mini-black-holes are a likely candidate for short gamma-ray bursts.

Credit or the opposite is occasionally misplaced. Osiander, not Copernicus, wrote the preface to the latter's *De Revolutionibus*. Knut Lundmark in 1930 was the first to use the phrase '*dunkle materie*' in describing the properties of galaxies, and tabulate its ratio to '*leuchtende materie*', not Fritz Zwicky. His ratios ranged from 6 for M33 to 100 for M81.

Still, the solid information greatly outweighs those slips and a few others, and you should have no hesitation in recommending *Dispatches* to friends and relatives who might want some idea of what you think is important about the Universe. And I will trust in the truth of the statement that Georges Lemaître heard from Odon Godart, his successor at Leuven, about the discovery of the cosmic microwave background and its meaning a few days before his death from leukaemia on 1966 June 20.

Conflict-of-interest statement: my copy of *Dispatches* was a review copy sent to Another Journal by the publisher. — VIRGINIA TRIMBLE.

Cruise Ship Astronomy and Astrophotography, by Gregory I. Redfern (Springer), 2018. Pp. 343, 23·5 × 15·5 cm. Price £22·99/$29·99 (paperback; ISBN 978 3 030 00957 1).

My first reaction when I saw this book was what can you write about this topic that requires 343 pages? Presumably, there were would be a good deal of imagery taken aboard various cruise ships illustrating what one can do. There would also be advice on how to take pictures from a moving platform which seems to conflict with the need for a stable platform to take good photographs. So what else might you need to cover?

The author of this book, a former US Navy Navigator, is a Special Interest Lecturer with extensive experience of delivering educational material to large numbers of guests aboard a range of cruise lines. He has interacted with thousands of what he describes as "shipmates" and has seen a whole host of attempts to capture the night sky, eclipses, and general sky phenomena in ways both good and bad. This has given him a unique insight into the seaborne astrophotographer.

The book is very well illustrated with imagery that ranges from black-and-white photographs that look more like an electrocardiogram to some excellent deep-sky imagery. There are also some interesting chapter titles such as 'Big Bang to Homo Erectus to Multi-Messenger Astronomy'; having read it, I still can't say I really understand the title despite the author's promise. I have to confess that I have never been on a cruise ship, but I think I have a better idea of what life is like on one of those vessels and how to cope with photography aboard ship.

The book contains nineteen chapters and is laid out in two distinct parts. The first part consists of 13 chapters and goes under the heading of 'Cruise Ship Astronomy'. It covers a wide range of topics including what to pack, how to enjoy the night sky, and a selection of different atmospheric phenomena you can observe at sea. The second part of the book covers the business of taking photographs at sea. All of the content is in a user-friendly format taking full advantage of the author's five decades of experience, and includes ten rules of thumb for conducting successful astrophotography at sea.

I think this is quite an interesting book and I would recommend it to someone who is about to try the cruising experience for the first time and wants to capture the beauty of a total eclipse of the Sun or the night sky at sea. It is inexpensive and can be fairly described as a go-to resource for the cruise-boat astrophotographer. — STEVE BELL.

THESIS ABSTRACT

TURBULENCE AND TRANSPORT IN STARS AND PLANETS

By Adam S. Jermyn

In this dissertation I have argued that the study of stars and gaseous planets has relied too heavily on simplifying assumptions. In particular, I have demonstrated that the assumptions of spherical symmetry, thermal equilibrium, dynamical equilibrium, and turbulent anisotropy all hide interesting phenomena which make a true difference to the structure and evolution of these bodies.

To begin I developed new theoretical tools for probing these phenomena, starting with a new model of turbulent motion which accounts for many different sources of anisotropy. Building on this I studied rotating convection zones and determined scaling relations for the magnitude of differential rotation. In slowly-rotating systems the differential rotation is characterized by a power law with exponent of order unity, while in rapidly-rotating systems this exponent is strongly suppressed by the rotation. This provides a full characterization of the magnitude of differential rotation in gaseous convection zones, and is in reasonable agreement with a wide array of simulations and observations.

I then focussed on the convection zones of rotating massive stars and found them to exhibit significantly anisotropic heat fluxes. This results in significant deviations from spherical symmetry and ultimately in qualitatively-enhanced circulation currents in their envelopes. Accordingly, these stars ought to live much longer and have a different surface temperature. This potentially resolves several outstanding questions such as the anomalously slow evolution of stars on the giant branch, the dispersion in the observed properties of giant stars, and the difficulty stellar modelling has to form massive binary black holes.

In the same vein I examined the convection zones of bloated hot Jupiters and discovered a novel feedback mechanism between non-equilibrium tidal

dissipation and the thermal structure of their upper envelopes. This mechanism stabilizes shallow radiative zones against the convective instability, which would otherwise take over early on in the planet's formation as it proceeds to thermal equilibrium. Hence tidal dissipation is dramatically enhanced, which serves to inject significant quantities of heat into the upper layers of the planet and causes it to inflate. This mechanism can explain most of the observed population of inflated planets.

Finally, I studied material mixing in the outer layers of accreting stars and developed a method for relating the observed surface chemistry to the bulk and accreting chemistries. This enables the direct inference of properties of circumstellar material and accretion rates for a wide variety of systems. — *University of Cambridge; accepted 2018 July*.

A full copy of this thesis can be requested from: adamjermyn@gmail.com

Here and There

AND ARGUMENTS ABOUT ITS ACCURACY

The [*New Horizons*] flypast of Ultima Thule may … help resolve the mysteries of the Big Bang 4·6 billion years ago and arguments about its cause. — *The Times*, 2019 January 2, p. 23.

NOT IN THE RIGHT VEIN

Despite being on the bleeding edge of astronomy, exoplanet science is not just the domain of professional astronomers. — *Astronomy Now*, 2019 February, p. 54.

ASTRO(MIS)NOMER

The beginnings of an answer came in a landmark 1957 paper written by Caltech astronomer Margaret Burbidge and her husband George, along with Hoyle and another prominent scientist, William Fowler. — *Los Angeles Times*, 2019 February 23, p. B2.

PRESUMABLY A RATHER SLIM VOLUME

An introduction to the subject of solar physics … by the same author, which covered most of the subject of solar physics (except for the solar interior and surface) published in 2004 … . — *New Millennium Solar Physics*, by M. J. Aschwanden (Springer), 2019, preface.