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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2021 December 10 at $16^{h}\ 00^{m}$

EMMA BUNCE, *President*
in the Chair

The President. Welcome, everybody, to the final Ordinary Meeting of the RAS during 2021 and I'm pleased to see people are now joining for today's programme. As always, it's set to be a good one, and we have two great speakers this afternoon. First of all, just the usual housekeeping. This meeting is taking place *via* webinar, and if you look at the top left of your screen you will see a small green shield, and that symbol means you're using the most up-to-date version of Zoom and that it is secure. And I also do need to advise you that this meeting is being recorded. As always, after the presentations, you can ask questions, but as you will be muted I would ask you please to use the chat facility which is found at the bottom of your screen and then your questions will go to the panellists. And today those questions will be read out by a member of Council, our 'A' secretary, Dr. Sheona Urquhart, so thank you to Sheona for joining us this afternoon.

The President. I'm delighted to introduce Dr. Cassandra Hall who is the recipient of the Winton 'A' award. Dr. Hall is an Assistant Professor of Computational Astrophysics at the University of Georgia, USA, and she is primarily using simulations to interpret and understand observational data. Dr. Hall earned her doctorate at the University of Edinburgh in 2017 and worked as a postdoctoral researcher at the University of Leicester and then became one of two people in the inaugural class of Winton Exoplanet Fellows from 2018. That is a programme which supports outstanding postdoctoral researchers working in exoplanet research. Dr. Hall relocated to the University of Georgia in 2020 where she now leads an Exoplanet and Planet Formation research group. She was awarded the University of Georgia's Lilly Teaching Fellowship in 2021 for dedication to teaching excellence with a commitment to equitable practices in teaching. Today, Dr. Hall is going to be speaking to us about 'Planet formation, substructure, and gravitational instability in protoplanetary accretion discs'.

Dr. Cassandra Hall. ALMA is located on the Chajnantor plateau in the Atacama Desert in northern Chile. It is a radio telescope, essentially a giant millimetre/submillimetre array whose antennae have revealed to us a whole new level of planet formation. There are about 400 billion stars in our galaxy and about 400 billion galaxies in the Universe, which means that there are about

10^{23} exoplanets in our Universe, if, statistically speaking, there is on average at least one planet around every star, and we know this is the case from exoplanet-search missions such as *Kepler*.

We are now at a point where we can not only search for exoplanets, but also search for them as they are forming around the host stars, and this is in some sense equivalent to looking back at the formation of our own Solar System four billion years ago to try and work out what happened, although it is happening today. Looking at these systems we can try to answer some of the biggest questions that humanity has asked, such as how did life form. Earth is special — nothing else in our Solar System is really like it. What were the conditions that allowed us to form — why do we have so much water, which is an essential ingredient for life? These questions are really important in understanding the place of humanity in the Universe. How did life begin on Earth? If we can search for signs of prebiotic chemistry molecules such as formaldehyde, for example, which are precursors to some of the essential chemicals needed for life, this may help us understand how life makes the leap from inert to self-replicating molecules. Recent surveys, for example with *ALMA*, have shown that planet-forming discs are teeming with the molecules needed to build complex biological compounds. And perhaps the biggest question, philosophically and theologically for society, is are we alone in the Universe? This is an important and impossible question to answer until we have conclusive evidence of life elsewhere.

The first step is to try to quantify how many small rocky planets are out there, and then try to understand how many of them may be anything like Earth. With the launch of new space missions such as *JWST*, we may find some answers that go some way towards helping us quantify the probability of life elsewhere in the Universe. There are many planets for which there is no Solar System analogue — there are planets many times the size of Jupiter closer to their host star than Mercury is in our own Solar System. We have lava worlds that orbit the host star in a few hours, with temperatures in the thousands of Kelvin, so there is this huge amount of diversity. One of the most pressing questions is how do we explain this diversity, how do we explain the huge variety that we see in the exoplanet population? What is it that is driving these differences? Exoplanets formed in protoplanetary accretion discs. The disc determines the most fundamental of exoplanet properties which is the mass, since the mass basically decides if a planet is going to be small and rocky like Earth, or large and gaseous like Jupiter, but also vital is the distance from the host star which determines the surface temperature. So exoplanets form in these protoplanetary accretion discs. The conditions inside these discs decide the most fundamental properties of these exoplanets, but we have a time-scale problem. The accretion discs, when we see them, are generally very young, most about a million years old and sometimes significantly less, but fully formed Solar Systems, such as our own, are around a billion years old, so the question here is what happens in the middle? To figure this out, we need computational simulations and models of protoplanetary accretion discs. The evolution and formation of planets and their environments happens on longer time-scales that we can observe. If we want to understand the diversity in the exoplanet architecture, then we need models of planet formation and accretion discs in order to do so.

Moving on to gravitational instability (GI) in protoplanetary discs. Accretion discs are ubiquitous in astrophysics — they are found on every rung of the cosmic ladder, all the way from active galactic nuclei down to accretion discs surrounding planets, and they exist because they formed from a collapsing cloud that has some latent angular momentum due to velocity anisotropy. Angular momentum must be conserved during the collapse, and this is the only shape that conserves this angular momentum. What happens in these accretion discs is that mass must move inwards to form the central protostar, but in order to conserve angular momentum, angular momentum must also be re-distributed outwards to allow mass to flow inwards. This happens through some torque — a torque is just a rate of change of angular momentum. The purpose of an accretion disc is for mass to move inwards so that the central protostar can grow.

We have built up a classification of protoplanetary discs, based on spectral energy distributions that correspond to the evolution of the disc. They begin as a collapsing cloud of gas before becoming more and more disc-like as they accrete the majority of their envelope, before finally becoming debris discs which stop reprocessing any light into longer wavelengths once most of the envelope is gone. These things, then, are really evolving objects. Over the course of their lifetime they shift a lot of mass onto the central protostar. The surface-density profile of these objects evolves over time, due to internal angular-momentum transport which can only come from a torque, or rate of change of angular momentum. In order to have this torque, we need some sort of viscosity.

One of the biggest problems in accretion-disc physics is what is the origin of this viscosity, basically as a turbulence that drives the angular-momentum transport. One possibility is turbulence from gravitational instability. This is set by the Toomre parameter for GI; the Toomre parameter needs to be less than about 2 for GI to set in. Pressure comes from a temperature sound speed stabilizing shorter-wavelength perturbations, but you also have rotation stabilizing longer-wavelength perturbations, and all of this is acting against gravity.

For a more massive disc, if you want to have it settled into a quasi-steady state where the spirals are continually replenishing and ebbing, you can have more efficient cooling and more dramatic spirals, to remain in this balanced state with the heating. On the other hand, if your disc is less massive you will have weaker heating and therefore require weaker cooling in order to remain balanced, and then you will have weaker spirals. A good question is what happens when these objects are no longer quasi-steady, and heating and cooling stop being balanced? So we have here a shearing-sheet simulation, where you look at a very small region of the disc, pick a fiduciary point that is co-rotating with the bulk of the disc, expand your equations of motion in first order, and solve for the perturbations. It just allows you to look at this deviation from the bulk motion so it's really just local conditions in the disc. What is interesting is that if the disc cools on a time-scale that is more rapid than it can collisionally heat itself up, it's going to collapse and fragment and form gravitationally-bound objects,

Now moving on to simulations and observations. As I just mentioned, if a disc cools faster than it can collisionally heat itself up, it will undergo fragmentation into gravitationally-bound objects. One of the key questions here is, can this fragmentation mechanism be a planet-formation pathway? We investigated this by running a suite of simulations, and what is interesting is that, to some extent, fragmentation can be a stochastic process. All of these discs qualitatively had identical initial conditions, they had the same mass, same surface-density profile, same temperature profile. The only thing that differed was something called a random-number seed used to initialize the placement of the actual fluid elements. That means there's just a very slight change in the noise distribution and the pattern and the underlying conditions, so qualitatively they are identical, quantitatively very slightly different but this is only really due to noise. What is really interesting is despite the identical physical initial conditions, qualitatively these simulations had different outcomes, although they all fragmented. They formed different numbers of fragments of varying mass and also at varying separation from the host star. We took these simulations and compared them to some population-synthesis models. These models make simplifying assumptions about the physics so that we can run hundreds of thousands of them. Generally speaking, we parameterize the disc into one dimension, for example, so that we can run many of them for a long time.

What we find is that we preferentially form fairly massive objects, typically the Jeans mass and then higher, often far away from the central star — these things will be scattered out to hundreds of AU and even further. A really good question to ask is, when does this fragmentation happen, when is it likely? So far there is not a huge amount of data for objects undergoing fragmentation right now, but it certainly seems to be possible at two extremes, for example, around 40-solar-mass stars, and also around one-solar-mass binaries.

So this begs the question, when does fragmentation happen? A PhD student at the University of Edinburgh, James Cadman, led a study to look at this by looking at simulations. He ran a suite of simulations that all have the same disc-to-star mass ratio. This means that the kind of gravitational stresses in the disc will be similar across the scales but, as we increase the central mass of the object, we hold steady the disc-to-star mass ratio, so in all cases it's 50%. We find that fragmentation is very heavily favoured around higher-mass stars, even for the same disc-to-star mass ratio.

One of my favourite systems is HR 8799 and we have observations covering seven years taken with the *Keck* telescope. The central object is a 1·5-solar-mass star, and based on the previous results for the same sort of disc-to-star mass ratio is more likely to undergo fragmentation. What is really interesting about this system is that you have really massive planets five to seven times the mass of Jupiter, forming very far away from the host star. In the classical core-accretion paradigm of planet formation, where planets form slowly through coalescence from grains to pebbles, and so on, it's very difficult to form these objects. There shouldn't be enough refractory material this far away from the star, but also it's an issue of time-scale. Gravitational instability offers an explanation to solve this paradox for very massive planets very far away from the central star, and this is consistent with observations more broadly. For example, the *VLT NACO* large programme was combined with an additional twelve surveys for a sample of 199 FGK stars in total, and they found that in this sample between 1% and 8·6% of the sample contained a planet that was consistent with having formed through gravitational instability at a 95% confidence level. What this tells us is that despite GI not being common, it is still statistically consistent with it being the dominant planet-formation mechanism for massive systems such as HR 8799.

At 1·6 microns, and in scattered light, you are only seeing the surface features about three scale heights above the mid-plane, whilst the disc images in the millimetre continuum band show beautiful spirals, so could be these be due to gravitational instability? We find that we can explain the spiral morphology in these observations by using gravitational instability but there are some strict caveats that must be met. The disc needs to be massive, the spirals must be compact, and the accretion rate must be high. The disc mass is something that is really difficult to pin down. We often make assumptions based on continuum observations which probe the dust, and then we make some assumptions about dust-to-gas mass ratio.

One of the problems that we have been faced with is the morphological similarity between gravitational instability and planets. Both gravitational instability and planets can explain spirals; if we could pin down the disc mass, we would be able to rule out one or the other but this is very difficult to do, so morphologically they are too similar, the disc mass is highly uncertain, and we need another way.

The system HD 97048 is an object that was found to have a planet through kinematics, and what happens is the spiral wake of the planet perturbs the gas velocity radially inwards and radially outwards at the location of the planet, and these spiral density waves are also present throughout the disc, but because the disc is viscous they appear washed out as you increase around in azimuth, so they're really strongest at the location of the planet. What is interesting here is looking at the simulations of gravitational instability. There appears to be no single point at which the velocity is perturbed, they are GI spirals throughout the disc, so we took the simulation and ran a radiative-transfer calculation to get molecular-line channel maps. What we found was something called the GI wiggle — this is a very strong signature of gravitational instability. It is a velocity signal, so you have to look at it using using gas, in this case it's ^{13}CO. It is present at all azimuths, and it's also independent of viewing angle and it was robust to rotation as well, so it's a really good signature to search for. But have we detected this GI wiggle? The answer is yes: in the $C^{18}O$ $J = 3–2$ transition of the Elias 2-27 system we very clearly see this GI wiggle, so it's really nice that we've had observational confirmation of this prediction from simulations.

Now I would like to look towards the future and to think about probing planet formation with the *SKA*. The earliest stage of planet formation is grain growth. If we want to look at the population of pebbles which are roughly centimetre-size grains, we need to be observing at wavelengths of 2–3 cm or so. I've been involved in the Cradle of Life working group for the *SKA*, and the goal of the group is to understand planet-forming conditions at longer wavelengths. The *Square Kilometre Array* is going to be the world's largest radio telescope — it's going to have thousands of dishes and millions of low-frequency antennas with science observations predicted to begin in 2028.

In this work we have presented the first predictions of what the *SKA* will see when observing planet-containing or planet-forming discs. With planets you get these spirals that you can see in the gas. What tends to happen in the dust instead is you get a ring-like structure, because the dust becomes trapped at local pressure maxima. If you're observing different wavelengths — if you're looking at micron wavelengths you would just be looking at the dust that is well coupled to the gas, you're going to see a very different morphology at these different wavelengths.

Something else that we hope to do with the *SKA* is to search for prebiotic molecules which are the precursors to life. Formaldehyde is one such molecule, and from simulations involving chemical networks we expect these things to be present forming fragments, and from recent *ALMA* observations we see that these these molecules are teeming in discs, but we can't specifically look for formaldehyde at those wavelengths. This is something that we want to search for using the *SKA*. Formaldehyde is a main precursor to nucleic acids, nuclear basis sugars, and amino acids, it's really one of the fundamental building blocks of life.

So to summarize, understanding why the known exoplanet population looks the way it does is one of the most interesting open problems in astrophysics, and our best bet is to understand the environments in which they occur, which means we need to do simulations of planet formation. Protoplanetary discs are the site of exoplanet formation, so if we want to understand them as fundamental properties, we need to understand protoplanetary discs. Gravitational instability may be the dominant source of turbulence and the reason that discs evolve at all at the earliest stages of a disc lifetime, so it's likely to be very important. GI is unlikely to form most planets but may form most of the very massive planets. We can now determine if spirals are due to gravitational instability thanks to discovery of this GI wiggle, now we have confirmation of this. Looking to the future, *SKA* has really promising capabilities for the earliest stages of planet formation and also for searching for prebiotic molecules.

The President. Thank you very much for that wonderful presentation, and for sharing your work with us. This afternoon I'm going to hand over to Sheona to see if there are any questions coming in, either by the Q&A or the chat.

Dr. Sheona Urquhart. Hi, thanks Emma, and also thank you Cassandra, that was a wonderful talk, but to be honest, we don't have any questions and I think that's because you presented it all so well and so clearly. If our attendees don't have any questions, do our panellists want to ask anything?

The President. I have probably a very naïve question. You mentioned measuring the mass of the accretion disc and how challenging that is. I suppose my question is how could that be improved or what is it that has to happen in order to help improve that estimate?

Dr. Hall. That's a great question. Trying to make inferences from the continuum emission involves making assumptions about the dust-to-gas mass ratio, which is not great because we do not know what this ratio is. Getting gas observations directly is better, but this is often hindered by foreground contamination or contamination from the envelope, and a new technique that seems pretty promising is using gas data to get rotation curves and fitting the rotation curves, including both the gravitational potential for the self-gravity, the capillary, and component, and the radial pressure component as well. Doing that, we've actually been able to recover the disc mass for the Elias 2-27 system, for example. And so higher spatial and spectral resolution, for these, is probably the best way forward.

Dr. Urquhart. Actually, we do have a question from Stanley. Hopefully this is a simple one for you to answer. "You mentioned the coalescence of pebbles. On what time-scales do the pebbles form from the gas?"

Dr. Hall. That's a great question, and the answer is uncertain. You know, traditionally in the core-accretion paradigm it takes millions of years but in some cases there's new evidence that other things happen, such as streaming instabilities. In this case, it could be much more rapid. The exact time-scale would depend on the properties of the disc, like the local gas density and what the local dust density is at the time that these things occur, so it's quite uncertain, and it's one of the things that is a very active area of research.

Dr. Urquhart. Thanks for that fantastic talk, Cass, and as we're running a little bit late we will move on to our second speaker.

The President. Our second speaker, Dr Ziri Younsi from UCL, is part of the team who received the RAS Group Award for Astronomy and is part of the *Event Horizon Telescope* team. So just by way of introduction then, Dr. Younsi is currently a UKRI Stephen Hawking Fellow in astrophysics at University College London. Previously Leverhulme Early Career Fellow at the same location, he has been working with the Event Horizon Telescope Collaboration since 2014. Dr. Younsi has been developing and performing supercomputer simulations of black holes and horizon-scale black-hole imaging, enabling comparison with and interpretation of observational images of black holes. He is one of just three UK scientists on the *Event Horizon Telescope* team and is a co-recipient of the National Science Foundation Diamond Achievement Award, the 2020 Breakthrough Prize for Fundamental Physics, and of course the RAS Group Award 'A'. So without further ado, I'm going to hand over to Dr. Younsi who's going to be speaking to us about the 'First image of a black hole from the *Event Horizon Telescope*'.

Dr. Ziri Younsi. It's a genuine pleasure for me to be giving this talk at the Royal Astronomical Society on behalf of the Event Horizon Telescope Collaboration (EHTC). As both a member of the team, and on behalf of the team, I would like to start by saying thank you very much for this award. It means a lot to us and we are very appreciative of this honour and recognition of our work.

I'm tasked with presenting an overview of a very broad project that spans many different areas of modern astrophysics. My background is as a theorist, working on theoretical calculations and computational simulations of how black holes behave, and how matter and radiation move around them. As such, I'll be approaching the topic from that perspective.

A computer-simulated image of the accretion flow around a supermassive black hole is very bright and asymmetric and there are many turbulent features. By the end of this talk I hope to convey a sense of what these black-hole images mean, and what we can learn from them.

The data used to produce the famous M 87 black-hole image were recorded at seven different telescopes across the globe in 2017. These data led to the publication of a series of six papers in a special issue of the *Astrophysical Journal Letters* in 2019 April. Today's discussion will touch on three of these papers, focussing on the meaning of the shadow image and its connection to General Relativity (GR), as well as the theoretical work underpinning the interpretation of this image and the constraint we were able to impose on the black hole's mass, independent of previous indirect measurements and through using strong-field GR.

In imaging a black hole, one must first ask oneself what a black hole is. In particular, it is important to understand how it interacts with its environment. Astrophysical black holes arise as a vacuum solution to Einstein's field equations of GR. Black holes are also incredibly 'simple' objects, and may be characterized (in GR) in terms of just three parameters: their mass, their spin, and their electric charge. The latter parameter is negligible for supermassive black holes since we anticipate that even if a black hole were to be imbued with some electric charge, it would rapidly reach charge neutrality with the surrounding plasma. As such, they are wholly characterized by their mass and spin.

There are several important regions around a black hole, the first of which is the eponymous event horizon, which is a defining feature of a black hole and delineates its boundary. Anything that crosses its boundary is causally disconnected from the rest of the Universe and forever trapped. We therefore cannot see beyond the event horizon since even light cannot escape. Close to the event horizon is the photon sphere, where light rays circulate around the black hole multiple times on unstable orbits. Moving further out, there is the innermost stable circular orbit of particles, which defines the innermost radius around the black hole where matter can orbit around the black hole and form a steady accretion disc. This is important because the accretion disc is the source of radiation that we seek to detect from the black hole. Since the black hole traps light and cannot be 'seen' directly, one instead seeks to resolve spatially the matter producing radiation at the edge of the event horizon, thereby imaging the silhouette of the black hole — this is the goal of the *EHT*.

So far, what I have presented is a rather simplified picture, since the extreme gravitational field of the black hole can severely bend light propagating in its vicinity. What one actually observes in black-hole images is very different to what one is accustomed to seeing. Considering the simplest possible approximation of isothermal matter isotropically distributed around a non-rotating black hole, the resulting image shows a dark central circle surrounded by bright emission that decays in intensity as the inverse square of the distance from the black hole. This dark circle is the 'shadow' of the black hole, and represents the lensed image of the unstable photon orbit, and can be thought of as the capture cross-section of the black hole itself. We expect all black-hole images to present this central dark shadow feature when viewed at frequencies where the accreting plasma is optically thin, *i.e.*, at the characteristic *EHT* frequency of 230 GHz.

Whilst there were several calculations of the 'shadow' boundary curve, and even of objects orbiting a black hole, it was Jean-Pierre Luminet who calculated the first proper direct image of a black hole in 1978, appearing in a French popular-science magazine prior to its journal publication in 1979. This was an image of a geometrically thin and optically thick disc of matter in the equatorial plane of a black hole, accounting for the gravitational lensing of light and the brightness asymmetry induced by Doppler effects. However, it wasn't until the advent of supercomputer simulations of magneto-hydrodynamics in GR that the highly turbulent, non-linear, and dynamical properties of the accreting plasma around a black hole could finally be studied.

The accreting plasma in the vicinity of the event horizon is extremely hot and luminous, owing to the presence of free electrons gyrating around the ambient magnetic field in the accretion disc and (bipolar) jets, emitting copious synchrotron radiation in the form of radio waves. It is this synchrotron radiation produced close to the event horizon that the *EHT* is designed to detect.

When constructing a telescope that can resolve the angular size of a distant (candidate) supermassive black hole, the resolution is effectively proportional to the size of the telescope and the inverse of the wavelength of the light received. One can attempt to increase the size of the dish, whilst also decreasing the observing wavelength. Engineering limitations restrict increasing the size, and atmospheric opacity and other dispersive processes limit the wavelengths accessible from ground-based telescopes. To provide a sense of scale, at perigee the Moon subtends roughly half a degree on the sky, whereas the diameter of the black hole in M 87 subtends 40 μas (*i.e.*, roughly one ninety-millionth of a degree). The *EHT* array in 2017 achieved a resolution of roughly 25 μas, resolving structure on the scale of the event horizon. This is akin to resolving with the naked eye, from here on Earth, an orange on the surface of the Moon. The EHTC achieved this through the technique of very-long-baseline interferometry (VLBI).

With this black-hole-image data in hand, the EHTC constructed a simulation library of several hundred thousand images of the potential state of the matter around the M 87 black hole at the time the data were recorded. Many parameters, including the black-hole spin, the magnetic state of the accretion disc, the size of this disc, the thermodynamic properties of the radiating electrons, the inclination angle of the observer, and the position angle of the source were allowed to vary. Whilst it was found that spin is not easily constrained, the central bright annulus surrounding the event horizon is a robust feature in all images, and the position angle and asymmetry of the peak image flux is consistent with what is observed. Assuming GR to be the correct theory of gravity, and taking the known distance to M 87, one obtains a constraint on the black-hole mass of 6·5 ± 0·7 billion solar masses, which is consistent with both previous stellar kinematics and gas-dynamics constraints. It is important to note that this new constraint on the mass was enabled by the presence of the 'photon ring', whose size and shape is only weakly dependent on the black-hole spin and observer orientation, and is a direct consequence of strong-field gravitational lensing of light emitted in the vicinity of the event horizon.

This first image of a black hole marks the beginning of an exciting new era in the study of astrophysical black holes and plasma physics in strong gravity. In the near-future the EHTC hopes to present the first images of the supermassive black hole in our Galactic Centre, Sagittarius A*. In the next few years, greater angular resolutions will be achieved through a combination of pushing to shorter observing wavelengths, expanding the number of telescopes on Earth, and optimizing their placement. In the years to come, we hope eventually to place telescopes in space, thereby enabling much higher-frequency observations, continuous source monitoring, significantly enhanced angular resolutions due to the much larger baseline separations, and even dynamical movies of the accretion onto supermassive black holes.

Please let me conclude by saying, on behalf of the Event Horizon Telescope Collaboration, thank you very much.

The President. Thank you so much, Ziri. That was really fantastic. I'm sorry you've had some issues with your slides. It seems we have technical gremlins all round today, but you've communicated a really difficult subject in such a clear way and I really appreciated the sort of visualisations and the explanations to go with it, of that image. I've learned a huge amount, so thank you very much for that talk.

Dr. Urquhart. We've had a couple of questions. The first one is from Phil Diamond, and he asked, "When might we see an image of another black hole?"

Dr. Younsi. That is the million-dollar question. I'm not at liberty to say, but what I can say is very soon.

The President. We have had a comment saying "two brilliant presentations". I think it's really good to pass that on and I totally agree. Hugh says "you'll never think of an orange in the same way again". The comparison with *Voyager I*'s position, I thought that was really helpful in giving some context. There is a question and it is, "How did Luminet create the first image of the M 87 black hole? What kind of mathematical methods did he use?"

Dr. Younsi. That's an excellent question. This was the late 70's. It has been well understood how geodesics, trajectories of particles, or mass-less models like photons, are actually mathematically defined in general, as there's an equation that you can solve called the geodesic equation of motion. What he did was to solve this equation of motion for light rays and he put a black hole down then took a simple thin disc of matter that has a particular emissivity profile and fired lots and lots of photons at this disc. A little bit like the image I showed earlier on in my talk, he was able to calculate this by running each particular light ray, which constitutes the pixel in the image that you saw on the supercomputer. I know it took many hours per pixel and he used India ink, I think, on Canson and negative photographic paper or plate even, and there were eight of the negatives of this so I think this took many days to weeks to actually complete, and it was published in a popular-science magazine in Paris in 1978 and subsequently in a journal in 1979. This was actually just an image of a generic black hole; one of the things that I neglected to mention in my talk was that because black holes are so simple, there's a scaling variance. If you double the mass of the black hole, you double its size, so whether it's a stellar mass black hole or supermassive black hole as far as that shadow is concerned, it always looks the same.

The President. I'm just going to ask one more quick question from Mike Edmonds: "Will there be quantitative data on the magnetic fields?"

Dr. Younsi. Thank you for the question. The short answer is, we already do have some quantitative data on the magnetic fields. It wasn't something I had a chance to touch on today, but we have published two papers in April this year, on the polarization of the light that we detected in M87 from the same 2017 data and what we found. Briefly, it's something very interesting about the topology and orientation of the magnetic field, as well as its strength, for the M87 accretion disc. Magnetic-field information gives you a means to constrain the topology of the magnetic field and by doing this, what we found was that it's mostly in a toroidal configuration, so a lot of that magnetic field is swirling around the black hole and there's a beautiful image which I wasn't able to show today where you can actually see the magnetic field almost being dragged onto the event horizon, the black hole. And what's more exciting about this for me, as a physicist and theoretical astrophysicist, is that with these data, you don't just get one parameter, total intensity, you actually get four Stokes parameters. You can determine things like linear and circular polarization, and so on. You can actually constrain the models that we have from these simulations I showed you, in these libraries, much more strongly, and you can make some very nice statements about the state of the matter around the black hole. It's almost certainly in what we call a magnetically-arrested state; it has a very high magnetic flux onto the event horizon. This is important because it explains things about variability in the time-scale over which these things happen, and I could go on and on. Please, check out those papers, they're really great papers.

The President. I think we had better draw this to a close this evening. It's been really fantastic to hear from both of you today. Thank you so much for your talks. To close the meeting, then, I give notice that the next monthly A&G meeting of the Society will be on Friday the 14th of January 2022. Apologies again for the technical issues at the beginning of the meeting and I just wanted to thank you all for coming today and for your continued support for the RAS throughout the year. It's hard to believe that we're at the end of another calendar year and still meeting virtually. I just want to wish you all a very pleasant and relaxing festive season. Please look after yourselves and I wish you all the best for 2022 and I hope to see you in London at some point next year.

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EMMA BUNCE, *President*
in the Chair

The President. Welcome to the first ordinary meeting of 2022 — it's great to see lots of people joining us for today's programme. And as people continue to join, I will go through our now-familiar on-line housekeeping before we start the meeting. So, as we all know, this meeting is taking place *via* webinar. If you look at the top left of your screen, you should see the small green shield, and that symbol means that you are using the most up-to-date version of Zoom and that it is secure. I do also need to advise you that this meeting is being recorded.

As always, questions can be asked at the end of the presentation, but as you will be muted, please can you use the Q&A facility which you should find at the bottom of your screen and your questions will go only to the panellists and will be read out by a member of the editorial team, Dr. Louise Alexander.

Before we go to the presentation, I would like to announce the 2022 RAS awards. The first award to announce is an Honorary Fellowship 'G' which goes to Dr. Ashwin Vasavada from NASA Jet Propulsion Laboratory. An Honorary Fellowship 'G' also goes to Professor Kathryn McWilliams from the University of Saskatchewan. A further Honorary Fellowship 'G' goes to Rob and Cathryn Wilcock from Winchcombe. An Honorary Fellowship 'A' is awarded to Dr. Morton Roberts from the National Radio Astronomy Observatory. The James Dungey Lectureship goes to Dr. Licia Ray, at Lancaster University, and the Gerald Whitrow Lecture goes to Professor Pedro Gil Ferreira from the University of Oxford. The George Darwin Lecture goes to Professor Alan Fitzsimmons from Queen's University, Belfast, and the Harold Jeffreys Lecturer will be Dr. Rhodri Davies from the Australian National University. The Group Achievement Award 'A' goes to the EAGLE simulations team made up of project leads from Durham University, Liverpool John Moores University, and Leiden University. The Group Award 'G' goes to the UK Fireball Alliance led by the Natural History Museum. The Winton Award 'G' goes to Tim Lichtenberg from the University of Oxford, and the Winton Award for Astronomy goes to Dr. Rebecca Smethurst from the University of Oxford. The Fowler Award for Geophysics goes to Dr. Beatriz Sánchez-Cano from the University of Leicester, and the Fowler Award for Astronomy goes to Dr. Matt Nicholl from the University of Birmingham. The Service Award for Geophysics goes to Professor Farideh Honary, from Lancaster University, and the Service Award for Astronomy goes to Professor Donald Kurtz from the University of Central Lancashire and the Northwest University in Mahikeng, South Africa. I'm now going to announce the RAS Education Awards: our Primary award goes to Rachael Wood from Southfield Primary School; the RAS Education Award for Secondary and Further Education goes to Jacob Harding from Sharples School; and the Higher Education Award goes to Professor Melvin Hoare from the University of Leeds. The Annie Maunder Medal is awarded to Nicolas Bonne, Jennifer Gupta, and Coleman Krawczyk from the University of Portsmouth. The Jackson-Gwilt Medal goes to Dr. Frank Eisenhauer from the Max Planck Institute. The Price Medal goes to Professor Hrvoje Tkalcic from the Australian National University. The Chapman Medal goes to Professor

Sandra Chapman from the University of Warwick. The Eddington Medal goes to Professor Alan Heavens from Imperial College London. The Herschel Medal goes to Professor Catherine Heymans from the University of Edinburgh, and this year's Gold Medal for Geophysics goes to Professor Richard Horne from the British Antarctic Survey. The 2022 Gold Medal for Astronomy is awarded to Professor George Efstathiou from the University of Cambridge. Many congratulations to all of our winners.

We will now continue with our programme for this afternoon and I'm delighted that we are welcoming Dr. Karen Aplin from the University of Bristol who's going to give the James Dungey Lecture this afternoon. Dr. Aplin is Associate Professor in Space Science and Technology at Bristol University and Visiting Professor at both the University of Reading, Department of Meteorology, and the Department of Electronic and Electrical Engineering at the University of Bath. She previously worked at the University of Oxford Department of Physics and the Space Science and Technology Department at Rutherford Appleton Laboratory. Her research programme seeks to understand and exploit electrical properties of atmospheres through novel instrumentation and experiments. I'm delighted to introduce Dr. Aplin's lecture entitled 'The charge of the spheres, sparking studies of the planets.'

Dr. Karen Aplin. [It is expected that a summary of this talk will appear in a future issue of *Astronomy & Geophysics*.] [Most of us have felt awe and wonder when experiencing — preferably, from a safe distance — the majesty of a thunderstorm. As well as its spectacular displays, lightning is a hazard, and affects atmospheric chemistry. It is associated with specific types of cloud and meteorological processes, as well as being implicated in the origins of life on Earth. For these reasons, lightning has long been seen as a significant phenomenon. However, there is more to atmospheric electricity than just lightning: its quieter and less well-known sibling exists in every planetary atmosphere as a continual flow of ions and electrons, and can form a global-scale electrical circuit with lightning acting as a 'battery'. The iconic *Voyager I* mission was the first to photograph an extra-terrestrial thunderstorm at Jupiter in 1979. Since then, lightning has been detected on most other Solar System planets.

On Earth, small currents away from thunderstorms can affect clouds, interact with particles from dust or pollution, and even potentially influence the weather. Similar non-thunderstorm processes may also act in other planetary atmospheres, such as Titan and Venus. The speaker discussed the unifying scientific background and context for the study of atmospheric electricity, and described past, present, and future observations.]

The President. Karen, thank you so much for a fascinating and really informative lecture — I've enjoyed it very much. I just wanted to say how sorry I am and how sad it is to hear the news of Don Gurnett. I was fortunate enough to work with him on the *Cassini* mission to Saturn, as did many of my colleagues in this community, so our thoughts are with his family and colleagues and friends.

I'm going to hand over to Louise now to ask any questions. I think we have some questions coming in from the audience.

Dr. Louise Alexander. Yes we do. We had a question towards the start of your talk from an anonymous attendee: "Are the intensity and wavelength of lightning discharges related to atmospheric pressure, temperature, and density?"

Dr. Aplin. If you imagine an atmosphere in its calm state, the lightning heats up the air and causes a local pressure wave, which is why we have thunder.

Lightning is just a big spark which dominates the atmosphere locally, so it's not directly affected by the background conditions.

Dr. Alexander. AC is asking: "Gamma and X-ray emissions have been detected on Earth emanating from lightning discharges. Have similar signals also being detected from other planetary objects, and what would the consequences be for such phenomena?"

Dr. Aplin. That's a really good question. Lightning, gamma-rays, and X-rays have been detected from thunderstorms on Earth. I don't think they've been detected at any other planets. That's a bit difficult to do because you need a radiation detector and you would also need a lightning detector. You need to know that it is lightning and it's hard to say exactly what the consequences would be, but there would certainly be ionization from the gamma- and X-rays, and sometimes they affect the lightning initiation, the way the lightning is ramped up. I think it would be interesting to work out where you would want to do that. I would suggest Jupiter or Saturn, because you've got lots of lightning, but you have to be inside the atmosphere to detect the gamma or X-rays, which might be hard, but that's an interesting question.

Dr. Alexander. Christopher would like to know: "What happens to charge on planets with no atmosphere. Is this a consideration for lunar exploration?"

Dr. Aplin. Yes it is. I stuck to atmospheres today, but you get a lot of electrostatic charging on the Moon and other places such as asteroids. I've done a little bit of work on places without atmospheres. They have a lot of charging from sunshine from the UV because atmospheres often screen out the UV light. As far as the charging of lunar dust is concerned, it's known to be very fine and so can get highly charged. I think it's been a problem for the astronauts who've been to the Moon, and I think it's something people need to bear in mind for future missions, definitely.

Dr. Alexander. Thank you, I don't think we have any more questions. I did just want to ask about fulgurites. I was quite interested in them: is there a chemical signature, or are they just identified from the texture?

Dr. Aplin. I don't know if there's a chemical signature, I think it's just like fused silica. I'm not a geologist but I bet there are some in the audience who are, so I don't want to embarrass myself, but it's a lot of heating and it can locally melt the sand and then it solidifies and they make quite a characteristic shape. I think there was a facility that was doing lightning testing of aeroplanes and I believe they work with an artist to make some fulgurites for an art exhibition. I think more is known about them now that they have been made artificially for quite a long time.

Dr. Alexander. An anonymous question now: "Is it possible that there are interesting electrical phenomena occurring in other planets that we do not see on Earth?"

Dr. Aplin. Yes, I'm sure there are but I don't know what they are! I started with Earth and it turns out that Earth is quite a good model because a lot of the phenomena it has also takes place in the Solar System. But yes, this new mushball stuff on Jupiter, we just don't see that on Earth because on Earth almost all the clouds are made of water, and the idea that you can have lightning in clouds that aren't made of water is a really big finding in atmospheric electricity. I'm sure that opens up a lot of possibilities for understanding things like where does the lightning on Neptune and Uranus come from? There'll be more and more as we get more missions and find out more hints about what's happening here.

Dr. Alexander. And Garth would like to know: "Are sprites a significant source of ionization for the D layer, the lowest layer of the Earth's ionosphere? Can they initiate atmospheric gravity waves or other travelling disturbances?"

Dr. Aplin. This is slightly outside my detailed technical expertise, and again, I'm sure that there are people in the audience who will know much more than me about this. I don't think sprites can be a significant source of ionization, particularly in the daytime, because you get UV from sunlight doing a lot of ionizing. I'm not sure also about the gravity waves, sorry.

Dr. Alexander. No, that's fine. Thank you very much. I think I will hand back to Emma now, but thank you very much for a wonderful talk.

The President. Perfect timing, thank you so much and thank you for answering the questions, Karen. It just remains for me to close the meeting this afternoon. Thank you all — as always — for joining us, and congratulations again to our award winners for 2022 and to Dr. Karen Aplin for giving the 2021 James Dungey Lecture. Finally, I give notice that the next monthly Ordinary Meeting of the Society will be on Friday the 11th of February, 2022.

RECEIVED WISDOM

By Phillip Helbig

Science is a self-correcting process. While a consensus implies a majority, what is consensus is not determined by majority vote *per se*, but rather by the majority being convinced (rightly or wrongly!) that something is correct. In some cases, that represents a reversal of an earlier situation. I briefly review several cases in cosmology (and astrophysics) in the last hundred years or so where the received wisdom at a particular time turned out to be wrong, leading to a new consensus. Whether that will happen for two relatively new topics remains to be seen.

Introduction

At least in some cases, there had been a real consensus among people who had thought about a problem, but it later turned out that that consensus had been wrong. I discuss some examples below. Many more details can be found in the references; the point here is not to give a detailed review, but to call attention to several topics which are similar in this respect. Also, my main point is to call attention to those episodes, rather than to explore the reasons involved (though I do comment on them briefly), which were certainly not all exactly the same. As my late history teacher used to say, "Just an observation, not a judgement".

Of course, scientists don't create theories in a vacuum and then see which are contradicted by random experiments. Although eventually a stable consensus is reached, that can be achieved by numerous routes. (I think of that as similar to the no-hair theorem for black holes: the final state is very simple and independent of a possibly very complicated past.) During such earlier phases, both the choice of theories and the choice of experiments depend on many non-scientific factors. The simple view is that a theory in contrast with experiment is falsified[1]. That is true as long as one can be sure that all aspects of the experiment are correct. In practice, while theories cannot be proved correct, only disproved, our faith in them can increase (or our Bayesian prior can be strengthened) if they continue to hold up in the light of new data and experiments. In such a case, the first, correct, reaction to an experiment which appears to disprove a theory is often to check the experiment. That was what Eddington meant by "it is also a good rule not to put overmuch confidence in the observational results that are put forward until they are confirmed by theory". While often used to caricature Eddington as an ivory-tower theorist (who also wrote "well, these experimentalists do bungle things sometimes"[2]), the full quotation is more interesting:

> Observation and theory get on best when they are mixed together, both helping one another in the pursuit of truth. It is a good rule not to put overmuch confidence in a theory until it has been confirmed by observation. I hope I shall not shock the experimental physicists too much if I add that it is also a good rule not to put overmuch confidence in the observational results that are put forward until they have been confirmed by theory.[3]

So it is a mixture of new data and prior belief which forms opinion. At least in part, prior opinion is based on previous data, so a contradiction between theory and experiment could falsify the theory, but it could also mean that the experiment is wrong. If the new experiment tests a previously untested prediction, then the situation is not immediately clear. If it is similar to previous experiments which agree with theory, then most would probably conclude that the new experiment is wrong, or at least should be checked before abandoning the theory (and the previous experiments, for which it needs to explain why they agree with the theory while the new one doesn't). Healthy scepticism is good, which is what Sagan meant by "extraordinary claims require extraordinary evidence"[4].* Shapley noted that one should be sceptical of observations: "No one trusts a model except the man who wrote it; everyone trusts an observation, except the man who made it." However, he also pointed out that "[t]heories crumble, but good observations never fade."

The combination of data and prior belief (also based on things which have a history of working well in science, such as Occam's razor and even a sense of beauty) is what is important. Of course, science can never absolutely confirm anything, only rule it out, but some ideas can be believed in with high confidence, and some of those can turn out to be wrong. That is an honest mistake, as opposed to holding on to a debunked idea or falsified theory for too long for irrational reasons.

Olbers' Paradox

I say little about this old chestnut here, since it has been discussed in detail by Harrison[6–15](yes, *that* Harrison[16]), including a chapter in his excellent cosmology textbook[17] (Chapter 24) and even in an entire book[18] on the topic; see also works by Wesson *et al.*[19] and Wesson[20]. Suffice it to say that the first person to glimpse the proper solution (our Universe is not old enough for it to have been filled with light) to the traditional riddle of why the sky is dark at night appears to have been Edgar Allan Poe[21] and that in this case there was not *one* consensus for a long time but rather various explanations had support

*Others had said something similar before Sagan; O'Toole[5] traces similar quotations from various people (including Pierre-Simon Laplace) back to 1708.

at various times. Some of the explanations were wrong even according to the state of knowledge at the time. For example, absorption by dust cannot explain Olbers' Paradox because the dust would heat up to be as bright as whatever is heating it. Other explanations, such as the hierarchical Universe of Charlier[22,23] (an idea, also in the context of a solution to Olbers' Paradox, going back to Lambert[24]), are valid solutions, but do not apply to our Universe. (Note that I am referring here to the traditional Olbers' Paradox. We now know that most of the photons in the Universe are in the cosmic microwave background (CMB). In that case, the expansion of the Universe explains why the CMB is no longer optically bright, as it is redshifted by a factor of 1000 or so. Expansion also plays a role in the traditional Olbers' Paradox, but is not the dominant effect[19,20].)

The cosmological constant

Whether, as claimed by Gamow[25,26], Einstein actually said that the cosmological constant was the biggest blunder of his life and, if so, what he meant by that is not completely clear[27–30]. However, it is certainly clear that he concentrated on models without a cosmological constant after the discovery of the expansion of the Universe, the most popular being the Einstein–de Sitter model[31]. At least with regard to the question of spatial curvature, though, the Einstein–de Sitter model, which has neither a cosmological constant nor spatial curvature, was a practical approximation for the data available at the time, not intended as anything more than a provisional model[32], though Einstein's belief that the cosmological constant is zero was certainly stronger. Even though Einstein was often wrong in his later years (see below), and even though the 'biggest blunder' story might not be true, his influence is partly to blame for the neglect of the cosmological constant from about 1930 to about 1990, although it was never completely out of fashion (*e.g.*, ref. 33). It was certainly true that observations weren't good enough to detect the cosmological constant until the advent of modern supernova cosmology, making use of the classic m–z relation[34,35], though there had been hints a few years before (*e.g.*, refs. 36–38). Of course, a (nearly) flat universe (perhaps inspired by inflation) and density parameter $\Omega_0 < 1$ ($\Omega = (8\pi G\rho)/(3H^2)$ where ρ is the density, G the gravitational constant, and H the Hubble constant; 0 refers to the current value because, in general, it changes with time) implies a positive cosmological constant; Peebles[39] found much earlier that such models fit the observations well. Some preferred to leave it out if possible to make things simpler, while others, noting that Nature uses all degrees of freedom available, preferred to leave it in with a value to be determined observationally. (Finding a value to be 0 usually indicates that there is some reason, such as a previously unknown conservation law.)

Departures from the Einstein–de Sitter model usually involved a smaller, but sometimes larger (*e.g.*, ref. 40), value for the density parameter Ω_0, usually with the (dimensionless) cosmological constant $\lambda_0 = 0$ ($\lambda = \Lambda/(3H^2)$, where Λ is the cosmological constant). That was probably due to the lack of enthusiasm for the cosmological constant, owing to the fact that observations weren't good enough to detect the value we believe that λ_0 has today, and perhaps also because calculations are easier than in a flat model ($\Omega_0 + \lambda_0 = 1$) or a non-flat model with (of course) $\Omega_0 > 0$ and $\lambda_0 \neq 0$. In the $\lambda_0 = 0$ case a spatially closed universe ($\Omega_0 > 1$) implies a universe which will collapse in the future and *vice versa*; the terms 'open universe' and 'closed universe' were sometimes used to refer to one of those aspects, sometimes the other, sometimes both. More general cases require more precise terminology.

Even after evidence which is now seen to support the idea of a positive cosmological constant had become available, for a while it was possible to find alternative explanations. For example, an early argument in favour of $\Lambda > 0$ was the combination of $\Omega_0 = 0{\cdot}3$ or so and a nearly flat Universe (motivated by inflation); such a Universe would also be old enough to accommodate the oldest objects within it without an unreasonably low value for the Hubble constant. However, some argued that Ω_0 had been underestimated and/or that the increase in the measured value with scale could be extrapolated all the way up to $\Omega_0 = 1$ (corresponding to a flat Universe without a cosmological constant). The combination of better observations for traditional measurements and new tests, such as the *m–z* relation for type-Ia supernovae, eventually led to a new consensus (although there are still a few who still believe that $\Lambda = 0$, perhaps even some reading this article).

Certainly for some, one reason to believe that $\Lambda = 0$ is that some estimates from quantum mechanics predict a huge value: many, many orders of magnitude larger than astronomical limits. It seemed more likely that some unknown mechanism results in an exact cancellation, rather than an almost exact cancellation leaving a very small but non-zero value. It would be interesting to know to what extent that argument was responsible for leaving Λ out of various analyses. On the other hand, General Relativity (GR) says nothing about quantum mechanics and the Λ in GR could be different from that resulting from quantum mechanics; perhaps the latter is cancelled exactly and the former is what is observed. Still others might have rejected the quantum-mechanics estimate since it is obviously very wrong in some sense. (See also the section below on the cosmological-constant problem.)

In any case, the belief that the cosmological constant $\Lambda = 0$ was a relatively strong consensus which has been completely overturned. However, that consensus was probably due mainly to lack of data, perhaps strengthening a prejudice in favour of $\Lambda = 0$. The current consensus is certainly due mainly to more and better data, not only from the *m–z* relation for type-Ia supernovae but also from the CMB.

The Einstein–de Sitter universe as the standard model

So although the cosmological constant never went completely out of fashion, there was almost a consensus that there is no cosmological constant (nor spatial curvature) as the Einstein–de Sitter model to a large extent became the standard model of cosmology from about 1970 (*e.g.*, ref. 41 and references therein), after the demise of the Steady State theory[42,43]. Observationally, the Einstein–de Sitter model, in which quantities of interest are easy to calculate, was a good working model, since observations were not yet good enough to detect significant departures from it, though observers had almost always favoured a value for the density parameter $\Omega_0 < 1$ (*e.g.*, ref. 41 and references therein) (1 being the value in the Einstein–de Sitter model).

By the 1990s, many had pointed out that observations were better fitted with a low-density model with a positive cosmological constant; values $\Omega_0 \approx 0{\cdot}3$ and $\lambda_0 \approx 0{\cdot}7$ were a good fit to the observations (*e.g.*, refs. 36–38). Although today the cosmic-microwave-background (CMB) data alone provide tight constraints on λ_0 and Ω_0, already 20 years ago the combination of constraints from the *m–z* relation for type-Ia supernovae[34,35] (which did indicate accelerated expansion but, at that time, essentially measured $\Omega_0 - \lambda_0$) and the CMB (which, at that time, essentially measured $\Omega_0 + \lambda_0$) shifted the consensus to the standard model which we still have today. My guess is that the estimated values of λ_0 and Ω_0 will not change much and that 'dark energy' will never be shown to be anything other than a cosmological constant (even if, in contrast to now, there were strong theoretical reasons for believing so, that could probably never be detected observationally, at least not *via* traditional observational cosmology[44,45]).

The Einstein–de Sitter model was a consensus, though not universal. Overbye[46] gives a fascinating glimpse into the thinking of the time:

> [The late David] Schramm, a neutrino advocate, was one who claimed to know the big picture. He liked neutrinos because they could "close" the universe and make omega a perfect 1·0. [In a footnote, Overbye discusses the misleading nature of that oft-repeated statement.] That was the answer required by inflation. If you understood anything at all about grand unified theories and inflation, you would realize that omega had to be 1·0. A true physicist could think no other way; it was the paradigm. A cosmologist's job was to reconcile the observations with that number.
>
> We were outside his office talking to one of his graduate students, a young woman who was doing numerical simulations of the universe. She was doing great, he said, but why was she doing these runs with omega equal to 0·2?
>
> That was what Davis and White did, she answered.
>
> Schramm told her to get rid of those runs. "You're thinking like an astronomer instead of like a physicist," he snorted, adding, "Simon White never understood inflation."

Ironically, when considering the observations, Schramm himself had co-written a good and important and influential paper on the topic[47], favouring a low value for Ω_0. While it is perhaps not surprising that theorists interested in inflation favoured the Einstein–de Sitter model, I was somewhat surprised that Allan Sandage, often thought to be a good example of an empirical observational cosmologist ("We never really did talk about cosmological models" he said when remembering his time with Hubble[48]), seemed very convinced that the Einstein–de Sitter model must be correct[49]. Of course, without a cosmological constant, the Hubble constant would have to be low (as Sandage had always claimed, even before the idea of inflation was popular or even invented) in order to reconcile the calculated and observed ages of the Universe were $\Omega_0 = 1$. The Einstein–de Sitter model implies a low Hubble constant, but the reverse is not necessarily true. My impression from hearing his lectures, which in that respect were more detailed than in the written version, is that he had latched on to the inflation-inspired Einstein–de Sitter model at least partly because it was compatible with his relatively low value for the Hubble constant, but also perhaps because it demanded a low Hubble constant.

Observationally, non-detection of spatial curvature implies only that the radius of curvature is too large for us to detect; it does not imply that the Universe must be exactly flat nor arbitrarily close to it, any more than the fact that we rarely notice the curvature of the Earth in day-to-day life implies anything other than that the radius of curvature is larger than the lengths we normally deal with. The idea that the Universe must be described as (almost) exactly flat stems from misguided ideas about stability (*e.g.*, ref. 41 and references therein) and/or from the belief that inflation must have made the radius of curvature of the Universe very much larger than we could detect.

Of course, the Einstein–de Sitter model is a special case of models without a cosmological constant. Although not completely accurate, to some extent one could say that while most believed that $\Lambda = 0$, observers thought that Ω_0 was to be determined observationally, and such observations tended to indicate a low value for Ω_0 (*e.g.*, ref. 50), while theorists preferred the Einstein–de Sitter model. As mentioned above, however, Sandage, an observer, was heavily influenced by theorists in his later years, becoming a firm believer in the Einstein–de Sitter model, whereas when younger he had been searching for two numbers, H_0 and q_0 (if $\lambda_0 = 0$, $q_0 = \Omega_0/2$; q is the deceleration parameter)[51]. Originally preferred for its simplicity, as originally intended, with time it became so popular that other models, even with $\Lambda = 0$, were considered almost fringe, as indicated by the story involving Schramm mentioned above. The fact that Peebles favoured the Einstein–de Sitter model was certainly influential, though I wonder whether his attempts still to make it work when it was becoming more and more obvious that it couldn't fit the observations were at least in part due to playing devil's advocate. So in this case the original reason for the consensus was relatively benign, but it held on longer than it should have due to continued support from influential figures in the field. Another reason, somewhat overlapping, is that some observations (*e.g.*, refs. 52–54) did seem to support, or at least be compatible with, the Einstein–de Sitter model, though uncertainties were large and the claim was too enthusiastic.

Gravitational waves

Einstein first thought that gravitational waves were real[55], then that they weren't, then again that they were real. The claim that they weren't was submitted to *Physical Review*, but was rejected. It was then submitted to the *Journal of the Franklin Institute*, but before publication it was revised to change the conclusion[56]. See the investigation by Kennefick[57] for some background; the incident led to Einstein never, with the exception of a letter to the editor in in response to a criticism of his unified-field-theory work[58], submitting to *Physical Review* again, since he objected to his paper being refereed.

Not only Einstein was confused. The confusion was not completely resolved until 1959[59], though momentum had been building since Feynman (using the pseudonym Mr. Smith) described a thought experiment at a conference in 1957. As this piece is not a review but is intended merely to call attention to some historical consensus shifts, I point readers to the excellent historical overview by Mauro *et al.*[60], which everyone interested in the topic should read. See also section 4 in the article by Trimble[61], which contains several references related to this story. The main ingredients in the confusion were the issue of coordinate *versus* real effects (comparable to the difference between the singularity at the horizon of a black hole, a mere coordinate singularity in Schwarzschild coordinates, and the real (within the context of GR) singularity at the centre) and doubt on the part of influential figures such as Einstein and Eddington. Essentially all remaining sceptics were convinced by the agreement of theory and observation regarding energy loss *via* gravitational waves in the famous binary pulsar[62], which can be seen as the first indirect detection of gravitational waves, which in the meantime have now been detected directly[63]. In this case, it was neither an honest mistake nor hanging on to an outmoded idea for irrational reasons (though the opinions of Einstein and Eddington probably played a small role), but rather it was a topic which had not been investigated in enough detail by enough people.

Black holes

Einstein also believed that black holes cannot form, at least not *via* astrophysical processes[64], primarily because he imagined the formation *via* a series of stationary states and argued that collapse would be prevented because particles would have to exceed the velocity of light. (He considered only circular orbits, but believed the result to be more general.) Penrose[65] demonstrated that, with some relatively uncontroversial assumptions, GR can produce singularities provided that gravity is strong enough (to create a so-called trapped region). (A related theorem by Hawking[66–68] states that, with similar assumptions, the Universe must have begun with a singularity, the Big Bang.) Penrose received the 2020 physics Nobel Prize for this work, a three-page paper in which the statement "The space–time manifold is incomplete" is expounded upon in a footnote: "The 'I'm all right, Jack' philosophy with regard to the singularities would be included under this heading!" While there was never a clear consensus that black holes cannot exist, evidence for bodies which are too compact to be anything else, such as the compact objects in X-ray binaries and the massive objects at galactic centres, has led over time to a consensus that they do. Similar to the case of gravitational waves, scepticism about black holes was due primarily to the topic not having been investigated fully. To some extent, the dislike of true (as opposed to merely coordinate) singularities might have made some sceptical of black holes in general. Today it is clear that such singularities follow from GR but also suspected by most that GR must be modified under such extreme conditions in order to be compatible with quantum theory.

Cosmological horizons

There was real confusion regarding the concept of cosmological horizons, perhaps in part due to the fact that in the early days the de Sitter model (a flat universe with no matter and a cosmological constant which expands exponentially; $\lambda_0 = 1$ and $\Omega_0 = 0$) was often used as a fiducial model and in that model the event horizon corresponds to the Hubble radius (at which the recession velocity is the speed of light) and is also constant in time, while in general neither is the case. For modern expositions of the topic of cosmological horizons, see Chapter 21 of the textbook by Harrison[17] and the papers by Davis & Lineweaver[69] and van Oirschot *et al.*[70].

The long-standing confusion was cleared up in a famous paper by Rindler[71], but, as Rindler himself recalls[72], it was sparked by further confusion documented in a debate in these pages. Whitrow[73] had claimed that the existence of (what was later to be known as) an event horizon in the Steady State model was an argument against that model. (Of course, we now know that the Steady State model is incorrect, but also that the existence of an event horizon is not *per se* an argument against it nor against any other model.) Bondi & Gold[74] responded, concluding with:

> We do not know what hurricanes will be directed against our cosmological edifice; but we are a little aggrieved to think that it is being credited with so little structural strength that Dr. Whitrow's puff could make it shudder.

Whitrow[75] was not amused, and clarified his claim. He attempted to clear it up[76] but appears to me to have been more confused; Hoyle[77] agrees: "When I wished to refer to an observer with an idealized telescope I referred to an observer with an idealized telescope; and when I did not wish to refer to an idealized telescope I did not refer to an idealized telescope." Annoyed, Whitrow told Rindler to "do something"[72], which ultimately led to Rindler's definitive paper[71]. To some extent the confusion was similar to that regarding gravitational waves and black holes, but on the other hand, as Whitrow's arguments demonstrate, also due to trusting one's intuition in a regime for which it is not suited.

Rindler noted that there are two types of cosmological horizons. One, dubbed the particle horizon, is, at a given time, the greatest (proper) distance (measured at the current time) from which a signal could have reached us. By symmetry, the corresponding sphere represents the furthest distance to which a signal emitted from our position could have reached. One can think of an expanding sphere of light emitted at the earliest possible time; its edge is the particle horizon with respect to the centre. While of course light travels *locally* at the speed of light, the expansion history of the Universe affects how the distance to the horizon changes with time. The other, dubbed the event horizon, represents the greatest distance from which a signal could travel to our position in the future. By symmetry, that corresponds to the greatest distance which a signal sent from our position could ever reach. Big-Bang models have a finite particle horizon; models which asymptotically expand exponentially have a finite event horizon. There are thus models with a particle horizon, an event horizon, both, or neither (*e.g.*, Einstein's original static model, and the relativistic equivalent of the Milne model with $\lambda_0 = 0$ and $\Omega_0 = 0$). The particle horizon is often thought of as the spatial surface at a given time (such as the present) while the event horizon is often thought of as a surface in space-time, though both can be thought of in both ways. (That is probably because two questions are considered more important than others: what are the furthest objects which can be seen now, and which events (in the sense of relativity) can affect us in the future.) In the spacetime sense, the particle horizon is the backward light-cone now and the event horizon the greatest possible extent of the backward light-cone in the future. Note that the particle horizon always increases in terms of proper distance and that the event horizon in the case of pure exponential expansion is at a fixed proper distance (it also corresponds to the Hubble sphere in that case, which is not true in general). See the paper by Rindler[71] and Chapter 21 in the book by Harrison[17] for more details.

On-going: the cosmological-constant problem

The cosmological-constant problem refers to the question as to why Λ is so small compared to the expectation from quantum field theory. That vacuum energy can produce a gravitational effect was noted already by Nernst[78]. Depending on definitions, the discrepancy can be as high as 120 orders of magnitude (*e.g.*, refs. 79,80) and has even been called "the worst theoretical prediction in the history of physics"[81]. Interestingly, most seem to believe the prediction from quantum field theory and postulate some mechanism which cancels the large expected value — or, since we now know that $\Lambda > 0$ though much smaller than that estimate, *almost* cancels it (which is perhaps conceptually more difficult, as it would seem to involve fine-tuning, whereas an exact cancellation could be due to some unknown symmetry principle). Of course, in GR there is no concept of any sort of vacuum energy from quantum field theory; Λ is simply a constant like the gravitational constant G (and indeed can appear in purely Newtonian theory). Schrödinger[82] seems to have been the first to suggest that a fluid with equation of state $p = -\rho$ would behave like a cosmological constant. Einstein[83] acknowledged that, but didn't see the point, preferring to keep the term on the 'geometric' rather than the 'matter' side of his field equation.

But if one, why not both? Weinberg[84] postulated that the prediction from quantum theory is correct, but that there is also a 'bare' or 'geometrical' cosmological constant which is negative and slightly smaller in absolute value than the 'matter' one, so that the net result is an effective cosmological constant which is slightly positive, the fine-tuning being explained by an appeal to the weak Anthropic Principle: an effective cosmological constant which is too negative would have led to a universe which collapsed after a short time, while a too positive one would have led to a universe which expanded too quickly for structure to form. Despite the influence of Weinberg, most still believe that there is a real cosmological-constant problem, perhaps out of distaste for arguments based on the Anthropic Principle.*

Bianchi & Rovelli[94] (in an expanded version of another article[95]) take a different point of view and dispute the reasoning leading to the prediction of an extremely large cosmological constant from quantum field theory. Even if one disagrees with them on that point, they still present several other interesting arguments, largely independent of that specific argument. They point out that what Einstein later thought of the cosmological constant is irrelevant and that the 'coincidence problem' (the fact that the energy densities due to matter and the cosmological constant are approximately equal at the current epoch, even though they have a different dependency on time) is overstated, giving three arguments against it. They also support the idea that a spatially closed universe makes more sense (see below) — all in all, one of the most sensible papers in modern cosmology. Their argument against the conventional derivation of something like a cosmological constant is too detailed to be summarized here, but their conclusion to the corresponding section is worth quoting:

> To trust *flat-space* QFT telling us something about the nature of a term in Einstein equations which implies that spacetime cannot be flat, is a delicate and possibly misleading step. To argue that a term in Einstein's equation is "problematic" because flat-space QFT predicts it, but predicts it wrong, seems a *non sequitur* to us. It is saying that a simple explanation is false because an ill-founded alternative explanation gives a wrong answer.

as is part of their general conclusion:

> But to claim that dark energy represents a profound mystery is, in our opinion, nonsense. "Dark energy" is just a catch name for the observed acceleration of the universe, which is a phenomenon well described by currently accepted theories, and predicted by these theories, whose intensity is determined by a fundamental constant, now being measured. The measure of the acceleration only determines the value of a constant that was not previously measured. We have only discovered that a constant that so far (strangely) appeared to be vanishing, in fact is not vanishing. Our universe is full of mystery, but there is no mystery here.

* Straumann[85] mentions (without citation) that Zel'dovich made a similar claim in 1967, though without explicitly invoking the Anthropic Principle. However, I haven't been able to find it explicitly mentioned in the literature, though it is perhaps implicit in a paper by Sakharov[86] on the idea of explaining the cosmological constant *via* vacuum fluctuations, something also explored by Zel'dovich[87,88] (see (also) the corresponding English translations and commented reprints[89–93]).

The cosmological-constant problem is similar to the flatness problem (see the penultimate section) in that most people who believe that it exists have probably not looked into it in detail themselves, but have absorbed it from textbooks (which usually just mention it rather than elucidate it) and so on as part of the 'lore'. It is thus good that Rovelli mentions the argument above in his recent book[96] (reviewed in these pages[97]), even though it concentrates on just the essentials; of course, avoiding misinformation should be considered essential.

There are two constants of Nature in the Einstein equation, Λ and G. Why do many see it as a puzzle that we don't understand the ultimate origin of Λ, nor its value, while the same questions are rarely asked about G? Of course, even if there is no contribution to an effective cosmological constant from vacuum energy, its value could still be determined by the Anthropic Principle. For that matter, the value of G could also be determined by the Anthropic Principle. I am not aware of any calculations of their values from deeper principles. Thus, within the context of GR, the value of Λ is no more puzzling than the value of G; the problem arises when trying to explain the small observed value despite the prediction of a large value from quantum mechanics.

For those who believe in the large value of Λ predicted by quantum mechanics and also willing to accept Weinberg's argument, then the problem is solved. However, due to its reliance on the Multiverse and the Anthropic Principle, not all are willing to accept it. Unless the prediction from quantum mechanics is fundamentally wrong for some unknown reason, then the problem still exists.

A detour into particle physics

For many, the belief that $\Lambda = 0$ exactly was based on the idea that, since the particle-physics expectation was that it should have an extremely large value, some unknown mechanism must cancel that contribution, and would cancel it exactly, because having Λ small but non-zero would imply a fine-tuned cancellation mechanism. (As mentioned above, Weinberg explained the small value by invoking the Multiverse and the Anthropic Principle, though not all accept his explanation.) Similarly, the observed small mass of the Higgs boson could be explained by a cancellation mechanism involving supersymmetry. In the former case, a small but non-zero value for $\Lambda = 0$ is observed, so we now know that no such cancellation mechanism exists (not that there had been a convincing argument for one). In the latter case, (at least most theories of) supersymmetry predicted that the *Large Hadron Collider* should have detected at least some of the predicted supersymmetric partners of the known particles, but that didn't happen. Thus, what appeared to many to be a convincing cancellation mechanism was ruled out, but since the mass of the Higgs boson is small, that small mass is now a puzzle. The two cases are similar in that the purported cancellation mechanism has been ruled out, but in the former case *via* the observed absence of cancellation and in the latter case *via* falsified predictions of the theory, leaving the observed small Higgs mass without an explanation. In both cases, vague belief in naturalness motivated both expectations, though supersymmetry at least had some additional motivation.

Not all refinements are paradigm changes

Of course, not all modifications of a working model indicate some sort of important change. The current cosmological standard model with $\Omega_0 \approx 0{\cdot}3$ and $\lambda_0 \approx 0{\cdot}7$ is at least approximately spatially flat (since $\approx 0{\cdot}3 + \approx 0{\cdot}7 = \approx 1$), but, contrary to what one sometimes hears, that flatness is not an important ingredient, for several reasons. First, the detection of non-zero spatial curvature would not involve any new physics. Second, in contrast to the discovery of the acceleration of the Universe or (were it to happen) the discovery that 'dark energy' is something other than the cosmological constant, there would be no qualitative, and only a small quantitative, change in our understanding of the Universe. Third, assuming flatness (as is sometimes done) is a practical matter: if a certain cosmological test is relatively insensitive to spatial curvature, then it makes sense to assume it as a prior in the analysis, since the observational evidence from other tests that the Universe is at least approximately flat is good; even if the test is sensitive to spatial curvature, in practice it probably cannot distinguish between perfect flatness and flatness to a very good approximation, though if there is a chance of detecting spatial curvature then one should make use of it. (Of course, perfect flatness can never be proved observationally, merely ruled out.) Fourth, even inflation does not predict a perfectly flat Universe, only one with a large radius of curvature. (Though, to be sure, it is not really clear what inflation robustly predicts; when a model with $\Omega_0 \approx 0{\cdot}3$ and $\lambda_0 = 0$ was still viable, there were many papers on 'open inflation', though to be fair such models were probably more contrived than the typical inflation model. While they did lend some credence to the 'inflation can predict anything' argument, it is fair to say that such models were not part of mainstream inflation and that most believed that (near) flatness was a robust prediction. The first convincing 'detection' of a positive cosmological constant was belief in the inflation-inspired flat universe together with observational evidence that Ω_0 is substantially less than 1[39].) Fifth, the origin of a universe with positive spatial curvature, and hence spatially finite, described approximately by a Friedmann–Robertson–Walker model, is perhaps easier to understand and/or more likely to be true because a spatially infinite FRW model is spatially infinite even at the Big Bang, hence its origin is more difficult to understand with respect to causality.

The flatness problem

Another on-going debate concerns the flatness problem. Two important aspects of the flatness problem are the fine-tuning problem (why was Ω so close to 1 in the early Universe?) and the time-scale problem (why is Ω so close to 1 today?). Briefly, there is no fine-tuning problem since all non-empty FRW models with a Big Bang have Ω arbitrarily close to 1 at the Big Bang, and there is no instability problem because arguments claiming that there is are based on a false analogy[98,99]. As many readers of this *Magazine* are aware, for the past several years I've been trying to convince the community that the flatness problem, as it is usually understood*, is based on a misunderstanding (*e.g.*, ref. 41 and references therein). Apart from obviously wrong responses†, one point of criticism, probably just thought more often than spoken as well, is that I must

* By that I mean the flatness problem as applied to a Friedmann model containing dust, a cosmological constant, both, or neither, which is usually what is assumed in the literature actually discussing the flatness problem, but perhaps not by those who mention it in passing or just think about it. Certainly when a specific time is introduced, such as setting initial conditions at a specific temperature, at least one aspect of the flatness problem is changed and some of the arguments against it no longer apply.

† "You just can't be right because I work on inflation" is something I have actually heard. Even after I had explained that inflation either happened or not independently of whether there is a flatness problem to solve, I still couldn't make any headway. It does seem, though, that many who have actually thought about it in detail agree with me. Whether we are right remains to be seen; I'm the first to admit that I haven't convinced everyone. One thing that we can agree on is that there are different opinions in the literature, with several well-known cosmologists arguing against the traditional understanding of the flatness problem (*e.g.*, ref. 41 and references therein).

be wrong because I don't agree with the consensus in the community. In this particular case, I don't think that there is a consensus as much as the flatness problem having become part of the lore. Very similar descriptions appear in dozens of textbooks (which almost always ignore the technical literature on the flatness problem but not on other topics mentioned, even if only briefly); like typos in reference lists, one can almost track who has copied from whom without actually checking very much.

Three other misconceptions are common. One is that denying the existence of the flatness problem would be some sort of paradigm shift as discussed above. However, that wouldn't change any observational conclusions. In the past, it could have weakened the belief in the flatness of our Universe, but that is now an observational fact ($\Omega_0 + \lambda_0 = 1$ to within a per cent or so). It could, and should, though, urge people to consider that non-zero spatial curvature might be detectable, unless there is a *robust* prediction (probably from inflation) that any such curvature would be undetectable. (Despite 'open inflation' models which were considered at some point, I think it is fair to say that near flatness is a robust prediction of inflation, but one needs to know exactly how near before one gives up the possibility of detecting spatial curvature.) Another is that it doesn't matter because we know that inflation happened. Without using near-flatness itself as evidence, while there is some evidence to support inflation, it is not yet a proven theory to the same degree as, say, the Big Bang. A third is that it doesn't matter whether there is a flatness problem in classical cosmology because there is in quantum cosmology and the early Universe must certainly be described by some theory which takes quantum mechanics into account. It *does* matter for the understanding of classical cosmology.

With regard to the possibility or usefulness of observationally trying to detect curvature, to some extent, one's expectations play a role. If one sees curvature as an additional parameter, one can ask whether a better fit to observations justifies allowing an additional parameter: since an additional parameter can be used to make the fit better, the advantage must be large enough to outweigh such a 'disadvantage' (*e.g.*, ref. 100). On the other hand, one's default expectation could be the general case and one would require evidence for vanishing curvature.

Conclusions

Throughout the history of cosmology, there have been phenomena for which there was a consensus with regard to their explanation which was overturned in a non-trivial way. (Of course, refinement is part and parcel of science and most advances do not involve any sort of revolution[101].) Nevertheless, if the only reason why one believes something is that there seems to be a consensus, one should be sceptical, because sometimes such consensus exists despite the facts, having taken on lives of their own in the manner of urban legends. As discussed above, some of those have been replaced with a new consensus on what is probably the correct explanation. Current examples which I think might be overturned in the future are the flatness problem and the cosmological-constant problem; it will be interesting to see for how long they will still be regarded as problems. Interestingly, the consensus in those cases is in regard to the problems themselves, rather than their solutions, particularly with respect to the cosmological-constant problem, for which there is no consensus regarding its solution. While one could see inflation as a consensus solution for the flatness problem, there are other arguments for inflation, and even universal belief in the non-existence of any sort of flatness problem would not be sufficient to rule out inflation. Understandably, most debate is not in fields where there is a perceived consensus, but rather in fields where it is clear that there is not a consensus, *e.g.*, regarding the existence of the Multiverse[102–104], fine-tuning[105–108], or the Anthropic Principle[109–113] (note that various combinations of those topics are often discussed together; they are not necessarily related, but can be). To some extent, the fact that cosmology is now a data-driven science, in contrast to the old days when there were only two-and-one-half or nine facts[114], has caused some to lose sight of such important debates (assuming they ever had them in sight at all). We need the data-driven science, but also discussion about fundamental principles.

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THE THIRD BODY IN RR LYNCIS REVISITED

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The times of minima of the bright, eccentric eclipsing binary RR Lyncis are reinvestigated. The data are sparse but on balance probably do support an earlier conclusion that the system contains a low-mass, < 0·1 $M_\odot$, third body, but there is no evidence that the orbit is eccentric. The likely period is about 16 years but the amplitude of the light-travel-time effect is small at 0^{d}·0019 and at the limit of detection.

RR Lyncis is a naked-eye detached eclipsing binary with a period near 9^{d}·95, having eclipses of depth ~ 0^{m}·3 and a small eccentricity. In a major reinvestigation of the system using radial-velocity and new *TESS* data together with previously published photometry, Southworth[1] has shown that the components have masses of 1·94 $M_\odot$ and 1·51 $M_\odot$, and $T_{\rm eff}$ = 7770 ± 200 and 7180 ± 200 K, corresponding to spectral types of A7 V and F0 V, respectively[2]. The secondary lies close to the zero-age main sequence and the primary has evolved roughly half-way to the terminal-age main sequence. The primary has been known as an Am star for many years but the *TESS* data also reveal that the system shows at least 35 low-amplitude pulsations in the δ Scuti and γ Doradus range (see Southworth[1]). The amplitudes are very low, < 0·3 mmag, and the excursions from the mean light-curve are < 3 mmag., so they will have no impact on the ground-based light-curves. The residuals from the mean light-curve are larger during primary eclipse, and smaller during secondary eclipse, suggesting that the secondary component is the source of the pulsations.

RR Lyn was discovered to be a spectroscopic binary by Adams[3], and the first single-lined orbital solution was given by Harper[4], a few years later in 1915. It was not until a decade later that it was identified as an eclipsing binary[5,6] (see Huffer[7]), and since then photometric studies have also been published by Magalashvili & Kumsishvili[8], Botsula[9,10], Linnell[11], Lavrov *et al.*[12], Khaliullin *et al.*[13], and most recently Southworth[1]. A number of separate times of minimum have also been published. All the eclipse timings were reviewed by Khaliullin & Khaliullina[14] who concluded that the system contained a low-mass third body with a period $P_3 = 39{\cdot}7 \pm 4{\cdot}2$ years in an extremely eccentric orbit with $e_3 = 0{\cdot}92 \pm 0{\cdot}02$. The excursion in the O–C diagram due to the light-travel-time effect (LTTE) is $0^{d}{\cdot}0015$ and corresponds to a body with a minimum mass $m_3 = 0{\cdot}10\ M_\odot$. It has to be said that the orbit is poorly sampled, particularly the parts that define the high eccentricity, and there only 13 data points, which degenerate to eight independent epochs, with the distribution of the data. Given the possibility of a third body Southworth calculated solutions with and without a third-light component and found that any contribution was insignificant at $3{\cdot}6 \pm 2{\cdot}3\%$, so this does not resolve the issue one way or the other.

It is now twenty years since Khaliullin & Khaliullina's study so it is an appropriate time to revisit the third-body orbital solution. Unfortunately, only a handful of additional timings are available but those provided by *TESS* are very high precision. In an effort to provide some consistency, all the available data have been re-analyzed using individual runs through the minima where possible, and composite light-curves where necessary[15]. The data from Huffer[7] and Botsula[9] each provide two composite timings and have had the heliocentric corrections applied, which are not mentioned in the original papers. Linnell[11] and Khaliullin *et al.*[13] provide high-quality light-curves in several filters and these have been analyzed separately. Additionally, a new minimum has been calculated for the *Hipparcos* data and one has been measured from the *MASCARA* data[16]. The times of minimum given by Caton and collaborators[17,18] are taken as published, but the minima from Koch[19] are relatively low precision so were not used by Khaliullin & Khaliullina, and are also not used here. Four timings from the *TESS* data have been taken from a study of possible apsidal motion in the system by Baroch *et al.*[20]. Finally, four isolated minima are available from the collections of the O–C Gateway and the BAV Lichtenknecker-Database, but only the timing by J. Ells[21] has been used as the one from J. Ebersberger[22] is discordant and the two by A. Paschke[23] have large uncertainties.

The observed times of minima (see Table I) are fitted to a standard form of a linear ephemeris for the eclipsing binary plus an offset due to the light-travel -time effect, so

$$HJD_k = T_0 + P_0 C_k + \Delta T_k, \qquad (1)$$

where T_0 and P_0 are the epoch zero and period of the close binary, C_k is the cycle number at minimum k, and ΔT_k is the LTTE offset at minimum k. The LTTE expression given by Irwin[24,25] has been used here, in which the light-travel time is given by,

$$\Delta T_k = A[(\cos E_k - e) \sin \omega + (1 - e^2)^{1/2} \sin E_k \cos \omega], \qquad (2)$$

TABLE I

Times of minima

HJD	*Error (d)*	*Min.*	*Cycle*	*O–C (d) Linear*	*O–C (d) LTTE*	*Band*	*Data set*
2425615·4956	0·0013	1	−1948·0	−0·0038	−0·0044	*B′*	Huffer
2425619·9741	0·0016	2	−1947·5	−0·0038	−0·0043	*B′*	Huffer
2434675·4581	0·0007	1	−1037·0	−0·0011	−0·0006	*B′*	Botsula
2434679·9364	0·0008	2	−1036·5	−0·0011	−0·0006	*B′*	Botsula
2437698·7597	0·0005	1	−733·0	−0·0010	−0·0014	*B*	Linnell
2437698·7598	0·0007	1	−733·0	−0·0009	−0·0013	*V*	Linnell
2437698·7604	0·0010	1	−733·0	−0·0003	−0·0007	*U*	Linnell
2437941·9201	0·0015	2	−708·5	−0·0006	−0·0006	*V*	Linnell
2438046·83761	0·00033	1	−698·0	−0·0006	−0·0003	*B*	Linnell
2438046·83765	0·00028	1	−698·0	−0·0005	−0·0003	*V*	Linnell
2438046·83992	0·00029	1	−698·0	0·0017	0·0020	*U*	Linnell
2444595·17158	0·00047	2	−39·5	−0·0017	−0·0004	*W*	Khaliullin *et al.*
2444595·17226	0·00045	2	−39·5	−0·0010	0·0003	*R*	Khaliullin *et al.*
2444595·17278	0·00037	2	−39·5	−0·0005	0·0008	*V*	Khaliullin *et al.*
2444595·17308	0·00050	2	−39·5	−0·0002	0·0011	*B*	Khaliullin *et al.*
2444988·49563	0·00017	1	0·0	−0·0021	−0·0003	*V*	Khaliullin *et al.*
2447220·67251	0·00049	2	224·5	0·0005	−0·0001	*C*	Caton *et al.*
2447524·4924	0·0010	1	255·0	0·0016	0·0004	*C*	J.Ells
2447568·75226	0·00038	2	259·5	0·0028	0·0016	*C*	Caton *et al.*
2448499·1081	0·0012	1	353·0	0·0003	−0·0016	*Hp*	*Hipparcos*
2448936·69194	0·00026	1	397·0	0·0010	−0·0006	*V*	Caton & Burns
2457145·8527	0·0013	2	1222·5	−0·0001	0·0018	*C*	*MASCARA*
2458846·45962	0·00003	2	1393·5	−0·0003	−0·0001	*C*	*TESS*
2458851·92653	0·00003	1	1394·0	−0·0001	0·0001	*C*	*TESS*
2458861·87150	0·00003	1	1395·0	−0·0002	0·0000	*C*	*TESS*
2458866·35005	0·00003	2	1395·5	0·0000	0·0001	*C*	*TESS*

where E_k is the eccentric anomaly at minimum k, e is the orbital eccentricity, and ω is the argument of periastron of the orbit of the close-binary pair in reaction to the motion of the third body. The constant, $A = a_{12} \sin i/c$ is the semi-amplitude of the light-travel time where $a_{12} \sin i$ is the projected semi-major axis of the orbit, and c is the speed of light. The fitting was performed using Markwardt's wonderfully robust implementation of the Levenberg–Marquardt algorithm through MPFIT[26] from the MINPACK-1 package[27]. The parameters fitted are the close-binary period and zero point of the linear ephemeris, P_0 and T_0, and similarly for the third-body orbit, P_3, the time of periastron T_3, as well the eccentricity, e_3, the argument of periastron, ω, and the semi-amplitude of the light-travel time, A.

The O–C diagram is shown in Fig. 1 and it is immediately clear that the data are very sparse. All the data shown have been included in the fit as the low-weight and discordant data have been omitted. The discordant data in particular make any solutions very fragile. As part of the process the phase offset of the secondary eclipse has also been determined and this was set at $\varphi_2 = 0{\cdot}45031$, largely based on the *TESS* minima, but because the LTTE amplitude is so low, small changes in this value have a large impact on the distribution of the O–C residuals. A full-parameter search of possible orbital solutions showed that the eccentricity is essentially unconstrained, and this is obvious from the plot. It is possible to find local minima in the parameter space with arguably significant eccentricities but none of the fits are compelling and the reduced χ^2_ν values are not significantly smaller than for circular orbits. Both

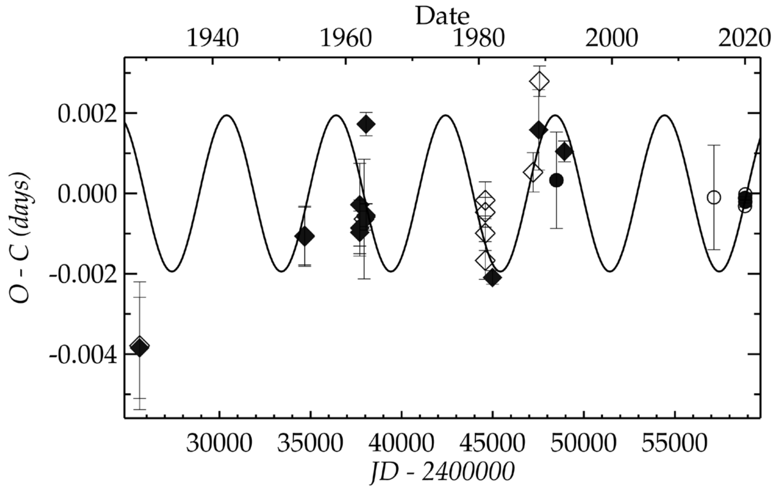

FIG. 1

The O–C diagram showing the weighted LTTE fit using the parameters for the short period given in Table II. The diamonds are nominally photoelectric data and the circles CCD data. Filled symbols identify primary minima and open symbols are secondary minima.

TABLE II

Light-travel-time solution

Parameter		*Short period*	*Long period*	
T_0	=	2444988·49772(11)	2444988·49755(11)	(JD)
P_0	=	9·94507096(17)	9·94507108(20)	(d)
A	=	0·00195(10)	0·00170(9)	(d)
e	=	0·0 (fixed)	0·0 (fixed)	
ω	=	0·0 (fixed)	0·0 (fixed)	(°)
T_3	=	2446922 ± 77	2446149 ± 137	(JD)
P_3	=	6010 ± 40	8379 ± 99	(d)
χ^2_v	=	8·038	8·376	
$a_{12}\sin i$	=	0·336(18)	0·294(16)	(AU)
$m_3 \sin i$	=	0·121	0·084	($M_\odot$)
K_{12}	=	0·61	0·38	(km s^{-1})

the circular and eccentric fits are sensitive to the initial parameters and this simply reflects the lack of constraint on the solution. The parameters giving the minimum χ^2_v for two circular orbits are listed in Table II and the solution for the shorter one is shown in Fig. 1. Derived parameters have been calculated from the well-known expressions for the mass function (see *e.g.*, Hilditch[28]),

$$f(m) = \frac{4\pi^2}{GP_3^2}(a_{12}\sin i_3)^3 = \frac{(m_3 \sin i_3)^3}{(m_{12}+m_3)^2} = \frac{(1-e_3^2)^{3/2}}{2\pi G} P_3 K_{12}^3 \qquad (3)$$

where m_{12} is the mass if the close-binary pair, m_3 is the mass of the third body, a_{12} is the semi-major axis of the binary pair in the third-body orbit, and K_{12} is the velocity semi-amplitude of the binary pair in reaction to the third body.

Taking the total mass of the binary as $m_{12} = 3{\cdot}45\ M_{\odot}$ from Southworth, then the minimum mass of the third body is $m_3 = 0{\cdot}12\ M_{\odot}$, similar to that given by Khaliullin & Khaliullina, and the velocity imparted on the binary pair is similarly low at $K_{12} = 0{\cdot}6$ km s^{-1}. The relatively poor χ^2_ν suggests that the orbit is a not a particularly good fit to the data — although fits to O–C diagrams are rarely good — but χ^2_ν for a constant period is three times larger, so perhaps it should not be judged too harshly.

In conclusion there is probably a low-mass third body in the RR Lyn system with a period of ~ 16 years, but the evidence is marginal. The data are sparse and there is ample opportunity for random excursions in the O–C diagram to align and give the appearance of a periodic variation. The argument against that is that the clusters of points at different epochs do have less scatter than the general distribution, and the solution is a not an unrealistic fit to the data.

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REDISCUSSION OF ECLIPSING BINARIES. PAPER 10: THE PULSATING B-TYPE SYSTEM V1388 ORIONIS

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V1388 Ori is an early-B-type detached eclipsing binary whose physical properties have previously been measured from dedicated spectroscopy and a ground-based survey light-curve. We reconsider the properties of the system using newly-available light-curves from the *Transiting Exoplanet Survey Satellite* (*TESS*). We discover two frequencies in the system, at 2·99 d^{-1} and 4·00 d^{-1} which are probably due to β Cephei or slowly-pulsating B-star pulsations. A large number of additional significant frequencies exist at multiples of the orbital frequency, 0·4572 d^{-1}. We are not able to find a fully satisfactory model of the eclipses, but the best attempts show highly consistent values for the fitted parameters. We find masses of 7·24 ± 0·08 $M_{\odot}$ and 5·03 ± 0·04 $M_{\odot}$, and radii of 5·30 ± 0·07 $R_{\odot}$ and 3·14 ± 0·06 $R_{\odot}$. The properties of the system are in good agreement with the predictions of theoretical stellar evolutionary models and the *Gaia* EDR3 parallax if the published temperature estimates are revised downwards by 1500 K, to 19 000 K for the larger and more massive star and 17 000 K for its companion.

Introduction

Detached eclipsing binaries (dEBs) are our primary source of measurements of the physical properties of normal stars[1–3]. Those containing high-mass stars are of particular importance because such stars dominate the light of young stellar populations[4,5] and the chemical evolution of galaxies[6], and give rise to a wide variety of exotic objects[7–10]. Theoretical models of massive stars remain limited by the imperfect understanding of several phenomena including internal mixing and convective-core overshooting[11,12], angular-momentum transport[13], and the effects of internal gravity waves[14,15]. Massive stars are typically found in multiple systems[16,17], and their evolution is dominated by binary interactions[18].

In this work we revisit the V1388 Ori system, using a recently-obtained space-based light-curve, with the aim of determining its physical properties to high precision[19]. Its spectrum was classified as B2 V by Walborn[20] and it has been used as a spectral standard star[21], before the discovery of eclipses in its light-curve from the *Hipparcos* satellite[22,23]. A detailed study of V1388 Ori was presented by Williams[24] (hereafter W09) based on 29 coudé spectra (resolving power R = 11 500) and a scattered V-band light-curve from the All Sky Automated Survey (ASAS, Pojmański[25,26]). W09 used the ELC code[27] to fit the light- and radial-velocity (RV) curves and measure the properties of the system. He also determined effective temperature ($T_{\rm eff}$) values of 20 500 ± 500 K and 18 500 ± 500 K for the two stars from the tomographically-reconstructed spectra of the individual stars.

Table I contains basic information for V1388 Ori. The *BV* magnitudes are from the *Tycho* mission[28] and are evenly distributed in orbital phase, so represent the average magnitude of the system. The 2MASS JHK_s magnitudes were obtained at a single epoch corresponding to an orbital phase of 0·4081 ± 0·0012, so represent the brightness of the system shortly before the start of secondary eclipse.

TABLE I

Basic information on V1388 Ori.

Property	*Value*	*Reference*
Henry Draper designation	HD 42401	29
Hipparcos designation	HIP 29321	22
Tycho designation	TYC 738-244-1	28
TESS Input Catalog designation	TIC 337165095	30
Gaia EDR3 designation	3342421035256268544	31
Gaia EDR3 parallax	1·3198 ± 0·0432 mas	31
B magnitude	7·424 ± 0·015	28
V magnitude	7·493 ± 0·018	28
J magnitude	7·506 ± 0·024	32
H magnitude	7·541 ± 0·027	32
K_s magnitude	7·551 ± 0·034	32
Spectral type	B2 V	20

Observational material

V1388 Ori has been observed on three occasions by the NASA *TESS* satellite[33]. It was observed in long cadence (600-s sampling rate) in sector 33 (2020/12/17 to 2021/01/13) but we did not use these data due to their coarser temporal sampling. It was observed in short cadence (120-s sampling rate) in sectors 43 (2021/09/16 to 2021/10/12) and 45 (2021/11/06 to 2021/12/02), and those data were analysed for this work.

We downloaded the data from the MAST archive* and converted the fluxes to relative magnitude. We retained only those observations with a QUALITY flag of zero, leaving 32 307 of the original 35 893 data points. The simple aperture photometry (SAP) and pre-search data-conditioning SAP (PDCSAP) data[34] were visually almost indistinguishable, the only clear differentiating feature being a 0·002-mag. variation in eclipse depth. We therefore adopted the SAP data as usual in this series of papers, and ensured that we fitted for third light in the analysis below. The light-curve is shown in Fig. 1.

Initial analysis of the light-curve

The components of V1388 Ori are significantly distorted due to their large fractional radii (r_A and r_B where $r_A = \frac{R_A}{a}$, $r_B = \frac{R_B}{a}$, R_A and R_B are the radii of the stars, and a is the semi-major axis of the relative orbit) so the light-curve must be analysed using a model incorporating Roche geometry. However, this is time-intensive due to the large calculation time required to model a large number of data points using existing Roche-geometry codes. We therefore performed a preliminary analysis with the JKTEBOP† code[35,36] in order to determine the orbital

* Mikulski Archive for Space Telescopes, https://mast.stsci.edu/portal/Mashup/Clients/Mast/Portal.html

† http://www.astro.keele.ac.uk/jkt/codes/jktebop.html

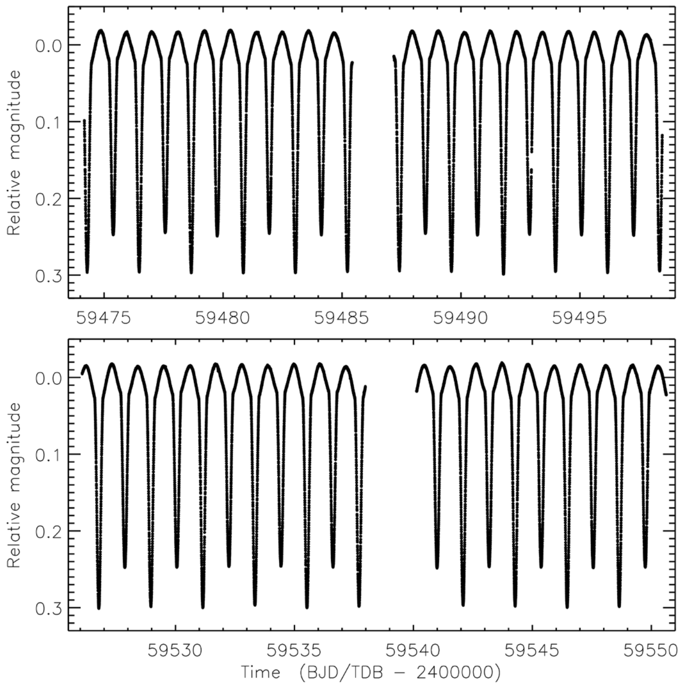

FIG. 1

TESS short-cadence SAP photometry of V1388 Ori from sectors 43 (top panel) and 45 (bottom panel). The flux measurements have been converted to relative magnitude and rectified to zero magnitude by subtraction of low-order polynomials.

ephemeris of the system, thus allowing us to convert all the data to orbital phase and bin into a small number of phased data points.

We fitted the full *TESS* sector 43 and 45 light-curve with JKTEBOP, assuming a circular orbit. We defined star A to be that eclipsed at primary minimum, making it the hotter of the two components (and also the larger and more massive component in the case of V1388 Ori), and star B to be its companion. The orbital ephemeris resulting from this fit is

$$\text{Min I} = \text{BJD/TDB } 2459485{\cdot}226609(16) + 2{\cdot}18703006(82)E \qquad (1)$$

where E is the cycle number since the reference time and the bracketed quantities indicate the uncertainties in the last digit of the preceding number.

We then sought to check this against a timing from W09. Only one timing is given by W09⋆ (their Table 3), which we converted from the UTC time-scale to TDB using the IDL routines of Eastman *et al.*[37]. The timing corresponds to an orbital phase of 0·5492 ± 0·0009, which is sufficiently distant from a time of eclipse to suggest that the orbital period of V1388 Ori may not be constant.

⋆W09 quote their reference time using the notation $T_{\rm IC,1}$ to denote the "time of inferior conjunction of the primary star". However, they also note that "this is the time of secondary minimum in the light curve". The two statements are in mutual conflict. Using our ephemeris we determined that the $T_{\rm IC,1}$ value in W09 is indeed a time of secondary eclipse, and therefore it is a time of *superior* conjunction of star A.

Using the ephemeris above, we converted each of the *TESS* sector 43 and 45 light-curves into orbital phase and binned them into 400 points equally distributed over phases 0 to 1. The two binned light-curves look almost identical. As a check, we phase-binned the PDCSAP data as well, and found that the slightly larger eclipse depths in the PDCSAP data occurred in both *TESS* sectors.

Frequency analysis

Following previous work on massive pulsators in eclipsing binaries[38], the residuals of the JKTEBOP fit to the unbinned SAP data were used to search for the presence of pulsations in V1388 Ori. We used the combined sectors 43 and 45 residual light-curve and calculated the discrete Fourier transform[39]. There are many significant peaks in the resultant amplitude spectrum, but almost all of them fall at integer multiples of the orbital frequency. Therefore, they probably do not represent independent pulsation-mode frequencies, but are rather a consequence of an imperfect binary model leaving residual signal at orbital harmonics (see below).

However, we detected two significant frequencies that do not coincide with orbital harmonics, namely $2{\cdot}9943 \pm 0{\cdot}0002$ d^{-1} and $3{\cdot}9987 \pm 0{\cdot}0004$ d^{-1}. We define *significant* to mean that the signal-to-noise ratio (S/N) is larger than five in the amplitude spectrum after all orbital harmonics have been removed, using a 1 d^{-1} frequency window in the amplitude spectrum centred on the extracted frequency to estimate the local noise level. The amplitudes of the two signals are 0·33 and 0·18 mmag, respectively. Additional variability at lower frequency is visually evident, in particular at 1·0 and 2·0 d^{-1}, but has S/N < 4 so formally falls below our detection threshold and is not significant.

These frequencies are typical of β Cephei or SPB pulsation modes[40–42]. The spectral type of V1388 Ori is consistent with such pulsations, so we conclude that the system is a new example of a massive pulsator in a dEB. Such systems are rare and could provide useful probes of stellar interiors through forward asteroseismic modelling. The closeness of these two frequencies to multiples of the Earth's rotational frequency is unlikely to be meaningful because the *TESS* data are not ground-based.

Fig. 2 shows the amplitude spectrum of the residuals of the JKTEBOP fit to the unbinned SAP data. A large number of frequencies are present at multiples of the orbital frequency ($f_{\rm orb} = 0{\cdot}45724$ d^{-1}). The highest-frequency of these is at $60f_{\rm orb}$ (not shown on the plot). The two significant frequencies are indicated in Fig. 2 using arrows. The available data are not sufficient to indicate which of the components these frequencies arise from. This can sometimes be determined by looking at the residuals of the fit to the eclipses, but in the case of V1388 Ori the variability during eclipse is dominated by the signals at multiples of the orbital frequency.

A Roche-geometry model of the light-curve

The light-curve binned into 400 points in orbital phase was modelled using the Wilson–Devinney code[43,44] in order to determine the photometric properties of the system. We used the 2004 version of the code (WD2004) driven by the JKTWD wrapper[45]. WD2004 uses Roche geometry to model accurately the light-curves of distorted stars in close binary systems.

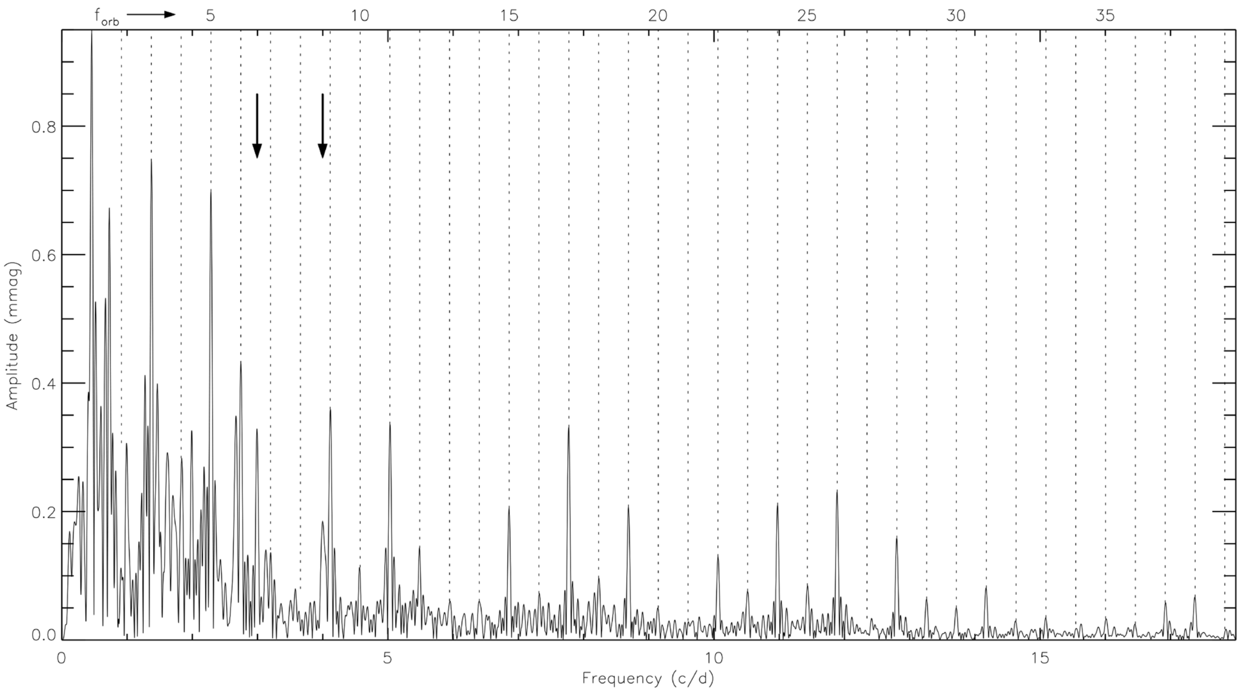

Fig. 2

Amplitude spectrum of the *TESS* light-curve of V1388 Ori after subtraction of the JKTEBOP binary model. The dotted lines indicate multiples of the orbital frequency, and the multiplicative factor is given above the top of the plot for each fifth integer. The arrows indicate the two frequencies that are significantly detected and do not coincide with orbital harmonics.

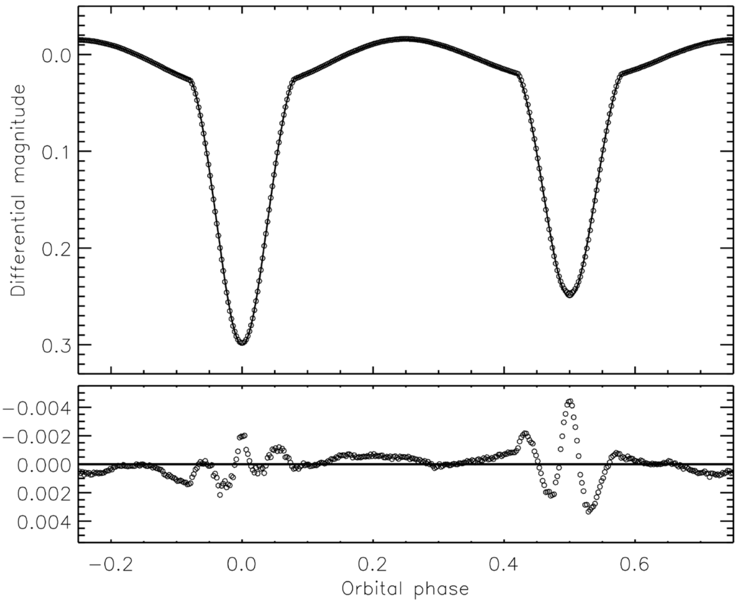

FIG. 3

Best fit to the binned light-curve of V1388 Ori using WD2004. The phase-binned data are shown using open circles and the best fit with a continuous line. The residuals are shown on an enlarged scale in the lower panel.

We have not been able to get a good fit to the light-curve. Our best attempt is shown in Fig. 3 and has significant residuals, as large as 4 mmag., through both eclipses. The reason for this remains unclear, as we have not encountered this precise problem in previous work on similar or different binary systems[19,38,46–48]. In what follows we discuss the default setup of the modelling process and our attempts to improve the fit.

Operation mode. The two relevant choices in WD2004 are mode 0 ($T_{\rm eff}$ values and light contributions are not forced to be consistent) and mode 2 (the $T_{\rm eff}$ values and passband-specific light contributions are forced to be consistent *via* the use of tabulated predictions from stellar model atmospheres). In our default approach we used mode 0, fixed the $T_{\rm eff}$ values at those from W09, and directly fitted for the contributions of the two stars to the total light of the system. As an alternative we chose mode 2 and fitted for the $T_{\rm eff}$ of star B, finding a very similar value (18 424 *versus* 18 500 K) and a similar quality of fit. We used a Johnson *R* passband as the closest available option to the *TESS* band, but obtained practically identical results with a Johnson *I* passband.

Numerical precision. Our initial solutions were performed with a low numerical precision of N1=N2=30 (see the WD2004 user guide[49]). Increasing this to the maximum value of 60 allowed a slight improvement in the fit.

Mass ratio. We fixed the mass ratio at the value of 0·695 ± 0·003 measured spectroscopically by W09. The alternative approach of fitting for this quantity gives a better fit to the light-curve but a mass ratio, 0·539, which is quite discrepant with the spectroscopic value.

Orbital eccentricity. V1388 Ori is expected to have a circular orbit due to its age (W09) and short tidal time-scales[50,51]. However, a small eccentricity does give more freedom to fit eclipse profiles because it allows the primary and secondary eclipses to have different impact parameters. If the argument of periastron, ω, is set to 90° or 270° it is possible to have an eccentric orbit where the secondary eclipse is still at phase 0·5. We tried this but were unable to improve the fit significantly. All solutions prefer at most a small eccentricity (0·01 or less) and there is no clear evidence for non-circularity.

Rotation rate. Tidal effects are also expected to have caused the rotation of the stars to synchronize with the orbital motion, so in our default solution we assumed synchronous rotation. Attempts to fit the rotation rate of star B failed because it has a negligible effect on the shape of the light-curve. Fitting for the rotation rate of star A yielded a determinate solution for faster rotation (1·7 times the synchronous value) but only a slightly improved fit. W09 measured the rotational velocities of the two stars from their spectral-line profiles, finding them to be consistent with synchronous rotation.

Albedo. We were able to find a significantly better model of the *TESS* light-curve by fitting for the albedos of the two stars. The residuals decreased from 0·96 mmag. to 0·51 mmag. and the residuals during eclipse became much smaller (Fig. 4). However, the fitted values of the albedo are 4·8 and 4·5, so are physically unrealistic[52]. Such a large albedo would require the stars to emit much more light in the *TESS* passband than is incident on them. We have seen a similar problem in the past (*e.g.*, KIC 10661783[45,53,54]) and suspect that it arises due to a physical effect not included in the WD2004 model. For our default values we therefore fixed the albedos of both stars to 1·0.

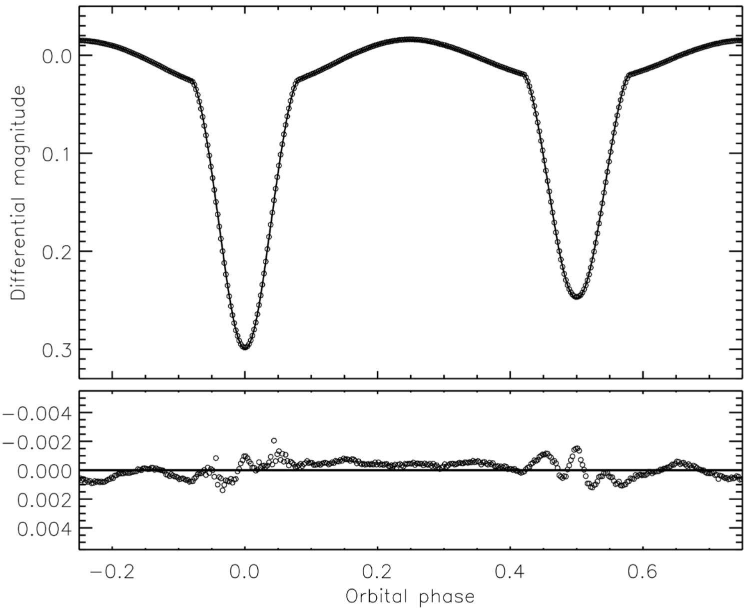

Fig. 4

Same as for Fig. 3 except for a solution where the albedos of the two stars were fitted.

Gravity darkening. Both components are expected to have a gravity-darkening exponent of $\beta = 1{\cdot}0$ (ref. 55). A significantly improved fit can be obtained by specifying values very different from this; a straight fit returns $\beta = 7{\cdot}7$ for star B, which must be rejected on physical grounds.

Reflection effect. WD2004 includes the option of a detailed treatment of the reflection effect[56] which we normally do not use because of the additional computing time needed. Use of the detailed treatment caused no improvement in the quality of the fit.

Third light. A small amount of third light is expected to be common in data from *TESS* because of the large pixel size (21″). We included this parameter by default and consistently obtained a small negative value. This is physically plausible if the background subtraction in the data-reduction process is imperfect, and has been seen before in a similar system[57].

Limb darkening (LD). Our original fits used the linear LD law and coefficients fixed at values interpolated from the tables of Van Hamme[58]. Use of the square-root or logarithmic LD laws gave almost identical results. Fitting for one LD coefficient per star allowed a small improvement in the fit. For our final fits we assumed logarithmic LD and fitted for the coefficients.

Inverted ratio of the radii. It is often possible to get relatively good fits to the light-curve of a dEB for both a certain ratio of the radii ($k = \frac{r_B}{r_A}$) and its reciprocal ($\frac{1}{k}$). We were indeed able to locate a solution with $k > 1$ for V1388 Ori, but the quality of the fit was significantly worse and the values of the fitted LD coefficients were very different from theoretical predictions based on stellar model atmospheres. The $k > 1$ solution is also discrepant with the spectroscopic light ratio measured by W09.

Data reduction. We obtained a fit to the PDCSAP data to compare to the default solution using the SAP data. The measured parameter values and residuals were almost identical between the two fits.

Subsections of the light-curve. We fitted the sector 43 and sector 45 light-curves separately, finding that the two solutions were extremely similar. The behaviour of V1388 Ori is therefore consistent over a time interval of at least 76 d.

Light-curve model. The JKTEBOP code was used to fit the phased light-curve, to provide a comparison to the WD2004 solution. In our experience JKTEBOP can obtain good fits even beyond the limits of its applicability[45]. We were not able to get a better fit without allowing the code to use unphysical values for the limb-darkening coefficients and reflection effect.

Final model of the light-curve

In light of the problems outlined above, we abandoned our attempts to get a *good* fit to the light-curve and settled for merely the *best* fit, from the point of view of low residuals whilst remaining physically reasonable. Our final parameter values are based on fitting the phase-binned sector 43 light-curves using WD2004 in mode 0, the simple reflection effect, the maximum numerical grid size of 60, a circular orbit, and the logarithmic LD law. The fitted parameters were the potentials of the two stars, the orbital inclination, the linear LD coefficient for each star, the phase of primary minimum, the light contributions of the two stars, and third light. The results are given in Table II.

To determine the uncertainties in the fitted parameter values we ran new fits with different input parameters or approaches. These fits comprised changing the mass ratio by $\pm 0{\cdot}003$; changing the rotation rate by $\pm 0{\cdot}1$; changing the albedo by $\pm 0{\cdot}1$; changing the gravity-darkening exponents by $\pm 0{\cdot}1$; a numerical grid size of 40 instead of 60; use of mode 2 instead of 0; use of the detailed

TABLE II

Summary of the parameters for the WD2004 solution of the TESS light-curve of V1388 Ori. Uncertainties are only quoted when they have been assessed by comparison between a full set of alternative solutions.

Parameter	*Star A*	*Star B*
Control parameters:		
WD2004 operation mode	0	
Treatment of reflection	1	
Number of reflections	1	
Limb-darkening law	2 (logarithmic)	
Numerical grid size(normal)	60	
Numerical grid size(coarse)	60	
Fixed parameters:		
Mass ratio	0·695	
Orbital eccentricity	0·0	
Rotation rates	1·0	1·0
Bolometric albedos	1·0	1·0
Gravity darkening	1·0	1·0
$T_{\rm eff}$ values (K)	20 500	18 500
Bolometric linear LD coefficient	0·5494	0·5760
Bolometric logarithmic LD coefficient	0·2339	0·2184
Passband logarithmic LD coefficient	0·5124	0·4881
Fitted parameters:		
Phase shift	−0·00003 ± 0·00002	
Potential	3·843 ± 0·030	4·788 ± 0·066
Orbital inclination (°)	77·50 ± 0·33	
Light contributions	10·11 ± 0·27	2·90 ± 0·11
Passband linear LD coefficient	0·24 ± 0·14	0·12 ± 0·20
Third light	−0·032 ± 0·023	
Derived parameters:		
Light ratio	0·287 ± 0·013	
Fractional radii	0·3242 ± 0·0022	0·1923 ± 0·0034

reflection effect; and linear instead of logarithmic LD. For each we calculated the changes in the values of the fitted parameters, then added these changes in quadrature to obtain the full error bars for the parameters. We also considered fits with the LD coefficients fixed at the theoretical values instead of fitted for, and with third light fixed at zero, but in both cases the residuals were significantly higher so we did not include the results in the error bars. The uncertainties reported by the differential-corrections-fitting algorithm in WD2004 are in all cases much smaller than the uncertainties quoted in Table II.

The fractional radii, $r_{\rm A}$ and $r_{\rm B}$, are the most useful results in Table II; $r_{\rm A}$ is highly consistent between alternative fits so is precisely determined; $r_{\rm B}$ varies more, the biggest differences being seen for numerical precision and a change of gravity-darkening exponent. As we were not able to get a good fit to the data, the uncertainty in $r_{\rm A}$ in Table II should be seen as a lower limit. In the following analysis we accounted for this by doubling the error bar. It was not necessary to do the same for $r_{\rm B}$ because its error bar was already significantly larger. In the analysis below we explicitly assume that the values of $r_{\rm A}$ and $r_{\rm B}$ are reliable even though we were not able to get a good fit to the light-curve.

The light ratio in Table II, $\ell_2/\ell_1 = 0{\cdot}287 \pm 0{\cdot}013$, is in excellent agreement with the spectroscopic value of 0·25 ± 0·05 determined by W09. The photometric light ratio is also expected to be be slightly higher than the spectroscopic value because the *TESS* passband (approximately 590–990 nm) is redder than the blue wavelength range covered by W09's spectra (425–457 nm).

Physical properties of V1388 Ori

We have calculated the physical properties of the V1388 Ori system using the r_A, r_B, and orbital inclination from Table II, with the error bar on r_A doubled. To this we added the period found above and velocity amplitudes of $K_A = 151{\cdot}4 \pm 0{\cdot}3$ km s^{-1} and $K_B = 217{\cdot}9 \pm 1{\cdot}0$ km s^{-1} from W09. The calculations were performed with the JKTABSDIM code[59]. To determine the distance to the system we used the apparent magnitudes given in Table I, bolometric corrections from Girardi *et al.*[60], and an interstellar extinction of $E(B-V) = 0{\cdot}18 \pm 0{\cdot}09$ mag from the STILISM* on-line tool[61,62]. The results are given in Table III.

We have measured the masses and radii of the two stars to precisions of 1·8% or better, so they are suitable for inclusion in the *Detached Eclipsing Binary Catalogue* (*DEBCat*†, ref. 63). We find slightly smaller masses than did W09 ($7{\cdot}24 \pm 0{\cdot}08$ and $5{\cdot}03 \pm 0{\cdot}04$ $M_\odot$ *versus* $7{\cdot}42 \pm 0{\cdot}08$ and $5{\cdot}16 \pm 0{\cdot}03$ $M_\odot$), but only because our measurement of the orbital inclination is higher. More interestingly, our radius measurements are much lower: $5{\cdot}30 \pm 0{\cdot}07$ and $3{\cdot}14 \pm 0{\cdot}06$ $R_\odot$ *versus* W09's $5{\cdot}60 \pm 0{\cdot}04$ and $3{\cdot}76 \pm 0{\cdot}03$ $R_\odot$. Our results are based on a *TESS* light-curve incomparably better than the ASAS data available to W09, so should be preferred; the error bars in the published measurements look too small. Although we were not able to find a good fit to the *TESS* data, the residuals are much smaller than the scatter of the ASAS data, so it is reasonable to expect that both datasets suffer from the problem. Our preferred distance measurement is from the K_s band, and is consistent with those from the other bands. Our initial distance measurement (see below) was 791 ± 24 pc, $1{\cdot}0\sigma$ longer than the parallax-based distance of 757 ± 25 pc from *Gaia* EDR3.

To investigate the discrepancy between the published and our own measurements of the properties of the stars, we have compared them to theoretical predictions from the PARSEC stellar-evolutionary code[64]. We did this in mass–radius and mass–$T_{\rm eff}$ parameter space as these are good diagnostic plots[46]. We find that the masses and radii of the stars are matched by predictions for an age of 33 ± 2 Myr and a heavy-element abundance of $Z = 0{\cdot}020$. This is probably consistent with solar abundance considering recent developments[65–67].

TABLE III

Physical properties of V1388 Ori defined using the nominal solar units given by IAU 2015 Resolution B3 (ref. 63). The $T_{\rm eff}$ values are 1500 K lower than those from W09.

Parameter	*Star A*	*Star B*
Mass ratio	0·6948 ± 0·0035	
Semi-major axis of relative orbit ($R^{\rm N}_\odot$)	16·352 ± 0·051	
Mass ($M^{\rm N}_\odot$)	7·237 ± 0·078	5·028 ± 0·038
Radius ($R^{\rm N}_\odot$)	5·301 ± 0·074	3·144 ± 0·056
Surface gravity (log[cgs])	3·849 ± 0·012	4·144 ± 0·015
Density ($\rho_\odot$)	0·0486 ± 0·0020	0·1617 ± 0·0086
Synchronous rotational velocity (km s^{-1})	122·6 ± 1·7	72·7 ± 1·3
Effective temperature (K)	19000 ± 1000	17000 ± 1000
Luminosity log ($L/L^{\rm N}_\odot$)	3·650 ± 0·044	3·018 ± 0·049
$M_{\rm bol}$ (mag)	−4·39 ± 0·11	−2·81 ± 0·12
Distance (pc)	759 ± 23	

*https://stilism.obspm.fr
†https://www.astro.keele.ac.uk/jkt/debcat/

Their T_{eff} values are higher than predicted by approximately 1500 K, which corresponds to slightly less than the difference between the B2 V and B2·5 V spectral type[68]. If we lower the T_{eff} values by this amount, we find a distance to the system of 759 ± 23 pc that is in almost perfect agreeement with *Gaia* EDR3. This represents our preferred set of system parameters as specified in Table III. A comparison with other similar sets of theoretical models[69,70] led to the same conclusions. The comparison is shown graphically in Fig.5.

We then performed the same comparison but with the system properties from W09. We found a rough agreement for $Z = 0{\cdot}060$ and an age of 22 Myr, but even at this extremely high metallicity (the highest available in PARSEC) the models underpredicted the radius and T_{eff} of star B by approximately 3σ and 2σ, respectively. We conclude that the published properties for this system are inconsistent with current stellar-evolutionary models. W09 showed a Hertzsprung–Russell diagram (their Fig. 7) in which the stellar properties

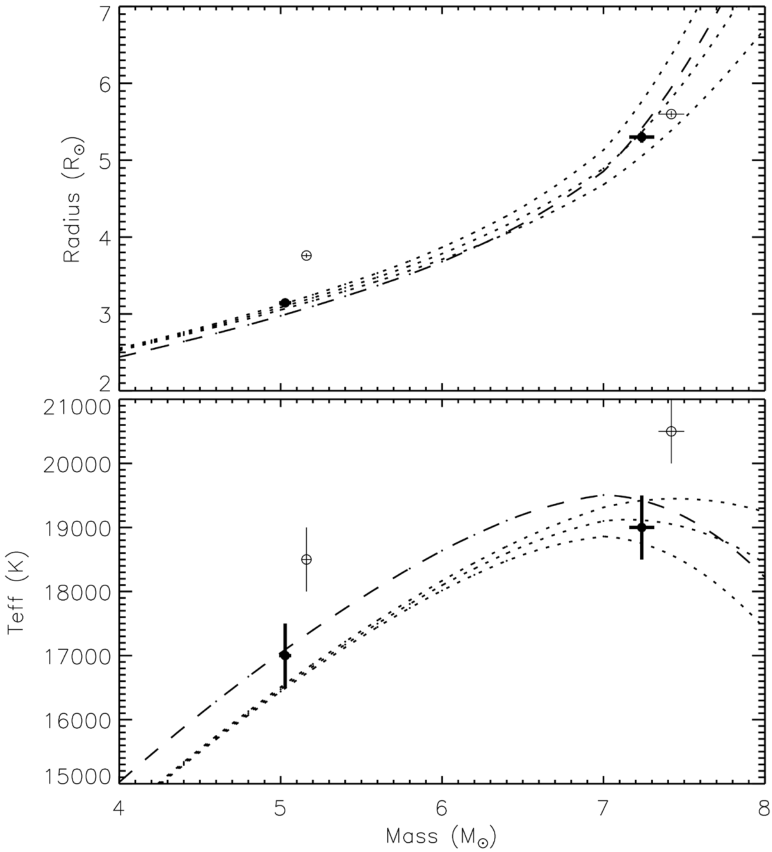

FIG. 5

Mass–radius and mass–T_{eff} plots showing the properties of V1388 Ori *versus* predictions from the PARSEC models[64]. The dotted lines are model predictions for $Z = 0{\cdot}020$ and ages of 31, 33, and 35 Myr. The dashed lines are predictions for $Z = 0{\cdot}014$ and an age of 35 Myr. The filled circles with thick error bars show the properties determined in the current work. The open circles with thin error bars show the values found by W09.

agreed with the Lejeune & Schaerer[71] isochrones for an age of 25 Myr, but the stars were significantly too massive to match the predictions from Schaller *et al.*[72] (a situation also noticed by W09). This highlights how a good agreement in a HR diagram is not sufficient for dEBs because mass is not directly involved; the use of mass–radius and mass–$T_{\rm eff}$ plots (Fig. 5) is better as it allows conclusions to be drawn both more easily and more robustly.

Summary and conclusions

V1388 Ori is a dEB containing two early-B stars with significant tidal distortions. We used the *TESS* light-curve and published spectroscopic results to study the system. The residuals of the fit of an eclipsing-binary model to the *TESS* data were used to obtain a frequency spectrum. We detected two significant pulsation frequencies in the system which could be due to β Cephei or SPB-star pulsation modes, making V1388 Ori one of the few known dEBs containing stars that show these types of pulsation. The frequency spectrum also shows a large number of peaks at integer multiples of the orbital frequency.

We modelled the *TESS* light-curve using the WD2004 code to determine the photometric parameters of the system. We were unable to obtain a good fit and, after extensive investigation, do not know why. Possible answers are an effect of the pulsations, issues with the data-reduction process, or the presence of surface inhomogeneities on the stars. Despite this, the best model of the *TESS* data is very well constrained and appears to be a significant improvement on previous work.

By making the assumption that the results of the Roche-model analysis above are reliable, and using published velocity amplitudes, we determined the physical properties of the component stars. These are in good agreement with theoretical predictions and with the *Gaia* EDR3 parallax for an approximately solar metal abundance and an age of 33 Myr, if a small decrease in the published $T_{\rm eff}$ values of the stars is applied. A previous analysis of the system (W09), using much more limited photometry, returned radius measurements in poor agreement with our own and with theoretical predictions. A new spectroscopic study of this system would be useful to check if the lower $T_{\rm eff}$ values we find are reasonable.

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We thank the referee for a positive and helpful report. This paper includes data collected by the *TESS* mission and obtained from the MAST data archive at the Space Telescope Science Institute (STScI). Funding for the *TESS* mission is provided by the NASA's Science Mission Directorate. STScI is operated by the Association of Universities for Research in Astronomy, Inc., under NASA contract NAS 5–26555. The following resources were used in the course of this work: the NASA Astrophysics Data System; the *Simbad* database operated at CDS, Strasbourg, France; and the arχiv scientific paper-preprint service operated by Cornell University.

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REVIEWS

The WSPC Handbook of Astronomical Instrumentation, Volume 4, edited by David N. Burrows, (World Scientific), 2021. In 5 volumes, 23 × 17·5 cm. Price £1536/$2387 (hardbound; ISBN 978 981 4644 31 0).

Volume 4 of *The WSPC Handbook of Astronomical Instrumentation* is dedicated to X-ray astronomical instrumentation. It includes reports from leading scientists and engineers illustrating the latest advances and challenges in the development of innovative optics, detectors, and techniques for upcoming X-ray missions. The past twenty years of X-ray astronomy have seen the ground-breaking discoveries of NASA's *Chandra* and ESA's *XMM–Newton* great observatories. Building on their legacy is a major challenge, but this *Handbook* shows that important steps have been taken in recent years to manufacture lighter optics, to innovate instruments' design, and to produce detectors with faster readout, improved spectral resolution, and lower noise.

The first part of the *Handbook* is dedicated to X-ray optics. It opens with an historical perspective and an overview of the current challenges. The topics discussed here include the innovative use of silicon wafers to produce mirrors, with the lightweight and large-effective-area silicon-pore optics and silicon meta-shell optics, the development of adjustable optics to achieve high resolution, and a chapter describing the very compact Lobster-Eye Optics design. This part closes with discussions of lightweight full-shell optics and of multi-layer coatings.

The second part illustrates the advances in X-ray-detector technologies, starting with a review of the main features of X-ray CCDs. Also included are chapters on DEPFET detectors that can achieve high speed and resolution, and hybrid CMOS detectors currently under development that promise rapid readout capabilities and high resistance to the harsh space-radiation environment. The *Handbook* closes with two shorter sections dedicated to grating spectrometers and to the techniques and telescope designs to detect X-ray polarimetry.

This *Handbook* provides a comprehensive overview of recent important developments in space X-ray instrumentation and it effectively illustrates the many challenges of the field. A good knowledge of the fundamental physics of optics and semiconductors is required to follow the topics and arguments being presented, but most chapters keep a discursive tone that will engage the reader and excite curiosity without dwelling in detailed, advanced mathematics. The *Handbook* can be a good reference for a higher-degree science student approaching the subject or for an expert in a similar field in astronomical instrumentation. The reader requiring an in-depth presentation of a specific topic will be guided by the rich reference lists included at the end of each chapter. — Claudio Pagani.

Multimessenger Astronomy, by John Etienne Beckman (Springer), 2021. Pp. 390, 23·5 × 15·5 cm. Price £24·99 (paperback; ISBN 978 3 030 68371 9).

Multimessenger Astronomy offers a comprehensive and scholarly excursion through the different components of what constitutes modern astronomy and astrophysics, passing in an historical order through developments in the electromagnetic spectrum from optical, radio, infrared, UV, X-ray, and gamma-ray wavelengths, and thence to those non-EM emitters — neutrinos, gravitational waves, cosmic rays — to cosmology and particle physics. The objective is clear: to illustrate how modern astronomy is benefitting from the pursuit of realms that are outside the traditional boxes, how well it may be doing so, and what are the costs (in terms of resources) to achieve that objective. The text is well-supported with photos and explanatory diagrams, though many of the images are too specialized to be fully appreciated without much more detailed descriptions of the relevant science.

In his introduction Beckman candidly (and correctly) claims that to write a book about this subject could incur writing also an encyclopaedia to explain the whole of astronomy. He opted out from such a gargantuan task by restricting himself to those aspects of astronomy and astrophysics with which he had had personal experience and interest. The result — this book — is nevertheless something of a micro-encyclopaedia and therein lies my chief complaint. What he has done — and generally quite successfully — is to roam steadily through the wavelength regimes of electromagnetic radiation, next to bring in information from other types of sources such as physical ones (*e.g.*, meteoroids and meteorites), and then — only then — to offer his flavour of what astronomers mean by 'multimessengers' and (more importantly) how we cope with them.

The book that he has produced is rather indigestible. It attempts to be comprehensive and is successful in parts, but its balance of developments does tend to favour Europe. Canada, for instance, is only mentioned in a passing reference to the team who first made Very Long Base-Line Interferometry work over a substantial distance (3074 km, in fact), while the study of meteoroids (as another example) makes no reference at all to the sterling work of Canadian Peter Millman who, almost single-handedly, elevated the study to a professional level, organized and energized international campaigns, and demonstrated unequivocally that the craters on the Moon and in old rock terrains upon Earth originated from meteorite impacts rather than volcanoes.

Multimessenger Astronomy benefits from a substantial degree of literature and internet searches. However, it might have been more convincing had its order been inverted so that it commenced by demonstrating the complementarity — or contradictions — of those multimessengers, and making them the central focus (together with a large assortment of examples), rather than as the brief afterthought that is presented here. Beckman could then have included some of the basics of stellar physics as deduced purely from optical studies, and how thinking has been modified by the inclusion of evidence from other wavelengths or frequencies (and he need not have felt obliged, for historical reasons, to explain the intricacies of photographic observing and then trimmed it to the extent of misrepresenting it).

The rather cramped format of the book adds to a sense of scrambling to include a great deal of material. That is a complaint to lay at Springer's door, of course, but another problem associated with covering everything in as much detail as possible is the risk of some contents becoming out of date before one even reads to the end. The writing flows well, though common misuses of grammar are there: confusing 'meteoroids' (the particles causing the flashes) with 'meteors' (the flashes themselves*), misusing 'comprise' and 'due', not applying enough commas to avoid ambiguities ... are just a few that stood out. — Elizabeth Griffin.

*As per the IAU definition

Experimental Astrophysics, by Elia Stefano Battistelli (IoP Publishing), 2021. Pp. 211, 26 × 18·5 cm. Price £75/$120 (hardbound; ISBN 978 0 7503 3117 3).

The idea behind this volume is an excellent one — to give budding astronomers or astrophysicists some understanding of many of the widgets they and their colleagues will be using to collect data. The focus is on electromagnetic radiation. This is perhaps just as well, as the introductory chapter includes photons and neutrinos among the cosmic rays, and says that meteorites can "provide us with very valuable information about the cosmo." As for the neutrinos, the classes of sources are "fusion of hydrogen, which produces ν_e"; "The Sun which produces solar neutrinos"; "Supernova explosions, which are associated with neutrino bursts"; and "A cosmic background ... today a 1·9K cosmic neutrino background." There is no indication of which sources yield mu and tau neutrinos or what can happen to them on their way to us. But "they can help provide information about celestial bodies that are too distant to detect using photons." I suspect the author meant that the cosmic neutrino background separated from matter before the CMB. As for those solar neutrinos, they "produce only one interaction for every 1036 target atoms." Well, the solar neutrino unit (SNU) is 10^{36} captures per target nucleus per second, and the solar flux is three or thereabouts.

Nevertheless, I turned eagerly to sections on devices I would really like to understand better — Zener *versus* Schottky diodes, lock-in amplifiers, Johnson noise, field-effect transistors (and with hopes for integral field units, but they are apparently not here — no index; though a quite detailed table of contents). Not, frankly, a lot of joy. A possible interesting use for the text would be to assign to individual students individual sections to be amplified, clarified, perhaps updated, and I would be happy to review the expanded, modified volume!

There is an extended Appendix of laboratory and observing activities (testing the Nyquist theorem, measuring the speed of light, calibrating an optical telescope). Some of these are perhaps too closely tied to the facilities of the author's institution (University of Roma la Sapienza). Some of the examples will date quickly, for instance, the relative costs of cooling 100 kg of copper from room temperature to 4·2 K, using just ^{4}He, *versus* pre-cooling with liquid N_2. But the principle that precooling is both cheaper and faster should remain! Helium-3 refrigerators, dilution refrigerators, and adiabatic demagnetization refrigerators also make cameo appearances.

We who teach for a living, or try to, sometimes think about how to set the minimum passing grades for the courses directed at engineering and pre-medical students. Would I want to cross a bridge designed by a B student or consider an operation to be performed by another such? Probably you would not want to write a review article based entirely on data gathered by astronomers whose only knowledge about instrumentation (noise suppression, *etc*.) came from this volume. But the underlying idea was undoubtedly an excellent one. All references and credits for images come from on-line sources, very frequently Wikimedia for the illustrations, so these are likely to vanish fairly soon, another suggestion that the book could be revised to advantage (probably no need these days for the properties of crown and flint glass, nor indeed the statement that photographic materials are more efficient than human vision; not quite true in any case, though they are really very much better at time exposures!). — VIRGINIA TRIMBLE.

Detection of Light, 3rd Edition, by George H. Rieke (Cambridge University Press), 2021. Pp. 370, 25 × 17·5 cm. Price £54·99/$69·99 (hardbound; ISBN 978 1 107 12414 1).

The first thing to emphasise is that this book is, as its title implies, purely about the physics, design, and operation of *detectors*. There is nothing about light gathering (*i.e.*, telescopes or other optical systems) and it does not even extend to cover astronomical instruments such as basic imagers or spectrographs. Nonetheless, this means that the focus of the book (and its target readership) is clear — if you are involved in building an astronomical instrument with a detector system, or want to understand better the details of detector operation when reducing and analysing astronomical data, then this is an excellent place to start.

The book is increased in breadth from earlier editions, and now covers the vast majority of the electromagnetic spectrum, from millimetre wavelengths to X-rays. It isn't really a book to be read from cover to cover — it is really too dry and detailed (with many equations, albeit none too complex) for that to be palatable. Rather, if one wants to understand a specific type of detector, including its strengths and weaknesses, then delving into the relevant chapter provides an excellent introduction, equipping the reader with the background required to move on to the more technically detailed material contained in the (up-to-date) 'further reading' list provided at the end of each chapter. A set of problems designed to deepen understanding is also provided at the end of each chapter, with answers for (some of) them provided in an Appendix. The book is also well illustrated, with numerous detailed diagrams, and the referencing is comprehensive.

In my view this textbook has three main strengths. First it is undoubtedly authoritative, as expected from a leading practising expert in the field. George Rieke has spent his career developing cutting-edge astronomical instruments and then conducting novel research with them. Indeed, he and Gillian Wright in Edinburgh have co-led the development, construction, and testing of the mid-infrared *MIRI* instrument recently launched on-board the *James Webb Space Telescope* (although if you are looking for a description of *MIRI* in this book you won't find it — instead see https://www.stsci.edu/jwst/instrumentation). The second impressive feature of the text is the efficient and effective way in which the underlying physics is explained at just the right depth required to understand each type of detector; a good example is the very digestible description of superconductivity (neatly sidestepping the horrors of BCS theory). The third strength of the book is the way in which it gets beyond simplistic, generic descriptions to explain how and why real-world detectors have been refined in design, clarifying the strengths and weaknesses of each modification or increased layer of sophistication (*e.g.*, when discussing buried channel CCD devices George provides a nice explanation of why these devices were developed, how they are made, and the resulting pros and cons of such CCDs for light detection). This third point means that the book could be usefully purchased as 'technical backup' by those involved in teaching, say, observational astronomy. It is also pitched at just the right level for masters-level or postgraduate students pursuing work involving the development of astronomical instrumentation, or indeed any area of physics involving photon detection. — James S. Dunlop.

Auroral Physics, edited by David J. Knudsen *et al.* (Springer), 2021. Pp. 409, 24 × 16 cm. Price £109·99/$149·99 (hardbound; ISBN 978 94 024 2121 7).

It seems hardly possible, but is true, that nearly twenty years have passed since the publication of the comprehensive landmark review volume *Auroral Plasma Physics* (Springer, 2003), edited by Paschmann, Haaland, and Treumann. Given the considerable body of work on this complex and multi-faceted research topic that has subsequently been published, it now seems appropriate for further stock to be taken on progress and remaining outstanding issues. The new volume, *Auroral Physics*, again the outcome of a workshop held at the International Space Science Institute in Berne, in 2018 August, and collected from individual articles previously appearing in *Space Science Reviews*, offers a comprehensive and detailed overview which emphasizes progress since the publication of the previous volume. Tightly packaged but clear and well-produced individual review chapters cover all aspects of research on Earth's aurorae, including, for example, three on quiet arcs (morphology, particle acceleration processes, generator mechanisms), as well as chapters on dayside and polar cap aurorae. Further chapters cover auroral dynamics, pulsating forms, mesoscale forms, and small-scale structures. It is perhaps as well that the project did not seek to cover aurorae of other planets as well, otherwise the more-than-400 packed pages of the present terrestrial-focussed volume would easily have doubled in size. — Stanley W. H. Cowley.

Dynamics of the Sun and Stars: Honoring the Life and Work of Michael J. Thompson, edited by Mario J. P. F. G. Monteiro *et al.* (Springer), 2020. Pp. 343, 24 × 16 cm. Price £129·99/$179·99 (hardbound; ISBN 978 3 030 55335 7).

Dynamics of the Sun and Stars, subtitled *Honoring the Life and Work of Michael Thompson*, contains, as its title indicates, the proceedings of a special conference organized to honour the life and work of Mike, as I knew him. I had the pleasure and privilege to know Mike throughout most of my scientific career and I was part of the large collaboration he had initiated prior to his sudden and untimely death on 2018 October 15. The conference to honour his life was held in Boulder, Colorado, in 2019 September.

The book's preface is from his wife, Kate Thompson, and it also includes an article by his son, Robin Thompson, an Assistant Professor of mathematical epidemiology at the University of Warwick, presenting results of his research on the threat of major outbreaks of vector-borne diseases, a line of research made quite relevant a few months later by the 2019 COVID outbreak*.

This conference covered a wide range of topics in solar and stellar physics, and the book's coverage of the proceedings is divided in five parts. The first, most appropriately and quite movingly, is a tribute to Mike. It presents very personal accounts by five of his close colleagues, collaborators, and mentors. The other four parts include 'Solar Interior and Dynamics', a chapter entitled 'From the Sun to the Star', a chapter on 'Stellar Dynamics', and finally comments on the future of the field. The articles in these proceedings range from review papers that give the reader a great exposure to the intricacies of the subjects covered (and, when appropriate, an emphasis on Mike's contributions), to recent results of work in progress, allowing the reader to get a good sense of where the field

*I should add that Robin became a regular fixture on British media, explaining to the general public models of outbreaks at the onset of the pandemic.

stands. The proceedings include contributions from both oral and poster presentations. The quality of the articles is a great tribute to the legacy of Mike's work and his impact on the field. Because of the nature of the conference, some of the authors have included photos of Mike in various situations, from spontaneous images snapped at scientific meetings or conference receptions to a photo of Mike playing keyboard at a Christmas party, and the back cover of a DVD containing Mike's Christmas carols. — Sylvain Korzennik.

Mystery of the Ashen Light of Venus: Investigating a 400-Year-Old Phenomenon, by John C. Barentine (Springer), 2021. Pp. 257, 23·5 × 15·5 cm. Price £22·99 (paperback; ISBN 978 3 030 72714 7).

I once spent a night in a camper van on the shores of Loch Ness. I didn't see Nessie, and didn't seriously expect to. Catching a glimpse of the Ashen Light of Venus has sometimes been likened to seeing the elusive Monster — not often reported, and the positive sightings sometimes doubtful or unconfirmed. I've even heard European astronomers refer to the Ashen Light as a phenomenon purely of the British amateur astronomer. But many of those who have rejected it have never made a serious attempt to see it themselves. Even some well-known amateurs have told me that they couldn't capture it upon their images, and after a few years, gave up trying. But it turns out that few of them were observing under the right conditions. Nor does Nature always cooperate. I spent a lot of time not managing to see kingfishers in my local country park, even though a few pairs are known to live there, but then one day casually sitting looking out of the window of my laboratory there was one equally casual kingfisher sitting a few metres from me, lunching in a nearby tree.

The cause of the Ashen Light of Venus has been a longstanding debate in astronomical circles since the 17th Century. It is an extremely faint glow sometimes seen to illuminate the night side of the planet, in whole or in part, but it can only be seriously looked for once the planet's phase has been reduced to a narrow crescent, due to the overwhelming bright glare from the sunlit hemisphere. True, there have been reported sightings at high phase or in full daylight, but these have rightly been rejected as illusory. After that, we are still left with a long list of observers — many of them highly respected — who have been convinced by the reality of the effect. The necessity of having a dark sky background immediately eliminates the majority of potential observers, for one is faced with observing the planet at low altitude. Today there are far more observers than 400 years ago, but also much more artificial lighting and intrusive urban sprawl with which to contend. Nor is it surprising that more observations have been made during evening elongations than morning ones. After all, astronomers do need to sleep sometimes.

Mystery of the Ashen Light of Venus (why no definite article?) is the second of only two books upon my bookshelves to explore this phenomenon. The first was written by the eccentric 19th-Century German astronomer Schorr, who used sightings of it to try to verify the existence of a Venusian moon. Barentine begins in a sceptical manner with a lesson in telescopic illusion ('The Martians That Never Were'). But he omits to mention that the Martian canals were disposed of by visual observations in 1909, not by the cameras of *Mariner 4*. It would have been better to have first set out a clearer description of the phenomenon in its daytime and nighttime forms, and some sort of overview, before plunging into the world of telescopic illusion. We do not get to Riccioli's first observation until page 33. In fact the latter's account illustrates the problem that descriptions of the extensions of the cusps (horns) of Venus very close to inferior conjunction (caused by atmospheric scattering of sunlight) can get mixed up with reports of the illuminated night side.

At least three times in his book Barentine claims there are no digital images of the Ashen Light, which shows he has not fully studied the recent literature. Taking visual waveband images of the Ashen Light will always be challenging from a technical viewpoint, but the author appears unaware that success was achieved by Gasparri in 2009 March[1]. In fact there was a positive visual report received from a very experienced observer on the same evening as the image.

And here I have to admit to my personal interest. During 2004–2019 I directed the Mercury & Venus Section of the British Astronomical Association, and received thousands of observations from a worldwide network of several hundred astronomers. It was in 2004 that the first amateur images of the night-side thermal emission of Venus were made, by Pellier in France. CCD cameras are very sensitive to infrared, making them ideal for recording the thermal emission. The emission is altitude-dependent, so lower-albedo features represent elevated areas. In recent years narrowband observations have captured remarkable topographic detail, including variable bright spots which just might be interpreted as evidence of volcanism. And here we have one explanation for the Ashen Light phenomenon: the tiny visible portion of the black-body radiation curve for Venus, an idea due to Professor Fred Taylor. The surface temperature of the planet is extremely close to that at which glass just begins to emit a dull red glow when placed in a furnace. Notwithstanding the issue that low levels of light may not adequately stimulate the colour receptors in the human eye, reports of colour in the Ashen Light almost always mention redness. The only debatable point is whether the feeble glow from the surface could be intense enough to be detectable telescopically.

In Chapters 3–4 the observations of 1643 to 1800 and 1800 to 1900, respectively, are reviewed. Making these historical sightings available in some considerable detail, with useful translations of the actual words used by the observers (and details about them personally and their careers), is a most valuable aspect of this book.

Having reached 1900 Barentine reviews the various explanations advanced for the Ashen Light, including the glowing-surface theory. But some of these ideas originated well after 1900, and this arrangement gives the impression of the story jumping forwards and backwards in time, which is something I found mildly irritating.

He then covers the epoch 1900–1980. By 1900, Barentine notes, the Ashen Light had found its way into many astronomy textbooks, but some authors like Danjon and Newcomb were too ready to write it off as pure illusion. Danjon even made the mistake of claiming that it had only been seen with refracting telescopes. Quite a lot of space is devoted to discussing the illusory 'darker than the sky' aspect sometimes reported during daylight, which is quite unconnected with the 'positive' twilight effect. It would have been better to have concentrated purely upon the twilight results. It was useful, however, to see the details of the Barbier observing campaign of the French Astronomical Society (1935) quoted in detail, and in English. But in 1935, though in a favourable eastern elongation, Venus did not cooperate with a display. This section includes some very useful references and a discussion of sky brightness values, always a key question. A flurry of sightings coincided with the high solar maximum in 1957, but it is incorrect to suggest that there were simply more observers looking at Venus in that decade. We then reach the spacecraft era, when sightings had considerably fallen off. This may indeed have rendered the phenomenon "a quaint curiosity from a bygone era" in the eyes of those professionals associated with the space missions. In 1988 there was another campaign, this time organised by Phillips and Russell in the USA. There were some positive sightings, but none appeared to correlate with geomagnetic disturbances. (Need they do so? It depends upon the mechanism responsible for producing the effect.)

Why does the Ashen Light vary so much in frequency? There are favourable and well-observed elongations in modern times where there have been no definite reports. Yet in 1957 there was an entire fortnight where there were reports (sometimes several) on every day. Many authors have sought a link with solar activity, but other high maxima have not correlated well with positive reports. A clue may come from the fact that the Light sometimes looks patchy. If really so, this might be due to absorption of the surface radiation by variable low-altitude cloud. This in turn poses the question about the variability of that low cloud. A currently active surface might be one explanation. After all, there is no doubt about extensive past volcanism, and the large disparities between the sulphur dioxide concentrations monitored during atmospheric entry by landing craft in different decades. *Pioneer Venus Orbiter* spectra showed a decline of atmospheric SO_2 and sulphate aerosol particles since the 1960s.

The upper Venusian clouds captured by UV imaging show a 4-day period. In fact this period can be traced back to 1927 in the UV records, and even back to the 1890s by means of the best drawings made in visible light. It is constant, and yet from time to time (as first pointed out by Boyer) parts of the characteristic 'Y' and 'psi' patterns disappear, only to reform weeks later. To account for the varying levels of the UV chromophores responsible, the idea of on-going volcanism is an appealing and not impossible one.

In Chapter 7 Barentine discusses the later spacecraft results, and after wisely rejecting lightning as a possible explanation, turns to the matter of oxygen emission upon the night-side. This is another plausible mechanism, in which oxygen radicals formed on the dayside by photo-dissociation recombine during the Venusian night to emit green light with a wavelength of 558 nm. Significantly, each detection of the green line emission from Venus was preceded by a solar flare or coronal mass ejection. The issue remains whether this airglow emission is sufficiently intense (or could continue long enough at times well removed from high solar maxima) in order to account for the ground-based sightings.

This is a thought provoking, nicely produced, and inexpensive book for the general reader, and the explanations of the diverse physical processes involved are always detailed and very clearly explained. However, not all published sources of Ashen Light sightings were explored by the author, so his statistics seem to me incomplete. Post-WW2 observations quoted are nearly all from the USA, but the long and unbroken series of reports by the BAA (which contain a significant number of (hopefully objective) sightings) for 1956–2020 hardly feature. Chapters dealing with optical illusions and lost planets are interspersed with observation and theory. I accept the point that some visual observers will always see what they wish to see, but the story of Neptune's discovery and the famous Vulcan episode, together with details of some well-known optical illusions and a discussion of the psychology of the observer, though interesting and well written, seem hardly relevant. Those personal opinions disposed of, I would still recommend the book.

Finally, as I complete this review, matters may have been overtaken by events, for the *Parker Solar Probe* has bypassed Venus and returned remarkable images that clearly reveal surface features of the night side in the visual waveband[2]. These show that the Ashen Light cannot be pure fiction, and doubtless there will be plenty more discussions of it in the future. But about the reality of old Nessie, I'm not so sure ... . — Richard McKim.

References

(1) R. J. McKim, *J. Brit. Astron. Assoc.*, **129**, 149, 2019. This paper contains a full account of the BAA Ashen Light sightings for the period 2007–2017, together with the images mentioned in this review.
(2) B. E. Wood *et al.*, *Geophys. Res. Lett.*, **48**, e2021GL096302.

The Human Factor in the Settlement of the Moon. An Interdisciplinary Study, edited by Margaret Boone Rappaport & Konrad Szocik (Springer), 2021. Pp. 317, 24 × 16 cm. Price £99·99 (hardbound; ISBN 978 3 030 81387 1).

This edited volume presents contributions by a number of authors who consider a range of scientific, social, economic, and political questions related to the future exploration and, perhaps ultimately, settlement of the Moon. It follows a similar volume edited by Szocik on *The Human Factor in a Mission to Mars* (Springer, 2019). Like all edited volumes of this type, the quality and usefulness of individual chapters is somewhat variable, but on balance I think the editors have done a good job in producing a wide-ranging summary of societal issues that will surely be relevant if and when humans choose to settle our nearest celestial neighbour.

I found the chapters dealing with the life sciences and associated medical hazards of living on the Moon to be the most interesting and informative; these chapters are all well-referenced and provide a valuable introduction to the relevant literature. The same is true of the chapter summarizing the lunar environment for non-specialists. The chapters detailing various sociological aspects of future human outposts on the Moon are more variable in their depth of analysis, but again contribute useful introductions to the literature (although one of those got a bit carried away by suggesting the possibility of terraforming the Moon — perhaps not strictly impossible[1] but surely implausible for the foreseeable future!). There is one chapter that summarizes the current international political and legal framework for human activities on the Moon, and this likewise provides a helpful introduction to the literature, although I felt the book would have benefitted from more contributions on this important topic. There is also an interesting chapter summarizing the importance of the Moon for different cultures on Earth, although I felt it could have been more comprehensive and would have benefitted from stronger references to the relevant historical and anthropological literature.

The one chapter to which I took exception concerns military uses for the Moon. Indeed, I think the whole idea of military activity in space needs to be actively resisted, and the authors appear unaware of existing international law on this topic. For example, they write (p. 174) that "there are no existing treaties that address the use of militaries on the Moon," whereas in fact Article IV of the Outer Space Treaty of 1967 clearly states that "[t]he establishment of military bases, installations and fortifications, the testing of any type of weapons and the conduct of military manoeuvres on celestial bodies shall be forbidden." Thus, their advocacy for the development of "missile silos", "weapons storage" and "customised military weapons suitable for the lunar environment" (pp. 182–184) are all contrary to existing international law, and hopefully will remain so. Rather than advocating a realpolitik argument that international military competition on the Moon is somehow inevitable, the authors of this chapter would have done well to reflect on William Hartmann's 'Golden Rule of Space

Exploration' to the effect that "space exploration must be carried out in a way so as to reduce, not aggravate, tensions in human society."[2]

My only other quibble with the book concerns the index. Whereas most of the chapters were well-referenced, the sparse index rather detracts from the book's likely value as an academic reference work. — IAN CRAWFORD.

References

(1) R. R. Vondrak, *Nature*, **248**, 657, 1974.
(2) W. K. Hartmann *et al.*, *Out of the Cradle: Exploring the Frontiers Beyond Earth* (Workman Publishing), 1984.

The End of Astronauts: Why Robots Are the Future of Exploration, by Donald Goldsmith & Martin Rees (Harvard University Press), 2022. Pp. 192, 22·5 × 14·5 cm. Price £20 (hardbound; ISBN 978 0 674 25772 6).

Humanity has been venturing into space for more than 60 years. During that time, around 600 people have experienced life in a weightless environment. With the exception of 24 astronauts who flew to the Moon and back, the vast majority of these have remained close to home, only a few hundred kilometres above our planet's surface. In contrast, robotic ambassadors have travelled millions or billions of kilometres from Earth, visiting all parts of the Solar System, with landings on such diverse — but inhospitable — worlds as Venus, Mars, and Titan. Closer to home, automated spacecraft also prepared the way for the people to land on the Moon.

Since the early years of the space age, a debate has raged between those who argue that human spaceflight is far less cost effective and efficient than robotic exploration, and those who argue that robots can never replace human intelligence and creativity, and that it is humanity's destiny to explore and colonize new worlds. In this book, Donald Goldsmith, a well-known science author, and Martin Rees, the UK's Astronomer Royal, have provided a new overview of the debate as they weigh the benefits and risks of human exploration across the Solar System.

Many of the points they put forward are familiar, but incontrovertible. In space, humans require air, food, and water, along with protection from potentially deadly radiation and high-energy particles. These life-saving measures result in much higher costs for human missions than robotic exploration. Meanwhile, robotic explorers have demonstrated the ability to investigate planetary surfaces efficiently and effectively, operating autonomously or under direction from Earth for many years. Although the authors accept the current limits of artificial intelligence, they insist that the capabilities of robots will steadily improve over time. Although a robot cannot equal a geologist's expertise at present, by the time a human geologist sets foot on Mars, this advantage will have diminished significantly. The authors argue that, as our robot explorers grow more competent, governments and corporations must ask, does our desire to send astronauts to the Moon and Mars justify the cost and danger? After weighing up the pros and cons, they argue that, beyond near-Earth orbit, space exploration should proceed without humans.

The book concludes with a chapter about the limitations of current space laws and the challenges that will arise as countries such as China challenge the old order and corporations race to privatize space exploration and colonization.

There is no doubt that the first space travellers inspired a whole generation of scientists and engineers, most of whom have reached or passed retirement age. Indeed, the first Apollo Moon landing was voted the most outstanding achievement of the 20th Century. As one of the generation who grew up at the dawn of the space age, and who was enthused and inspired by the remarkable exploits of the first space pioneers — robotic and human — I enjoyed the careful, well-written analysis presented in this book.

The demise of human space travel is difficult to envisage, but, as the authors conclude, although we may decide that humans belong in space despite the dangers and expense, it will be semi-autonomous robots that act as the pathfinders and personal assistants. — Peter Bond.

Extraterrestrial: The First Sign of Intelligent Life Beyond Earth, by Avi Loeb (John Murray), 2022. Pp. 239, 20 × 13 cm. Price £9·99 (paperback; ISBN 978 1 529 30484 8).

Carl Sagan once said that "extraordinary claims demand extraordinary evidence". As such, some might doubt Avi Loeb's non-mainstream claim that 'Oumuamua (a Hawaiian word for 'scout'), which everyone agrees is a body which passed through our Solar System in 2017 October but originated outside of it, is an artefact of an extraterrestrial civilization. However, Loeb (according to ADS, author of 897 works; his own 87-page CV (including publications) lists a few more as well as two patents), who has been at Harvard since 1993 and one of the few there to rise through the ranks from assistant to full professor (and that in just four years), makes the point that while there is not that much evidence, the extraterrestrial-artefact hypothesis is orders of magnitude more likely than competing ones; in that sense, the claim is not so extraordinary. The main evidence is its non-ballistic orbit, which indicates an acceleration in addition to that explicable by gravity, namely one directed away from the Sun and following an inverse-square law with respect to the distance from the Sun, which together with observations compatible with it having a very flat pancake shape leads to his hypothesis that 'Oumuamua is a light-sail; no known natural process could produce an object of such form. Loeb himself was involved in Yuri Milner's Breakthrough Starshot project, the goal of which is to transmit close-up information from the Alpha Centauri star system within Milner's lifetime. The proof-of-concept study envisages using a laser array with a power of about 100 GW to accelerate a thousand or so objects of centimetre size and gram mass, known as StarChips, to 20 per cent of the speed of light within a few minutes. The project, on a financial scale comparable to that of the Large Hadron Collider, could be completed on time with near-future technology. 'Oumuamua is much larger, on the scale of a few hundred metres (observations are compatible with Loeb's very flat pancake or with a cigar shape), but it is clear that a technology not much more advanced than our own could have manufactured it, if it is indeed a light-sail.

Loeb details several other arguments, involving 'Oumuamua's albedo, lack of change in its presumably rotation-induced light-curve (making the out-gassing explanation for its anomalous acceleration, which in such a case would also just coincidentally be the same as if caused by reflected sunlight, unlikely), *etc.*, to support his hypothesis, including an interesting discussion of the fact that 'Oumuamua was at the local standard of rest before encountering our Solar System. Copious references are provided to his and others' technical papers (both those supporting his hypothesis and those supporting more conventional explanations). All in all, a convincing argument. But the book is about not just 'Oumuamua, but also the fact that his hypothesis is perceived to be unlikely not because of the evidence, but because of a refusal to think outside of the box. Loeb's involvement in the Breakthrough Starshot project certainly primed him for that hypothesis, recalling Pasteur's dictum that fortune favours the prepared mind. Despite its popular appeal, few scientists actually work on SETI (the search for extraterrestrial intelligence), and Loeb also criticizes some of those who do for being too closed-minded, concentrating on communication when 'astro-archaeology' might provide much more evidence of extraterrestrial civilizations.

The book is also a memoir in that Loeb recounts various episodes from his life which, apart from being interesting in themselves, have influenced his thinking. Where I agree to disagree regards his belief that the discovery of extraterrestrial intelligence, whether *via* his hypothesis about 'Oumuamua becoming generally accepted or *via* other, perhaps more spectacular means, would somehow significantly transform the thinking of humanity in general. Similar claims have been made about the Earthrise photo from *Apollo 8* and Sagan's Pale Blue Dot. (I'm sure that I wouldn't see that differently were I not writing a few days after the start of the war against Ukraine.) "[S]uch a discovery would also affect the way we behave and how we interact with each other", while Tegmark[1] (reviewed in these pages[2]) arrives at very similar sentiments from the opposite premise, namely his belief that we might be the only intelligent life in the (observable) Universe. (My impression is that most who have thought about the matter believe that there are, or at least have been or will be, many extraterrestrial civilizations, but also that it is unlikely that we will have evidence for their existence in the foreseeable future.)

I do agree, though, that 'Oumuamua should change our thinking in a different way. It was discovered by the *Pan-STARRS* telescope. While specifically designed to look for moving and/or variable objects (such as potentially dangerous near-Earth asteroids), like many surveys (which are the life-blood of astronomy, according to Paul Schechter) it has discovered objects not considered when the survey was being planned. But if we take seriously the idea that 'Oumuamua is an artefact of an extraterrestrial civilization, then chances are that another such object will be detected before long, especially if we look more avidly for it. We need to have experts ready to study it: astro-archaeologists, perhaps even astro-linguists, astro-sociologists, or astro-economists. We should have the technology to approach it closely or even intercept it. That could have happened earlier. Not only if, in an alternate reality (Loeb temporarily indulging in a bit of Multiverse speculation, an idea which he otherwise thinks to be not very useful), the effort which went into World War II would have gone into science. Would we have been able to photograph 'Oumuamua at close range? Capture it? (That line of thought was inspired by an unpublished and, until 2016, unknown article by Winston Churchill, who wrote many popular-science pieces, asking the question whether we are alone in the Universe. Perhaps history would have been different had that article been published. On the other hand, as Loeb notes later in the book, Otto Struve had written on methods of exoplanet detection in 1952[3] but that had little effect; ADS lists 106 citations — a considerable number for an article in this *Magazine* — but only six are before 1993.)

While the idea that 'Oumuamua is an artefact of an extraterrestrial civilization is perhaps a bit 'wild', Loeb's analysis is very down-to-Earth (so to speak) and evidence-based. He tends to be sceptical of topics such as string theory and the Multiverse, both with little hard evidence to support them. That in itself, in my view, shouldn't be a reason not to work on such topics; like with some of the ideas favoured by Loeb, thinking outside the box might produce tangible results. I agree, though, that there is a danger that people, especially those without tenure, might tend too much to work on popular topics and it would be nice to see more funding for astro-archaeology, even at the expense of other topics (though I would take it from string theory rather than the Multiverse).

There are a few black-and-white figures throughout the text. My only real complaint is that at least some of them could benefit from being larger and/or in colour. The main text is followed by literature references (indicated by page number and enough quoted text to match the text to the reference, rather than by numbers in the text) and suggestions for further reading, both grouped by chapter; the book ends with an eight-page small-print index. In the interest of space, I am leaving out some other topics discussed in the book; they are worth reading about (even if one doesn't agree with every detail), but those mentioned above alone are reason enough to read the book. The fact that the book is well written and almost completely free of the objects of my usual criticisms (typos, matters of style, *etc.*) strengthens my recommendation. Loeb provides enough biographical details to convey that he is probably not a native speaker of English, but one wouldn't notice that from reading the book, which is better written than many by native speakers. Even if you don't agree with everything, indeed, even if you don't even agree with his main thesis, the book is still worth reading. — PHILLIP HELBIG.

References

(1) M. Tegmark, *Our Mathematical Universe* (Allen Lane, London), 2014.
(2) P. Helbig, *The Observatory*, **134**, 150, 2014.
(3) O. Struve, *The Observatory*, **72**, 199, 1952.

The Pluto System after New Horizons, edited by S. Alan Stern *et al.* (University of Arizona Press), 2021. Pp. 663, 28·5 × 22 cm. Price $65 (about £48) (hardbound; ISBN 978 0 8165 4094 5).

NASA's *New Horizons* interplanetary space probe flew past Pluto at a speed of 50 000 km/hr, and at a miss distance of 12 500 km, on 2015 July 14. The probe-facing surface of Pluto and Charon was imaged, atmospheric composition and airglow was monitored, rings were sought for, infrared studies mapped the chemical composition of the surface, and temperatures were measured. Other moons, Nix, Hydra, Styx, and Kerberos, were imaged. After Pluto the craft voyaged on for a further three and a half years to visit the Edgeworth–Kuiper Belt object Arrokoth. Four years after the Pluto flyby two hundred scientists met for a four-day conference to discuss the mission results. The book being reviewed contains the twenty-eight papers from that conference.

The densities of Pluto and Charon indicate that we are dealing with differentiated bodies consisting of roughly ⅔ rock and iron plus ⅓ ice. Like all Solar System bodies, the temperature increases as one moves towards the centre, and there is a reasonable possibility that Pluto has a subsurface ocean about 100 km deep. The surfaces of Pluto and Charon reveal cratered plains, mountainous ridges, valleys, scarps, and icy glacial features. Pluto has an atmosphere that varies drastically with the orbital position, and with Milankovitch cycles of obliquity.

It is amazing how our image of Pluto and Charon has changed. The pixellated blobs revealed a few decades ago by the *Hubble Space Telescope* have been replaced by colourful detailed images of almost half the surfaces. This book quite rightly concentrates on what has been learnt. As is to be expected from a volume in this University of Arizona planetary series, the standard of production is exemplary. I was slightly worried that too little time was spent on the unknown. One of the tasks of this volume was surely to convince the reader that these exemplars of Edgeworth–Kuiper Belt members need to be reinvestigated. The speedy and distant flyby needs to be followed by orbiting craft. But this next step, which is clearly difficult and costly, may be many decades away.

I have always been fond of Pluto. Being found a decade before I was born, I regard it as our modern planet. I was disappointed when it was demoted to 'dwarf' status. It might be 'dwarf' by modern definition but it is a giant in terms of surface complexity and atmospheric variability. As a Solar System body of interest, it knocks the other outer planets into a cocked hat. This book will do much to rocket Pluto back into the major league where it rightfully belongs. — David W. Hughes[†].

[†]The Editors are deeply saddened to report that David Hughes passed away on 2022 June 6. It is hoped that a tribute will appear here in due course.

Stories of Astronomers and Their Stars, by David E. Falkner (Springer), 2021. Pp. 302, 23·5 × 15·5 cm. Price £24·50/$34·99 (paperback; ISBN 978 0 030 80308 7).

The initial thought I had before diving in to the book itself was that the title sounded like a rather good idea. Compile a list of stars which are known by their discoverers or principal investigators, and attach a biography of the astronomer to each. So we could have the stars connected with Edward Emerson Barnard, Antoni Przybylski, Tabitha Boyajian (Tabby's star), and Adriaan van Maanen to name but a few. However, David Falkner in his second book has chosen to write a general description of stellar astronomy with biographical emphasis on some of the great stellar astronomers of the past. Many of the names are familiar and are treated in considerable detail, and there is a welcome inclusion for the rather neglected Charles Wolf and Georges Rayet, but it could be argued that there is not much of a case for the entry for Johann Georg Liebknecht, who thought that the 8th-magnitude star located between Mizar and Alcor was a planet and was roundly criticized for it, to be included.

The book begins with a 30-page verbal description of the northern and southern heavens as they appear at the equinoxes and at the solstices. These are accompanied by half-page maps of the sky, which, given the absorbent nature of the paper used would not bear much exposure in the night air and a dedicated star atlas would also be a useful accessory.

There are a small number of infelicities in the text: "the blue and white ends of the spectrum", the "infamous" Wolf 359 and the "notoriety" of Mizar, and, with reference to the Harvard College Observatory plates, "The northern and southern sky surveys are the only records we have of what the sky looked like between 1885 and 1992". The man who painted Isaac Newton was Sir Godfrey Kneller, and Kai Strand was at Sproul and not Dearborn. It seems difficult to believe that the best orbit for 61 Cygni is from 1948. There have been hundreds, if not thousands of high-class photographic, CCD, and space-based astrometric observations since then to increase the database significantly. In addition, the author uses a 43-year-old book to confirm the status of the hypothesized planet around 61 Cyg B, when resorting to the internet would have shown that the best supporting evidence for planetary companions in the 61 Cyg system comes from the *Gaia* spacecraft, which, strangely, the author has not mentioned anywhere.

Tackling the whole of stellar astronomy from the earliest days is a considerable task but the author covers it all well and in a straightforward and clear manner, and this book does have the advantage of being one of the publisher's more affordable volumes.— Robert Argyle.

A Tale of Two Infinities, by Gianfranco Bertone (Oxford University Press), 2021. Pp. 125, 22·5 × 14·5 cm. Price £19·99 (hardbound; ISBN 978 0 19 289815 9).

Over 60 years on from C. P. Snow's jibe about the lack of awareness of the second law of thermodynamics within some sections of society, it is indeed heartening to see eminent scientists still spending time on outreach projects such as this book. Ideally the dual goal of these enterprises should be not only to feed the appetite of the already scientifically curious, but to tempt the previously scientifically incurious to look beyond their comfort zone and cultural-bubble boundaries. This book will undoubtedly provide a meal for the former and in regard to the latter the author is to be congratulated on the provision of many a literary amuse-bouche using references to literature and art (wow, a quantum physicist who has heard of Dante and Shakespeare!) as not only an illustration of the similar landscapes science and the arts often explore, but more particularly as a means of illustrating the close parallels between artistic and scientific imagination.

No matter how laudable a goal it might be to produce a text "accessible to the general reader with no former knowledge on the subject" or one that "will find its way into the hands of young readers who will be inspired to follow", achieving that is still as difficult as ever. Perhaps authors in this genre would do well to bear in mind Lord Reith's vision for the BBC's role to "inform, educate and entertain". All three contribute to success, but getting the balance right is the tricky bit. Does this book succeed on this basis? Well, yes, but with perhaps inevitable caveats.

It is strikingly brief, coming in at barely 100 pages, and starts with a full 35 pages of scientific context (orders of magnitude in nature, from cosmologically large to quantum small) intertwined with historical background (both scientific and personnel) before diving into the main themes. Some readers will no doubt appreciate the gentle introduction (inform) and the foibles of some of the scientific personalities mentioned might even raise a chuckle (entertain), but others will quickly respond, or eventually wish they had, with a 'nothing new to see here, move on'. The final two-thirds of the text (educate) survey the ever-growing importance of data gleaned from the complements to electromagnetic radiation, *viz*. gravitational waves, cosmic rays, and neutrinos (dubbed "messengers from space" though it probably sounds sexier in Italian) and the role they are expected to play in our future understanding of dark matter, dark energy, and the physics of black holes. Brevity unfortunately often comes at the cost of clarity, with many explanations potentially as puzzling as the basic statements. As a result, too often, insufficient space or time is allocated for the benefit of readers who may need a helping hand with unfamiliar concepts and terminology. Having taken advice from a reader typical of the non-specialist readership mentioned above, I fear that as a result there may well be too many foreclosures before the stirring Epilogue is reached; an Epilogue incidentally that has a distinct JFK (… not because they are easy…) or even Henry V at Agincourt feel to it. In fact, read the Epilogue first — you might then be psychologically better prepared to survive the irritations of the main text.

Throughout, the author lets his enthusiasm for the subject and its research enterprises shine through, which is an excellent feature given that the momentum of presentational enthusiasm can help vault many a deficit in an author's or speaker's explanatory clarity. At times, though, this enthusiasm drives an optimism about future progress which might seem a little excessive or even naïve. Bertone's glass is definitely more than half full, but Popperians might require a little more nuance in his apparent faith that new observations will have the final say in confirming any future theoretical frameworks. He does, though, also admit of the possibility that in the face (presumably one can no longer say merely "light") of new data, the foundations of the scientific cathedral, to which he likens the Standard Model(s), might crumble at any time. Sensibly, he maintains a foot in both camps and it's all good, tense, wild-frontier stuff.

So far, so good, but my main grievance is reserved for the publisher. The book was originally published in Italian and OUP claims the copyright for the English translation. Whether that means it was done in-house, the author provided his own translation, or it was outsourced to translation software, one can only speculate. Either way, where is the quality control a reader could rightly expect in a publication of this nature? I would be upset, and rightly so, were I to read a translated work of fiction that contained as many glitches in sentence structure and word order or in ambiguous phrases, wild tense changes, and un-English expressions, not to mention failures like referencing the number of a chapter when they are actually not numbered, nor the paltry index that nevertheless helpfully contains separate entries for 'Nobel Prize' and 'Nobel prize'. No, this cannot be dismissed as tetchy pedantry or a philistine's lack of appreciation of the author's distinctive literary flair. For many readers, trying hard to concentrate on the narrative, these defects will at best be distracting (the ill-timed concert-hall cough) or at worst confusing and potentially serious impediments to understanding. Is the phrase "rotating about itself" just awkward or indicative of a novel dynamical phenomenon? It would have taken a literate, native English speaker, preferably with a science background (aye, there's the rub?), no more than a couple of hours to find and correct these problems and at what extra publication cost? Does OUP care about reputational cost? One would hope so. Harping back to the excellent Epilogue, it would be fascinating to know what a forensic linguist would make of a comparison between it and the main text.

The inevitably shining reviews contained in the book and on the dust jacket are from a selection of A-list popular-book authors. I am surprised that apparently none of them could have had any influence with OUP over the shortcomings noted above. But then I was amused to notice a letter for sale from an antiquarian bookseller. The letter is from George Gershwin written in 1927 to a publisher expressing his regret that he had not yet been able to read a new play by Philip Barry. Gershwin writes: "I am sure that it is an interesting piece of writing and I would be only too glad to have you use a quote from me on the jacket... and feel that if you would care to tack my signature on to a few words of praise it would be perfectly justifiable…" — Dave Pike.

Hubble, Humason and the Big Bang. The Race to Uncover the Expanding Universe, by Ron Voller (Springer), 2021. Pp. 445, 23·5 × 15·5 cm. Price £24·99/$34·99 (paperback; ISBN 978 3 030 82180 7)

I owe the author of this book an apology, since I approached it with negative expectations. The title and subtitle led me to expect yet another hagiography of Hubble, recycling the common myths about his role in the discovery of cosmic expansion. In reality no-one contributed as much to this development as Vesto Slipher, who had single-handedly demonstrated by 1917 that galaxies recede on average. In his 1929 'discovery' of this expansion, Hubble plotted Slipher's correct redshifts against his distance estimates, which turned out to be almost pure junk (see arXiv:1301.7286). But in the event, the current book is by no means uncritical of Hubble and it seems to paint a fair picture of his career and scientific contribution, albeit concentrating on the later period around the 1930s. During this time, Hubble's work was closely tied up with the contributions of Milton Humason, and the book is very much a dual biography that nicely contrasts these very different men.

The book begins with a whistle-stop tour of developments in cosmology up to about 1928, which is well done on the whole, although there is alas almost no mention of Slipher. But some amends are made thereafter with a chapter that imagines the first meeting between Hubble and Humason in 1928. By this time, Slipher had redshifts for 43 galaxies and had covered pretty well all objects that were bright enough for his telescope, so the field was set to be taken over by the larger telescopes on Mount Wilson. The work of Slipher was the foundation, although the picture painted of him is equivocal: at one point he is described as "a living legend", but elsewhere we read that "nabbing a useful spectrum from M31 was no problem at all for the average astronomer", whereas no astronomer had succeeded in this task until Slipher showed how it could be done. In any case, it was clear that this sort of spectroscopy was important; but it was also difficult. With inefficient photographic plates as detectors, getting a signal required accumulating light on a single plate for many nights in a row. You sat in the cold and dark doing nothing but keeping the target centred on crosshairs for perhaps eight hours, and then the same again the following night. Hubble lacked either the skill or the stamina for this task, but Humason had both.

The story of how an uneducated mule driver became employed as the leading observer at the world's leading telescope is familiar in outline, though no less astonishing through repetition, and it is well told here. There is plenty of biographical detail, and it succeeds in bringing Humason to life, rather than simply burying the reader in facts. Humason's efforts were superhuman: in 1928 September he undertook a run of 11 nights amounting to 80 hours of observing and yielding spectroscopic redshifts for precisely two galaxies. One can only wonder what he would make of the fact that the present-day *DESI* instrument can measure over a million redshifts in the same time. After his heroic efforts (which doubled the redshift record, to over $z = 0{\cdot}01$ for the first time), Humason had a breakdown and did no observing for six months. But his return demonstrated that Humason was far more than Hubble's slave, as Humason pushed for specific improvements to instrumentation in order to make spectroscopy faster and more practical. As a result, he was able to continue advancing the redshift record: by the mid-1930s he had reached the limit of what the 100-inch telescope could do, collecting nearly 200 redshifts up to a maximum of $z = 0{\cdot}13$. Further progress would require the larger aperture of the 200-inch, but the onset of war meant that this arrived too late for Humason to be able to use it. The central chapters of the book interweave the various strands of this story in an interesting way: Humason's measurements; the history of cosmological theory and the influence of the Mount Wilson data; the history of the development of Mount Wilson through the war years to the commissioning of the 200-inch.

What was Hubble doing during all this time? Having made a great individual contribution with the 1924 detection of Cepheids in M31, an uncharitable reply would be that much of the post-1928 period was spent in self-promotion. Certainly, Voller does not pull his punches in his analysis of Hubble's character: "... fuelled by ambition, an ego that bordered on narcissism...", and he dissects elements such as Hubble's adoption of a fake English accent following his time in Oxford. But although Hubble comes across in this account as an unpleasant character, aloof and jealous, he is also treated with a degree of sympathy.

Hubble's father seems to have been a complete monster: insistent that his son should study law despite a consuming interest in science; and it is fascinating to read how Hubble managed to continue his scientific education in secret while obeying his father's wishes. But the old man at least had the good grace to die around the time Hubble graduated, although even then a change of direction away from law was not trivial as Hubble was now responsible for the financial support of his family. Facing such hurdles must have moulded Hubble's personality, but perhaps also given him a drive that contributed to his scientific successes. It is one of the achievements of Voller's book that he succeeds in giving a rounded picture of this complex individual.

Lastly, the book is also fascinating for the depth with which it treats Hubble's attitude to cosmology. He was sceptical about whether spectroscopic redshifts truly indicated velocities, and tended to believe that light lost energy with distance rather than that the Universe was truly expanding. The reason for this was his measurement of the galaxy number–magnitude counts, which appeared to have a slope inconsistent with a uniform distribution. This incorrect conclusion was partly due to erroneous assumptions about spectral shape (the K-correction), but also incorrect input from the Caltech theorist Tolman, who assumed that the distance–redshift relation remained linear. The latter mistake was corrected by Mattig, and I was previously unaware of this historical significance of his work.

Overall, then, I can strongly recommend this book as a skilful and informative mixture of vivid biography and the history of science at a unique time when our picture of cosmology was first coming into focus. — John Peacock.

More From The Library

Astronomy for All, by Bruno Hans Bürgel, translated from the German by Stella Bloch (Cassell and Company, Ltd.), 1911. Pp. 352, 23 × 15 cm. Price £25·70 *via* Amazon. Purchased at auction in a set of books owned by William Tyler Olcott, who acquired this one on 1919 March 22.

Bruno Bürgel (1875–1948) was primarily a popularizer of astronomy. The German version of *Astronomy for All* was his first major effort in this direction. In it, he is prepared to consider the possibility that the Andromeda Nebula could be a separate galaxy, comparable in size and mass with the Milky Way. And, after a (not terribly successful) attempt to explain how observations of transits of Venus across the face of the Sun could be used to determine the Sun's distance from Earth, he issues a charming invitation: "My readers are cordially invited to the next Venus transit of June 8th, 2004, at 9 a.m."

Just about unique to this book, however, is Chapter III 'Women as Astronomers'. Some of his examples shortly, but he claims that (in contrast to sometime in the past) "studious women, today, on the contrary, have every possible encouragement held out to them." Not, perhaps, quite the experience of those more directly concerned!

So, who were his women astronomers? First, "Frau Hevel" (Elizabeth Hevelius) by virtue of working with her husband. Agreed. "Maria van Lewen" claimed as a friend of Kepler who supplied him with material for his laws. Oops. Real person, Wiki'd as Maria Cunitz (1610–1664), but the generation after Kepler, though she did indeed apparently calculate things using his laws.

Madame Lepaute (1723–1788), described as working with Lalande and contributing to a calculation of the return of comet Halley. Duchess Louise of

Gotha, described as having provided the initiative for "the first astronomical congress in 1798". Real congress, but this convening of "the celestial police" to look for the missing planet between Mars and Jupiter is generally credited to von Zach.

Caroline Herschel — yes, with all honour. Sonia Kowalewski — primarily a mathematician, credited with a book on the rings of Saturn. Madame Flammarion and Mrs. Asaph Hall as collaborators with their husbands. Eight Americans, with a total confusion of titles, but surnames Bruce, Draper, Leland, Durrey, Klumpke, Somerville, Mitchell, and Fleming (several actually known as donors, but all at least real women). Elizabeth Brown, a founder of the British Astronomical Association and a not-very-successful eclipse chaser. And, finally, "lady computers" at Melbourne, Cordova, Columbia, Albany, Cape of Good Hope, and Paris.

Is this dog's breakfast better than no recognition at all? Yes, at least marginally, the more so as the author ends by saying that, while none of the women have made any world-disturbing discoveries, neither have the majority of their male colleagues. "... and the conscientious worker is no whit less important" (than the genius). Should he have known about, and mentioned, Leavitt's Law (the period–luminosity relation for Cepheid variables)? Not really; the book is just a little too early, and the recognition of the importance of the correlation and its attribution to her took a little longer.

As with any volume more than a century old, there are, of course, lots of places where one could say, "no", "not quite", "well sort of", and so forth, but the happiest thought is that Bürgel would have expected that, by the time of that 2004 transit of Venus (yes, it happened, and in 2012 as well), women really would be holding up half the sky! Maybe for the next transit in 2117 December?
— Virginia Trimble.

Here and There

A PUZZLE INDEED

While there may not be as many pieces in this space-themed mini jigsaw as there are stars in the solar system ... — *Lakeland Christmas Catalogue*, p. 18, 2021.

BY JOVE!

Quick Quiz, Question 5: How many moons in our solar system are bigger than Mercury?
Answer, Page 55: Two, Ganymede and Titan, which orbit Jupiter.
— *New Scientist*, 2022 February 12, p. 53.