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SONNE UND MOND, OR, THE GOOD, THE BAD, AND THE UGLY: COMMENTS ON THE DEBATE BETWEEN MOND AND ΛCDM

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MOND, MOdified Newtonian Dynamics, attempts to explain deviations between, on the one hand, observations, and, on the other hand, expectations based on the gravitational forces due to baryonic matter — *via* a change in the law of gravitation rather than *via* dark matter. Despite its *ad-hoc* nature, MOND is still interesting because the observations on which it is based are undisputed and it is at least unclear whether the corresponding phenomena arise naturally in the context of mainstream astrophysics. However, the debate is often not healthy, which impedes progress. As someone with a stake neither in MOND nor in conventional astrophysical explanations of the corresponding phenomena, I suggest some ways to improve the debate on what is certainly an important topic.

Introduction

'*Sonne und Mond*' means 'Sun and Moon' in German. In English, MOND refers to MOdified Newtonian Dynamics*. The discussion of MOND begs for a new acronym, SONNE, meaning 'Surely One Need Not Exaggerate', since much of the debate regarding MOND is characterized by attacking straw men, caricatures of what the other side actually claims. That is one aspect of the 'ugly' part of the debate; I will discuss others below. Each side also has its good and bad, namely areas where theory explains observations well and areas where it doesn't, respectively. Leaving out areas which neither or both explain well, the good of one side is the bad of the other and *vice versa*.

A 'cartoon version' of the debate has MOND supporters on one side, objectively observing the Universe and formulating simple rules which explain

*The German word is pronounced with a long 'o'; the English acronym with a short 'o.'

a wide variety of phenomena while, on the other side, hidebound defenders of the ΛCDM* orthodoxy are blinded by their allegiance to a Kuhnian paradigm. I have often met young people who had been intrigued by MOND, perhaps even done some work on it, then moved on, turned off by the exaggerated rhetoric. While I frame no hypotheses regarding possible explanations of MOND phenomenology†, I am convinced that that phenomenology is worth investigating and that there are at least strong hints that there is no credible explanation within ΛCDM. As such, I criticize the rhetoric of some in the MOND camp, not to detract from MOND but rather to attract more people to it by shedding more light and less heat on the debate.

A more objective description of the situation is as follows. Both sides, of course, recognize their own strengths. They also recognize their own weaknesses: MOND enthusiasts are still in search of a valid relativistic theory of MOND (*e.g.*, refs. 1,2) while entire conferences are devoted to problems with ΛCDM (*e.g.*, ref. 3)‡. Most MOND supporters agree that dark matter works well in many areas (*e.g.*, ref. 2), and of course mainstream (used here as a contrast to supporters of MOND) scientists are aware of the problems of MOND. The two remaining areas are problematic: recognition of the successes of MOND by those in the mainstream camp, and criticism of ΛCDM by those who support MOND. At least in part, the first of those areas is due to ignorance of the literature. In part that can be explained by the fact that ΛCDM is only now just beginning to explore things at the scales at which MOND phenomenology occurs, thus many experts in ΛCDM, and even more so those interested in matters even more cosmological, have had no need to investigate MOND phenomenology. Mainstream astrophysicists concerned with galactic dynamics tend to be more familiar with MOND. In fact, James Binney, who literally wrote the book on galactic dynamics[4], is a supporter of MOND§. Just as the name for

*ΛCDM refers to a universe the main components of which are the cosmological constant Λ and cold dark matter (CDM), 'cold' here meaning not moving at relativistic speeds. While cosmological models can be classified according to their contents — Λ or some other sort of 'dark energy' (essentially a smoothly distributed component with negative pressure with, unlike Λ, an equation of state, perhaps time-dependent, other than $p = -\rho$), various matter components such as cold, warm, or hot dark matter (and whether those are baryonic, leptonic, or something else), radiation, *etc.* — the term ΛCDM is usually used in the context of structure formation. The term 'DM' is often used for 'dark matter'. Both Λ and the matter content affect the expansion history of the Universe, often expressed as the change in the scale factor with time, $R(t)$, which sets the stage, so to speak, while the details of structure formation depend on the various matter components.

†By 'MOND phenomenology' I mean those observations which are often invoked as being easily explicable *via* MOND, independently of any sort of explanation whatsoever, *i.e.*, just the observations themselves.

‡In 2015 January I was at the conference in Oslo reported on by Bull *et al.*[3], called 'Beyond ΛCDM'; most people there were working on extensions or alternatives to ΛCDM, though some were working on MOND. However, it's not easy to make such alternative ideas work. George Efstathiou was there, defending the orthodoxy, who set the bar low by saying that if anyone had an alternative to ΛCDM which did nothing more than explain all the current observational data just as well as ΛCDM — not even requiring predictions, much less confirmed predictions — then he would give them a job. I don't think that he has hired anyone as a result.

§http://www-thphys.physics.ox.ac.uk/user/JamesBinney/MOND-2.ppt alternative medicine which works is 'medicine', the fact that MOND has some support among mainstream scientists can obscure the fact that it has contributed something to mainstream science. Although ignorance of the literature is a serious problem, one can do little more than point those interested in ΛCDM to the extensive MOND literature (*e.g.*, ref. 1 and references therein).

Just as problematic is the second area: criticism of ΛCDM by some MOND enthusiasts. However, I have more to say about that, because there are many misunderstandings involved. There is of course no problem if such criticism is the exposition of problems with ΛCDM which are recognized by the ΛCDM side, though it is sometimes not stated that it is possible that those problems might be solved within the context of ΛCDM, as science is a way of thinking and not a collection of facts (though the latter can be a result of the former). I do, however, see a big problem with criticism of a straw-man version of ΛCDM: not only is that wrong, but it turns people who would otherwise be interested in MOND away from working in the field, as some conclude (wrongly) that there can be nothing interesting to MOND if some supporters have to resort to such primitive tactics. Below, I discuss those problematic attacks. I hope that the MOND community can recognize them for what they are and distance themselves from them. Also, in dialogue with mainstream scientists, the MOND community should focus on MOND phenomenology and the fact that that phenomenology is independent of whatever explanation for it turns out to be correct; those so attracted will encounter theory soon enough.

As pointed out by Sanders[2], MOND often works well where ΛCDM has problems and *vice versa*. Of course, there are also problems with ΛCDM, such as over-predicting the number of satellite galaxies, which MOND has nothing to say about, as well as problems with MOND, such as the lack of a relativistic theory, which doesn't directly correspond to a success of ΛCDM.

It is difficult to estimate the relative sizes of the two communities, for at least two reasons. First, many who work on MOND also work on more-conventional astrophysics. Second, most astrophysicists work neither on MOND nor on structure formation, but perhaps sympathize with one or the other field without that being publicly known, which can affect things such as funding and allocation of observing time. In any case, the MOND community is much smaller, consisting of perhaps a few dozen people.

Of course, there is neither anything new about nor wrong with debate. Some debates are based on misunderstandings, some on obscure technical points, some just on alternative hypotheses. Many remember debates about the value of the Hubble constant or even the one between supporters of the Big Bang and Steady State hypotheses. I certainly don't object to debate *per se*, merely to unhealthy debate.

The plan of this paper is as follows. After a brief introduction to dark matter and MOND, I discuss areas where MOND explains observations well (and conventional theory does not, at least not clearly) before discussing areas which are problematic for MOND but not for conventional astrophysics and cosmology. I mention some typical examples of unfair attacks on MOND from mainstream science and then, in more detail, unfair criticism of ΛCDM by some MOND enthusiasts, using a particular paper as an example, then discuss other reactions to the same paper, all positive. Finally, I offer some suggestions for improving the debate.

Dark matter: basics

MOND attempts to provide an alternative to dark matter.* In this context, 'dark matter' refers to non-baryonic matter of unknown composition which, according to the 'concordance model' of cosmology, makes up the bulk of matter in the Universe (though the bulk of the mass–energy is due to the cosmological constant). Neutrinos are non-baryonic dark matter ('dark' also implies 'transparent', as the point is that there is no electromagnetic interaction), though of course not unknown and their total mass is not a significant fraction of nonbaryonic matter. It is actually not known where about 30 per cent of the baryons are[5]; this is dark matter in the narrower, conventional sense of the term (*i.e.*, they don't emit light), and of course not even all known baryons emit or absorb significant amounts of electromagnetic radiation. In the rest of this article, I use 'dark matter' as shorthand for 'non-baryonic matter of unknown composition'. Typical values for the concordance model are 70 per cent cosmological constant, 25 per cent dark matter, 4 per cent known baryons, and 1 per cent unknown baryons; ≈ 7 per cent of baryons are in stars, *i.e.*, ≈ 0·35 per cent of the total mass–energy of the Universe (*e.g.*, ref. 5). (In recent times, the energy density of neutrinos and photons has been negligible. However, because the density of relativistic particles increases with the redshift z as $(1+z)^4$ as opposed to $(1+z)^3$ for non-relativistic matter, in the early Universe those components played a larger role, even dominating in the very early Universe, but that is not germane to the present discussion.) In astrophysical and cosmological contexts, dark matter is detectable only *via* its gravitational interaction. Hence, if observations cannot be explained by known matter, one can invoke dark matter — or a change in the law of gravity.

The first suggestion that there are significant amounts of dark matter in the Universe is often attributed to Zwicky[6], who noted that the velocities of galaxies within the Coma cluster are too high to be bound if the only source of gravitation in the cluster are the galaxies, though a few years earlier Lundmark[7] had noted that the mass-to-light ratios of spiral galaxies, based on dynamical measurements of the masses *via* rotation curves, are appreciably larger than 1, arriving at a tentative value of ≈ 100 for Messier 81. Smith[8] later came to similar conclusions regarding the Virgo cluster. However, the concept and sometimes even the name (perhaps in another language — both Zwicky and Lundmark used the German term '*dunkle Materie*') had been mentioned before by the likes of Thomson[9], also known as Lord Kelvin (concluding that "perhaps a great majority of [stars] may be dark bodies"), Poincaré[10,11], Poincaré & Vergne[12] ('*matière obscure*'), Öpik[13], Jeans[14], Kapteyn[15], Lindblad[16] , and Oort[17] (computing "[t]he amount of dark matter"), all in the context of the Milky Way (and concluding that, in contrast to the situation in clusters of galaxies, the amount of dark matter was similar to or less than that in stars). The concept of dark matter to explain motions not due to the gravitational attraction of visible matter goes back at least to the prediction by Bessel[18] of unseen companion stars in order to explain the proper motion of Procyon and Sirius. Slightly later, Neptune was discovered due to its gravitational influence on Uranus[19–22], although it had been observed by Galileo who, however, thought it to be a fixed star or satellite of Jupiter, and later by others as well before it was recognized as a planet. Le Verrier, who was the first to publish a calculation of the position of

*At least in some respects. While there is no *a priori* reason that both dark matter and MOND could not exist, most MOND enthusiasts probably would like to see MOND, or some extension of it, obviate the need for all dark matter.

Neptune[23], also noticed an irregularity in the orbit of Mercury, the precession of its perihelion, which also led to the prediction of a new planet, Vulcan, which, however, does not exist, the explanation for deviations from motion expected from the gravitational effects of visible objects in this case being modified gravity (Einstein's general theory of relativity) rather than dark matter (see Levinson[24] for an interesting historical account).

Apart from familiar objects such as planets, it was also realized that dark clouds (now known as absorption nebulae) exist, due to obscuration of stars presumably lying behind them[25]. That became more obvious when photography started to be used in astronomy, although it was at first not clear whether there were regions with no stars or whether stars were there but obscured[26]. Barnard[27] compiled a catalogue of 182 such objects. Such observations, based on obscuration rather than gravitational effects, are somewhat more direct. Dark stars were mentioned by Clerke[28]; she speculated that the mass in dark objects might be greater than that in luminous ones. (For a time, all variable stars were believed to be eclipsing binaries; sometimes, the dimmer companion was too faint to be detected directly, hence it was unknown how dark it actually was.) After it was realized that some stars have a lifetime shorter than the age of the Galaxy, the possibility of dark stars as burned-out remnants arose.

Even earlier, Michell[29] had described what later came to be known as black holes, perhaps the first explicit mention of dark matter in an astronomical context, unless one counts Philolaus's invisible counter-Earth from more than two thousand years ago. To be sure, no dark-matter candidate before Lundmark[7] was an indication that most of the mass of the Universe was in dark matter, and none of those before Thomson[9] that it might be even a significant fraction of the total mass. Of course, the question whether dark matter was baryonic or non-baryonic was a non-issue until it was realized that Big Bang nucleosynthesis predicts that most of the mass of the Universe★ is non-baryonic (*e.g.*, ref. 30) — at least if $\Omega_0 \approx 1$ — although as long as there were no firm lower limits on the mass density in the Universe, it was considered possible that the Universe might consist chiefly or only of baryons (*e.g.*, ref. 31).†

★The density of various kinds of matter is often expressed by the parameter $\Omega = (8\pi G\rho)/(3H^2)$, where G is the gravitational constant, ρ the density, and H the Hubble constant. Ω is used rather than ρ for two reasons. First, historically, many quantities were known up to some power of the Hubble constant. Also, the value of Ω, rather than the density itself, is useful for describing the evolution of the Universe. The constant factor is such that $\Omega = 1$ denotes, for $\Lambda = 0$ (no cosmological constant), the boundary between a universe which is spatially closed (finite) ($\Omega > 1$) and one which is spatially open (infinite) ($\Omega \leq 1$; $\Omega = 1$ implies that the universe is spatially flat); in the former case, the universe collapses after expansion to a finite value of the scale factor; in the latter, it expands forever. (For the experts: $\Omega \leq 1$ implies an infinite universe only if a trivial topology is assumed, *i.e.*, the universe is not something like a higher-dimensional torus.) The curvature parameter $k = \mathrm{sign}(\Omega+\lambda+1)$ indicates whether the spatial curvature is positive ($k = +1$), negative ($k = -1$), or zero ($k = 0$). For $k = 0$, a flat universe, possibly with $\Lambda \neq 0$, $\Omega \leq 1$ implies that the universe will expand forever, otherwise it will collapse after expansion to a finite value of the scale factor. If both Λ and k are non-zero, Ω is still a useful parameter but $\Omega = 1$ no longer has any particular significance. (Note that my use of Ω refers only to matter, sometimes called Ω_m or Ω_{matter}.) Similarly, $\lambda = \Lambda/(3H^2)$. The subscript 0 denotes present-day values, *e.g.*, H_0, Ω_0, and λ_0.

†The paper by Gott *et al.*[31] was very influential, making a case for a low-density Universe. It begins with a quote from Lucretius, urging the reader to "[d]esist from thrusting out reasoning from your mind because of its disconcerting novelty. Weigh it, rather, with a discerning judgment. Then, if it seems to you true, give in." In other words, conclusions should be based on observation, rather than theoretical prejudice. Belief that $\Omega \geq 1$ is mocked as being due to "theological or other grounds". Ironically, Schramm later became a strong advocate of $\Omega = 1$, like many due to the combination of beliefs that inflation implies a flat Universe and that $\Lambda = 0$, and criticizing those who considered those who considered $\Omega < 1$ for "thinking like an astronomer instead of like a physicist" (ref. 32, p. 336).

A few decades after Zwicky's suggestion of dark matter in the Coma cluster, observations of flat rotation curves (*i.e.*, after an initial rise due to increasing enclosed mass, the radial velocity of stars as a function of radius does not decline — as expected based on the mass distribution of the stars — but rather stays approximately flat out to the limits of observation) in spiral galaxies (*e.g.*, ref. 33), and somewhat later similar observations of neutral hydrogen in the radio[34], provided the classic observational basis for MOND. (Like Zwicky and dark matter, there were also earlier observations of flat rotation curves which, for some reason, have not been as influential, such as the work of Babcock[35].) See the reviews by Trimble[36,37] and Bertone & Hooper[38] for an extensive history of dark matter.

MOND: basics

Milgrom[39] suggested that the observations of flat rotation curves could be explained by modifying the gravitational force law, leading to Newton's second law being, at least in this case,

$$F = m\mu\,(|a|/a_0)\,a = m\mu(x)a, \qquad (1)$$

where μ is a positive, smooth, monotonic function with the limits $\mu \approx 1$ for $x \gg 1$ and $\mu \approx x$ for $x \ll 1$; some examples are $\mu = x/(1 + x)$ and $\mu = x/\sqrt{(1 + x^2)}$. Note that the modification occurs at an acceleration scale, not a length scale*

The constant a_0 is a new constant of nature with the dimension acceleration which, observationally, is found to be $\approx 1{\cdot}2 \times 10^{-10}$ m s^{-2}. That does not mean, however, that there is some minimum acceleration; the force still falls off with distance, albeit more slowly than the inverse-square law, namely inversely proportional to the distance once the Newtonian acceleration drops below a_0. One can easily show that the Newtonian law of gravitation modified *via* Eq. (1) leads to $v = \sqrt[4]{(GMa_0)}$, where G is the gravitational constant and M the mass of the galaxy. Thus, once one is far enough away from the centre of the galaxy that the Newtonian acceleration $a \ll a_0$, the rotation velocity should depend on the mass but not on the distance from the centre of the galaxy. (To be sure, the mass might increase slightly with radius even in the regions where $a \ll a_0$, and indeed some rotation curves are seen to rise slightly.)

MOND: successes

MOND was constructed in order to explain flat rotation curves. There is nothing remarkable in a simple explanation (Eq. (1)) for a simple observational fact, and of course MOND is an *ad-hoc* explanation for flat rotation curves, with no real physical motivation. By the same token, flat rotation curves are not a prediction of MOND, and not even a post-diction, since not only were they known at the time MOND was formulated, they were the motivation for MOND in the first place. What is remarkable is the fact that Eq. (1) explains a

*There had been previous attempts to modify Newton's law of gravity. For example, Laplace[40] had suggested that, due to propagation at finite speed, the force of gravity acting on a moving body should not be purely radial. While it is true that gravity propagates at a finite speed — the speed of light, c — relativistic effects cause aberration effects to appear only at higher order in v/c than Laplace expected (*e.g.*, ref. 41). Seeliger[42] had proposed a long-distance cutoff in order to avoid the problem of an infinite gravitational potential in an infinite, homogeneous universe. See Norton[43] for the history of this idea. Also, Finzi[44,45] had suggested a modification of Newton's law of gravity, though based on distance rather than acceleration, to explain the dynamics of galaxy clusters, flat rotation curves, and other apparent mass discrepancies.

large number of other observational phenomena, some of which were predicted by MOND, while some were surprises but easily explained within the MOND context. Some of the most important (*e.g.*, refs. 1,2) are

• the tight relation between a galaxy's total baryonic mass and its asymptotic rotation velocity, known as the baryonic Tully–Fisher relation, though arguably that is implied by the flat rotation curve;

• Renzo's rule: features in the light (and hence mass) profile of the galaxy have corresponding features in the rotation curve at the same radius, which seems unlikely if the rotation curve is due to the influence of dark matter at much larger radii;

• mass-discrepancy–acceleration relation, (the square of) the ratio of observed velocity to that attributable to baryonic matter, which increases as the acceleration decreases;

• the Freeman limit to the observed central surface-mass density in spiral galaxies (related to the core–cusp problem, since ΛCDM tends to predict higher central densities); the maximum observed surface density is $\approx a_0/G$;

• $1/r$ rotation curves for high-mass spiral galaxies;

• mass discrepancies in tidal dwarf galaxies;

• long-term stability of orbits of satellite galaxies.

There is no space here for a review of MOND, but also no need, since many reviews are available. Famaey & McGaugh[1] present many figures which make the phenomena above, and many others, very clear (and of course such phenomena need to be understood whatever the underlying explanation). McGaugh[46] gives a good introduction to MOND phenomenology, concentrating on predictions[47] which have been confirmed.

MOND: failures

It appears that MOND cannot completely eliminate the need for dark matter, especially in galaxy clusters.[48–55]. Whether that is a problem is a matter of taste (depending on whether one considers a combination of MOND and dark matter to be a viable scenario), although it is interesting that the missing matter could be baryonic (since the location of about 30 per cent of the baryons is unknown[5] and their mass is sufficient to make up the missing matter) and hence no dark matter in the commonly used sense would be needed (which would then not really be a problem for MOND at all). Some relativistic versions of MOND have been ruled out (*e.g.*, ref. 56), though that is not a problem for MOND *per se*. It has been claimed that the Bullet Cluster rules out MOND[57], though the case is not as clear cut as many still believe: the implied collision velocity is very high[58], which is unlikely in ΛCDM[59], though not in MOND[60]. Also, the Train-Wreck Cluster (Abell 520) provides a counterexample[61] which is difficult to explain in ΛCDM, while MOND explanations for the Bullet Cluster have been proposed[62,63]. I won't take sides here, but rather note that claims such as "the Bullet Cluster falsifies MOND" or "the Bullet Cluster proves the existence of dark matter" are at best exaggerated.

The most serious problem for MOND seems to be that dark matter works exceedingly well for explaining the power spectrum of CMB isotropies as well as the formation of large-scale structure in the Universe, which many or even most MOND enthusiasts admit (*e.g.*, ref. 2 and references therein). (Without dark matter, concentrations of which can grow while those of baryonic matter are still prevented from doing so due to interaction of radiation, fluctuations at the level observed in the CMB could not have evolved to those at $z = 0$ in the time available.) Moreover, there doesn't seem to be an explanation for such anisotropies within the context of MOND. While it is true that such calculations would be more complicated in MOND, and also that it is difficult to get funding, personnel, computer time, *etc.* to do them, that does not imply that they would be successful if done. (Similarly, proponents of ΛCDM shouldn't argue that the various small-scale problems will be resolved once baryons are fully taken into account, the simulations are higher resolution, *etc.*; that might be true, but one cannot say so before they have been done.)

Another problem is that no-one has been able to construct a relativistic version of MOND which is not overly complicated and/or *ad hoc* while at the same time respecting well-tested conservation laws and other basic principles of physics.

Unfair criticism of MOND

A common criticism of MOND is that it is an *ad-hoc* theory, its only motivation being observational. The same is true for dark matter, of course.* The difference is that an unknown type of matter can more easily be accommodated than new physics, especially if the latter has no theoretical motivation. Thus, on balance, the criticism that MOND is *ad hoc* would be more valid if indeed there were a 'theory of MOND' to be criticized. However, most MOND supporters argue that the important point, at least for now, are the observations, which need an explanation, Milgrom's modification of the law of gravity or other schemes being merely approximations to some proper theory. However unrealistic or unbelievable effective MOND theories are, criticizing them should not detract from the observations, 'MOND phenomenology', which ultimately has to be explained by *any* theory which claims to be a valid description of the Universe. Even if there is some explanation completely different from MOND, there still needs to be an explanation of why such a simple, one-parameter, empirical fit works so well. (To be sure, if the dark matter is in WIMPs — Weakly Interacting Massive Particles — then that would also be 'new physics' in the sense of 'physics beyond the standard model'†, but arguably not as radical as the new physics required if the explanation of MOND phenomenology is indeed some relativistic version of Milgrom's *ansatz*. Also, while there have been many predictions of new particles, I think it is fair to say that the primary *astrophysical* motivation for dark matter is *ad hoc*.)

There have been attempts to show that MOND phenomenology naturally falls out of ΛCDM. Simple calculations (*e.g.*, ref. 66) have been rebutted by MOND supporters (*e.g.*, ref. 67), while those rebuttals have been largely ignored by those working in ΛCDM. More-involved calculations (*e.g.*, ref. 68) might indeed demonstrate that MOND phenomenology has a basis in conventional astrophysics, and while complexity should not be a mark against a theory (some phenomena are complicated; of course, that does not imply that the underlying

*It is certainly true in the context of astrophysics and cosmology. To be sure, SUSY and other extensions to the standard model of particle physics have predicted previously unknown massive particles, often with the suggestion that one or more of them could be the dark matter. There is, however, no experimental evidence in favour of such extensions to the standard model, and none of those particles has been detected.

†Here, I mean the standard model of particle physics. Otherwise, except for two instances where it is explicitly stated otherwise, I refer to the standard model of cosmology. The latter term is older[64] and the former (due to Weinberg and others) intentionally modelled on the latter[65].

theory must be complicated), in practice it needs to be demonstrated that that is a robust result, not dependent on various parameterizations, approximations, *etc.*, which is difficult to do as long as no numerical simulations are completely free of such devices (*i.e.*, it is too challenging to compute everything from the primitive equations). In other words, proponents of ΛCDM shouldn't simply claim, without evidence, that more-complicated simulations will explain all observations. (Similarly, MOND supporters shouldn't claim — without evidence — that a proper relativistic theory of MOND will clear everything up.) At the same time, if the answer does lie in complex calculations, it is at least surprising that those can be reduced to a simple one-parameter fit. Some critics of MOND argue that it is unfair for MOND supporters to criticize simulations if they have no simulations of their own for comparison, perhaps suggesting that those would look bad for MOND if they existed. There are, however, two other explanations. First, while respectable astrophysicists do work on MOND, it is much more difficult to get funding for such projects — if the money is for computing time, one is competing with more-conventional science; if the money is for personnel, many will avoid working on MOND for fear that it will hurt their career.* Second, without a proper theory of MOND, it is not clear how to set up such simulations; even programming Milgrom's simple parameterization is much more difficult than using standard Newtonian theory while proper MOND simulations are much more involved.†

Unfair criticism of conventional astrophysics and cosmology

Well-established MOND phenomenology seems to attract less attention among mainstream cosmologists than other problems in galaxy formation, such as the missing-satellites problem, the core–cusp problem, and so on. My guess is that that is due to the fact that any explanation of such well-established observations would have to be very, very good, and it is clear that numerical simulations are not yet refined enough to attempt such an explanation. The other problems are less well defined and could conceivably have other explanations. It makes sense to work on things where some progress might be made. That can create the impression, though, that important observations — those supporting MOND — are being ignored because it is difficult to explain them theoretically.

While my criticism of straw-man attacks by some MOND supporters applies to more than one person, for concrete examples I will quote from Merritt[69] (as all direct quotes — unless indicated by another reference — are from that paper, I will omit the corresponding reference in what follows). That is not because I think that that work is a particularly good (or bad, depending on the point of view) example of such wrong-headed attacks on ΛCDM, but because the author states clearly what he is trying to do and attempts to remain objective, but the tenor is still that of a straw-man attack. Not all critics of ΛCDM are that vocal, at least not in print.

Already in the abstract is the claim "... dark matter and dark energy … were invoked in response to observations that falsified the standard model as it existed at the time", where it is also made clear that falsification in the sense of Popper[70] is meant. According to Popper, a falsifiable prediction is one which, if

*It is of course difficult to document 'difficulty in getting funding', but I have heard that from several people and it seems believable.

†Note that galaxy-formation simulations use Newtonian gravity, not GR, and that that is justified.

confirmed to be false, rules out the hypothesis in question. Since most do not believe that dark energy* and dark matter have falsified the standard model, how can one makes sense of that claim? Normally, it makes no sense to invoke some explanation if the model has been falsified, unless that which is invoked is part of a new model which replaces the old one, but such replacement did not occur; it is still essentially the same standard model. To be sure, "the standard model as it existed at the time" implies that dark energy and dark matter modified the standard model, but that is part and parcel of normal science. Rarely if ever does a theory predict essentially everything; on the contrary, theories of that type (*e.g.*, ref. 72) are not mainstream and often crackpot. What Merritt seems to be implying is that dark energy and dark matter are some sort of epicycles, *ad-hoc* explanations, *dei ex machina* called in to save appearances. That implies that dark energy and dark matter are something added to the standard model, as opposed to being merely refinements of it. But is that the case?† Also, as noted above, concluding the existence of dark matter from the motion of visible matter has a long history, and usually the existence of what was at least initially perceived to be dark matter was later confirmed by non-gravitational means. Almost no-one saw such events, even before the objects were later detected by other means, as some sort of contradiction of Newtonian gravity.

The standard model is based on General Relativity (GR).

Merritt seems to believe that matter detected only *via* gravitational effects is somehow an addition to GR, especially if what is usually termed dark matter, *i.e.*, non-baryonic matter (see above), is meant. However, GR says nothing about the sources of the gravitational field. Indeed, the word 'baryon' did not even exist when Einstein developed GR (it would be coined in 1953 by Einstein's friend and biographer Pais[73]). At the time, matter was known to consist of atoms; indeed, Einstein himself contributed to the development of atomic theory, determining Avogadro's number in his doctoral thesis[74] and sometimes the unit 'einstein' is used for (the energy of) a mole of photons. However, the composition of atoms was still unknown, and the neutron was not discovered until later by Chadwick[75], reported in a one-page paper which earned him the 1935 Nobel Prize in physics. Thus, the claim that some new sort of matter, no matter how it is inferred, somehow falsifies the standard model is certainly untrue if referring to GR. The alternative is that the standard model specifies what types of matter there are. Of course, the working hypothesis, in the sense of Occam's razor, is that there is only matter one knows about, but the discovery of new types of matter in no way invalidates that hypothesis any more than the discovery of gorillas invalidated Linnaeus's binomial-classification scheme. That criticism is tantamount to claiming that we, at this point in the history of cosmology, for some reason *must* be aware of all types of matter in the Universe.

*Carroll[71] has pointed that many things are dark and everything has energy; unfortunately, his much better term 'smooth tension' has not caught on.

†To be sure, Merritt discusses Popper's "conventionalist stratagems", *i.e.*, ways of evading the consequences of a falsifying experiment. Leaving aside doubts about the quality of the observer and/ or the observations (which no-one seriously claims with respect to MOND phenomenology), those include *ad-hoc* modifications or modifying definitions. The question is not whether those exist and have been (ab)used by some in the past; the question is whether dark matter and dark energy fall into one or more of those categories, as Merritt seems to think.

The case of dark energy is even more straightforward, since the cosmological constant (the simplest form of dark energy, constant in time and space; there is no evidence that anything more complicated is needed) appeared in the first paper on relativistic cosmology[76]. To be sure, Einstein introduced it for what is now known to be the wrong reason, but nature is independent of the history of science on the planet Earth: he could have introduced it from the beginning, and some, following the particle-physics motto that anything not forbidden must happen (and the burden of proof is on those who claim that it doesn't happen, since that implies a new symmetry, conservation law, quantum number, *etc.*), believe that that is what he *should* have done. The fact that Einstein later distanced himself from the cosmological constant is of course not an argument against it; Einstein was often wrong, especially in his later years. Rather, it became a free parameter in the theory. A parameter can be zero, or be so small as to be practically unmeasurable, so, again following Occam's razor, it was often set to zero — but non-zero values were invoked if they provided a better fit to observations. That is not some sort of epicycle, but rather learning from observations, which is an essential part of science. It is not 'new physics', which *any* theory of MOND (as opposed to some other explanation for MOND-like effects) would have to be. The fact that it was often assumed to be zero was due to the fact that observations were long compatible with a zero cosmological constant coupled with the fact that including it makes calculations more difficult, thus more of a practical matter. Nevertheless, from time to time interest in it revived when it appeared that it was required by observations (*e.g.*, refs. 77–79). The fact that it was assumed to be zero before the change to the current standard model with a positive cosmological constant is more an accident of history. There were also many who were comfortable taking it into account, even if in some cases that was perhaps to remind ourselves that we didn't know enough to set it to zero (*e.g.*, refs. 80–82); others thought it to be of fundamental importance, such as Eddington in his *Fundamental Theory* (*e.g.*, ref. 72).* One reason might have been to avoid the age problem, which was more acute when H_0 was believed to be well over 100/km/s/Mpc, though for Eddington an additional reason was that it allowed a universe without a Big Bang (*e.g.*, ref. 91, p. 58). Lemaître also stuck with his model (*e.g.*, ref. 89), which not only had no age problem (for the appropriate value of λ_0) but also had a quasi-static phase which, it was believed at the time, could provide enough time for structure to form. On the observational side, de Vaucouleurs didn't assume λ_0 to be zero (*e.g.*, ref. 90). If the cosmological constant had been invented to explain the acceleration of the Universe, then the critics would have a point, but that is not the case. Even the interpretation of the cosmological constant as vacuum energy has a long history (*e.g.*, refs. 92,88,93,94), so that is not a modern invention either.

I would be exaggerating only somewhat if I said that Merritt's criticism makes the same mistake as the arguments regarding the shape of the Earth which were criticized by Asimov[95], who noted that a refinement is not the same as a

*Lemaître advocated essentially the same cosmological model throughout his career (*e.g.*, refs. 83–89), one in which the cosmological constant played an important role. Gérard de Vaucouleurs also favoured models with a positive cosmological constant, which was necessary in order to have the Universe old enough with his high value of the Hubble constant, though he tended to emphasize the data (*i.e.*, his value of the Hubble constant) rather than extrapolations from them, due to his rather positivist philosophy. Although he shared his belief in the cosmological constant with Eddington, de Vaucouleurs put much more emphasis on observations and much less on theory. In fact, de Vaucouleurs also wrote a couple of papers in French in the late 1940s which were probably an indirect attack on Eddington's *Fundamental Theory*[90].

revolution: the idea that the Earth is a sphere is not falsified by the refinement that it is an oblate spheroid, or slightly pear-shaped, or whatever shape current observations say that it is. There is a difference between evolution and revolution and one shouldn't throw the baby out with the bathwater.

Kuhn[96] appears already in the introduction. That is not surprising, given that Merritt's paper is ostensibly about comparing ΛCDM and MOND with regard to their utility and status as scientific theories. What troubles me is that Merritt tacitly assumes that Kuhn's ideas about paradigm shifts are at least roughly correct. Yes, it is true that most scientists working within a given paradigm aren't willing to admit that it is fundamentally flawed — but because usually there is no evidence that it is, not because they are blind to alternatives. Merritt is assuming that that which he wants to prove is true, namely that science progresses by revolution, not evolution. Actually, the reverse is more common, especially when the debate is among scientists and not, say, between science and the Church, as in the cases of Galileo, Bruno, Copernicus, Darwin, *et al.* I am not alone in claiming that that idea is essentially wrong. I was happy to discover that Rovelli[97] shares my criticism. Many readers will have heard of Kuhn, but not many will have read his works. Thus, basing argumentation on that of Kuhn could be seen as 'pulling a fast one'; the foundations of the argument have to be justified by more than just quoting an 'authority'. Apart from criticism of his ideas *per se*, it seems to me that Kuhn is hoist with his own petard. Assume that his ideas are not scientific (or just wrong); in that case, scientists don't have to worry about them. Assume that they are scientific (and thus presumably capable of being, but not yet, ruled out, like other accepted scientific ideas); in that case, then they apply to Kuhn's ideas themselves, so those will someday be replaced by another paradigm, so again there is no need to worry about them.

According to Merritt, the standard model of cosmology "purports to describe the universe" from the time of Big Bang nucleosynthesis (BBN) or even earlier. Not only is there good agreement with respect to the time of BBN and thereafter, though that needs to be distinguished from more-speculative ideas about earlier times (*e.g.*, ref. 98), but also the language is inappropriate and unnecessarily derogative, as he does not state that MOND 'purports' to describe something.

After listing several well-known 'anomalies' of the standard model, Merritt states that "these discrepancies are rarely described as falsifying; they are presented rather as problems that remain to be solved from within the existing paradigm". That paints a cartoon version of a theory: it must explain everything as soon as it is developed, and if not, then it is to be discarded when the first puzzle is presented. Most of those problems are associated with details of structure formation as studied by numerical simulations, and it is clear that those have finite resolution, do not include all physics, and so on, so it should be expected that not everything is resolved (pun intended). That is not to say that none of those will lead to an overthrow of ΛCDM, merely that it is too early to tell. Another mistake is conflating those details of small-scale structure formation with ΛCDM in general or even the 'standard model'. That is almost as bad as the creationist ploy of implying that debate (whether real or not, whether understood by the creationists or not) within the evolutionary community implies that evolution itself must be wrong. There is also a double standard: MOND does not explain everything either, but Merritt does not claim that it has been falsified. Also, Merritt contrasts revolutionary research with "puzzle solving", but actually all MOND-theory papers are also puzzle solving, so that description fits MOND as well as ΛCDM.

Another question is how serious those anomalies are. Sometimes, the failure of standard-model simulations to reproduce observed structures is cited as a failure. That is certainly unfair if that applies to dark-matter-only simulations, which obviously cannot reproduce any effects due to baryonic matter. While there are claims that including baryonic physics solves those problems (*e.g.*, ref. 68), it is still unclear if there is a consensus here. The fact that such simulations are technically complicated and thus not easily checked is a practical problem, but not a problem of principle. Some things *are* complicated. For example, the climate of Earth is difficult to simulate in detail, though no-one doubts that it is explicable by the known laws of physics. Consensus has been reached here probably because more CPU time has been spent, and also because all components of the Earth are known, so it is easier to check simulations against reality. (At the same time, mainstream astrophysicists shouldn't claim — without evidence — that 'adding more physics' to the simulation will automatically clear everything up.)

"At the same time, there *have* been instances since the 1960s where anomalies were interpreted by the community as being incompatible with the cosmological model as it existed at the time. A famous example is the discovery around 1998 that the expansion of the universe is accelerating, rather than decelerating as the standard model had predicted." [Emphasis in the original.] First, that is almost backwards: this is a case where the observations fit perfectly with a theory which was almost 80 years old; merely the value of one parameter, the cosmological constant, was shown to be significantly larger than zero. The perhaps surprising thing is that 1920s cosmology is sufficient to describe the large-scale Universe, even today. Yes, many used the Einstein–de Sitter model as a 'standard model'; some did actually believe that that must be correct (*e.g.*, ref. 99), but for others it was merely a case of using the simplest model until observations demanded that it be made more complex; refining a model when more data are available is evolution, not revolution. Second, with regard to claiming that the standard model "has been falsified many times since the 1960s", note that that time frame could have been cherry-picked so as to have a theory without the cosmological constant, and before the need for dark matter became pressing, as a starting point. Why then? Third, while a narrow definition of the standard model at a particular time could have specified a decelerating Universe, both the cosmological constant and dark matter had been discussed in the 1930s and even earlier; one could just as well say that the framework was already in place then, but for convenience some parameters were set to zero for practical reasons until there was good evidence to the contrary — and this version is more accurate. Fourth, accelerating-universe models have existed since the second relativistic model was proposed by de Sitter[100], based to some extent on some earlier papers published in English but in the Netherlands[101,102]. (Note that de Sitter's model was originally perceived as static, due to the coordinates used, but it makes sense to think of it expanding exponentially when compared with other Friedmann–Robertson–Walker (FRW) models.[103]) The model favoured by Lemaître[83,84,85,87,88] was also of that type*, and the full range of models, both accelerating and decelerating, were routinely examined and taken for granted (*e.g.*, refs. 80–82). Thus, what Merritt calls a revolution was actually only a better measurement of one parameter, and didn't change the underlying framework, despite the hype. At the same time, the anomalies which he claims

*Lemaître[83] was actually the first person to calculate a value for the Hubble constant, now known to be much higher than the correct value. Perhaps he favoured an accelerating model with a long quasi-static phase because the time since the Big Bang can be much longer than the Hubble time $1/H_0$ in such models.

are seen as puzzles to be solved within the current paradigm might actually lead to its overthrow (as mentioned above, it is too early to tell). Fifth, the acceleration of the Universe has not been measured; that is an interpretation which follows from the standard cosmological model (*i.e.*, observations are used to derive the cosmological parameters and those in turn imply that the Universe is accelerating). In other words, the fact of acceleration follows from the standard model (though there are also other cosmological models in which it would follow from the observations), and thus cannot be used as an independent datum used to evaluate the standard model.

The above points occur in the introduction and first two sections. As such, it is somewhat surprising to read at the beginning of section 3 that "neither the content of the current model of cosmology, nor the methodology that led to that content, are [*sic*] being critiqued here". Merritt also finds it confusing that astrophysicists use the term 'acceleration' to mean both "rate of change of velocity" and "gravitational force per unit mass"; however, were that not the case, then Newton's second law and the weak equivalence principle would not hold. While some theories of MOND do modify the law of inertia, I don't see any confusion when astrophysicists in general use the term 'acceleration' in both senses.

"The standard model of cosmology deals with this anomaly [*i.e.*, accelerated expansion] in a different way: *via* an auxiliary hypothesis. It is postulated that the universe is filled with a fluid, called 'dark energy', that has whatever properties are needed to convert the predicted cosmological deceleration into an acceleration, and in just such a manner as to reproduce the observed dependence of galaxy redshift on distance." First, while it is true that some think of dark energy as a fluid with a particular (perhaps time-dependent) equation of state, not only does that idea go back several decades (*e.g.*, refs. 92,88,93,94), but many or even most researchers assume that dark energy is nothing more than the cosmological constant, which has a very specific equation of state ($p = -\rho$, where p is the pressure and ρ the density), among other reasons due to the fact that there is no observational evidence that anything more complicated is needed. Second, whether the cosmological constant is due to a fluid with negative pressure, or is on the 'geometry' instead of the 'matter' side of the Einstein equation, or is some combination of the two (*e.g.*, refs. 104), is still unclear. Third, even those who suggest that dark energy is something more complicated than the cosmological constant do not do so "in just such a manner to reproduce the observed dependence of galaxy redshift on distance"; there is no need, since a cosmological constant, with the only free parameter being its value, not only explains the m–z relation but also does so with a value which is consistent with other (somewhat less direct) determinations (hence the term 'concordance model'). Rather, those ideas have other motivations, often to explain the 'coincidence problem', though not only I have doubts that that is really a problem at all (*e.g.*, ref. 105, the long version of an article which appeared in *Nature* as part of a debate with Rocky Kolb). Fourth, the idea that it "has whatever properties are needed to convert the predicted cosmological deceleration into an acceleration, and in just such a manner as to reproduce the observed dependence of galaxy redshift on distance" is absurd. The cosmological constant, which, as noted above, was not invented to explain any recent observations, has precisely one free parameter, its value. On the contrary, if anything is remarkable, it is that 1920s cosmology still explains all the data of the m–z relation. Fifth, more sinister is the accusation that cosmologists just make up something to save the appearances, akin to angels pushing along the stars in their courses. That should be contrasted to MOND's idea of an acceleration scale a_0, which is completely *ad hoc*, with no theoretical motivation whatsoever, and constructed precisely to explain specific observations, having "whatever properties are needed … in just such a manner as to reproduce the" observations. (What makes MOND interesting at all is the fact that that hypothesis made testable predictions which have been confirmed.) Merritt creates the impression that one can 'fit anything' with dark energy. Depending on the definition, that might be true, but irrelevant. The fact is that one value of the cosmological constant can fit all observations (and there is just one parameter, its value). Again, the cosmological constant wasn't invented to explain any current observations; dark energy is adjustable, but the simplest version, the cosmological constant, explains all the data, and was not invented to explain any current data, so that whole line of argument breaks down.

When discussing $p = -\rho$, Merritt compares negative pressure to the negative mass of phlogiston. I doubt any historian of science would agree with that comparison, which is another caricature. More damning is the claim that "there is a choice for [the energy density] ε and p that is particularly convenient: the energy density is set to a constant value (with respect to time), and the pressure is also assumed to be constant and equal to ε", as if those were picked from an infinite number of possibilities in order to fit the data. Actually, it is the other way around; there are sound mathematical reasons why the cosmological constant has exactly that form; in the words of Carroll[106], it leads to the left-hand side of the Einstein equation (in this case, the cosmological constant is part of the 'matter' side; the two formulations are mathematically equivalent) being "the most general local, coordinate-invariant, divergenceless, symmetric, two-index tensor we can construct solely from the metric and its first and second derivatives". Far from being chosen to fit the bill from an infinite number of possibilities, as Merritt implies, there are objective reasons for just that choice and no other. Moreover, it was introduced in that form by Einstein[76] almost a hundred years before the discovery of the acceleration of the Universe.

"The dark energy hypothesis allows one to fit any observed cosmic expansion by adjusting the dependence of ε and p on time". That is true, but misleading for two reasons. First, as mentioned above, those who postulate a dark energy more complicated than the cosmological constant do so for other reasons. Second, a cosmological constant with no additional free parameters fits all the data. Just because a more complicated hypothesis is not falsifiable doesn't mean that the basic hypothesis should be rejected.

Merritt claims that dark matter is not falsifiable because no conceivable laboratory experiment could detect it, as if astronomical evidence is somehow inferior. There are at least three problems with that claim. First, it is not clear that dark matter consists of individual elementary particles, though he gives the impression that almost everyone who believes in dark matter believes that it is some sort of WIMP. That is an exaggeration; primordial black holes have not been ruled out as dark-matter candidates[107], though certain mass ranges have (*e.g.*, ref. 108), and other ideas such as superfluid dark matter (*e.g.*, ref. 109) or macroscopic dark matter[110] seem more promising than WIMPs. Second, its properties are not known well enough that the lack of detection in a particular experiment rules it out: absence of evidence is not evidence of absence. The neutrino was postulated in 1930 and mentioned in print somewhat later[111]*, but not detected until after more than two decades[114,115], although we knew

*In a possibly interesting parallel to the current discussion of dark-matter particles, Fermi first tried to publish his idea in *Nature*, which rejected it "because it contained speculations too remote from reality to be of interest to the reader"[112]; *Nature* later admitted that that was a big mistake[113].

how many neutrinos were passing through the detector, since both the source and the flux produced by the source were known. Third, that is a case of the pot calling the kettle black, since a modified gravitational-force law also has not been detected in the laboratory, and perhaps never can be. While that might not be possible in principle, due to the external-field effect[39], it is unfair to claim that non-detection of particle dark matter represents a failure, since there could be reasons why that is not (yet) possible and/or some or even all dark matter is not in that form. In principle, there is no reason why all dark matter must be of the same form; even if it is in the form of WIMPs, there could be several forms — there are several stable particles in the particle-physics standard model, yet they make up only about 15 per cent of the matter (and only about 5 per cent of the total mass–energy density) of the Universe. (More important than the number is that those particles form nuclei, atoms, molecules, macroscopic objects. The assumption that most of the mass of the Universe (dark matter) must be one non-interacting particle is probably unjustified; why shouldn't the world of dark matter be as rich as our world is? During the age of exploration, newly sighted land was often drawn as a small island on maps, possibly long but thin if the coastline was long; some later turned out to be continents.)

Merrit claims that "the mass discrepancy–acceleration relation has been dealt with *via* the third of Popper's conventionalist stratagems: It has been ignored." That might have been true to a large extent at the time of writing, but in the meantime that phenomenon has been addressed (how well is another question) within the context of ΛCDM (*e.g.*, refs. 116–119); it is fair to say, though, that only now are simulations beginning to become detailed enough to investigate such small-scale phenomena at all; that goes for other aspects of galaxy morphology as well, not just those interesting in a MOND context. Again, the standard model is caricatured. Dark energy and dark matter are not necessarily in conflict with the mass-discrepancy–acceleration relation, though I don't think that they yet explain it in a completely convincing way. Elsewhere, Merritt argues that, by adjusting the parameters, dark energy and dark matter can explain *any* observation; he can't have it both ways. The main question here is not whether ΛCDM can explain that observation, but rather whether it can do so convincingly while making other predictions which could falsify that explanation. However, it should not be counted as a mark against a theory if the first, simple version (*e.g.*, dark-matter-only numerical simulations) cannot explain all details, especially at scales where such explanation was not expected. (The interesting aspect is that dark-matter-only simulations are as good as they are at getting the large-scale structure of the Universe correct.)

"*None* of the texts mentions the mass discrepancy–acceleration relation. Only two … mention the existence of the universal acceleration scale a_0." [Emphasis in the original.] The texts are from a list of "graduate-level text[book]s on cosmology and/or galaxy formation" published or revised after 2005; the list is intended to be complete, and does not include conference proceedings, popular, or semi-popular books. I could easily list several popular and semi-popular books which do mention MOND, but let us play by Merritt's rules. His list does include one volume of conference proceedings[120], which, as the proceedings of a Les Houches school, is rather obviously not a textbook, but let's ignore that and the fact that I could easily list several conference proceedings which mention MOND. He does miss one textbook[121] which does mention MOND, even though it is more orientated to theory than are most textbooks at a similar level[122]; though perhaps it was not available when the article was written. However, I'm not surprised that most such textbooks don't mention

MOND, since those are books which cover a wide range of topics (*e.g.*, galaxies *and* cosmology), necessarily leaving out most details of most topics. Let us take another example. As Merritt agrees, the *m–z* relation, especially for type-Ia supernovae, is an important cosmological test. That depends on the calculation of luminosity distance as a function of redshift for various cosmological models; the data are then used to fit for the cosmological parameters Ω_0 and λ_0. Often, it is tacitly assumed that the Universe at least behaves as if it were homogeneous with respect to light propagation, though small-scale inhomogeneities can significantly affect that calculation. A recent review, limited only to the simplest inhomogeneous models, of the so-called ZKDR or Dyer–Roeder distance lists 285 references[123]. That can be compared with the MOND review by Famaey & McGaugh[1], which has 518 references (though of course many are to papers which do not mention MOND at all, so the numbers of references to the corresponding topics are comparable). However, the ZKDR distance is also practically never mentioned in the books on Merritt's list. I see neither evidence for a conspiracy nor for over-arching ignorance; despite the interest of those concerned with such topics, even particularly interesting trees might not be mentioned when writing a book about a forest. Significantly, he notes that two textbooks which are not mainstream also "fail to mention either the acceleration scale or the mass-discrepancy–acceleration relation", as if disappointed that the authors of those books did not succumb to the the-enemy-of-my-enemy-is-my-friend fallacy. (Although written after Merrit's article, note that a recent major review (more than 80 pages, more than 300 references, already more than 160 citations) in a leading journal[38] does mention MOND, even though it is a review of dark matter.)

"Nothing in the pre-existing model (*ca*. 1970) pointed toward the need for dark matter or dark energy; the observations that motivated these hypotheses came as a complete surprise." That is true as far as it goes, but it doesn't go far enough; as mentioned above, the ideas of both dark energy (or at least the cosmological constant) and dark matter had already been around for decades in 1970. It is true that convincing evidence for dark energy (though not for dark matter, in clusters if not yet in individual galaxies) was lacking, but, as argued above, that was simply a practical matter of setting a parameter of the theory to zero until observations required otherwise.

The general tenor is that while MOND has made successful predictions (which is true), the standard model of cosmology has not (which is false). A good example of such a prediction is the CMB power spectrum. That was predicted long before it was observed, and *no additional parameters are needed to explain it*. Yes, the *values* of the parameters have to be determined by observation, but those fitted to the power spectrum agree with other measurements (which is why the current standard model is called the concordance model). The idea that a theory should have no free parameters is demanding too much. (Yes, the standard cosmological model has more parameters than MOND, but it also explains more.) Of course, even most MOND enthusiasts agree that MOND cannot explain the CMB power spectrum and many even say that that is evidence for dark matter (though believing that MOND, rather than dark matter, might be a better explanation in other contexts). There are good arguments for dark matter where MOND is *not* an alternative. Also, as mentioned above, dark matter doesn't have to be in the form of WIMPs. To summarize my criticism of Merritt, MOND might be a more elegant explanation in some areas, and that is played up, but areas in which it just doesn't work at all, and dark matter does, are played down. In particular, he doesn't seem to think that the lack of a relativistic theory is a big disadvantage for MOND. Of course, MOND might be some sort of effective theory, an approximation to an unknown relativistic theory, and in practice relativistic theories are not needed in many applications. However, as a scientific *theory*, which seems to be one of Merritt's main points, lack of compatibility with General Relativity is certainly a major deficit. He also assumes a false dichotomy: it might turn out that both MOND, or something like it, and dark matter are needed. My most important criticism of Merritt, however, is that he attacks a straw-man version of ΛCDM.

Other reactions to Merritt's article

At the time of writing, Merritt's article has, according to ADS, 15 citations. My motivation for writing this article is that Merritt is often cited as if he had proved that the MOND paradigm is somehow superior to ΛCDM. Hossenfelder[124], referring to WIMPs as dark-matter candidates, states that "[t]he expected cross-section has been repeatedly revised to stay below experimental bounds", as an example of bad science. Apart from the fact that there are probably better references than Merritt for that (though perhaps the intention is to direct readers to Merritt's critique of standard science, which is similar in tone to that of Hossenfelder), it is obvious that any prediction which remains must be compatible with experiment, in this case the lack of direct detection of WIMPs. Revising theories in the light of new evidence is part and parcel of science. Another interpretation is that the prediction of interaction cross sections based on the so-called WIMP miracle is simply wrong, which does not falsify WIMPs *per se*. Pawlowski[125] cites Merritt for pointing out that the success of a theory does not constitute proof of correctness, which hardly needs a citation. He goes on to point out correctly that a theory can be tested by comparing observations in regimes which played no role in its development to the predictions it makes in such a regime, noting that in the case of on galaxy scales, one doesn't test ΛCDM itself, but rather its realization *via* numerical simulations, with all the caveats that implies. Again, it seems that Merritt is cited merely to call attention to his paper, not because he has anything relevant to say on the matter. Indeed, Merritt often doesn't distinguish between ΛCDM and its realization *via* numerical simulations. McGaugh[126], discussing the still controversial result of the *EDGES* observations involving neutral-hydrogen absorption at high redshift[127], notes that it is problematic for ΛCDM but would be expected in a purely baryonic universe (of course, in other contexts even MOND supporters see a need for dark matter (*e.g.*, ref. 2), and while a pure MOND universe contains no non-baryonic matter, a universe with no non-baryonic matter doesn't necessarily have anything to do with MOND); he states "that there are remarkable genuine successes and apparently insurmountable hurdles for both approaches", MOND and ΛCDM, which is a perfectly valid and balanced statement, so it seems strange to back it up with a citation to Merritt's very unbalanced work. Traunmüller[128] cites Merritt because he agrees with him that "cosmologists interpret … falsifying observations even as tantamount to the *discovery* of dark matter or dark energy" [emphasis in the original], repeating the straw-man picture of mainstream cosmology. Massimi[129], in a very good and balanced article by a philosopher, incongruously cites Merritt for his "excellent discussion".* Neves[130], in an article involving Immanuel Kant and "the fuzzy degree of scientificity", cites Merritt for his idea

*In the acknowledgements, she notes that she had presented earlier versions of the paper at three conferences, and I also heard her give a talk on it at the 'Dark Matter and Modified Gravity' conference in Aachen in 2019 February. It was an excellent talk.

that dark matter and dark energy "are auxiliary hypotheses that were invoked in response to observations that falsified the standard model", agreeing with that sentiment. However, he seems rather ignorant of the history of cosmology, calling inflation "an *ad hoc* and a *posteriori* mechanism" in order to solve crucial observational problems in the standard model. (Actually, Guth[131] was trying to solve the monopole problem — not a problem with cosmology, but with particle physics, involving theories which have now been ruled out due to their falsified prediction of the time scale of proton decay — and later noticed that inflation could solve the horizon and flatness problems.) He later mentions the flatness issue as one of the problems*, ignoring the fact that it has been shown by many authors not to be a problem at all but rather a misunderstanding (*e.g.*, refs. 133–144). Chan[145,146] cites Merritt for noting that "it is doubtful that the baryonic matter can rigorously control the dark matter density profile in many dwarf galaxies" (paraphrased in the second citation), which is a major component of 'MOND phenomenology', so it seems strange to cite Merritt here rather than one of the many primary sources. Alagnostopoulos *et al.*[147] invoke Merritt in support of the claim that "there are too many *ad hoc* hypotheses (*e.g.* dark energy, dark matter)" which are needed for "explaining the phenomena" in a paper about "Dynamical Space-time Cosmology (DSC) that unifies dark energy and dark matter" which "includes a Lagrange multiplier, which is coupled to the energy momentum tensor and a scalar field which is different from quintessence". I leave it to the reader to decide which is more *ad hoc*. Milgrom[148], in a very interesting article† with as many footnotes as pages, cites Merritt in three neutral contexts: as one of several authors of discussions of MOND *vs*. DM, in a discussion of convergence with regard to the measurement of Avogadro's number (with which I have no bone to pick and which is a good parallel to a_0 in MOND phenomenology), and in connection with the 'Balkanization' of ΛCDM (but citing Merritt only in a footnote). That probably exists in some form, but Merritt exaggerates by seeing it as proof of the demise of ΛCDM, whereas Milgrom is more reserved: "This might not be decisively fatal for a theory, but it is arguably a bad omen for its fate." (In contrast to Merritt, who claims to be neutral, Milgrom "write[s] from the viewpoint of a MOND advocate" and his "article is not meant as a balanced presentation of the Mond-vs.-DM paradigm struggle".) Nevertheless, his account is actually more balanced than that of Merritt, for example noting that "[a] 'cosmological constant"' is a "straightforward addition to GR". Sanders[149], again in a contribution to the Aachen meeting, mentions Merritt only in passing, though for his "general discussion of the modern cosmological paradigm". Chan & Popolo[150] cite Merritt for general support that MOND might be a better theory than ΛCDM. Like Hossenfelder, Singirikonda & Desai[151] cite Merritt only in connection with lack of direct detection of WIMPs. McGaugh[46] mentions Merritt only as support for the idea that "[t]he gold standard for scientific predictions are those made in advance of the observation", a statement which is certainly true but not due to Merritt, though of course his claim is that MOND

*In a fascinating glimpse into the thinking at that time, Brawer[132] notes that neither the horizon problem nor, especially, the flatness problem were considered to be important issues until inflation suggested a solution to them. Her thesis, containing many direct quotations and a full interview with Guth, demonstrates the many views on those topics even then. It appears that Guth made an extra effort in his paper to convince the community that the flatness problem is, in fact, a problem (and thus that inflation offers a solution).

†Based on his talk at the Aachen conference mentioned in a previous footnote.

measures up to that standard better than ΛCDM. Benisty & Guendelman[152] cite Merritt, along with two others, as a general reference to the cosmological-constant problem! On-line, McGaugh[153], usually somewhat more balanced, heaps high praise on Merritt's article, "a genuine page turner that should be read by everyone interested in cosmology".

In summary, there is not one citation critical of Merritt. Rather, the citations are either gratuitous, perhaps intended to draw attention to his paper, or used to shore up similar sentiment by the author of the citing paper, the interesting exception being the founder of MOND, Milgrom, whose citations could be described as natural but neutral.

Suggestions for improving the debate

I hope that the above makes clear my recommendations for improving the debate. MOND phenomenology needs to be taken seriously and *any* valid theory must explain it in detail; disagreeing with MOND supporters concerning other things should not be a reason to shy away from that. (It is impossible to disagree on the observations themselves as they are not at the limits of technology or whatever, though of course there can be debate about the interpretation.) It is important to separate MOND phenomenology — which clearly exists and must be explained — from current effective theories of MOND, which might be completely wrong. Any non-MOND explanation, though, needs to be correct in detail and held to the same standard as in other areas of comparison between theory and observation. At the same time, MOND supporters should recognize that the current concordance model of cosmology is based mainly on observations and attempt to lure mainstream astrophysicists into explaining the interesting MOND phenomenology, rather than claiming that they are motivated by other factors and incapable of improving their model to accommodate more observations or that all such improvements are *ad hoc*. (The Novel Probes Project is an initiative to create a forum connecting theorists and observers with respect to tests of gravity on astrophysical scales[154], though it unfortunately does not properly include MOND.)

Mainstream cosmology is not falsified by the non-detection of dark-matter particles, nor by the fact that simulations (especially those without baryons) fail to reproduce all features of observed baryonic matter, and it is certainly not in conflict with GR. Neither was dark energy invented to save the phenomena. There might be some significance to the fact that $a_0/c \approx H_0$, suggesting an ultimately cosmological explanation for galaxy-scale phenomena; for the investigation of such matters, a more interdisciplinary approach might be more fruitful.

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Note added in proof: Recent work suggests that the problem of missing baryons could be less severe, *e.g.*, F. Nicastro *et al.*, *Nature*, **558**, 406, 2018; and J.-P. Macquart *et al.*, *Nature*, **581**, 391, 2020.

REDISCUSSION OF ECLIPSING BINARIES. PAPER 1: THE TOTALLY-ECLIPSING B-TYPE SYSTEM ZETA PHOENICIS

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ζ Phe is a bright binary system containing B6 V and B8 V stars. It has deep total and annular eclipses, a slightly eccentric orbit with a period of $1^{d}.669$, apsidal motion, and a third body on a wider orbit. The *Transiting Exoplanet Survey Satellite* light-curve and published radial velocities of this system are analyzed to determine masses of $3{\cdot}91 \pm 0{\cdot}06\,M_{\odot}$ and $2{\cdot}54 \pm 0{\cdot}03\,M_{\odot}$ and radii of $2{\cdot}84 \pm 0{\cdot}02\,R_{\odot}$ and $1{\cdot}89 \pm 0{\cdot}01\,R_{\odot}$. The resulting distance to the system is in agreement with its trigonometrical parallax. The physical properties of the stars, with the exception of the effective

temperature of the secondary component, can be matched by the predictions of several sets of theoretical stellar evolutionary models for a solar chemical composition and an age of 70–90 Myr. A spectroscopic analysis of this system is encouraged for the determination of the photospheric chemical composition of the stars, plus improved measurements of their masses and effective temperatures.

Detached eclipsing binary star systems

Eclipsing binary stars form one of the cornerstones of our understanding of the physics of stars, as they are the primary source of direct measurements of stellar masses and radii. Calculation of these properties can be performed using only observational data, geometry, and orbital mechanics[1,2]. Of the many types of eclipsing system, perhaps the most important is the detached eclipsing binary systems (hereafter dEBs), which have experienced no mass transfer so are representative of normal stars.

The measured properties of dEBs have been used to determine how stars evolve[3] and check the predictions of theoretical models of stellar structure and evolution[4–7]. The strength and mass dependence of the phenomenon of convective-core overshooting has been calibrated using dEBs[5,8–11], empirical relations of the properties of stars have been fitted to the properties of dEBs[12–14], and they have been utilized to determine the primordial helium abundance and the helium-to-metals enrichment ratio[15,16].

dEBs are also excellent distance indicators because it is possible to determine the luminosity of a system from the measured radii and effective temperature ($T_{\rm eff}$) values of the component stars. They can be used as direct distance indicators if one trusts bolometric corrections calculated from theoretical model atmospheres, because in this case there is no need to calibrate against nearby examples. An alternative approach is to use empirical surface-brightness relations calibrated on nearby stars[17–20], in which case dEBs can be turned into standard candles. The local distance scale now rests on results from the *Gaia* satellite, but dEBs remain useful for determining the distances to nearby galaxies such as the LMC[21,22], SMC[23,24], M31[25], and M33[26]. In turn, the cosmological distance scale can be anchored on nearby galaxies using distances from dEBs[27].

Another use of dEBs is to probe the interior structure of stars either *via* apsidal motion[28–30] and other tidal effects[31], or by direct determination of the physical properties of a pulsating star. The types of pulsating star found and studied as components of dEBs include δ Scuti[32,33], γ Doradus[34], β Cephei[35], SPB[36], stochastic pulsators[37,38], EL CVn systems[39,40], red giants with solar-like oscillations[41–43], and stars showing tidally-influenced modes[44,35,45,46].

Current status of the study of dEBs

dEBs have historically been difficult to study in detail due to the large amount of observing time needed to obtain good light and radial-velocity (RV) curves. Compilations of dEBs with precise mass and radius measurements, published by Popper[47,48], Andersen[49], and Torres *et al.*[13] contained 25, 36, 45, and 94 systems, respectively. Harmanec[50], Malkov *et al.*[51], Eker *et al.*[52], and others have published larger catalogues with relaxed qualification criteria, but the most useful dEBs for testing theoretical predictions are those for which mass and radius measurements to 2% (or preferably 1%) precision are available[49].

The situation is now changing due to a deluge of high-precision light-curves that are literally falling out of the sky from space telescopes such as *CoRoT*[53], *Kepler/K2*[54], and now *TESS* (the *Transiting Exoplanet Survey Satellite*)[55]. These missions were all conceived to facilitate the discovery of transiting planetary systems with transit depths as small as 0·01%, so find it trivial to both detect and produce high-quality light-curves of dEBs with eclipse depths up to 50% or more. In future the *PLATO* mission[56] will push the boundaries to even higher-precision data.

Progress has also been made on spectroscopic methods. Many of the dEBs in the catalogues given above were observed using photographic spectroscopy, and the RVs from these studies remain the best available — an example would be the subject of the current work. More sophisticated approaches, such as the use of large-format CCD detectors and échelle spectroscopy on the hardware side, and cross-correlation, spectral disentangling, Doppler tomography, and broadening functions on the software side, have greatly improved the measurement precision readily achievable in RV studies. Spectral disentangling[57,58] has several clear advantages: it requires no template, considers all spectra simultaneously, and produces not only the spectroscopic orbits of the stars but their individual spectra in a form suitable for determination of the $T_{\rm eff}$ values and chemical abundances[59,60].

Konacki *et al.*[61] measured RVs for double-lined binaries to precisions as good as 2 $\rm m s^{-1}$ using a cross-correlation approach. A recent study of the dEB AI Phe[62] used RVs obtained with this method, the *TESS* light-curve of the system, and a wide variety of analyses performed independently by multiple researchers, to determine the masses and radii of the component stars to precisions of 0·2%. An accompanying work[63] obtained the $T_{\rm eff}$ values of the two stars to 0·4% using the *Gaia* parallax and apparent magnitudes of the system.

Rediscussion of eclipsing binaries

The author maintains the *DEBCat*★ (*Detached Eclipsing Binary Catalogue*[64]) list of dEBs for which masses and radii have been measured to precisions of 2% or better, although this is not treated as a harsh cut-off. *DEBCat* was created in 2005 by updating the list of well-studied dEBs given by Andersen[49], and has since been maintained by including new systems and new results on existing systems.

A cursory inspection of the contents of *DEBCat* revealed that many of the entries were based on data and analyses obtained several decades ago, and that space-based high-precision light-curves are available for most of the systems listed in the catalogue. We expect that new analyses of these systems would in most cases lead to significantly improved physical properties, particularly radii. We therefore decided to commence a series of studies of suitable systems. Most of the new data will come from *TESS*, as this has a much greater sky coverage than other sources of high-quality light-curves, but other databases will be considered where appropriate. No spectroscopic analyses will be performed, in order to allow many dEBs to be studied within the limited time available. Detailed spectroscopic studies of these objects by other workers are highly encouraged in order that precise masses, $T_{\rm eff}$ values, and chemical compositions could be measured for many of these systems.

★https://www.astro.keele.ac.uk/jkt/debcat/

Another consideration is the continued improvement in measurement of the physical constants and solar quantities used in the study of dEBs. The astronomical unit was redefined to be an exact quantity by the International Astronomical Union (IAU) 2012 Resolution B2, and a set of nominal properties of the Sun were delineated in IAU 2015 Resolution B3[65]. Although the effects of this are small, they are no longer negligible by current standards. As an example, the radius of the Sun found by Brown & Christensen-Dalsgaard[66] is smaller by 0·03% than the value recommended by the IAU. This motivates the systematic reanalysis of dEBs using the same physical constants.

The title chosen for this series of papers is 'Rediscussion of Eclipsing Binaries', for two reasons. Firstly, it fits the scientific aims extremely well. Secondly, it references an illustrious series of works produced by Daniel M. Popper, beginning with Z Her[67] and culminating with V380 Cyg, VV Ori, and V1765 Cyg[68]. Our primary aim is the curation of the *DEBCat* catalogue, but other systems will be covered when interesting results are obtained.

In this first work, we consider the bright and early-type dEB ζ Phoenicis. This came to our attention whilst searching for a nice totally-eclipsing dEB for an exam question. The *TESS* light-curve is stunning and suitable for a significant improvement in our understanding of the system. It was also found that the uncertainties in the masses had been underestimated in previous works, something that has been noticed for other systems studied using similar spectroscopic material[69].

The dEB ζ Phoenicis

ζ Phe (Table I) was reported to be a spectroscopic binary by Wilson[77] from photographic plates taken at Lick Observatory. RVs and a double-lined spectroscopic orbit were presented by Colacevich[78] based mostly on the Lick material; *Simbad* misreports the subject of the latter publication as δ Phe. Hagemann[79] obtained 80 photographic spectra and was able to measure RVs for the primary on 71 plates, and for the secondary on only 16 plates. In his *Rediscussion of Eclipsing Binaries, Paper 9*, Popper[80] presented a spectroscopic orbit of both components of ζ Phe based on 21 photographic spectra, finding velocity amplitudes and thus masses significantly different than previous studies. The most recent spectroscopic study of this system[81] was based on 31 high-dispersion spectra obtained using photographic plates, and yielded precise velocity amplitudes for the two stars.

TABLE I

Basic information on ζ Phe

Property	*Value*	*Reference*
Bright Star Catalogue designation	HR 338	70
Henry Draper designation	HD 6882	71
Hipparcos designation	HIP 5348	72
Hipparcos parallax	10·92 ± 0·39 mas	73
Gaia DR2 ID	4913847589156808960	74
Gaia parallax	14·68 ± 0·73 mas	74
B magnitude	3·908 ± 0·014	75
V magnitude	4·014 ± 0·009	75
Spectral type	B6 V + B8 V	76

The discovery of eclipses in ζ Phe was made by Hogg[77], who obtained unfiltered photoelectric observations of the star. The primary eclipse is annular and the secondary eclipse is total. Dachs[82] obtained photoelectric observations on the *UBV* system and Knipe[83] observed one primary eclipse. Extensive photometry in the Strömgren *uvby* system was obtained and analyzed by the Copenhagen group[84,85]. From these results and his own RV analysis, Andersen[81] measured the masses and radii of the two components of the eclipsing system to precisions of approximately 2%. Since then, a complete light-curve has been published by Shobbrook[86].

ζ Phe is known to have two fainter nearby companions[87,88,85]: a star of magnitude $V = 8{\cdot}2$ at 6″·4 forms RMK2, and one of magnitude $V = 6{\cdot}8$ at 0″·5 is designated RST1205, and both are significantly fainter than the eclipsing system. A subsequent measurement[89] gives a separation of 0″·5486 and a magnitude difference of $2^{m}{\cdot}7$ mag for RST1205. Andersen[81] identified RST1205 in his spectra, making ζ Phe a triple-lined system, and measured its RV from 26 of the plates. The RV of that object was found to be roughly in agreement with the systemic velocity of the eclipsing system, suggesting that the three stars are gravitationally bound. This has been confirmed[90] with the measurement of the orbital period of the third body (220·9 ± 3·5 yr) from astrometric observations.

One remaining characteristic of ζ Phe to be discussed is the presence of apsidal motion. This was first noticed by Dachs[82], and an apsidal period of $U = 44{\cdot}2 \pm 6{\cdot}5$ yr was measured by Giménez *et al.*[91]. The most recent measurement of the rate of apsidal motion is $\omega = 6{\cdot}16 \pm 0{\cdot}20$ deg yr^{-1}, corresponding to $U = 58{\cdot}4 \pm 1{\cdot}8$ yr[90].

Observational material

TESS was launched by NASA on 2018 April 8 into a highly eccentric orbit around the Earth with an orbital period half that of the Moon. It is currently engaged in a photometric survey of 85% of the celestial sphere with the aim of identifying extrasolar planets through the transit method[55]. It includes four cameras with 10·5-cm apertures, each with four CCDs, that together image a 24°×96° strip of sky. Each pixel subtends a solid angle of 21″×21″. The observations are performed through wide-band filters with a high response function between 600 nm and 1000 nm.

TESS observes individual strips of sky for two orbits ($27^{d}{\cdot}4$) with a break near the midpoint for download of data to Earth *via* the NASA Deep Space Network. Such a unit of observation is called a sector, and on its completion *TESS* moves on to the next sector. As it is designed to detect shallow planet transits, the light-curves are of very high quality and thus well suited to studies of large-amplitude variable stars such as dEBs. A total of 200 000 stars were pre-selected for high-cadence observations (summed into a 120-s sampling rate). Full-frame images are also captured at a cadence of 1800 s, and subsequently clipped into an effective integration time of 1425 s by a cosmic-ray-rejection algorithm.

The data are processed and released as light curves by the *TESS* Science Processing Operations Center for stars observed at high cadence[92]. Two versions are available: simple aperture photometry (SAP) and pre-search data conditioning (PDC). The latter is based on the former but undergoes additional processing to remove signals which might obscure shallow transits, a procedure that risks removing astrophysical signal so is not suitable for use on objects showing deep eclipses.

ζ Phe was observed* using *TESS* in Sector 2 (2018 August 22 to 2018

* https://heasarc.gsfc.nasa.gov/cgi-bin/tess/webtess/wtv.py

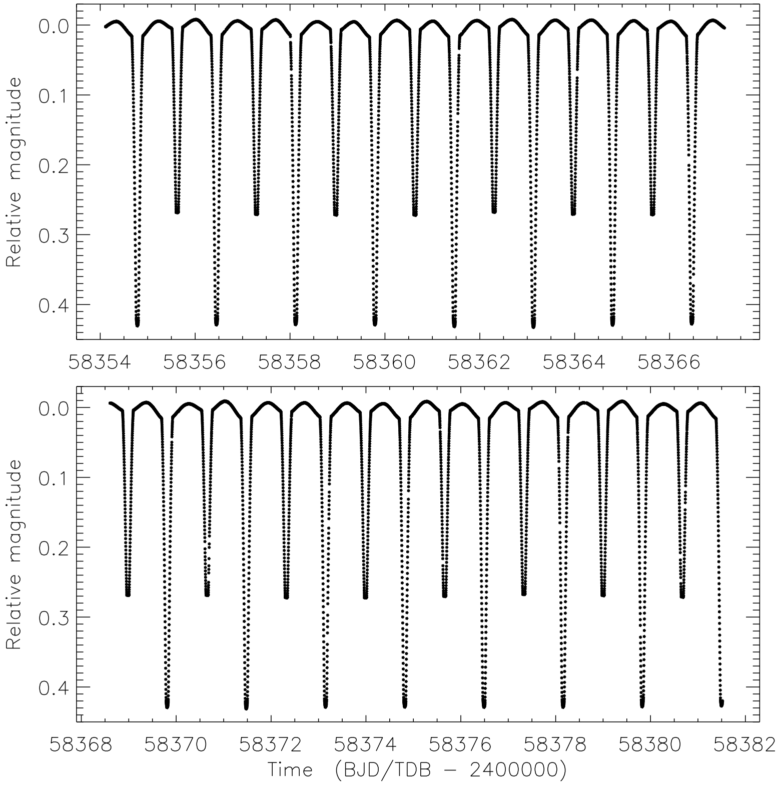

FIG. 1

TESS simple aperture photometry of ζ Phe from Sector 2. The upper and lower plots show the observations either side of the mid-sector pause for data download.

September 20), in high cadence. These data were downloaded from the Mikulski Archive for Space Telescopes (MAST) archive★ and the SAP measurements were converted into magnitude units ready for further analysis. Only data with no flagged problems (QUALITY = 0) were retained, and the data uncertainties were ignored because they have little variance and are far too small. Five data points were rejected as being 4σ outliers, leaving a total of 18 278 for analysis (Fig. 1).

Light-curve analysis with JKTEBOP

A preliminary analysis of the *TESS* light-curve was performed using version 40 of the JKTEBOP code† [93,94]. JKTEBOP approximates the stars as spheres for the purposes of calculating eclipse shapes and as ellipsoids for calculation of proximity effects.

★ https.//mast.stsci.edu/portal/Mashup/Clients/Mast/Portal.html

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

We designate the primary star (the one eclipsed during the deeper eclipse) as star A, and the secondary star as star B. In the current case, star A has a larger mass, radius, and $T_{\rm eff}$ than star B. Parameters of the fit included the sum and ratio of the fractional radii of the stars ($r_{\rm A} = R_{\rm A}/a$ and $r_{\rm B} = R_{\rm B}/a$, where $R_{\rm A}$ and $R_{\rm B}$ are the true radii and a is the orbital semi-major axis), the orbital inclination, and the central surface-brightness ratio. The orbital period and a reference time of mid-eclipse were also fitted, and no constraints from historical data were applied in order to avoid the complications arising from the known apsidal motion and third-body effects in the system. We included limb darkening using the quadratic law with the linear coefficients fitted and the quadratic coefficients fixed to values from Claret[95]. The Poincaré elements, $e \cos \omega$ and $e \sin \omega$, where e is the orbital eccentricity and ω is the argument of periastron, were included as fitted parameters. Finally, third light was included as a fitted parameter because the two fainter nearby stars are much closer than the pixel scale of *TESS*.

Uncertainties were calculated using Monte Carlo and residual-permutation algorithms[93]. The results of this process were very encouraging, with uncertainties in $r_{\rm A}$ and $r_{\rm B}$ of roughly 0·1%. However, $r_{\rm A}$ was found to be sufficiently large that the spherical approximation used in JKTEBOP is inaccurate at the level of approximately 1%[35]. This caveat applies also to previous studies that relied on the EBOP code[85]. It was therefore necessary to switch to a more sophisticated code.

In preparation for a more refined analysis with a slower code, we used the best-fitting orbital ephemeris from the JKTEBOP fit to phase-bin the light-curve into 388 data points. These were sampled ten times more finely through the eclipses in order to retain all important temporal information in the light-curve whilst avoiding the computational expense of calculating a fine grid of out-of-eclipse data points.

Light-curve analysis with the Wilson–Devinney code

The phase-binned light-curve from the previous section was modelled using the Wilson–Devinney code[96,97], which implements Roche geometry to represent accurately the tidally-distorted components of binary star systems. We used the 2004 version of the code[98], hereafter called WD2004, driven *via* the JKTWD wrapper[99]. Because WD2004 does not include the *TESS* passband, it was operated in mode 0 with the $T_{\rm eff}$ values of the stars decoupled from their light contributions.

For the initial fits the fitted parameters were the potentials of the two stars, their light contributions, the amount of third light in the *TESS* passband, e, ω, and the orbital inclination. The rotational velocities of the stars were set to be pseudosynchronous with the orbital motion, the gravity-darkening exponents were set[100] to 1·0 and the $T_{\rm eff}$ values were fixed at the values given by Andersen[81]. The mass ratio, defined as the ratio of the mass of star B to that of star A, was set to 0·649.

Limb-darkening coefficients were taken from Van Hamme[101], and limb darkening was initially specified using the linear law. We immediately found that at least one coefficient for each star had to be varied in order to get a good fit. For reference, the initial values were $u_{\rm A} = 0{\cdot}196$ and $u_{\rm B} = 0{\cdot}227$ and the fitted values were $u_{\rm A} = 0{\cdot}274$ and $u_{\rm B} = 0{\cdot}464$, so there was a need for stronger limb darkening than theoretically predicted. Further experimentation showed that the square-root limb-darkening law was significantly better and that the logarithmic was slightly better still, in all cases with at least one of the two coefficients for each star fitted. For the final model the logarithmic law was adopted and the linear coefficient for each star was fitted. Fitting for both coefficients for each star was not an option as this is not implemented in WD2004; it would be very unlikely to help due to the known strong correlations between the coefficients[102,93,103–105].

Our attempts to fit for the rotation rate of the primary star yielded values close to pseudosynchronous, and for the secondary star the fits were unstable. In both cases these parameters had no significant effect on the best-fitting fractional radii, which are the two parameters of greatest interest obtainable from the light-curve. Fitting for the gravity-darkening exponents was similarly unsuccessful.

Fitting for albedo returned a much better fit but for physically unexpected values of the albedos. The best compromise was to fit for the albedo of the secondary component, which always drifted up to values of roughly 1·4, but to fix the albedo for the primary component to 1·0 to avoid getting negative values. Albedo is the ratio of the light emitted to the light incident on the surface of the star, so is expected[106] to be between 0·0 and 1·0. An albedo above unity

TABLE II

Summary of the parameters for the WD solution of the TESS light-curve of ζ Phe. Uncertainties are only quoted when they have been robustly 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)	50	
Fixed parameters:		
Orbital period (d)	1·6697739	
Primary eclipse time (BJD/TDB)	2458366·46953	
Mass ratio	0·649	
Rotation rates	1·0	1·0
Gravity darkening	1·0	1·0
$T_{\rm eff}$ (K)	14 400	12 000
Bolometric linear limb-darkening coefficient	0·7497	0·7274
Bolometric logarithmic limb-darkening coeff.	0·0709	0·0721
Linear limb-darkening coefficient	0·38 ± 0.17	0·39 ± 0.10
Logarithmic limb-darkening coefficient	0·1732	0·1890
Fitted parameters:		
Phase shift	−0·00226 ± 0·00001	
Potential	4·583 ± 0.022	4·999 ± 0·016
Orbital inclination (°)	89·14 ± 0·11	
Orbital eccentricity	0·0116 ± 0·0024	
Argument of periastron (°)	307 ± 12	
Third light	0·1220 ± 0·0031	
Light contributions	8·464 ± 0.042	2·573 ± 0.041
Bolometric albedos	1·0 (fixed)	1·47 (fitted)
Derived parameters:		
Light ratio	0·3040 ± 0·0051	
Fractional radii	0·2572 ± 0.0013	0·1710 ± 0·0006

is therefore not expected, but is physically possible if some flux from shorter wavelengths is reprocessed and emitted in the *TESS* passband. From the author's experience[99,107,35] albedo can compensate for unrelated inaccuracies of the binary model and often rises above 1·0 when that model is confronted with high-quality data. The treatment of albedo is often a significant contributor to the uncertainty of the final results, and this is the case in the current work.

Table II summarizes the results of the work above. We designate the final fit to be that with the maximum numerical precision and with the logarithmic limb-darkening law. The fitted parameters were the potentials, light contributions, and linear limb-darkening coefficients of the two stars, the orbital eccentricity, argument of periastron, orbital inclination, the phase of primary eclipse, third light, and the albedo of the secondary star. Third light is given as the fraction of the total light of the system at phase 0·25, so is not on the same scale as the light contributions of the two eclipsing stars.

The best fit (Fig. 2) is good, with an r.m.s. *versus* the phase-binned data of 0·3 mmag, but is still dominated by systematics. The high-frequency systematics during eclipse can be attributed to the numerical precision of the code. The low-frequency systematics outside eclipse indicate that the proximity effects implemented in WD2004 are only an approximation and exceed the Poisson

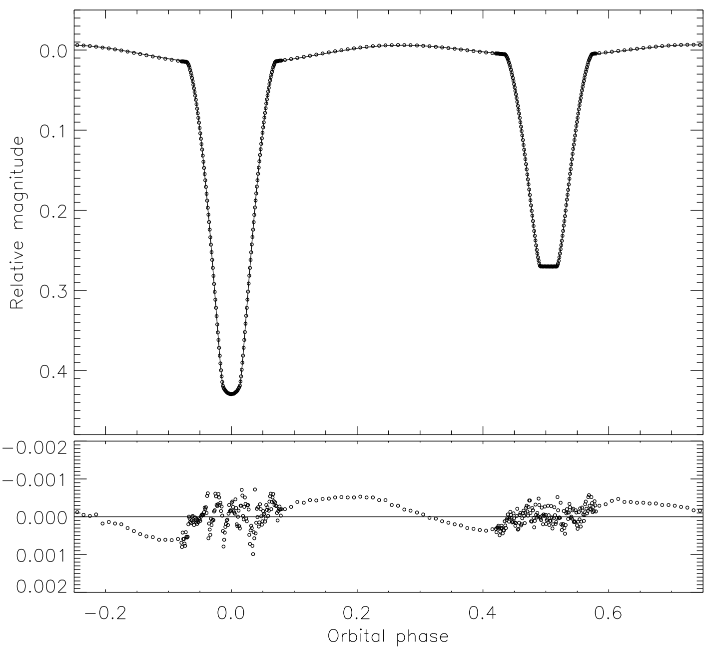

FIG. 2

Best fit to the *TESS* light curve of ζ Phe 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.

noise in this case. In order to test if this could be caused by the Doppler boosting effect[108–110] the input data were adjusted to account for the expected amount of Doppler boosting using the approach outlined by Loeb & Gaudi[109] and the fit was rerun. This yielded a slightly worse fit with parameters well within the error bars given in Table II, so this effect is not to blame for the imperfect fit. For the record, the expected amplitude of the Doppler boosting effect for ζ Phe is approximately 0·74 mmag.

Error analysis for the light curve

As the Poisson noise in the *TESS* data is negligible, we have calculated the uncertainties in the fitted parameters by comparing fits obtained with different sets of input parameters. These sets differed from the final fit by changes to the limb-darkening law used (linear or square-root *versus* logarithmic), the way albedo was fitted (primary only, secondary only, both stars), the numerical precision specified in WD2004, rotation, and gravity darkening. The effect of each of these was determined for each fitted parameter by differencing the fitted value with that from the overall best fit, and these were added in quadrature to give the final uncertainties.

These uncertainties are included in Table II and show that the solution is very well determined. The dominant uncertainty in the fractional radii arises from the treatment of albedo. Until a clear improvement in the understanding of this parameter is obtained, the uncertainties in the fractional radii cannot be significantly lowered. The current uncertainties are only 0·5% and 0·4%, so are well within our target of 1% precision.

WD2004 computes formal uncertainties from the covariance matrix, and the user guide[98] cautions against their adoption as the true uncertainties of the fitted parameters. The formal uncertainties have been found to be too small in several cases[111,112] and the current analysis allows this to be quantified in the case of negligible Poisson noise in the observational data. The formal uncertainties exceed the true uncertainties for the fitted parameters listed in Table II by factors ranging from 6·6 (for the light contribution of the primary star) to 131 (for the linear limb-darkening coefficient of that star). It is clear that the choice of model is a critical contributor to the uncertainties in the solution in data of high quality.

Finally, it is interesting to consider the difference in results between the preliminary fit with JKTEBOP and the final fit with WD2004. This is restricted to the fractional radii for brevity, for which it amounts to 0·45% for r_A and –0·02% for r_B. The values for the more distorted primary star differ by roughly the size of the final error bar, and for the secondary star by a negligible amount. This supports the use of JKTEBOP for stars which are relatively undistorted (see also Maxted *et al.*[62]).

Physical properties of ζ Phe

Armed with the orbital inclination and fractional radii from the light-curve analysis above, it is now possible to calculate the full physical properties of the system. This in turn requires some results from spectroscopic analysis: the velocity amplitudes and $T_{\rm eff}$ values of the stars. For the latter we adopted the values proposed by Andersen[81], $T_{\rm eff;A} = 14400 \pm 800$ K and $T_{\rm eff;B} = 12000 \pm 600$ K.

Velocity amplitudes were also given by Andersen[81], but these were reanalyzed in order to check the uncertainties. The RVs were copied from Andersen[81] and fitted with a Keplerian orbit using JKTEBOP. The two stars were not required to have the same systemic velocity, and the fitted values differ by 4·0 ± 2·1 km s^{-1}. The RVs were not supplied with data errors so a single value was chosen for each star to force a reduced χ^2 of $\chi^2_\nu = 1{\cdot}0$. Thus it was assumed that all RVs for each star are of equal precision, and that the precision can be estimated from the scatter around the best fit. Uncertainties in the velocity amplitudes were calculated using Monte Carlo simulations. The resulting velocity amplitudes are $K_A = 131{\cdot}4 \pm 0{\cdot}7$ km s^{-1} and $K_B = 202{\cdot}5 \pm 1{\cdot}3$ km s^{-1}. The uncertainties in these values are modestly larger than those given by Andersen[81] (0·6 and 1·0 km s^{-1}, respectively). The RVs and fitted orbits are shown in Fig. 3.

The physical properties of the ζ Phe system were calculated from the photometric and spectroscopic results using the JKTABSDIM code[19]. JKTABSDIM was modified for this work to use the standard physical constants and stellar properties adopted by the International Astronomical Union (IAU) 2012 Resolution B2 and 2015 Resolution B3[65]. The uncertainties of all input parameters were propagated to all output parameters using a perturbation

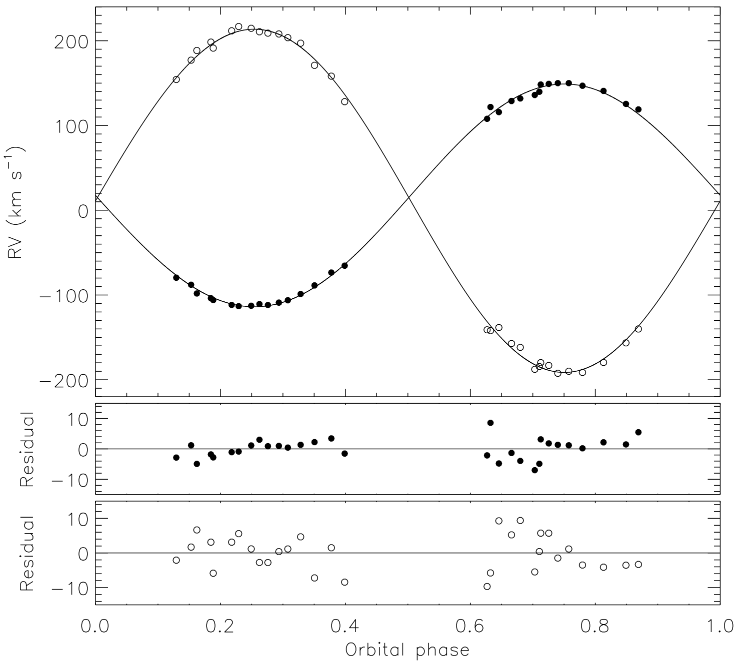

FIG. 3

Spectroscopic orbit of ζ Phe. RVs of the primary and secondary stars are shown with filled and open circles, respectively. The fitted orbits are shown using solid lines. The lower panels show the residuals of the fit.

TABLE III

Physical properties of ζ Phe. The T_{eff} values are from Andersen[81].

Parameter	*Star A*	*Star B*
Mass ratio	0·6490 ± 0·0053	
Semi-major axis ($R_\odot$)	11·022 ± 0·048	
Mass ($M_\odot$)	3·908 ± 0·057	2·536 ± 0·031
Radius ($R_\odot$)	2·835 ± 0·019	1·885 ± 0·011
Surface gravity (log[cgs])	4·1249 ± 0·0052	4·2917 ± 0·0038
Density ($\rho_\odot$)	0·1715 ± 0·0027	0·3788 ± 0·0044
Synchronous rotational velocity (kms^{-1})	85·89 ± 0·57	57·11 ± 0·32
T_{eff} (K)	14400 ± 800	12000 ± 600
Luminosity log($L/L_\odot$)	2·49 ± 0·10	1·82 ± 0·09
M_{bol} (mag)	−1·49 ± 0·24	0·19 ± 0·21

analysis. The results are given in Table III and agree well with those from Andersen[81].

The distance to ζ Phe has been measured to be 91·6 ± 3·2 pc from its trigonometric parallax found using the *Hipparcos* satellite[73]. The analogous measurement from the *Gaia* satellite[74] is less good because the binary system is so bright. Using the *Hipparcos* B and V magnitudes of the system[75] and bolometric corrections from Girardi *et al.*[113], we find a distance to ζ Phe of 87·3 ± 3·3 pc. This consistency check is sufficiently successful to support the T_{eff} values used for the stars; an increase of 800 K is needed to bring the two distances into exact agreement. We have not been able to find reliable photometry in other widely-used passbands, limiting the distance comparisons that can be made.

Comparison with theoretical models

The measured masses, radii, and T_{eff} values have been compared to the predictions of several sets of theoretical models to gauge the level of agreement between observation and theory. The comparisons were performed in the mass–radius and mass–T_{eff} planes in order to have as direct a link as possible with the observational results[115]. An approximately solar metal abundance was assumed for the stars in the absence of direct spectroscopic measurements of their photospheric compositions.

The PARSEC models[114] fit the stars within the error bars in the mass–radius diagram for a fixed fractional metal abundance of Z = 0·017 and an age of 80–90 Myr. In the mass–T_{eff} plot the agreement is good for the primary star (0·5σ lower than the measured value) but not for the secondary star (2σ lower). A more metal-poor chemical composition improves the fit to the T_{eff} values but at the expense of the quality of fit in the mass–radius plane (see Fig. 4).

The Teramo models[116] tell a similar story but using Z = 0·0198 and giving an age of 70–80 Myr. No significant difference to the fit is seen for models with and without convective core overshooting. Finally, the Yonsei–Yale models[117] provide an essentially identical fit to the other grids of models.

ζ Phe is therefore a young system with an approximately solar chemical composition, but the sets of theoretical models considered agree much better with each other than with the observed properties of this binary system. A detailed spectroscopic analysis of the two stars to determine precise T_{eff} values and photospheric chemical abundances would be useful.

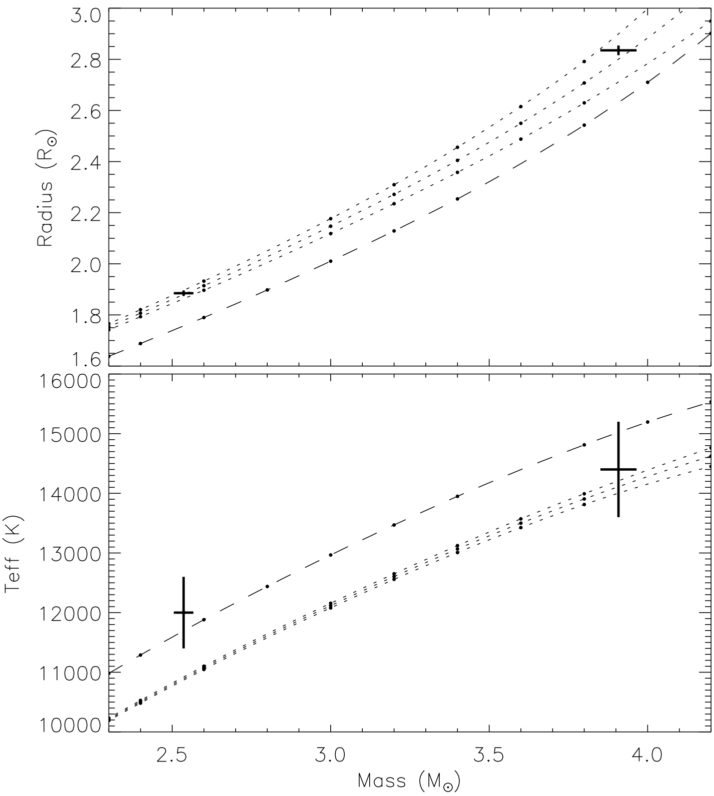

FIG. 4

Mass–radius and mass–$T_{\rm eff}$ plots showing the properties of ζ Phe *versus* PARSEC models[114]. The dotted lines are model predictions for $Z = 0{\cdot}017$ and ages of 70, 80, and 90 Myr. The dashed lines are predictions for $Z = 0{\cdot}010$ and an age of 80 Myr. The small circles show the points at which the models have been tabulated; the lines have been generated by interpolation between these points.

Conclusion

The bright eclipsing binary system ζ Phoenicis has been analyzed using the light-curve of this system from the *TESS* mission and published RVs. The *TESS* light-curve has essentially negligible Poisson noise and a perfect fit could not be obtained using the Wilson–Devinney code. The radii of the stars are nevertheless determined to precisions of better than 1%, where the uncertainties were obtained by considering the best-fitting parameters found using a variety of plausible assumptions in the modelling process. The masses of the stars are measured to 1·5% precision, using RVs measured from photographic spectra. The distance to the system determined from stellar radii, apparent magnitude, and bolometric corrections agrees with that found using the *Hipparcos* satellite.

The masses, radii, and $T_{\rm eff}$ of the primary star are well matched by theoretical predictions for a roughly solar metallicity and an age of 70–90 Myr, but the $T_{\rm eff}$ of the secondary star is higher than predicted by approximately 2σ.

A new spectroscopic study of this system would be extremely helpful in refining the measurements of the masses and $T_{\rm eff}$ values of the stars, and establishing their photospheric chemical compositions and rotational velocities. Such spectra already exist in the archive of the European Southern Observatory (ESO) and are available for use. Once these results have been obtained, and because the two component stars have significantly different physical properties, ζ Phe will be able to provide an exacting test of the predictions of stellar evolutionary theory.

Acknowledgements

I would like to thank Pierre Maxted, Kresimir Pavlovski and an anonymous referee for their comments on this work at the draft stage. The following resources were used in the course of this work: the ESO archive; the NASA Astrophysics Data System; the *Simbad* database operated at CDS, Strasbourg, France; and the arXiv scientific paper preprint service operated by Cornell University.

Afterword

The writing of this paper began, partly by coincidence, on the day that I learned of the death of Johannes Andersen (1943–2020). Johannes was an inspiration in his careful analysis of so many eclipsing systems, and in his seminal 1991 review paper. I would like to dedicate this work to both Johannes, whom I knew only briefly, and to his colleague Jens Viggo Clausen (1946–2011). Jens Viggo was a wonderful and hospitable boss for my first postdoctoral position, at the Niels Bohr Institute in Copenhagen where both he and Johannes worked.

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CORRESPONDENCE

To the Editors of 'The Observatory'

The Herschel Obelisk in Cape Town

While on a visit to Cape Town a few years ago I took the opportunity to visit the obelisk that marks the site of John Herschel's historic 20-ft reflector, the world's first all-sky survey telescope. John had already used this telescope to repeat his father William's survey of the northern skies from Observatory House, Slough, in 1825–33 before dismantling it and transporting it to the Cape of Good Hope. From there he studied the southern skies from 1834 to 1838, completing a unique one-man all-sky survey with a single instrument.

When John packed up the telescope for his return to England he placed a small granite cylinder at the pivot point of its rotatable mounting. A stone obelisk was later built over the spot, which the historian Brian Warner has described as 'almost a shrine'[1]. As Cape Town has grown the 'shrine' has become incorporated in the grounds of The Grove Primary School in Claremont, a residential suburb, and remains little-known and little-visited. As we approach the 150th anniversary of John Herschel's death in 2021 this is perhaps a suitable occasion to remember the monument and correct some of the misinformation that has grown up about it.

John Herschel and family arrived at the Cape of Good Hope in 1834 January. Within a week he had found a suitable residence with surrounding land at Feldhausen ('field houses') some 6 km southeast of the city in a sheltered spot inland of Table Mountain. A month later he had erected the 20-ft reflector in a small orchard to the north of the house, as shown in the famous drawing that forms the frontispiece to his *Results of Astronomical Observations Made at the Cape of Good Hope*.[2] The white hut to the right of the 20-ft in the drawing housed John's 5-inch *Tulley* refractor used for double-star measures. Table Mountain towers in the background, while the residence itself is off to the left.

At the end of 1834 March John was writing enthusiastically to Caroline Herschel, his aunt, about the merits of this site: "While it has been blowing a perfect hurricane in Cape Town I have been able to sweep with the 20-feet without inconvenience."[3] By contrast, the professional Cape Observatory, opened in 1828 on a low hill in sight of Table Bay, was notoriously exposed to the wind.

After four richly productive years of celestial surveying, in 1838 February John dismantled the 20-ft for the final time. He sold the Feldhausen estate but retained ownership of a plot of land 63 ft in diameter surrounded by a ring of fir trees where the 20-ft had stood. At the centre he placed a small pillar of local granite, inscribed with his initials and the year of his departure, 1838.

Later that year members of the South African Literary and Scientific Institution, of which John had once been president, formed a committee to raise funds for a more elaborate memorial. An Egyptian-style obelisk was decided on. During the planning stage Thomas Maclear, director of the Royal Observatory at the Cape, had written to tell Herschel that it would be "a massive granite monument"[4] but for practical and economic reasons this was changed to sandstone blocks from the quarry at Craigleith near Edinburgh.[5] This is the same durable stone from which many of the great buildings of Edinburgh were constructed, including the City Observatory on Calton Hill. However, John remained under the impression that the obelisk was of granite even after it had been built.[6] He never returned to South Africa so he never saw the monument for himself.

The stone blocks for the monument arrived at the Cape in 1841 August. Their assembly was delayed several months by the need to raise further funds, but by the end of the year construction was under way, under the energetic supervision of Colonel Griffith George Lewis of the Royal Engineers. Although Maclear had promised John that "Your pillar will not be touched or disturbed", in practice it was necessary to remove it to complete the foundations of the monument, after which it was replaced with what Lewis described as "mathematical correctness".[7]

Before the pillar was replaced a time capsule was buried beneath it containing coins, commemorative medals, published engravings of nebulae observed by John at Slough, and some local charts, all in a glass bottle sealed with pitch and finally encased in a block of teak. The capsule remains there today, awaiting a time "when calamity or curiosity will once again uncover these treasures", as Warner wrote.[8]

The obelisk consists of seven tapering blocks of yellowish Craigleith sandstone on a square base 6 feet wide and 6 feet high, surmounted by a pyramidal capstone that was laid in a topping-out ceremony on 1842 February 15. The overall height from base to peak is 20 ft, the same as the length of Herschel's telescope tube. Whether this was intentional or a happy coincidence is not stated anywhere that I can find.

The sides of the base are aligned on the four compass points. The south side, which faced the Feldhausen manor house, is open so that the original Herschel pillar can be seen (Lewis in his report published in the Memoirs of the RAS wrote that the opening was on the east side, but this is clearly incorrect) (Plate 1). Plaques in English, Latin, and Afrikaans are affixed to the remaining three sides, all added at a later date.

The Herschel family gave the piece of land containing the monument to the Claremont Municipality in 1906. The obelisk was adopted as a South African national monument under the Natural and Historical Monuments, Relics and Antiques Act in 1936.[9]

How did this national monument end up in the grounds of a school? In 1885 the Grove School was founded in a new building next to the Feldhausen residence, which for a while was used as accommodation for boarders (the Grove was an alternative name for Feldhausen). The old house, much altered, became the Herschel Hotel in 1934 and was eventually pulled down in 1958 to make way for a Hebrew nursery school, now a synagogue, which lies on the south side of present-day Grove Avenue in Claremont.[10] On the north side of Grove Avenue lies the current Grove Primary School, much enlarged and entirely rebuilt since its founding, and the obelisk lies in what is now its entrance courtyard (Plate 2). Since 1972 the obelisk has featured centrally in the school's coat of arms, surmounted by three stars, and its wireframe outline appears in the ironwork of the school gates.

John Herschel's return to England in 1838 marked the end of his observing career, and also that of the 20-ft telescope. The Herschel Era, begun by his father in Bath some 60 years earlier, was over. As well as the 'shrine' of the obelisk, 'holy relics' from John's visit to the Cape are still to be found: the octagonal wooden tube of the 20-ft telescope, along with one of the three 18-inch mirrors that John took with him to South Africa, was donated by the Herschel family to the National Maritime Museum, Greenwich, in 1958 and is now on display at the National Air and Space Museum in Washington, on long-term loan.

Yours faithfully,
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REVIEWS

As the World Turns: The History of Proving the Earth Rotates, by Peter Kosso (World Scientific), 2020. Pp. 273, 23·5 × 16 cm. Price £50 (hardbound; ISBN 978 1 78634 817 3).

Fans of the Bristol-based musical group Massive Attack, especially their 1991 album, *Blue Lines*, will be in no doubt that the Earth spins on its axis as their song, *Hymn of the Big Wheel*, repeatedly tells us that it does. In Tom Stoppard's 1972 play, *Jumpers*, the fictional philosopher George Moore reports a conversation between Wittgenstein and a friend: "Tell me (said Wittgenstein) why do people always say it was *natural* for men to assume that the sun went round the earth rather than that the earth was rotating?" His friend said "Well, obviously, because it just *looks* as if the sun is going round the earth". To which the philosopher replied, "Well, what would it have looked like if it had looked as if the earth was rotating?" And there, between Massive Attack and Tom Stoppard, you have the heart of this thoroughly engaging book.

We know the Earth spins on its axis, even if it looks to some as if the Sun goes round the Earth. How do we know? As Peter Kosso points out not only do we know that the Earth rotates but we know how fast. The time spent by Galileo under house arrest; between his conviction and his death amounted to almost eight years, or 3122 days. To put it another way, during this time the Earth rotated on its axis 3122 times with respect to the Sun. As all readers of this *Magazine* will know, it rotated a few more times with respect to the stars — in fact in that reference frame it completed eight more rotations — as if an extra eight days got magicked out of thin air. Kosso describes rotation as a two-place property; it needs two pieces of information to be fully described. Much like the property 'taller' — something isn't just taller, it is taller than something. Equally, to say something rotates needs the addition of rotates with respect to what. All motion is relative. The Earth rotates relative to the Sun, and by a different amount with respect to the stars and the very fact of these two references allows for the possibility of a rotation with respect to a reference frame attached to the Earth itself, with respect to which it does not rotate at all. Hence in such a frame the Earth rotated exactly zero times for the entirety of Galileo's incarceration. Which colours his alleged muttering "And yet it moves" after his conviction.

The statement that all movement is relative may seem a very thin thread on which to hang a book of almost 300 pages. Kosso describes observations and experiments that have been carried out since the Pythagoreans (specifically Philolaus of Croton), around 400 BC, first conjectured the movement of the Earth. He details measurements that have been made since those times to establish this movement as a fundamental truth that is now so obvious we no longer need to be even shown the evidence. We just know it to be true. He unpicks the experiments and observations and demonstrates that because all motion is relative, to say something moves or rotates is not a trivial matter. Kosso treats with respect the historical practitioners and does not, as many histories of science do, regard the early natural philosophers as primitive fools, chained to religion, myth, and superstition. He gives careful weight and consideration to their thinking and to the available information, and places it in the context of theories they had at their disposal to frame their thought. He carefully looks at what counts as evidence and examines the scientific method including the much vaunted Popperian ideal of falsification — and just how rigorously that ideal is applied. He describes some of the difficulty in explaining science. Lip service is often paid to experimental proof and evidence but Kosso appreciates that science can only work, at some point, by simply accepting some very counter-intuitive words of designated experts. For example, multidimensional universes or dark energy that even the experts must infer rather than being able to detect directly, and topically in the middle of the global coronavirus pandemic he notes the conceptual difficulty of being told — with no direct evidence from our eyes — of the existence of harmful microscopic entities that by the simple expedient of washing our hands we can remove and preserve our health. It is part of the strength of science that these pronouncements are accepted by the vast majority of the world's population. Likewise without evidence of our eyes the majority of the world's population are happy to accept that the Earth moves — and it is not a case of having to accept observations that would require special apparatus to see, such as required to see the Higgs boson or even coronavirus. The Earth is big, and moving very fast — a ball of rock 6400 km radius spinning at a dizzying 1700 km/h at the equator. That should be very hard to miss or ignore.

The story is told semi-chronologically, *i.e.*, mostly in historical order but with excursions back and forward in time as the argument demands, and without formal mathematics or any diagrams but instead using clear descriptive text. There are just two equations in the entire book — one, Newton's second law of motion relating the force on an object to its acceleration, and the other for the orbital velocity of an object around another body. Thus Kosso does not lean on mathematics to provide short cuts to understanding but instead provides careful precise descriptions of the question being asked and the details of the measurement being undertaken. The lack of equations is not a problem, but the lack of diagrams could be. The descriptions needed to explain the locations and centres of the various deferents, epicycles, equants, and eccentrics of early models of the Solar System, become quite involved and some careful reading is needed — with a sketch pad at hand. These detailed scientific accounts are leavened by brief biographies of the key scientists and their personal foibles; the vegetarian Pythagoreans, didn't eat beans; neither did Copernicus, Tycho, Kepler, or Galileo manage to get university degrees; the team sent to Peru by the French Royal Academy of Sciences to measure the ground length of 1° of latitude were gone for ten years — the team sent to Lapland took only 1 year; and so on down the chaotic history of science. All of the observations from your school days are here — dropping stones from towers or down mine shafts, Newton's spinning bucket of water and its equivalent of measuring the Earth's equatorial bulge, Foucault's pendulum, and straightforward observations from spacecraft either in low Earth orbit or remotely on perilous journeys to the outer planets. All are given deep consideration with constant reference to the original question — how do we unambiguously know that the Earth moves? And do these observations answer that question?

In summary this is a lovely, elegant book which reminds us that physics is not an exercise in mathematics but a self-consistent system of thought based on measurement and informed observation which depends on interpretation by the human mind in the context of the science of the day. It is a valuable reminder of the underlying human quality in physics that gets lost in the "shut up and calculate" methodology of the more esoteric branches of the science. So good is Kosso at getting to the heart of understanding with clear detailed descriptions that I very much hope that there will be more books from him. I thoroughly enjoyed reading *As the World Turns* and would particularly enjoy reading what he might have to say on other conceptually challenging subjects such as quantum entanglement — but at a perhaps more reasonable price than the £50 suggested for this book.

In discussing matters of fact and belief Tom Stoppard's fictional philosopher notes that "… knowledge is only a possibility in matters that can be demonstrated to be true or false, such that the Bristol train leaves from Paddington. .. and even then only on the understanding that all the observable phenomenon associated with the train leaving Paddington could equally be accounted for by Paddington leaving the train … ."

The last word must go to Galileo who in 1632 stated, "…whatever motion comes to be attributed to the Earth must necessarily remain imperceptible to us and as if non-existent, so long as we look only at terrestrial objects". The book ends with a lovely story about Galileo's finger which is set up and described so beautifully that it would be impossible and heavy handed of me to attempt to summarize it here. Give yourselves a post-pandemic treat and read the book. — BARRY KENT.

The Birth of Modern Astronomy, by Harm J. Habing (Springer), 2018. Pp. 565, 24 × 16 cm. Price £109·99/$159·99 (hardbound; ISBN 978 3 319 99081 1).

The title of this book may well be a misnomer. I believe most historians equate the beginning of modern astronomy with the invention of the telescope or perhaps the era of William Herschel, or maybe even the development of spectroscopy. This volume might more properly be entitled 'The Remarkable Growth of Astronomy after the Second World War', since it features wartime research in radar, computers, electronics, rocketry, nuclear physics, metallurgy, and other basic and applied sciences and advanced technology far beyond what would have occurred in a peacetime environment. Scientists and engineers engaged in urgent military research pushed back the boundaries of knowledge and made many new discoveries. Anxious to resume their former peacetime pursuits, they brought these newly honed skills back to their former studies. In what would become a booming post-war economy, all fields of human endeavour would benefit; the way food is cooked and preserved, medical procedures, and the construction of aircraft and automobiles to name but a few. Astronomy became one of the biggest beneficiaries. The ability to access new areas of the electromagnetic spectrum, to peer deeper into the cosmos with telescopes of much larger aperture, and to understand the inner workings of the stars because of Earth-based nuclear research led to whole new categories of astronomical research that could not have been imagined in the first part of the 20th Century.

Nuclear physics is the hand-maiden of astrophysics. Research performed shortly before, during, and just after the war involved the invention and use in laboratories of devices like the cyclotron, bevatron, early nuclear reactors, and other machines that demonstrated the validity of theories about the atomic processes occurring in the interiors of stars which gave them their heat lasting billions of year in some cases. Without this research we would know little more about the interior working of the stars than Ptolemy. Hans Bethe, a nuclear physicist, received the Nobel Prize in Physics (at this time there was no prize in Astronomy) for explaining these processes. In the last 70 years astronomy has achieved a growth that is perhaps greater than in all of its previous history. Habing has attempted to cover all of these developments in one large volume and, with a few glitches, has amazingly almost accomplished this. Unfortunately, because of the limited space allotted for this review and the large number of topics, I cannot comment on everything and so will limit myself to some highlights and discrepancies, leaving a more detailed analysis to the reader.

After the preface and acknowledgements there is a nine-page table of contents. The 17 chapters are divided into sections and before the first chapter is a 24-page prologue that states the author's aims. I encourage that this be read first: it will establish the framework of the author's aims. The many references are placed at the end of each appropriate chapter rather than at the end as in many books. An epilogue and index complete its organization. The book is remarkably well illustrated with many very sharp photographs and explanative charts and diagrams. This is one of its star qualities, making it a joy to thumb through.

Page xxiv of the prologue illustrates one of the author's prejudices: at the top is a photo of Wernher von Braun with most of his rocket team in New Mexico shortly after WW II. Below on the same page is a wartime photo of von Braun surrounded by Nazi officers with the caption "von Braun in 1941 with his friends of the time". I can well understand Habing's feelings because his country, Holland, was occupied during the war. But I do not believe he is giving von Braun a fair shake. Dennis Piskewiscz, in his book *Dark Moon*, maintains that von Braun should have been tried at Nuremburg. The truth is that von Braun never launched V2s at England. That was done by SS General Kammler who disappeared at the end of the war and was never brought to justice. Heinrich Himmler, the architect of the 'final solution', together with Kammler organized the V2 and other production at Dora. When on a visit to check on production, von Braun complained about the treatment of the labourers and the SS told him in no uncertain terms that arrangements could be made for him to join them. It is hard for us in a free post-war world to imagine what it is like to live in an evil dictatorship where everyone's existence is subject to a supreme tyrant. There are several instances where von Braun tried to intervene outside of the view of the SS for some inmates but to no avail. They are mentioned in Michael J. Neufeld's 2007 book *Von Braun: Dreamer of Space/Engineer of War*. Himmler wanted control of the V2 programme and eventually had von Braun and two associates arrested and jailed for 'treason' on the spurious charges that he talked of spaceflight at a social get-together. General Dornberger, Wehrmacht and not SS, von Braun's military commander, went to Albert Speer, minister of armaments and good friend of Hitler, and Speer got Hitler to write a letter ordering Himmler to release von Braun on parole. Eventually his two companions were also released. Undoubtedly if the war had gone on longer Himmler would have eliminated both von Braun and Dornberger, and if von Braun had been killed we might never have had a Jupiter-C or Saturn rocket. I believe America would still have reached the Moon but it would have taken a few years longer.

Away from controversies over the American space programme, there are a couple of other points I'd like to make. The first concerns Fritz Zwicky, whom Habing correctly credits with predicting dark matter and neutron stars, but which many other writers have failed to do. This is contained in one of the 'shaded boxes' which contain historical information that is for the most part correct and interesting. However, the true story about Walter Baade on p. 46, is that when the Japanese attacked Pearl Harbor and brought the USA into the war, Baade, who had never given up his German citizenship was classified as an enemy alien and confined to a radius of 5 miles around his home. Mt. Wilson and the 100-inch telescope was 17 miles away. Because of his gregarious lifestyle, Edwin Hubble, who had been a major in WW I had a lot of influence with the US Army and the FBI and persuaded them to allow Baade to include

Mt. Wilson in his sphere of activities. Being almost the only active astronomer at the Carnegie Observatories because of the war, Baade had almost complete use of the 100-inch. Taking advantage of the wartime blackouts of Los Angeles he had a beautiful dark sky. Baade took many plates of objects in our Galaxy and discovered Population I and II stars. In early 1943 the *Astrophysical Journal* called 'stop press' and inserted full-page, high-quality copies of those plates. It was a seminal moment in astronomical history.

In the prologue, on page xxxi, I believe Habing's use of the word 'morphology' is incorrect. The technique of morphology is explained and developed by Zwicky in his book *Discovery, Invention, Research — Through the Morphological Approach* (Macmillan, 1969). However, in the bulk of the book Habing reaches his stride; astrophysics rather than history is his forte. For the most part he does a good job of describing many modern developments in the subject. This part of the work makes it well worth purchasing. I did have some misgivings with some of his techniques. In his treatment of superluminal motion (p. 280) he points the reader to Wikipedia for an explanation; adding a couple of paragraphs to explain it would not have added appreciably to the length of the book, and would have made it more self-contained. That approach is repeated in several other places.

I would advise that an up-to-date first-year general astronomy text be kept at hand for cross reference. Even the initiate who has come to work in a highly specialized area of astronomy may not realize that in the twenty, thirty, or even forty years since he or she received their introduction to the field great changes have taken place. Even in the time it took to research, write, publish, and distribute this book there have been discoveries made that will be worthy of being included in the next edition if the author chooses to produce one. From my own reading I've found that we are truly in a golden age of astronomy. There are more astronomers at more observatories and other facilities doing more research than in all of the previous history of astronomy combined.

In spite of some shortcomings, this book does an amazing job of bringing this science up to date. In Chapter 17 on p. 535, Habing outlines future research methods, and in the epilogue (p. 555) proclaims that dark matter, dark energy, and inflation are the biggest mysteries of modern cosmology. We shall see what the future holds! — LEONARD MATULA.

The World According to Physics, by Jim Al-Khalili (Princeton University Press), 2020. Pp. 313, 18·5 × 12 cm. Price £12·99 (hardbound; ISBN 978 0 691 18230 8).

The author of this book is well known for his instructive and entertaining radio and television programmes concerning the history and development of physics and chemistry, and I have enjoyed reading about such matters ever since opening the pages of *Mr Tompkins in Wonderland* back in 1940, so I welcomed this opportunity to tune in to his current assessment of *The World According to Physics*.

The book is aimed at the interested layman, and in this context covers the ground thoroughly and clearly. The author avoids equations and other mathematical illustrations of the concepts which he describes, but is able nevertheless to present the mysteries inherent in the subject in a manner which is easy to follow. Special and General Relativity, quantum theory and the Standard Model of particle physics, the four fundamental forces, and the role of thermodynamics, together with the means of our progressive discoveries, the cosmic microwave background, the *Large Hadron Collider*, and indeed just about all of the major factors involved in modern physics and cosmology are set out and brought into perspective.

On reaching the end, however, the lay reader may be left with a vague feeling of dissatisfaction. This is not really the fault of the book, but of its subject, which is still very much a work in progress. In his commendable wish to present the situation, warts and all, the author starts the book by mentioning the serious problems faced by cosmology today — dark matter, dark energy, the preponderance of matter over antimatter, the problem of unification — and later, after he has outlined what we do know and have achieved, which as he says is remarkable in its range and practical value, he concludes by discussing current theories of multiverses, multi-dimensions, and other abstruse concepts. This is of course a fair reflection of the world of physics as it is today, but it does not paint the sort of picture that the average reader expects from an authoritative textbook, and may understandably leave some of them rather dismayed.

Considered as a primer, the book would benefit from chapter summaries because for a layman there is a lot to take in, some of which is distinctly strange, and regular recapitulation helps to avoid a build-up of bafflement; and a glossary is needed not only for the rapid identification of the multitude of particle names and other technical terms involved in quantum theory but also for picking up the nuances of the author's meaning and use in the context of his book of non-technical words such as time, space, field, and symmetry. Time, for instance, as the author points out, varies in its significance to Special Relativity, to quantum mechanics, and to thermodynamics, and space is far from emptiness.

But in calling this book a primer I do not do it justice. It is in its way a testament to the author's enthusiasm for the subject, his belief that its importance outshines the problems it has thrown up, and his confidence that a grand unified theory is not unattainable and well worth pursuing. He rightly makes it clear that such pursuit must depend upon observation and experiment and a willingness to amend even well-established theories in the light of new evidence, and I appreciated his account of utilising a mistake he made in his assembly of information for a television presentation to emphasize to his audience the importance of recognizing and acknowledging errors in moving science forward. I also enjoyed his recollection of being startled by the multitude of eager responses sent to him when he invited an audience to give him a common-sense explanation for the behaviour of particles in the classic two-slit experiment.

Two other points about the author and his book are perhaps worth making. The first is that, as indicated above, he is an avowed Realist, as was Einstein, and repudiates the Copenhagen quantum philosophy which he refers to as "Bohr's practical dogma". He describes Einstein's explanation of gravitational pull as being "a measure of the curvature of space–time", but the apparent simplicity of this explanation is compromised by his later discussion of quantization and mention of gravitons. While gravitons, like dark matter, have not yet actually been found, it is not unfair to say that the idea of gravity being a consequence of mass distorting space–time (matter tells space how to bend; space tells matter how to move) is less readily comprehensible to the average lay mind than the thought that gravitons pull matter together like gluons bind quarks. All quantum particles are well known to be strange, or to paraphrase Feynman not wholly understandable, and it is temptingly simple for a non-mathematical mind to attribute gravity to gravitons rather than the distortion of a curved four-dimensional concept called space–time. The author mentions gravitons in the context of his discussion of string theory *versus* loop quantum gravity as ideas for unification. He understandably doesn't take sides in this respect, but he could with advantage have told the reader a little more about gravitons, and made it clearer that loop quantum gravity extends quantization way beyond string theory, coming up as it does with discrete lumps of space and of time (I have seen the latter referred to as "chronons" in a recent book called *The Clock Mirage* by Joseph Mazur). And if time is not continuous, then, as Martin Bojowald has pointed out, the Big Bang singularity problem might not exist.

But such matters rest upon mathematics, which brings me to my second point about the book under review. The author makes it clear that mathematics provide an essential tool for physics, and emphasizes the success of the equations worked out for relativity and quantum mechanics, adding that he regards the latter as "the most powerful and important theory in all of science". But he also points out that the only true measure of the efficiency and power of mathematical theories is whether they describe phenomena in the real world against which we can test them, and to the intelligent layman this is an important point. What with multiverses and multi-dimensions, there does seem to be a risk that the elegance and harmony of mathematics to those who are gifted in the subject can become something of a will-o'-the-wisp leading them into a mire of irrelevance if not factual impossibility. A truly satisfactory solution to unification will need to be comprehensible without the depth of mathematics which will doubtless be needed to support and hopefully make use of it.

On a more mundane note, the book under review is compact and pleasingly presented, hardbound albeit without a wrapper and, unusually, with pages printed so as to have wider margins on the inside than the outside, which makes for easy reading. It includes six small diagrams which are rather disappointing when one remembers the author's vivid and instructive television scenes (and in no way match up to the entertaining and illuminating drawings which John Hookham provided all those years ago for *Mr Tompkins in Wonderland*) but one of which (no. 4 on page 188) sets out "the roads to Unification" and is well worth perusal. The author has included a useful list of 51 books for further reading, and this is a boon because although he has not short-changed the reader in its textual content, the subject is so interesting and the author's approach to it so unpedantic and enthusiastic that many readers may wish to extend their knowledge of it.

Indeed, a book of this sort cries out for supplementary information if the reader is fully to savour its content, and my enjoyment of it has been enhanced by having to hand John Gribbin's *Companion to the Cosmos* and *Q is for Quantum*. — Colin Cooke.

Einstein on Einstein: Autobiographical and Scientific Reflections, by Hanoch Gutfreund & Jürgen Renn (Princeton University Press), 2020. Pp. 197, 26 × 21 cm. Price £30/$35 (hardbound; ISBN 978 0 691 18360 3).

Albert Einstein is the extreme example, perhaps the only example, of a physicist whose every letter and laundry list deserves to be preserved, translated, and published. The primary process is coming from the Einstein Papers Project, now headquartered at the California Institute of Technology (though ownership resides with the Hebrew University in Jerusalem, which was Einstein's sole heir), under the directorship of the charming Dr. Diana Kormos Buchwald. Meanwhile, more or less in parallel, Hanoch Gutfreund, professor emeritus at HUJ, and Jürgen Renn, a director at the Max Planck Institute for the History of Science in Berlin, have been publishing facsimiles, translations, and commentaries of and about some of the key documents. They began with *The Road to Relativity: the History and Meaning of Einstein's 'The Foundation* (*Grundlage*) *of General Relativity*' in 2015 (see my review in these pages, **136**, 195, 2016), continued with *The Formative Years of Relativity: The History and Meaning of Einstein's Princeton Lectures* in 2017 (also reviewed in these pages, **138**, 73, 2018); and have now come to the present volume, containing two scientific autobiographies: (*i*) a fairly long one from about 1946, written in German and translated by Paul Arthur Schilpp (of whom more shortly), with fine tuning by Peter Bergmann; and (*ii*) a much shorter one, written very shortly before he died, again in German, and appearing here in English translation for the first time. The translator was Karen Margolis, who appears in neither acknowledgements nor the index.

We have, therefore, of about 183 pages (excluding references and index), about 31 pages of pure Einstein, and the rest mostly Gutfreund & Renn, with brief quotations and paraphrases of Einstein, and a good deal of Schilpp. Both autobiographies end with Einstein indicating that he thinks he has made significant progress toward his nearly lifelong goal of a Unified Field Theory, encompassing both gravitation and electricity and magnetism, with a structure, deterministic and continuous, like that of General Relativity, rather than the quantized, probabilistic structure of quantum mechanics as developed from about 1926 onward.

Before trying to explain what is going on here, I would suggest that the strongest reason for reading most of the material is to try to understand how Einstein thought he should lead his life, how he thought he did, and, perhaps, how you might want to lead yours, or think you should.

Now about Paul Schilpp. He was a philosopher who, in the 1930s, had the idea that much of the debate and uncertainty about his subject arose because we could not ask questions of the departed. Thus he set out to create a Library of Living Philosophers. Each volume would begin with a chapter by its title character, followed by comments, questions, disagreements, and so forth from a dozen or two other living philosophers, to which the lead chap would be invited to reply. The series began with John Dewey (1939), continued with Santayana, Whitehead, G. E. Moore, Bertrand Russell, Ernst Cassirer, and then Einstein was invited to lead off the 7th volume as a philosopher-scientist, with the subsequent chapters to come from both physicists and philosophers interested in his views. Later volumes edited by Schilpp featured Carnap, Buber, Popper, and Sartre (the last he edited), and others I had not previously heard of. An editorial board took over the project, which included (as of 2017) 35 volumes, the first woman, Marjorie Grene, appearing in 2002. Most of the titular philosophers wrote in English or German, and Henri Bergson, with whom Einstein had a public dispute shortly after World War I is not among them. The longer autobiography was Einstein's chapter for 'his' book, and ends, "This exposition has fulfilled its purpose if it shows the reader how the efforts of a life hang together and why they have led to expectations of a certain kind." Pages 25 to 104 are the authors' comments, with AE quotes interspersed, on Einstein's life, work, and the progress of science going on around him.

In section III we find out who was asked by Schilpp to contribute to the volume, who did, who didn't, paraphrases of what they said, followed by (again mostly paraphrases of) Einstein's unpublished responses to criticisms. Those who wrote (or provided an essay that had been written for some earlier purpose) were: Physicists: Sommerfeld, de Broglie, Pauli, Born, Heitler, Bohr, Philipp Frank, H. P. Robertson, Percy Bridgman, Milne, Lemaître, Infeld, von Laue, and Dingle (perhaps these should be called physicists and cosmologists); Mathematicians: Karl Henger, Kurt Gödel; Philosophers (or in the authors' views physicist-philosophers) I hadn't heard of: Ilse Rosenthal-Schneider (the one woman, though L. Susan Stebbing might have been interesting to hear from), Henry Margenau, Hans Reichenbach, Victor Lenzen, Filmer Northrop, Gaston Bachelard, Aloys Wenzl, Andrew Ushenko, and Virgil Hinshaw. A few familiar names appear in the list of those who agreed but did not write: Hermann Weyl, Paul Epstein (a friend of Fritz Zwicky), and Yakov Frenkel. At least as interesting are those who declined to contribute from the get-go: Russell and Whittaker among the philosophers; and Bethe, Dirac, Feynman, Schwinger, Schrödinger, Chandrasekhar, Zwicky, and Valentine Bargmann (then Einstein's assistant at Princeton, who might have felt his job would be at risk). Lev Landau never responded, and may well not have received the invitation. Einstein started out to write individual responses to the contributions, but soon found this inconvenient and repetitive (they exist, unpublished) and instead provided responses to groups of chapters he perceived as related. About eight of the writers go unmentioned. The groups us 'Observatoriers' might find meaningful are two: (*i*) Born, Pauli, Heitler, Bohr, and Margenau, and (*ii*) Milne, Infeld, and Lemaître. Again we get paraphrases and brief quotations. To the first group (described as his highly esteemed colleagues) he tries yet again to explain what he has against quantum mechanics in the form they have developed it, agreeing that the "statistical quantum theory" has brought great progress to physics, but maintaining that it has failed to fulfil the basic aim of all physics, "the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observations or substantiation)".

Dingle might have appeared with the cosmologists, but does not, and AE's unpublished response is to the effect that he did not understand either its essence or its aim. Since Dingle never accepted even Special Relativity, this is perhaps not surprising. The main response to the cosmologists was Einstein's sporadic attempt to keep a cosmological constant out of his equations, though Richard Tolman had long maintained that this could not be done (because it is in effect the second integration constant of a second-order differential equation), and the only question is its numerical value. Tolman had already advanced arguments against the value zero, and most of us now accept that, in suitable units, it is presently comparable with the other terms in the equation that begins $H_0^2 =$ a bunch of stuff.

There is much of Einstein the person here — his horrified opposition to American racism (expressed in a commencement speech at Lincoln University in Pennsylvania, a historical black college, where he spoke of it as "a disease of white people") and his support for unlimited post-WWII Jewish immigration to Palestine (accompanied by opposition to the new state of Israel, and by the expressed hope that the two communities could live peaceably together). The late, latter, short autobiography explains within that a major motivation is to express his gratitude to his early friend, fellow student, and collaborator Marcel Grossman. Grossman unfortunately had died nearly 20 years before. He is remembered in a series of biennial Marcel Grossman conferences, which (get out your abacus) every six years coincide with the triennial meetings of the International Society on General Relativity and Gravitation. They met together most recently in Valencia, Spain, in 2019 July. — VIRGINIA TRIMBLE.

S. Chandrasekhar. Selected Correspondence and Conversations, edited by Kameshwar C. Wali (World Scientific), 2020. Pp. 317, 24 × 16 cm. Price £75 (hardbound; ISBN 978 981 120 832 4).

Some human beings have been so important that every letter and laundry list they ever wrote is worth preserving and perhaps publishing. For physics, Albert Einstein and the Einstein Paper Project* (headquartered at the California Institute of Technology under the direction of Diana Buchwald) define the class. For Americans, probably George Washington and Abraham Lincoln; for the British perhaps Winston Churchill.

At the other extreme are us ordinary folks who struggle to get even an abbreviated CV into an archive. Subramanian Chandrasekhar† comes somewhere in between.

Author Kameshwar C. Wali of Syracuse University has already published a biography called simply *Chandra* (with University of Chicago Press), in 1991, when the subject was still alive and read the text. He then edited, for Chandra's 100th birthday, *A Scientific Autobiography: S. Chandrasekhar* with World Scientific. In addition, other books of essays on Chandra's legacy have appeared shortly after his death and in connection with the centenary. I've even been co-editor of one of these (with, of course, the other editor doing all the work). Can there possibly be anything left to say? Well, yes, with reservations.

The bad news first: I have never encountered a book so badly in need of, and completely lacking, an index and footnotes explaining who is who and what is what. Not to be defeated by this, I actually bought and read both of the earlier Wali-on-Chandra volumes and (accidentally) two copies of the Legacy volume edited by G. Srinivasan of the Raman Research Institute in what used to be Bangalore (now Bengaluru). They helped — moderately. The remainder of this commentary will alternate between tidbits that every Chandra fan might appreciate and a broader overview of what is here and a very partial sorting out of the difficulties in reading it. Two major surprises pop up. First (in letter exchanges with his father, concerning the possibility of Chandra returning to India to a university position), he says he doesn't really like the idea of teaching or advising students. This from the man whose lectures were legendary‡ and whose PhD students at the University of Chicago were uncountable, in the sense that published and webbed numbers fall a bit under fifty — and each list is missing a few. Second, in correspondence while Chandra was in Cambridge, UK, and his future wife Lalitha Doraiswamy was back in Madras (*etc.*), they were of the opinion that, though they shouldn't have children immediately, they would want to in the future. They never did.

*In the volumes I read cover-to-cover, representing 1914–1918, there are no actual laundry lists, but there are several items rather like grocery shopping lists and meal plans. This is also true of the Chandrasekhar letters in the present volume, because each was for a time coping with limited food supplies, Einstein during the major shortages in Berlin near the end of WWI and Chandrasekhar as a vegetarian in Cambridge and London in the early 1930s, where he found the situation not very different from what had faced Ramanujan and Gandhi earlier in the century.

†Someone needs to write a few paragraphs on South Indian personal names, but this is probably not the place. Chandra's father was C. Subrahmanyan Ayyar; his paternal grandfather Ramanathan Chandrasekhar; and his great grandfather Ramanathan (1837–1906). The family were Shaivite brahmans (sharing the surname Ayyar or Iyer) and native speakers of Tamil, though all the letters in this volume, apart from a few stray words, were originally written in English.

‡The legend that some of his students eventually gave lectures from notes they had taken in his courses was true (I was in a few of them). The legend of driving through snow for 100 miles to give a lecture for two students, both of whom won the Nobel Prize, has, like bread containing yeast, elements of truth. Roads were blocked, so he took a train. The actual class had at least seven enrolled students, while C. N. Yang and T. D. Lee, the only ones who came, were auditors.

What, besides an index and explanatory footnotes, is not here, and why did I ask for the review copy? In the same time frame, I have been writing about Lodewijk Woltjer (1930–2019) who spent a postdoctoral year with Chandra in 1958–59, Guido Münch (1921–2020), who was Chandra's student (PhD 1946) and was later a faculty colleague at Chicago, and E. Margaret Burbidge (1919–2020) who, with her husband Geoffrey R. Burbidge, was at Yerkes/Chicago/McDonald Observatory (then owned by U. Chicago) in 1957–62. Surely, thought I, even if there are no letters written by or to any of the three, they will be mentioned in some context, adding to my insights about them. Nope; *Nada*; *Niets*; (and if you think my English, Spanish, and Dutch leave a great deal to be desired, wait until you encounter my Tamil!)*. Indeed, even in the *Biography* and *Autobiography*, if you blink, you will miss the few mentions of those four.

As for what is here, the longest section consists of letters between Chandra and his father, written sporadically between 1930, when the 20-year old sailed (well, under steam) to England and on to Cambridge, and 1936, when he returned to marry Lalitha and then moved to Chicago.

Correspondence with father

Chandra generally writes to "Dear Babuji," (an affectionate term of respect;) and signs "Yours affectionately, Chandrasekhar" (sometimes "S. Chandrasekhar"). In return, he is addressed as "My Dear Ayya," (his father being known as C. S. Ayyar), with the signature "Yours affectionately, C. Subramanya." Things that will help to understand this section of the volume: (*i*) Chandra's mother Sitalakshmi (née Divan Bahadur Balakrishna) Ayyar died in 1931 May of what was perhaps long, painful stomach cancer. Chandra in letters to his father often extended "*namaskarams*" to her — affectionate respect, it sounds like. (*ii*) Near the end of his Cambridge stay, Chandra writes that he still wants to be a mathematician, not an astrophysicist†. (*iii*) Chandra was one of ten children, the oldest son (born when his mother was 18 or 19), with three younger brothers (Vishwam, Balakrishnan, and Ramanathan), two older sisters (Rajam and Bala), and four younger sisters (Savitri, Vidya, Sarada, and Sundari). These pop up in the letters, most often with father worrying either about their health (and therefore what they should eat) or, increasingly, about their marriages when "oldest brother" declines to return to India as soon as he receives his PhD (1933) and marry an appropriate younger woman. Lalitha, also born in 1910, was 11 days his senior, which they treated as a joke; the families perhaps less so. (*iv*) He speaks of a landlady as a middle-aged woman of about 35, when he was perhaps 21 or 22. (*v*) The father is of course on principle a vegetarian, but he urges one of his daughters to eat eggs when she is not well, and encourages Chandra, if necessary for his health, to consume some fowl

* Of the Tamil words untranslated by Wali, "babuji" suggests to American ears characters in the Charles Schulz cartoon *Peanuts*, because one of the girls insists on calling one of the boys "my sweet baboo", getting the answer, "I am not your sweet baboo". "Namaskarams" suggests "namasti", which some of us started using as a greeting when shaking of hands and even fist bumps disappeared into the murky Covid19 waters.

† We think we all know what an astrophysicist is (indeed Chandrasekhar helped to define the class). If you want to know what makes a mathematician, read, if you have not done so before, G. Harold Hardy's *A Mathematician's Apology* (CUP, 1940), with a 1967 preface by C. P. Snow. Within are many surprises. The one concerning Ramanujan, the first man from the Indian subcontinent to be elected to the Royal Society — Chandra of course was second — is the bit that belongs here, but you must read it for yourself.

and/or meat. Much later in life, Lalitha and Chandra preferred if possible to move away from where others were eating fish, fowl, or meat. (*vi*) C. V. Raman (Physics Nobel 1930; the C. = Chandrasekhar) was Chandra's uncle. The three famous Indian physicists of the period, Raman, Bose, and Saha, were very far from collegial. Ramanujan (1887–1920) was long gone; Chandra in due course helped to locate his widow, living in poverty, arranged for her to have a better pension, and rescued the passport photo that is really the only image we have of him. (*vii*) Chawla, who seems to be a slightly older friend from Tamil Nadu is Savadaman D. S. Chawla (1907–1995), a Cambridge PhD who was a student of J. E. Littlewood and had himself at least 25 PhD students, all but one at the University of Kansas and Pennsylvania State University, after moving from the University of Punjab to the US. (*viii*) A Mrs. Lonsdale, with whom Chandra stayed in London a couple of times was Dr., later Professor, Kathleen Lonsdale (1903–71), who was working with Bragg at the Royal Institution at the time. She was an X-ray crystallographer, the first to show that benzene rings are flat, and one of the first two women elected to the Royal Society (London) in 1945.

Correspondence with future wife

The next-longest section consists of letters between Chandra and Lalitha, beginning soon after he arrived in Cambridge in 1930 and continuing sporadically until his 1936 return to India. Their initial engagement and its termination occurred by mail (letters typically taking about three weeks in each direction — airmail existed, but was expensive). But when they met again in Madras in 1936, there was no doubt on either side that they loved each other and wished to marry. This happened quickly, working around some of the traditions of Hindu weddings. Chandra, incidentally, had to borrow from his father the price of her voyage from England to the US. The volume ends with transcripts of interviews between the author and Lalitha after Chandra's death. In these she gives a clearer picture than I have seen anywhere else of the difficulties of having dark skin in a small American town (Williams Bay, the site of Yerkes Observatory).

Music was always part of their lives. Chandra's father was a violinist and musicologist. Lalitha played the veena, an Indian string instrument which looks slightly less complex than a sitar, but probably is not. Their deep, lifelong affection for each other surely has some light to shed on Chandra's ability to work with women graduate students and colleagues throughout his career at Chicago, without ructions. Lalitha had earned degrees in physics before leaving India, had taught and been the headmistress of a women's school, and was working at the Raman Research Institute in Bangalore at the time Chandra returned to India. Some of their letters spoke of the possibility of working together in due course, and she would have liked to continue classes and on to a PhD. This was simply not possible in Williams Bay, where the only science, apart from Chandra's own work, was observational astronomy. She attended some of his classes when they first arrived, but never really settled into the communities either of students or of faculty wives. The frankest description of the unpleasantness she, and sometimes they, encountered is in her discussion with Wali at the end of this volume. Part of this, for instance at hotels, was plain, old-fashioned American racism. Some perhaps just small-town folks not knowing how to interact with what were probably the first people from India they had ever met. Chandra was the first Indian to lecture at the University of Chicago, which is presumably why his appointment came directly from President Hutchins.

Correspondents in need of introductions

That Chandrasekhar must have had a father and was married to Lalitha for something like 60 years are probably general knowledge among astronomers. Eddington comes at the end here (though not in the book), after several sections overflowing with named individuals, writers, addressees, and folks mentioned in passing, that contributed to my overflowing list of "who hes?" First K. S. Krishnan, who writes (1934–38) to "My Dear Chandrasekhar" and signs, "with best regards, Yours affectionately, K. S. Krishnan," and is addressed as "My dear Krishna Iyengar" from "Yours ever, S. Chandrasekhar"; Krishnan was the elder by 17 years, which did not impede a close personal friendship. He was the student of C.V. Raman at the time of discovery of Raman scattering, which led to the Nobel Prize. Some desultory web browsing suggests that he was, as it were, Jocelyn Bell to Raman's Antony Hewish, or Cesare Lattes to Raman's Cecil Powell (Physics Nobel 1950). Despite the friendship, on a later occasion Chandra recommended against the appointment of Krishnan to some university position on the grounds that he had specialized in some narrow branch of mathematics.

Leon Rosenfeld (1904–1974)* gets a short section because he was Chandra's closest friend from among the physicists met in a brief visit Chandra had made to Bohr's institute in Copenhagen, and he was hoping Rosenfeld might persuade Bohr, Pauli, or others of the quantum physicists to state openly that Chandra was right and Eddington wrong on the issue called "relativistic degeneracy", of which more shortly. The "scattered letters written by both Chandra and Rosenfeld from 1935–1948" promised in the Table of Contents are not there, and the last couple of pages of the section contain letters of 1935 and 1938 from Eddington to Chandra.

In the middle come "Miscellaneous Letters" written by Chandra to Robert Hutchins (Chancellor of the University of Chicago during the early part of Chandra's tenure there), Harlow Shapley, Henry Norris Russell, and Andres Weil (a mostly-French mathematician, and part of the group who published collectively as Bourbaki). Confusingly, the section actually begins with a few additional Eddington items. Of particular interest is the factoid that the questioner who provoked the response "who is the third?" concerning the number of people who understood relativity in 1919 was Ludwig Silberstein (a study in himself). And Eddington's report of his independent discovery of Nova Herculis, during a postprandial walk from dinner in Trinity College to his home at the Observatory, says "In returning from Trinity one evening, he thought Dirac was 'twisted'." This is apparently a concatenation of two or more originally meaningful sentences, and is the funniest of very many typographical errors in the volume.

The letters that actually belong in this section deal with (*i*) negotiations for Chandra to visit Shapley's Harvard in 1935, (*ii*) Shapley again later on Chandra's election to the American Astronomical Society, (*iii*) negotiations involving Henry Norris Russell and others for Chandra to become Russell's successor at Princeton in 1946–47 (he had actually accepted the offer, then changed his mind and stayed at Chicago after further discussions with Chancellor Hutchings),

*A Belgian-born theorist, who made contributions toward nuclear physics, statistical physics, applications of General Relativity, quantum field theory, and the foundations of what eventually became quantum electrodynamics.

(*iv*) discussions with Russell about the difficulties of having the International Astronomical Union meet in Leningrad in 1951, and (*v*) discussions with Weil (then in Brazil) in 1947 about the possibility of a mathematics professorship at Chicago for the latter. Weil declined the position he was eventually offered over issues of salary, teaching, and title. Meanwhile he mused about the right choice of a visiting professor for the Saõ Paulo department of mathematics to teach astronomical computing, and wondered whether Turing (of King's at Cambridge) would be the proper man for the task.

Now about Eddington and Chandrasekhar

The present volume is a bit confusing. The letters in the "Eddington" section come mostly from Chandra's years at Chicago/Yerkes and deal with convection in stars, the award of Eddington's knighthood, effects of the early years of WWII in both Cambridge and Chicago, a 1933 invitation from Eddington (writing as 'we' meaning that the invitation came also from his sister) to tea, with two letters touching on the topic of their disagreement, one from 1935, just preceding an RAS meeting where both were to speak, and one from 1939 during the lead-up to a Paris meeting where again both were to speak, the conference topics being novae and white dwarfs. In both, Eddington says that he intends to proclaim that relativistic degeneracy does not exist and, therefore, there is no upper limit to the masses of white dwarfs supported by degeneracy pressure. Bits in the other sections make the conflict clearer. I will attempt here to put things in something like chronological order, with the understanding, please, that any complicated story has to begin with the Big Bang or the Garden of Eden and contain an enormous number of tangled threads.

Robert Emden's (1862–1940, both Switzerland) 1907 volume *Gaskugeln* had put the structure of polytropes on a firm footing. Polytropes have pressure and density in the relationship $P = K\rho^{(n+1)/n}$ where K and n are constants. Eddington, by the time of his 1926 book (*The Internal Constitution of the Stars*) had shown that ordinary stars could be described by $n = 3$ polytropes. Yes, for real stars temperature, T, has to come in there somewhere, but Chandrasekhar's 1939 book, *Stellar Structure*, explains how one gets around this, at least for stars in convective equilibrium. Mathematical physicists named Lane, Ritter, and Kelvin (yes, that one) also come in here, but we will skip them. In the same time frame Ralph H. Fowler (1889–1944), who had returned from active duty in WWI to a lectureship in mathematics at Trinity College (and from 1932 the Plummer Chair in Theoretical Physics at the Cavendish Lab) showed that a gas whose pressure was due to completely degenerate electrons, with kinetic energy negligible compared to their rest-mass energies, could be described by $n = 1{\cdot}5$, that is pressure proportional to density to the 5/3 power.

This Chandra knew as he sailed from Bombay westward, but he folded in the idea that, as density ranged ever higher, the occupied momentum states would correspond to kinetic energies comparable with, and eventually exceeding, $E = mc^2$. This corresponds to pressure proportional to density to the power 4/3, like a gas of photons, and is highly unstable. This material was published in 1931 partly in *MNRAS* (**91**, 446) and partly in *ApJ* (**74**, 81). He had intended all along to use his fellowship from the government of Madras to work with Fowler. This he did, completing a thesis on polytropes distorted by rotation or a companion for a 1933 PhD. That is the context in which Eddington invited Chandra to tea at the Observatory and scheduled him to give a talk for the Observatory Club.

Chandra described himself as, during this period, caught between Eddington, Milne, Jeans, and Fowler and their conceptions of stellar structure. His first description of Dirac was "shy and fuzzy", but he later very much appreciated Dirac's intelligence and collegiality.

Chandra then returned to the white-dwarf problem, incorporating General as well as Special Relativity, which, by making gravity close to dense configurations stronger, lowered the possible maximum white-dwarf mass. This work appeared in 1935 (*MNRAS*, **95**, 20) and as Christian Møller & S. Chandrasekhar (*MNRAS*, **95**, 673, 1935). Discussions of the 4/3 power law and its implications for stellar structure were also published by E. C. Stoner, W. Anderson, and Y. Frenkel in the early 1930s, and Chandra explicitly cites them in his 1939 book. I will, therefore pass over recent suggestions that that maximum mass should be called the Stoner–Anderson limit or some such.

On 1935 June 12, leading up to an RAS meeting, Eddington wrote to "Dear Chandrasekhar" saying he was "anxious to see a defence of the relativistic degeneracy formula published so that (he) can focus his attack", going on to say that Møller ('Miller', unfortunately, in the present volume) and Chandrasekhar don't understand standing waves, or boundary conditions, or progressive waves. Part of the attack is obscured by another major typo, so that page 221 says "In 3 you say that you might just as well have used but p. 3 is not a function of the operation." In 1936, Eddington wrote to the American Consul General in London a letter explaining that "D. S. Chandrasekhar … has practiced the professor of astronomy for more than tow years." More typos, but the (presumably correct) letter enabled Chandra to get a visa to visit the United States (eventually of course remaining there and becoming a citizen after WWII).

The totally unexpected attack by Eddington on Chandra at that 1935 January RAS meeting surely played a very major role in Chandra's decision to leave Cambridge, although his Trinity Fellowship had a year or so to run. Chandra had been meeting regularly with Eddington during the previous four months during which the former had been working on the exact equation of state for degenerate matter and its implications for white-dwarf structures and masses, and apparently never found out why Eddington had raised no objections until he could do so in public. The meeting participants appear mostly to have supposed Eddington was right and Chandra had made some fundamental conceptual error that would require massive degenerate stars or cores to continue collapsing until gravity was so strong as to keep the radiation in. Eddington thought this ridiculous; you don't have to be told that we now call these black holes.

The aging Eddington

What on earth could have got into Eddington? That he changed with time is obvious and not surprising. Back when Cecilia Helena Payne first arrived at Newnham College, Cambridge, she found his lecture on General Relativity and the 1919 eclipse expedition so inspiring that it turned her professional goal in life to astronomy (for which we are unquestionably all the better), and she regarded him with respectful adoration the rest of his life (which ended in 1944 November, they having last met at that 1939 Paris conference). Twenty years down-stream in 1941 Eleanor Margaret Peachey heard him give a talk at an RAS meeting (probably on either convection in stars or Cepheid pulsation mechanisms) and found him (according to her oral history from 1978 with David H. DeVorkin) "a crabby little man ... who looked into the corner of the room and gave a boring talk". His treatment of Chandra and relativistic degeneracy comes in between.

Eddington's list of publications was 'many per year' for many years. It went, however, through a relative minimum in 1933–34. This was the time when his interests were moving from main-stream astronomy and astrophysics to his own view of the cosmos and the beginnings of his *Fundamental Theory* (published posthumously and never taken seriously by most of the scientific community, although Clive Gregory, who was Margaret Peachey Burbidge's thesis advisor, found it interesting).

Not mentioned in any of the items cited here, is that, in 1933 June, Charles John Agnew Trimble (1883–1958) who had been Eddington's best personal friend from their first days in Cambridge until his death, had suffered a nervous breakdown that had required him to retire from his faculty position at the Royal Mathematical School of Christ's Hospital. CJAT is not a traceable relative of mine (our Trimbles left County Antrim back in the 1700s), but the coincidence of the name naturally encouraged me to take an interest in him and write about him for a different publication. He was Eddington's companion on many cycling expeditions, camping trips, and so forth, and also never married. It is pure speculation on my part to wonder whether this great unhappiness of his dearest friend could have somehow affected how Eddington interacted with other people. Trimble turned to private teaching, outlived Eddington by more than a decade, and began a biography of his old friend at the request of Eddington's sister, Winifred. The biography was completed and published by Allie Vibert Douglas, Eddington's only female co-author (see also in these pages **139**, 114, 2019).

The echoes lingered long

In 1978, a book by I. S. Shklovsky called *Stars: Their Birth, Life, and Death*, appeared in English translation; *Sky and Telescope* sent me their review copy. The review was published some time in 1979 and included my surprise at two items. The first was Shklovsky devoting many paragraphs to proving that planetary nebulae were a phase at the end of the life of a star. This was well known on our side of the Iron Curtain. But Ambartsumyan, a man then of great influence in the Soviet astronomical community, had insisted that they were stars being born. I did not hear from Ambartsumyan! But, second, I quoted Shklovsky as saying that the Chandrasekhar mass limit for white dwarfs had really been discovered by Jacov Frenkel. That issue of *S&T* had barely hit the newsstands when there arrived at my office a sizable manilla envelope from Chicago. In it were actual original reprints of a couple of Chandra's papers and photocopies of a couple of others, annotated in red ink, to make clear that he was unambiguously the discoverer. The Nobel committee agreed a few years later, and Chandra was never unfriendly when we happened to meet up at later conferences, but he clearly still felt strongly about the issue 45 years after he first published the Chandrasekhar limit! Incidentally, the Frenkel paper appeared in German in *Zeitschrift für Physik*, though in one of the other Wali books, he puts it in a Soviet journal. Some weeks after the envelope arrived, I sat down with a friend who was a native speaker of German and a photocopy of the Frenkel paper. She read all the short words, and I read the long ones (this seems to be quite an efficient way to get something translated, if you have the right friends). Her first reaction was that German had not been Frenkel's first language, and we could both see that he had not in fact discovered the Chandrasekhar limit. Eighty-five years after the fact, one has to say that Eddington had not understood the Pauli exclusion principle and came down on the wrong side of the wave–particle dualism, and that Frenkel's equations imply a density to the 4/3 law equation of state, but that he did not fully explore its consequences, while Chandrasekhar did.

Acknowledgements

Special thanks go to Editor Stickland for descending deep into the virtual vault of old issues of *The Observatory* to figure out what 1941 talks by Eddington might have been heard by E. Margaret Peachey (later Burbidge). Friend and colleague Alak Ray of the Tata Institute kindly confirmed my impression that this was not the place to try to explain traditional Tamil personal and family names. — VIRGINIA TRIMBLE.

The Little Book of Cosmology, by Lyman Page (Princeton University Press), 2020. Pp. 138, 22·5 × 14·5 cm. Price £16·99 (hardbound; ISBN 978 0 691 19578 0).

This little book is successful because, despite the title, it doesn't attempt to cover all of cosmology. As the caption of an image of the cosmic microwave background (CMB) as seen by the *WMAP* satellite states, "The goal of this book is to explain this image and what it tells us about the universe." The author, a professor of physics at Princeton, has done significant work related to the CMB, so this is a book written by an expert, but for a very general readership. The level is typical for a popular-science book, and four short appendices provide a bit more detail which will be useful for some readers without cluttering the main text. Although the focus is on the CMB, the scope is broad, with chapters covering the basics of cosmology, the composition and evolution of the Universe, mapping the CMB, the standard model of cosmology, and brief sketches of current hot topics in cosmology; at the same time, the selected topics support the goal of understanding what the CMB can tell us about the Universe.

As the author states in the preface, "[o]ne of the challenges in presenting recent developments in science is to pitch ideas at the right level for the reader". While any level would probably appeal to some readers, a common problem is the lack of a constant level, often correlated with the distance of the topic from the author's area of expertise. Fortunately, that is not a problem here. The book is well written; in particular, I found the descriptions of dark matter and structure formation and the final chapter — both the brief survey of some current topics and the summary of the book — better than those in similar books. Page manages to present the standard material clearly and at the same time mention details usually left out in books at this level, all without losing track of the main narrative.

There are few actual typos and I have fewer complaints with regard to matters of style and so on than with most books I've reviewed in these pages. There are no serious mistakes and only a handful of things which I think could have been explained a bit better. My only serious complaint is that the index, at less than two pages, is too short, even for such a little book. I believe that this is the first book I've read with the chapter numbers and names at the bottom of the page (though page numbers are at the top). I was happy that there are footnotes rather than endnotes. In keeping with the minimalist theme, there are no references. There are a handful of black-and-white figures throughout the book as well as something rarely seen these days: eight colour 'plates' on slick paper about two-thirds of the way through the book. The CMB has provided some iconic scientific images: the essentially perfect black-body spectrum with error bars smaller than the width of the curve, the power spectrum of fluctuations, and the map of the same; they are all here.

The book is one of at least three in the series of 'little books in physics' (the other two concerned with black holes and string theory), which in turn is part of the *Science Essentials* series, which covers not just physics, but I have not (yet) read any of the others.

Apart from the 'interested layman' readership, the book should also appeal to scientists from other fields who might have heard about the CMB and/or cosmology but are satisfied neither with accounts which are so broad that they are lacking in depth nor with plodding through a more advanced text just to get the basic idea. I recommend this book to both groups. —PHILLIP HELBIG.

The Dark Energy Survey: The Story of a Cosmological Experiment, edited by Ofer Lahav, Lucy Calder, Julian Mayers & Josh Frieman (World Scientific), 2020. Pp. 350, 15 × 23 cm. Price £85/$98 (hardbound; ISBN 978 1 78634 835 7).

This book is something of a mixed bag, but in a positive sense. In addition to Chapter 1, the introduction, the twenty-seven following chapters cover, in four parts, the technical aspects of building the Dark Energy Survey (DES), dark-energy science, non-dark-energy science, and more-general topics discussed by two anthropologists, a philosopher (Michela Massimi on the use of the Bayes factor in cosmology; she is also mentioned in a very different context in my article in this issue), two visual artists, and a poet, as well as a general assessment of DES and the future of dark energy by David Weinberg. Those topics in the first part, and occasionally those in the fourth, can be heard about at conferences, but the technical aspects of such projects are usually published separately from the science, so it's nice to have it all in one place. Most of the authors and editors are closely involved with DES (usually used without an article in the book), so this is mostly the 'inside story', although the external views in the last part are an interesting addition, especially Chapter 23, 'An Anthropological Angle: Credit and Uncertainty in the Dark Energy Survey', which, among other things, addresses the sometimes precarious working conditions of those without permanent jobs who contribute significantly to the project. (Of course, that is a general problem, not just with DES.)

At 418 pages (including the front matter, part of which is a two-and-one-half page foreword by the Astronomer Royal, Martin J. Rees), coverage is reasonably thorough. The first part, 'Building the Dark Energy Survey', discusses not only the technical aspects of the construction, but also the political, sociological, and personal events which played a role in the conception and realization of DES. The construction was driven by the four emphases of DES, namely the magnitude–redshift relation for type-Ia supernovae, large-scale galaxy clustering including baryon acoustic oscillations (BAO), galaxy clusters, and weak gravitational lensing. DES produces images in five bands as well as photometric redshifts using a purpose-built instrument on the 4-m *Blanco* Telescope at the Cerro Tololo Inter-American Observatory in the Chilean Andes; depending on a complicated algorithm, fields (chosen to overlap with other surveys such as the Sloan Digital Sky Survey and microwave observations made by the *South Pole Telescope*) are observed several times. In some cases, such as type-Ia supernovae, redshifts of the objects and/or their host galaxies are obtained from other instruments. As Paul Schechter said, surveys are the lifeblood of astronomy, and DES has also provided data which can be used for other goals, such as galaxy evolution, quasars, strong gravitational lensing, studies of relatively nearby objects (the Local Group, the Milky Way as a whole and stars within it, and the Solar System), and optical follow-up of gravitational-wave events.

"Listed authors of chapters are usually those who served as working group project coordinators, as recognition for their contribution to DES over the years (not necessarily for writing the chapter). Parts of some chapters are based on interviews with them, or on their published work." Although it is thus not clear who wrote what, kudos goes to the editors for producing a work which, despite the multitude of sources, is very uniform in style, level, and quality. The only actual mistake I noticed is the often repeated claim that "[t]he idea of dark matter was first posited by Fritz Zwicky in 1933".* One passage referred to the time of writing that chapter, several years ago, and could have been updated to the time of production of the book. Otherwise, there are only minor technical issues: some figures are too small to be legible, some are scans with visible pixels though the originals perhaps exist in higher quality, and many captions mention colour in black-and-white figures (though since this is a paperback galley proof, hopefully the final version will include colour figures; note that the standard information above refers to the hardbound version). My pet peeve of missing hyphens in two-word adjectives is present in abundance, though relieved by some unintentional humour such as "naked eye observers" (either that, or DES was even more fun than I thought).

Some results are presented in Parts II and III, but they are necessarily preliminary, as not all the data have been analysed yet. More valuable in the long run are thus the introductions to the various scientific topics, both in general and in the context of DES. So far, the standard 'concordance' model is confirmed: $\Omega_{\mathrm{matter}} = 0{\cdot}331 \pm 0{\cdot}038$ (assuming a flat universe; relaxing that assumption doesn't change things much, *i.e.*, $\lambda + \Omega_{\mathrm{matter}} = 1$ within the uncertainties); $w = -0{\cdot}978 \pm 0{\cdot}059$ (the ratio of pressure to density in dark energy), *i.e.*, within the uncertainties the value for a 'pure' cosmological constant (there is also no evidence that w changes with cosmic time).

Some might have wished for sensations, but for me, 'no news is good news'. More important than confirming the concordance model is perhaps the greater precision. Interestingly, the chapters on non-dark-energy science contain things not necessarily expected at the beginning of DES, such as optical confirmation/follow-up of gravitational-wave events and the discovery of dwarf planets in the outer Solar System, including indirect evidence for 10-Earth-mass Planet Nine. There is much to look forward to: 300 million photometric redshifts, 200 million galaxies with measured shapes, 380 thousand galaxy clusters, thousands of supernovae, a couple of dozen newly discovered satellites of the Milky Way, a hundred strongly lensed QSOs, 100 million stars, 300 new trans-Neptunian objects, a score or so new Jupiter Trojans, 200 thousand main-belt asteroids, several hundred Kuiper-Belt objects.

There are a few figures throughout the book. The only matter after the final chapter is a description of the US Department of Energy approval process and several DES group photos. (An index would be nice; perhaps the final version will contain one.) Some of the chapters contain reference lists.

This is a nicely produced book which should appeal to a wide readership; it would be nice to have similar books about the many other observational projects currently underway or planned. — PHILLIP HELBIG.

*For some discussion of this point, see my article in this issue.

Alien Oceans: The Search for Life in the Depths of Space, by Kevin Peter Hand (Princeton University Press), 2020. Pp. 248, 24·5 × 16·5 cm. Price £22/$27·95 (hardbound; ISBN 978 0 691 17951 3).

When viewed through the eyepiece of a decent-sized telescope, the moons of the outer Solar System offer tantalizing clues as to their possible nature. Europa sports a bluish-white tint, suggesting an icy surface, but the more diminutive disc of Titan has a distinct orange colour, which we now know indicates a photochemical smog.

Writing this review at the height of the Covid-19 global pandemic, alien oceans sound like a faraway fantasy, while even the idea of travelling to the British seaside was rendered problematic. So Kevin Hand's book arrived at an opportune moment, and provided a nice antidote to current worries. *Alien Oceans* is written on solid foundations, is certainly engaging, and has proven to be an informative introduction to those subsurface oceans that must surely exist upon Europa, Enceladus, and Triton, and perhaps other moons.

Hand served as deputy-chief scientist for Solar System exploration at NASA's Jet Propulsion Laboratory, and is therefore well-placed to describe future missions in search of life. *Alien Oceans* nicely chronicles the sequence of events by which we have garnered evidence for these distant seas. The author likes analogy, and although his descriptions of spectroscopy will be too simple for readers of this *Magazine*, they are clear and fulfil the needs of the intended popular-science market. He explains the traditional concept of a Goldilocks world, where the Three Bears might find their porridge is neither too hot, nor too cold. But life might exist outside the Goldilocks zone, given the right environment. Here, however, writing as a chemist, I found his Ferris-wheel analogy for explaining the permanent dipole of water *via* the theory of covalently bonded electrons to be unsatisfactory. The existence of this strong dipole is the really key point for the possibility of subsurface oceans, because the lower density of ice than water — resulting from a rigidly geometrical (and space-wasting) arrangement of hydrogen bonds in ice — is essential to the survival of aquatic life. If ponds froze from the bottom upwards on Earth, many of their inhabitants would hardly survive our winters. If alien species exist, they will be safely insulated by a thick icy crust from the constant winter overhead.

The author has first-hand experience of extreme environments on Earth through trips to dry valleys in the Antarctic and *via* submersible visits to the famous 'black smokers' of the oceanic depths, and is enthusiastic in presenting the case for life beneath the moons of the outer Solar System. Given the right chemistry and enough tidal heating, life could have developed beneath the fractured and moving crust of Europa. Arthur C. Clarke may have been prophetic to have singled out Europa in his sequel to *2001*. Missions to go and actually look are planned, despite the enormous technical difficulty in drilling through layers of ice hardened by extremely low temperature. And how about life in or around the carbon-rich oceans and lakes upon the surface of Titan?

Kevin Hand conducts us on an exciting tour, and it's one that I can warmly recommend. — RICHARD MCKIM.

2021 Guide to the Night Sky, by Storm Dunlop & Wil Tirion (Collins), 2020. Pp. 112, 21 × 15 cm. Price £6·99 (paperback; ISBN 978 0 00 838904 8).

This inexpensive and very handy reference to the night sky in 2021 follows numerous successful predecessors reviewed in these pages — clearly the format is well tried and tested! The Introduction sets the scene with a host of useful definitions, while the following chapter on The Constellations introduces the sky maps for which Wil Tirion is justifiably famed. Then comes a chapter on the phenomena associated with the Solar System — phases of the Moon, eclipses (although the NASA website continues to be preferred to the excellent one provided by the UK's Nautical Almanac Office), the location of the visible planets, and notes on any comets likely to grace our night skies.

Then we get to the excellent month-by-month sky guides depicting the view to the north (from a UK/Ireland perspective), the view to the south, and the positions of the Moon and planets, all in three-double-page spreads. Finally there is a list of dark sites in the British Isles, a glossary, and sundry other useful information. Every home containing the smallest flicker of interest in the night sky should have a copy. — DAVID STICKLAND.

Annual Review of Earth and Planetary Sciences, Volume 47, 2019, edited by R. Jeanloz & K. H. Freeman (Annual Reviews), 2019. Pp. 606, 24 × 19·5 cm. Price from $440 (print and on-line for institutions; about £363), $116 (print and on-line for individuals; about £96) (hardbound; ISBN 978 0 8243 2047 8).

In these days of lockdown, a fat volume of review papers is most welcome. The 2019 volume of *AREPS* reviews diverse topics that range from Earth's deep mantle to exoplanet atmospheres. Perhaps the least represented subject is traditional geology.

The subject of exoplanets is perhaps the best represented in the 2019 volume. Several papers cover aspects including their great diversity — they range from rocky bodies to deep gaseous ones — the dynamics of their atmospheres, and the effects of their clouds. As regards planets in our own Solar System, an interesting paper interprets what is known about sedimentary processes on Mars, concluding that the planet was both habitable and able to preserve evidence for the same, raising the stakes on the revolutionary question of whether it ever actually did support life. We are updated on research on Pluto in a paper on its atmosphere (yes, it has one!) which is primarily of nitrogen with methane and carbon monoxide as minor constituents.

Considerable space is also allocated to terrestrial water. One chapter discusses how water might be cycled through the deep Earth in regions below transition-zone depths, *i.e.*, at depths greater than 660 km. If so, it must strongly influence the dynamics of this poorly understood region. Ocean chemistry through the Phanerozoic is usefully summarized in its own chapter, as is the use of isotopes in studying the water cycle. Moving to matters related to climate change, details of the origin and behaviour of glacial meltwater in supraglacial streams and rivers is reviewed, along with the response of marshes to climate change and sea-level rise. Overlapping the theme of terrestrial water is a paper on some novel applications of GPS, which include processes related to environmental change including the movement and loss of groundwater, in addition to geological hazards.

A sprinkling of papers on traditional geological topics are still to be found. Palaeontology is represented by a review of reptiles and mammals, in particular from Madagascar, whose evolving distribution and morphologies inform Gondwana breakup. A useful contribution provides a good summary of that oft-mentioned but rarely explained phenomenon: the destruction of the North

China Craton. An update of the perennial issue of the link between flood basalts and mass extinctions completes the traditional geological content.

This year's *Annual Review* collects papers on some unusual and compelling subjects, and those on exoplanets are particularly welcome. But could we have a bit more cutting edge geology in future volumes? — Gillian Foulger.

From the Library

The History of the Telescope, by Henry C. King (Dover, reprint), 1979. Pp. 456, 23 × 16 cm. Price $29·95 (about £23) (paperback; ISBN 0 486 43265 3).

Before I proceed to a review of this book I would like to present a brief survey of the author's life. King was born in 1915 and lived an active life until he passed away in 2005. In the 1930s he met Constance Lubbock, the granddaughter of William Herschel and was granted access to Herschel's books and papers. During the Second World War he served as the Inspector of Instruments for the Ministry of Aircraft Production at Ruislip near London. After the war King became the first Scientific Director of the London Planetarium, from 1956 to 1966, and then the director of the McLaughlin Planetarium in Toronto for a short time. He also served as President of the BAA from 1958 to 1960. He thus had unprecedented access to important astronomical documents and acquired an extensive knowledge of both mechanical and optical technology. He used these abilities to write several landmark books. Though written long before the advent of *Gemini*, *Keck*, *Hubble*, and the other giant instruments now under construction, *The History of the Telescope* is celebrated by most experts as the best treatment of the subject up to the date of its appearance in 1955. King lived for 50 years after its publication but never revised or updated it. In 1979 Dover published a paperback complete with the almost 200 illustrations.

The other important books by King are *The Background of Astronomical Exploration of the Universe from the Astrolabe to the Radio Telescope* (in co-operation with John Millburn) in 1954; *Wheelwright of the Heavens: the Life and Work of James Ferguson FRS* in 1988; and *Geared to the Stars*, the history of planetaria, orreries, and astronomical clocks, also with Millburn in 1978, another landmark book considered to be the definitive work on these subjects. To the best of my knowledge that book was never reprinted and commands an unbelievably high price on the secondary market; it was written before computerized and digital planetaria arrived on the scene. It also was never updated or revised but remains the finest work on the history of those devices until its publication. Younger readers may find it hard to believe that King's research on these books was performed before the age of the internet, when every source had to be physically obtained and studied in hardcopy, yet nothing written subsequently has equalled his thoroughness.

Returning to the present volume under review, it is organized into a foreword, preface, 19 chapters, and an index. Because of the immense amount of detail in the book, I will keep this review brief and urge the reader to acquire the easily obtainable Dover edition.

The author gives a thorough historical background describing in detail pre-telescopic astronomical instruments such as the quadrant, triquetrum (known as Ptolemy's Ruler), astrolabes, and armillary spheres. None of these devices provided any magnification or detail; they merely made it possible to determine more accurately positions of the various celestial bodies. Chapters II and III go on to present a more detailed account of the invention and development of the telescope than any other book I've read. Although primitive telescopes were constructed before Galileo's, he is given credit for developing it into a useful astronomical instrument and using it to conduct the first optical observing programmes and keep some of the first systematic records.

On p. 48 it is mentioned that no great improvements in telescope design were made until René Descartes published his *La Dioptrique* in 1637. On p. 31 the first reference I have ever seen to the invention that would later be called 'binoculars' was made by Prince Maurice of the Netherlands States-General to Hans Lippershey seeking to obtain his invention for the Dutch military; I quote, "enquiring of him whether it would not be possible to improve upon it, so as to enable one to look through it with both eyes", which means, I suggest, that two be put together. The chapters then proceed forward through the long French telescopes, the achromatic lens, the Cassegrain and its cousins like the Gregorian, Newton's reflector, the Herschel and Rosse telescopes, and the 'usual suspects', but, unlike most other comparable histories, King goes into detail about many rarely-heard-of, obscure individuals who contributed to the wonderful rich tapestry that is the history of the telescope. The invention and perfection of the various astronomical instruments that vastly increased the ability of the telescope to broaden our knowledge of the Universe are also outlined with the competence only a person with King's expertise could present.

The book's time period ends with the 120-inch Lick reflector, built after the 200-inch on Palomar Mountain, to become the second largest in the world, and the early history of radio astronomy which was just entering its era of early growth and development. Amateur telescope making, which was coming into an era of great expansion, is not ignored either.

One minor flaw I discovered is that while mentioning the 18-inch Palomar Schmidt the vagueness of who was most responsible for it, Fritz Zwicky or Walter Baade, is not addressed. I shall say more about that in my forthcoming review of the most recent biography of Zwicky! Although both Baade and Zwicky are mentioned there is nothing about Zwicky's famous supernova search that began with a small lens on the roof of the Robinson Laboratory of Astrophysics on the Caltech campus in Pasadena, and went on to be continued by the 18-inch Schmidt and later the 48-inch Schmidt at Palomar.

This is not just an old book — it is a great old book and I found it fascinating. — Leonard Matula.

CORRIGENDUM

On page 238, in the *Correspondence* section of the 2019 December issue, it states that "The entire series of 12 Gemini flights used the Douglas Thor, another home-grown US rocket." However, it should have read "the Martin Titan", also a home-grown rocket. This was a gross error on my part and I should have caught it; after all, I grew up watching that programme and collecting much information on the Titan. — Leonard Matula.

ASTRONOMICAL CENTENARIES FOR 2021

Compiled by Kenelm England

The following is a list of astronomical events, whose centenaries fall in 2021. For events before 1600 the main source has been Barry Hetherington's *A Chronicle of Pre-Telescopic Astronomy* (Wiley, 1996). For the 17th to 19th Centuries lists of astronomical events came from Wikipedia and other on-line sources, supplemented by astronomical texts. For 1821 early issues of *Astronomische Nachrichten* were used. Discoveries of comets, asteroids, novae and other objects for 1921 appeared in the February issue of *Monthly Notices of the Royal Astronomical Society* in the following year. There were also references from *Popular Astronomy*, *Astronomical Journal*, *Journal of the British Astronomical Association*, and *Publications of the Astronomical Society of the Pacific*. Professional discoveries and observations were followed up in *Astronomische Nachrichten*. Details of individual astronomers were supplemented by articles published in *Biographical Encyclopedia of Astronomers* (Springer, 2007). Gary Kronk's *Cometography* Volumes 1–3 (Cambridge, 1999–2007) provided details on all the comets. Finally NASA's *Five Millennium Canons of Eclipses* and planetary tables were consulted for information on eclipses and planetary events.

1921

January 14: Cuno Hoffmeister (Sonneberg, Germany) observed several meteors belonging to the rather weak ρ Geminid meteor shower.

January 17: Birth of Epaminondas George Aristotle Alexander Stassinopoulos. He is a Greek astrophysicist, who worked on the Earth's radiation environment for NASA (1961–2006) and continued publishing until 2016.

January 17: Vesto Melvin Slipher (Lowell Observatory) reported very high red-shifts in the spectra of the spiral galaxies NGC 584 and NGC 936.

January 18: Death of Wilhelm Julius Foerster. Born in 1832, he was Professor of Astronomy at Berlin University in 1863 and Director of the University Observatory (1865–1903). He co-discovered asteroid (62) Erato with Otto Leberecht Lesser (1830–87) in 1860. He worked on calculating ephemerides for the rapidly growing number of asteroids.

January 20: Death of Mary Watson Whitney. Born in 1847, she was an American astronomer, Director of the Vassar College Observatory, Poughkeepsie (1888–95), where she observed double and variable stars, comets, and asteroids. She promoted women in science, especially astronomy; retired 1915.

February 3: Birth of Ralph Asher Alpher. He was an American cosmologist, who in 1948 wrote a doctoral dissertation on the nucleosynthesis of elements in the Big Bang. Working with Robert Herman (1914–97), he predicted the Cosmic Microwave Background radiation, discovered in 1964, but was overlooked for the 1978 Nobel Prize for Physics. In 1955 he designed re-entry vehicles. Professor of Physics and Astronomy at the Union College, Schenectady, New York (1987–2004); died 2007.

February 20: Birth of Joseph Albert Walker. He was an American test pilot, who flew the X-1, Douglas Skyrocket, and X-15 research aircraft. He reached 100 km twice in 1963 and also flew NASA's *Lunar Landing Research Vehicle* (later crashed by Neil Armstrong); killed in an aircrash 1966.

March 1: The Soviet government established a laboratory for rocket research in Moscow.

March 11: William Reid (Cape Town, South Africa) discovered a small nebulous 9th magnitude comet. It slowly brightened in April and was widely observed by professional astronomers. At the end of April it could be seen with the unaided eye and in photographs displayed a tail 3-degrees long. The comet reached perihelion on May 10. ($q = 1{\cdot}008$ AU) and then began to fade, before being lost in the Sun's glare on July 4. It was photographed again in October and November [Comet C/1921 E1 (Reid)].

March 11: Death of Sherburne Wesley Burnham. Born in the United States in 1838, he was by profession a court reporter, but astronomy was his passion. He discovered nearly 1300 new double stars and published his double-star catalogues in 1900 and 1906, revitalising that field of astronomy in the 20th Century.

April 8: An annular solar eclipse passed across the Arctic, Scandinavia, and Scotland. The partial phase was visible from a large part of North Asia, Europe, and North Africa [Saros 118].

April 9: Birth of Mary Winston Jackson. She was an American mathematician and aerospace engineer at NACA (1951–8) and NASA (1958–85); died 2005. Her character appears in the 2016 film *Hidden Figures*.

April 11: Edward Emerson Barnard (Yerkes Observatory) recovered comet 7P/Pons-Winnecke, despite a close encounter with Jupiter. It brightened rapidly during May and was closest to the Earth on June 12 (0·141 AU), when the coma was 10 arcminutes across. It reached perihelion on June 13 ($q = 1{\cdot}041$ AU) and was at it brightest (mag. 6·5), but only visible from the Southern Hemisphere. The comet rapidly faded and was last seen on September 5 [Comet 7P/Pons-Winnecke].

April 15: Birth of Georgi Timofeyevich Beregovoi. He was a Soviet test pilot, who joined the Cosmonaut Team in 1964. Backup commander for the cancelled *Voskhod 3* mission, he was commander of *Soyuz 3*, making 81 orbits on 1968 October 26–28. He rendezvoused but failed to dock with the unmanned *Soyuz 2* spacecraft. Director of the Cosmonaut Training Centre; died 1995.

April 22: A total lunar eclipse was visible from the Americas and Australia [Saros 130].

April 24: Alexander Dmitrievich Dubiago (Kazan Observatory) discovered a comet (mag. 10·5) despite moonlight. It remained a faint object, reaching perihelion on May 5 ($q = 1{\cdot}115$ AU) and was last seen on July 7. The comet's orbit was distinctly elliptical with a period of about 60 to 80 years, but the comet remains lost [Comet D/1921 H1 (Dubiago)].

April 29: Birth of Cornelis de Jager. He is a Dutch solar physicist, editor of *Solar Physics*, Director of Utrecht Observatory (1960–86) and General Secretary of the IAU (1967–73). He was a leading investigator on NASA's *Solar Maximum Mission*.

April 30: Birth of Roger Lee Easton. He was an American scientist at the U.S. Naval Research Laboratory (1943–80). He worked on Project Vanguard in 1955, Naval Surveillance System in 1959, and designed navigational satellite systems Timation, NTS, and NAVSTAR (now GPS); died 2014.

May 3: Death of William Robert Brooks. Born in 1844, he was an American astronomer who concentrated on the search for and observation of comets. Between 1881 and 1911 he visually discovered 31 comets, 21 of which bear his name. The short-period comets are D/1886 K1 (Brooks 1) (lost since it discovery in 1886), 16P/Brooks 2 (nearly disrupted by a very close encounter with Jupiter in 1886), and 12P/Pons-Brooks (due to return in 2024; recovered 2020 June). His last comet was the brightest, the naked-eye comet C/1911 01 (Brooks).

May 14: A very large sunspot was observed from Britain and the United States.

May 14–15: A major geomagnetic storm disrupted telegraph services. Bright aurorae were widely seen.

May 23: Birth of James Benjamin Blish. He was an American science-fiction writer of short stories, novels, and *Star Trek* books; coined the term 'gas giant' in 1952; died 1975.

May 31: Death of Sir John Herschel. Born in 1837, the son of Sir John Herschel and grandson of Sir William Herschel, he served in the British Army and observed the total solar eclipse of 1868 August 18 in India; elected FRAS.

June 4: Death of Jean Baptiste Aimable Gaillot. Born in 1834, he was a French professional astronomer at the Paris Observatory under LeVerrier. He compiled positional observations of stars and improved orbital calculations of the planets for *Connaissance des Temps*.

June 28–July 5: A number of meteors belonging to the June Boötid meteor shower were observed from Japan, India, Europe, and the United States; linked to the comet 7P/Pons-Winnecke, which made a close approach to the Earth in mid-June.

July 18: Birth of John Herschel Glenn. He was an American test pilot, selected as one of NASA's *Mercury Seven* astronauts in 1959. He was selected to fly the cancelled *Mercury-Redstone 5* mission but flew on board *Mercury-Atlas 6* on 1960 February 20. His capsule *Friendship 7* completed three orbits before returning safely. He resigned from NASA in 1964 to enter politics and was Senator for Ohio (1974–99). His second spaceflight was on 1992 October 25 on board Shuttle STS-95 *Discovery*, which remained in orbit for 8 days and 22 hours; died 2016.

July 22: Birth of Ronald Newbold Bracewell. He was an Australian engineer at the Radiophysics Laboratory, Sydney, working on radar and radio astronomy. Elected FRAS in 1950, he studied the ionosphere, the orbits of early artificial satellites, and discovered polarization in radio waves from the active galaxy Centaurus A; died 2007.

July 27: Despite attempts as early as 1920 November, comet 2P/Encke was finally recovered by James Francis Skjellerup and William Reid (Cape Town, South Africa) low over the horizon (mag. 9·5). It remained a difficult object and was only visible from the Southern Hemisphere until August 13 [Comet 2P/Encke].

August 6: H. C. Emmert (Detroit, Michigan) reported an object 'fully as bright as Venus in twilight' in the western sky just after sunset.

August 7: The British amateur astronomers Nelson Day (Ferndown, Dorset) and S. Fellows (Wolverhampton) reported a bright object seen around sunset.

August 7: William Wallace Campbell (Lick Observatory, California) reported that 'a star-like object certainly brighter that Venus' was seen by himself and five others 7 minutes before sunset. These reports could be of a single comet but this would place it close to the Earth. More likely, a cluster of comets, part of the Kreutz sungrazer group.

August 9: Maximillian Franz Josef Cornelius Wolf and Cuno Hoffmeister (Sonneberg Observatory, Germany) reported "a number of luminous bands lay across a clear sky", moving NNE and fading at dawn.

August 11: Astronomers at Lick Observatory began detecting nebulosity around Nova Cygni 1920 [V476 Cygni].

August 15: Comet 25D/Neujmin2 returned to perihelion (q = 1·333 AU). Predictions at the time showed that it was very poorly placed. An object found by Grigori Nikolayevich Neujmin (Simeis Observatory, Crimea) was thought to be a possible observation, but this was disproved, when the comet was recovered in 1926; lost ever since.

August 21: Birth of Victor G. Szebehely in Hungary. He worked on space research for various American organizations including NASA, studying satellite orbital dynamics and space debris; died 1997.

August 28: Birth of John Herbert Chapman. He was a Canadian physicist, who studied radio propagation and the ionosphere; director of the *Alouette 1* satellite project and a strong influence on the setting up of the Canadian Space Agency in 1990; died 1979.

September 1–8: Alice Grace Cook and John Philip Manning Prentice (Stowmarket, Suffolk) observed nine meteors, discovering the γ Aquarid meteor shower.

September 12: Birth of Stanislaw Lem. He was a Polish science-fiction writer of short stories and novels, including *Solaris* (1961) and *His Master's Voice* (1968), but his influence was hampered by lack of translations from Polish; died 2006.

September 21: Death of Henri Bourget. Born in 1864, he was a professional astronomer at Toulouse Observatory, working on stellar positions. Elected FRAS in 1903, he became Director of the Marseilles Observatory, concentrating on accurate timing.

October 1: A total solar eclipse passed over Antarctica. The partial phase was visible from the South Atlantic and most of South America [Saros 123].

October 2: Birth of Albert Scott Crossfield. He was an American test pilot, who flew the Douglas Skyrocket and X-15 research aircraft; died 2006.

October 7: Frederick Charles Leonard (Lick Observatory) obtained a spectrum of 40 Eridani C as a nearby M-class red dwarf.

October 16: A partial lunar eclipse was visible from Asia, Europe, Africa, and the Americas [Saros 135].

October 17: Death of Loetitia Crommelin née Noble. Born in 1866, she was the wife of the British astronomer Andrew Claude de la Cherois Crommelin (1865–1939) and accompanied him to view the total solar eclipses of 1900, 1905, and 1912; member of the BAA.

October 21: Birth of Ingrid van Houten-Groeneveld. She was a Dutch astronomer at the Leiden Observatory, working on the Palomar–Leiden Survey, with her husband Cornelis Johannes van Houten (1920–2002) and Tom Gehrels (1925–2011) which discovered 4637 asteroids. In 1966 with her husband she discovered comet 271P/1960 S1 (van Houten-Lemmon), which was lost until 2012; she died in 2015.

November 4: Birth of Mary Sherman Morgan. She was an American rocket scientist involved in developing fuels for the Juno 1 (launching *Explorer 1*) and Redstone (launcher for suborbital Mercury missions); died 2004.

November 6–10: A noted display of the Northern Taurid meteor shower was observed by John Philip Manning Prentice and Alice Grace Cook (Stowmarket, Suffolk).

November 9: Albert Einstein received the Nobel Prize for Physics for his work on the photoelectric effect.

November 13: Ida Elizabeth Woods (Harvard) reported finding a 7th magnitude nova in Puppis on an archive plate taken on 1902 November 19. The nova slowly faded and was last recorded on 1903 June 3 [DY Pupis].

December 12: Death of Henrietta Swan Leavitt. Born in 1868, she was an American astronomer, who joined the Harvard College Observatory in 1902. She worked on stellar photometry, discovering 2400 new variable stars. Her studies of variables in the Magellanic Clouds led her to discover the period–luminosity relationship of Cepheid variables — one of the cosmic yardsticks for measuring the Universe.

The amateur astronomer Hester Periam Hawkins (1846–1928) published the romantic novel *Stella Maitland; Love and the Stars*, set in a background of astronomy; also elected FRAS.

1821

January 2: Birth of James Croll. This British geologist studied the Earth's climate and looked to astronomy to explain ice ages by changes in the Earth's orbital eccentricity. He noted that astronomers could not explain the Sun's energy over geological time and tried a model of two stars merging; died 1890.

January 21: Jean Nicolas Nicollet (Paris Observatory) and Jean Louis Pons (Marlia, Italy) discovered a comet in Pegasus. It was small and faint, and Nicollet noted a tail 0·5 degree long. The comet steadily brightened to be visible with the unaided eye and have a tail 3·5 degrees long in February. In March it was magnitude 3 but very low at sunset and disappeared, reaching perihelion on March 22 (q = 0·092 AU). In April the comet put on a fine display for observers in the Southern Hemisphere [Comet C/1821 B1 (Nicollet-Pons)].

March 4: A total solar eclipse passed over the East Indies and the Indian Ocean. The partial phase was visible from the West Pacific, East and Southeast Asia, Western Australia, and South Africa [Saros 127].

June 15: An eucrite meteorite landed near Juvinas in the Ardèche, France. Its basaltic composition suggests that it came from asteroid (4) Vesta [Juvinas meteorite].

August 15: Birth of Marian Albertovich Kovalsky. He was a Polish–Russian astronomer, Director at the Kazan University Observatory. He calculated the

Moon's motion and measured stellar positions. By analysing the proper motions of stars, he derived the Sun's motion in the Milky Way; Associate of the RAS 1863; died 1884.

August 16: Birth of Arthur Cayley. He was a British mathematician, mainly on pure mathematics and its application to astronomy. Editor of RAS *Memoirs* and *Monthly Notices* (1859–82) and President of the RAS (1872–4); died 1895.

August 19: Birth of Edwin Dunkin. He was a computer and then assistant at Greenwich Observatory in 1840, becoming Chief Assistant in 1881. Elected FRAS in 1845 and FRS in 1876, he became President of the RAS (1884–6); wrote numerous articles; died 1898.

August 21: Birth of Ernest Amédée Barthélémy Mouchez. He was a French naval officer drawing up coastal maps. He observed the Transit of Venus in 1874 and became Director of the Paris Observatory in 1878. One of the main driving forces behind the *Carte du Ciel* project; died 1892.

August 27: An annular solar eclipse passed over the Atlantic and the Southern United States. The partial phase was visible from West Africa and most of the Americas [Saros 132].

August 31: Birth of Hermann Ludwig Ferdinand von Helmholtz, the German physicist and mathematician. In astronomy he worked with Lord Kelvin on the model of the Sun's energy from gravitational contraction. It remained the general view among astronomers but did not satisfy the long time required by geologists. He died in 1894.

September: First issue of the periodical *Astronomische Nachrichten*.

September 14: Birth of Moritz Ludwig Georg Wichmann. He was a German astronomer at the Königsberg Observatory, observing minor planets. In 1853 he calculated the distance to the then-fastest-moving star Groombridge 1830 (Argelander's Star); died 1859.

November 18: Birth of Franz Friedrich Ernst Brünnow. He began his career in Germany, being present at the discovery of Neptune in 1846. Then he was Director of the University of Michigan and Dudley Observatories in the United States. He was appointed Astronomer Royal for Ireland (1865–74) but retired with failing eyesight. He set high standards of observations; died 1891.

November 28: Death of the Reverend Samuel Vince FRS. Born in 1749, he was a British clergyman, mathematician, and astronomer. Senior Wrangler at Cambridge University in 1775, awarded the Royal Society's Copley Medal in 1780, he was Plumian Professor of Astronomy at Cambridge (1796–1821); author of *A Complete System of Astronomy*.

November: Sir Thomas Macdougall Brisbane, governor of New South Wales, set up the Parramatta Observatory near Sydney.

December 4: Birth of Ernst Wilhelm Leberecht Tempel. He was a German amateur astronomer with a passion for comets. He discovered five asteroids: (64) Angelina, (65) Cybele, (74) Galatea, (81) Terpsichore, and (97) Klotho. Among his 21 comet discoveries were the short-period comets 9P/Tempel 1, 10P/Tempel 2, 11P/Tempel–Swift, and 55P/Tempel–Tuttle; died 1889.

December 21: First astronomical observations made at the Parramatta Observatory, Sydney.

Alexis Bouvard (Paris Observatory) published a set of tables on the motion of Uranus. He had to leave out Flamsteed's observation of 1690 as well as Lemonnier's, as he could not reconcile them with more recent positions. The problem was only solved in 1846 with the discovery of Neptune.

The Royal Society awarded Copley Medals to Edward Sabine and John Herschel.

Ignaz Venetz proposed the ice-age theory for the Earth.

Thomas Rowlandson published the drawing *Viewing the Comet*, caricaturing the popular interest in comets, rekindled by Comet C/1821 B1 (Nicollet–Pons).

(about) Death of Karl Joseph König. Born in 1751 he was a Jesuit astronomer at the Mannheim Observatory, the Palatinate (now Germany). In 1785 he created the constellation Leo Palatinus in honour of Elector Karl Philip Theodore (reigned 1742–99).

1721

January 13: A partial lunar eclipse was visible from North America, Asia, Europe, and Africa [Saros 110].

January 27: A total solar eclipse passed over Cape Horn and the Southern Ocean. The partial phase was visible from a large part of South America and New Zealand [Saros 136].

May 28: Birth of Placidus Fixlmillner. He was an Austrian Benedictine priest and Director of the Kremsmünster Observatory. He observed the Transit of Venus in 1769 and other astronomical objects in the Solar System. He was involved in the early calculations of the orbit of Uranus and kept finding that the planet was not following the calculations exactly; died 1791.

July 9: A total lunar eclipse was visible from the Americas [Saros 115].

July 24: A partial solar eclipse was visible from a large part of Asia and Northern and Central Europe [Saros 141].

August 31: Death of John Keill. Born in 1671, he was Savilian Professor of Astronomy at the University of Oxford (1712–21) and popularized Whiston's theory that the Biblical Flood was caused by a comet impact.

November 16: Birth of Johann Essaias Silberschlag. He was a German natural philosopher and astronomer, who observed the Brocken Bow; died 1791.

James Bradley was appointed Savilian Professor of Astronomy at the University of Oxford.

1621

January 16: Birth of Magnus Celsius. He was a Swedish mathematician and astronomer, grandfather of Anders Celsius (1701–44).

March 5: Birth of Thomas Street. He was an English mathematician, astronomer and influencer, supporter of Kepler's views and in 1661 published *Astronomia Carolina: a new theorie of Coelestial Motions*, used by Newton, Halley, and Flamsteed; died 1689.

April 10: An iron meteorite landed near Jalandhar, Punjab State, the Mughal Empire (now India) [Jalandhar meteorite].

May 21: An annular solar eclipse passed over Siberia, Russia, Scandinavia, and Southern England. The partial phase was visible from most of Asia, Europe, and North Africa [Saros 112].

May 22: The Chinese reported a 'red star' in the east, which could have been a comet or nova in Pisces.

June 4: A partial lunar eclipse was visible from the Americas [Saros 124].

July 2: Death of Thomas Harriot. Born in 1560, he acted as geographer on Sir Walter Raleigh's second voyage to Virginia. He made a number of astronomical observations with a cross-staff. In July 1609 he drew a sketch of the Moon with a small telescope. He also observed the moons of Jupiter, sunspots, and comet C/1618 W1; corresponded with Kepler but did not publish his own observations.

July 7: Death of Achyutha Pisharadi. Born in Kerala, India, in about 1550, he was a member of the Kerala school of astronomy and mathematics. He wrote on calculating eclipses and the position of planets; also a Sanskrit scholar.

September 1: Death of Baha' al-Din Muhammad ibn Husayn al-'Amili. Born in 1547, he was a Muslim scholar, mathematician, and astronomer. He was chief legal officer at the court of Shah Abbas the Great (ruled 1588–1629) in Isfahan, Persia (now Iran), He wrote over a hundred books, many on astronomy and tried to solve the motions of the Moon and Mercury, which the Ptolemaic system could not fully explain.

November 13: A total solar eclipse passed over Cape Horn. The partial phase was visible from South Africa and the southern half of South America [Saros 117].

November 29: A partial lunar eclipse was visible from most of Asia, Europe, Africa, and the Americas [Saros 129].

Death of Giovanni Paolo Gallucci. Born in 1538, he was a Venetian writer on astronomical and other instruments. He wrote *Theatrum Mundi* (*The Theatre of the World*) on astrology, with a star atlas and maps of the constellations.

Johann Schreck introduced the telescope to China.

John Bainbridge (1582–1643) was appointed the first Savilian Professor of Astronomy at Oxford University.

Willibrord Snellius discovered the law of refraction (Snell's Law).

1521

January: Antonio Pigafetta, who was accompanying Magellan, recorded observing the Magellanic Clouds.

February 7: The Chinese saw a star like a fire in the southeast. It turned white and moved from east to west. Then it became bent like a hook and disappeared. This could be a bright fireball or aurora or, over a number of nights, a comet moving away from the Sun.

April 7: A total solar eclipse passed over Japan, Korea, China, India, and East Africa. The partial phase could be seen from most of Asia, the Middle East, and North and East Africa [Saros 121].

April 27: Death of Ferdinand Magellan in the Philippines. Born in about 1480, he was a Portuguese navigator and explorer, whose ship the *Victoria* completed the first circumnavigation of the World. The Magellanic Clouds are named after him, although for a long time they were known simply as the Greater and Lesser Clouds.

September 30: An annular solar eclipse passed over South America. The partial phase was visible from most of the Americas [Saros 126].

Birth of Pontus de Tyard. He was a Catholic priest and Bishop of Châlons (1578–94). He wrote *L'Univers ou discours des parties et de la nature du monde* (*The Universe or A Discourse on the Parts and Nature of the World*), which included astronomical values calculated by Copernicus; died 1605.

1421

January 9: Japanese astronomers discovered a comet with a tail 5-degrees long in the northwest after sunset.

February 17: A total lunar eclipse was visible from Asia, Europe, and Africa [Saros 104].

March 3: A partial solar eclipse was only visible from the South Pacific [Saros 130].

August 13: A total lunar eclipse was visible from the Americas. The partial phase could be seen from Europe and Africa [Saros 109].

August 28: A partial solar eclipse was visible from Japan, Korea, China, Siberia, and Scandinavia [Saros 135].

December 27: The Chinese saw a 'guest star' (nova or comet).

1321

June 26: The inhabitants of Novgorod in Russia observed a solar eclipse. "There was a sign in the sky before morning service; the sky being clear, the sun suddenly grew dark for about an hour, and was like a moon of five nights; and there was darkness as on a winter night, and it filled out gradually and we were glad." The eclipse was annular across Japan, Siberia, and Russia and 93% from Novgorod.

An eclipse of the Sun was seen in the early morning from Bohemia. The eclipse was 97% from Prague. Levi ben Gerson (Orange, Southeast France) observed mid-eclipse one hour after sunrise, when cloud intervened. The eclipse was total over Northeastern, Central, and Southwestern Europe, including the French city of Bordeaux. It was 91% from Orange [Saros 106].

July 10: Levi ben Gerson (Orange, Southeast France} recorded the start of a lunar eclipse before sunrise. He could not tell if mid-eclipse took place before sunrise because of clouds on the western horizon. As a Jewish writer he dated the eclipse to the 9th. The eclipse was partial over most of Europe, but at Orange the Moon set before the end of the eclipse [Saros 118].

July 31: Death of Abu al-Abbas ibn Muhammad ibn Uthman al-Azdi al-Marrakushi [known as Ibn al-Banna]. Born in Marrakesh in 1256, he was a Sufi scholar and polymath, writing about a hundred books, half on mathematics and astronomy. His work on planetary motion was based on the works of Ibn al-Zarqali and Ibn Ishaq and required extensive calculations. As well as his students, his works influenced many later astronomers. He also wrote on the practical subjects of timekeeping for daily prayers and calculating the direction of Mecca. He acted as court astrologer for the Marinid sultan Abu Sa'id (ruled 1309–31).

September 14: Death of Dante Alighieri. The Italian poet, born in 1265, made extensive use of medieval astronomy in his poem *Divina Commedia* (*The Divine Comedy*). Heaven is seen as a series of spheres as in the Ptolemaic system.

December 19: An annular/total eclipse passed over central South America. The partial phase was visible from South America [Saros 111].

1221

January: Korean astronomers observed a comet in Ursa Major.

May 23: Chhiu Chhang-Chhun and a party of diplomats were travelling from China to Korakorum and Samarkand to see Genghis Khan. They observed a total solar eclipse from the Kerulen River, Mongolia. It was total over Northern China, Mongolia, and Northern India (maximum totality 5 min. 43 sec.), being 93% in the Mongol capital at Korakorum [Saros 115].

November 23: Birth of Alfonso X 'the Wise' King of Castile. Son of Ferdinand III, he became king in 1252. He set up a programme of translating Arabic texts into Latin. As well as astronomical texts, it included writings on the design of instruments for making observations. A number of scholars worked on the Alfonsine Tables, which became the standard text of astronomical positions for European astronomers, including Copernicus. Alfonso did not neglect his political duties and was a competitor to be Emperor of Germany; died 1284.

1121

April 4: A lunar eclipse was observed from China. The total lunar eclipse was visible from most of Asia, Europe, and Africa [Saros 98].

May: Michael the Syrian recorded that "on a Monday night there appeared a full arc." This was a lunar halo or an aurora.

It was recorded that "an immense fire ejected flames lasting six hours about dawn." This was a bright auroral display.

1021

May 25: Chinese astronomers observed a comet in Leo. It moved past Leo and was last seen on August 8.

921

February 21: Birth of Abe no Seimei, Japanese astronomer and court astrologer; died 1005.

December 17: In Ireland a lunar eclipse was recorded. The eclipse was total over Asia and Europe. In Ireland totality ended as the Moon set [Saros 88].

A spectacular shower of meteors was seen from Narni in Umbria, Central Italy.

A stony meteorite fell near Narni in Umbria [Narni meteorite].

821

February 27: Chinese astronomers discovered a comet in Crater. On March 7 it was near to β Virginis, although the report also said it was very close to Mercury (then in Pisces).

March 15: The Chinese observed a close conjunction of Venus and the Pleiades.

April 19: The Chinese observed a close conjunction of Venus and β Tauri.

July: Chinese astronomers saw a comet near the Pleiades in the morning sky. It had a tail 10-degrees long and remained visible for 10 days.

October 11: The Chinese observed a close conjunction of Venus and η Virginis.

721

The Japanese empress Gensho (683–748, ruled 715–24) was impressed by seeing a halo around the Sun.

521

There is a record that "a great mantle . . . stretched [across the sky] and every colour was present in it." This described a very bright auroral display.

(about) Death of Ammonius. Born in Alexandria in about 440, the son of Hermias, Head of the Academy in Alexandria. He was a Neoplatonist philosopher, who succeeded his father in 485. He made observations of planetary occultations and conjunctions and used an astrolabe to measure the longitude of Arcturus in order to calculate the precession of the equinoxes. He saw the Universe as eternal and not deterministic, putting him in opposition to both Christians and astrologers.

421

January – February: The Chinese observed a 'guest star' in Crater sometime in the lunar month January 20 to February 17. This may have been a nova or, more likely, a tail-less comet.

May 17: Agapius recorded that a solar eclipse was observed from Syria. The eclipse was annular across North Africa and was 34% at Antioch in Syria [Saros 83].

321

May 7: Chinese astronomers noted that 'within the Sun there was a black spot', an observation of a sunspot visible with the unaided eye.

221

The Roman writer Sextus Julius Africanus completed his work the *Chronicon*, an ecclesiastical chronology from Creation to the year AD 221 with information on the calendar.

80 BC

September 20: Chinese astronomers observed a solar eclipse from Chang'an, where it was nearly total. The eclipse was total over Northeast China and

Siberia but about 60% at Chang'an [Saros 62].

October – November: The Chinese observed a 'guest star' in Ursa Major. This may have been a nova or, more likely, a tail-less comet.

(about) Geminus of Rhodes was writing the *Isagoge* (*Introduction to Astronomy*). He was a pupil of the Greek philosopher Posidonius. He mentioned the theories of the Babylonians and the Greek astronomers Euctemon and Calippus. He thought that the stars were not on the surface of a sphere but at different distances.

280 BC

Aristarchus of Samos observed the summer equinox.

(about) Aristarchus of Samos observed the Earth's shadow on the Moon during an eclipse and calculated that the Moon's radius was about 1/3 of the Earth's.

(about) Aristarchus of Samos proposed a heliocentric model of the Solar System.

(about) Birth of Chrysippus of Soli in Asia Minor. He was a Stoic philosopher with an Earth-centred view of the Universe surrounded by spherical shells for the Moon, Sun, the planets, and finally the fixed stars. This followed the model set by Plato and Aristotle; died in Athens 207 BC.

(about) Birth of Conon of Samos. He was a Greek mathematician and astronomer at the court of King Ptolemy III of Egypt (ruled 246–222 BC) in Alexandria. He collected records of eclipses and named a group of stars as Coma Berenices in honour of Queen Berenice II; died about 220 BC.

(about) Ptolemy II King of Egypt (ruled 308–246 BC) appointed Zenodotus as the first Head of the Alexandrian Library.

380 BC

(about) Birth of Calippus of Cyzicus. He studied under Eudoxus and developed his model of the Universe, which contained 34 spheres around the Earth to allow for the unequal seasons. His subtraction of one day every four Metonic cycles improved the accuracy of the Moon's motion and the Greek calendar to 11·3 minutes per year [Calippic cycle].

(about) Birth of Menaechmus in the Thracian Chersonese. He studied under Eudoxus and discovered conic sections; died about 320 BC.

(about) Birth of Pytheas of Massilia (Marseilles). He was a Greek geographer and explorer, who sailed around the coast of Britain and observed the altitude of the Sun far to the north; died about 310 BC.

480 BC

September 22: Pliny the Elder recorded that a comet with a curved tail appeared when the naval battle of Salamis was fought between the Greeks and the Persians.

October 2: A solar eclipse discouraged the Greeks from pursuing the Persians after the battle of Salamis. The eclipse was annular across North Africa and about 50% in Southern Greece [Saros 65].

The Greek philosopher Harpalus accompanied the Persians, acting as an engineer. He was a mathematician, who had modified Cleostratus' cycle of lunar motion.

(about) Death of Pythagoras of Samos. Born in about 570 BC, he developed the Pythagorean school of philosophy, mainly mystical but with a strong mathematical basis. He taught an Earth-centred universe and recognized that the morning and evening stars were apparitions of the planet Venus. In the 540s BC he migrated to Southern Italy, where he died.

(about) Death of Heracleitus of Ephesus. Born in about 540 BC, he believed that the Earth was flat, the Sun was created each morning, and the stars were fainter, because they were further away. Each celestial body was placed in an inverted bowl in the sky.

(about) Birth of Philolaus of Croton. He was a Pythagorean philosopher, who modelled the Universe as a series of spheres, including the Earth, Sun and Moon, moving around a central fire. He created a complicated theory to explain why the central fire was not visible; died about 385 BC.

(about) Birth of Leucippus of Miletus. He was a Greek philosopher, who developed a theory of atoms. He considered Earth as cylindrical with eclipses caused by the tilt of the Earth. He wrote *Megas Diakosmos* (*Great World System*), but most of his ideas were taken up by his student Democritus; died about 420 BC.

Here and There

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The Importance of Physics to the Economics of Europa: a study by Cebr for the period 2011–2016 — *Europhysics News*, **51**, no. 1, 12, ref. 1, 2020.