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 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

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

†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,

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 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

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 January 2015 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`

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 overpredicting 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 ΛCDM 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 non-baryonic 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

*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, obliviate the need for all dark matter.

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 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 aborption 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)

*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

— 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).*

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,

$$F = m\mu\left(|a|/a_0\right)a = m\mu(x)a\,, \tag{1}$$

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 $\approx 1.2 \times 10^{-10}$ m s^{-2}. That does not mean, however, that there is some minimum acceleration; the force still falls off

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).

†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.

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 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 obseved 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 [60,61]. Also, the Train-Wreck Cluster (Abell 520) provides a counterexample [62] which is difficult to explain in ΛCDM, while MOND explanations for the Bullet Cluster have been proposed [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 accomodated 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 an approximation 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 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 every-

*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].

thing 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 ΛCDM 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 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,

*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 ΛCDM galaxy-formation simulations use Newtonian gravity, not GR, and that that is justified.

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

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 the 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

*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 Universe.

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

*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 then 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].

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 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 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 en-

*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 since the time since the Big Bang can be much longer than the Hubble time $1/H_0$ in such models.

ergy 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 $-\varepsilon$", 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 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

[*]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].

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 luminostiy 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 to 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 interested in such topics, even particularly interesting trees might not be mentioned when writing a book about a forest. Interestingly, 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 correctly point out 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 ΛCDM 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 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 problem 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,

*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 February 2019. It was an excellent talk.

†In a fascinating glimpse into the thinking at that time, Brawer[132] notes that that neither the horizon problem nor, especially, the flatness problem was considered to be an important issue 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.

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 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 maintream astrophysicists into explaining the interesting MOND phenomenology, rather than claiming that they are motivated by other factors and incapable of improving their model to accomodate 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.