June 20 2015

Hi Stacy,

Let me take this opportunity–i.e., your inquiry on possible departures from some zeroth-order predictions of MOND–to share with you some of the more general thoughts that have been in my mind for the last N years. It is a bit rambling as I write more or less as it comes, but I think there are important lessons here.

In testing MOND for the last thirty years we have been twice lucky. 1. Nature has obliged us with some rather clear-cut systems that are rather well understood, and that also probe MOND in regions where it departs greatly from standard dynamics. The thought that it could have easily been otherwise has often haunted me: Disc galaxies could have been messier, or LSBs not exist, M/L could have vary much within galaxies, HI not present to large radii, etc. (For example, Nature has not been so generous in connection with GR, which has not been tested where it counts most–the strong-filed regime–even after 100 years.) 2. The second lucky circumstance was that practically all these clean tests of MOND, test predictions that follow essentially from only the simple basic tenets (as I explained e.g., in the MOND-laws paper). So we did not have to know the exact theory.

I do think we are now at some sort of a crossroads. It seems that, barring some surprises, we may have pretty much exhausted the tests in the comfort zone defined by the above two characteristics. I have to say that I thought so even some five years ago, but there did come some surprises (such as the two x-ray ellipticals of Humphrey et al. that provided nice tests of ellipticals to large radii, the M31 dwarfs that we studied, and the galaxy weak-lensing results of Brimioulle et al.); so maybe there will be more surprises.

There are also interesting deductions on formation and evolution of objects, such as bulge formation, the M31-MW history, discs of satellites, etc., but these can be only corroborative, as obviously they are not as clear-cut as RCs.

But barring that, we are now, as it were, scratching the bottom of the pot, data wise, having to look at systems that are much less well understood, having to make all sorts of assumptions, and/or systems in which different MOND theories could indeed make very different predictions, especially if we are probing MOND near a_0 where the MOND effects are of order unity, which makes them even more theory dependent.

You may have noticed that (as a result of the above view of the situation) besides continuing to construct theories, I have been concentrating, in the last N years, on 1. defining and investigating more precisely what exactly we can predict from the basic tenets alone. 2. exploring the range of what otherwise may be possible within MOND theories of all sorts, using various toy theories that satisfy the basic tenets.

We may well be in for a period where we will need much more help from theorists, similar to what happens just now with the standard model of particle physics. We could, for a while, put aside further tests and try to construct a MOND theory based on the more robust tests we have already, and on "pure thought" such as symmetry principles, and the connection with cosmology. We need such a MOND theory, anyway, to describe cosmology.

There has indeed been a flurry of such MOND theories just recently, but unfortunately I think that none of them is a full-fledged "final" theory. In case you have not been following this activity here are links to some from the last few months:

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http://arxiv.org/abs/1506.00730
http://arxiv.org/abs/1505.05146
http://arxiv.org/abs/1504.00870
http://arxiv.org/abs/1504.00475
http://arxiv.org/abs/1412.0430
http://moriond.in2p3.fr/J15/transparencies/2_monday/2_afternoon/4_Khoury.pdf
https://www.youtube.com/watch?v=UTIeJ-asLao&feature=youtu.$be
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(The last is Verlinde's talk; he discusses MOND quite a bit. In fact, he claims to be able to reproduce it, but I doubt it.)

The other approach (not excluding the first) would be, indeed, as you suggest, to try and take hints from these rather less reliable "tests". This is naturally closer to what you are good at. But then you run the risk of being led astray by red-herring anomalies, if there is something wrong with the way we interpret the measurements in such systems. There are periods in the life of a paradigm when people have to be patient and wait for a better theory before finer tests can be made.

And there are, indeed, many things that could go wrong with the observations and their interpretation, none of which escapes you, I am sure. In measuring sigmas in low-dispersion pressure-supported systems you have binaries: at a level of a few km s^{-1} dispersions I don't even see how you could exclude binaries. Solar mass binaries with such velocities have periods of thousands of years. In our paper on M31 dwarfs we mentioned some serious sources of uncertainty, but then, because the results were rather satisfactory, we just ignored these potentially serious sources of systematics. One that I remember is the fact that to measure dispersions you look at a certain stellar population that lands itself better to the task, but it is not clear that this is the dispersion that enters the MOND expression we use, which is the MASS WEIGHTED dispersion over the whole system. Another worry is indeed whether these are in virial equilibrium. Tidally affected systems would tend to have larger-than-virial velocities, and still-collapsing systems (such as still-forming tidal dwarfs) would have lower-than-virial velocities. Low-velocity rotating discs may require large and uncertain asymmetric-drift corrections, etc., etc. And another is the systematics I am sure besets the Disc Mass funny result, which we have discussed extensively.

On the other hand, certainly, some of the anomalies which have been, and will be, discovered must have to do with us using unjustified zeroth-order predictions of MOND.

I do not know which way things will go, and what sort of a theory the MOND paradigm will converge to eventually.

Describing phenomena, such as the z-dynamics in discs, in terms of different interpolating functions (IF) is clearly a poor man's description. I don't know that the departure from zeroth order will be describable in such terms, but in default of something better we may at this stage try to "parametrize" the next order in this way. But, in any event, we use the term IF rather loosely, and the concept requires sharpening.

It's definition is really only clear when the IF is introduced already at the level of the action, as is done in existing theories. But now we do not want to assume that, as I am sure a good MOND theory will not have that. So what is an IF more generally?

In connection with RCs, the use of an IF ASSUMES that the measured acceleration (from the RC) is a universal function of the mid-plane Newtonian acceleration at the same location. This is not the case even in the known MG theories (AQUAL, QUMOND). My old work with Brada showed that for the disc models we studied, using the algebraic relation gives correctly the velocities calculated in AQUAL to better than about 20 percents for the same IF and the same value of a_0. (You can probably improve the agreement if you use different IFs for the two schemes, because the differences seem to be systematic.) We have always swept this under the rug, not using AQUAL to do RCs, relying on the fact that in MI theories the rotational acceleration is a function of the mid-plane Newtonian acceleration. The continued use of this algebraic relation, which assumes strict functional dependence, has been vindicated by the great agreement of MOND RCs with the observed one, epitomized by the nice and tight "discrepancy-acceleration" correlation (for RCs). Perhaps this does point to MI rather than MG. If we do not count your recent Disc Mass study, which is for face-on galaxies, there was never a systematic study of RCs in MOND using MG, such as AQUAL or QUMOND. Perhaps this would not have been as successful, perhaps it would have pointed to another IF?

Now note that in MI theories there is not even generally a thing such as "the MOND acceleration at a given position or a given time" (unless you define "acceleration" not as $d\mathbf{v}/dt$, but by some generalization, as happens in relativity; see below). Only in MG theories, where there exists a potential $\phi(\mathbf{r})$, such that all bodies have $d\mathbf{r}/dt = -\vec{\nabla}\phi$, do you have that. In MI theories the accelerations of different particles at a given position (and time) can be different depending on properties of their orbits.[1] So how would you even define an IF for z-motions, or other general motions? It is only for RCs that the orbit is unique for all particles. And then, a conjunction of several things leads to a uniquely and universally defined IF: you only have the use of a_0 in MOND, the only quantity with the dimensions of acceleration that you can define for a circular orbit is v^2/r, and the derivation from an action. This happens even without an introduction of an IF in the action. The emergent IF is related to the action as it is defined for circular orbits, where it then must be some function of v^2/ra_0 on dimensional grounds.

But let's say that you still want to define, by some brute force, effective, observational way, some IF that pertains, e.g., to z-dynamics. Of course it will have to somehow involve contributions from different particles on different orbits, that are now at the same location. I don't quite know how to do that, but let's proceed. What would be the meaning of such an IF? Clearly it is some function that connects the DML with Newtonian dynamics, but what does it describe, and function of what is it? You could say, as is usual, that we want to plot some predicted acceleration, let's say the g_z at some $\mathbf{r}$ (remember that there need not be such a thing; you would have to force a description in terms of some potential field) as a function of the Newtonian acceleration there, or as a function of $g = |\mathbf{g}|$. Then you would plot what you call "the inverse mass discrepancy" which is $\eta \equiv g_z^N/g_z$ (g_z^N being the Newtonian value) say as a function of g/a_0, and you would want to call this the IF $\mu(x)$.

But then don't be surprised if μ does not describe a very tight (or even a not so good) correlation, and it may turn out to depend on galaxy, on position, etc. Remember, it is only the RC that has to be unique in this sense.

It is also important to note what MOND's basic tenets predict for such a μ.

1. It should go to 1 for large x, so as to restore Newtonian dynamics for $a_0 \to 0$.

2. It should satisfy $\mu(x \ll 1) = \alpha x$. But note, importantly, that we do not have to have $\alpha = 1$. The proportionality $\mu \propto x$ for small x is dictated by scale invariance of the DML, quite simply: η is some Newtonian acceleration–which scales as $g^N = MG/r^2$–divided by a measured acceleration $g = v^2/r$. So, η scales as MG/v^2r. Since v and M do not change under scaling, η scales as r^{-1}. But g also scales as 1/r, so SI, says that η scales as g, namely $\mu(x) \propto x$. In

[1] This is not unheard of: it is even true in special relativity, which, as a departure from Newtonian kinematics may be considered a MI theory. After all it is $d[\gamma(v)\mathbf{v}]/dt$ that is the same for all particles subject to the same F/m, not $d\mathbf{v}/dt$.

the case of the IF that pertains to RCs, we normalized the value of a_0 so as to give equality $MGa_0 = V_\infty^4$, with which convention you put $\alpha = 1$. But this convention does not fix the small-x coefficient of IFs for other correlations.

3. The transition in the IF should occur $\sim a_0$.

So, for example, you could have $\mu(x) = 3x/(1+3x)$, or $2x/(1+4x^2)^{1/2}$, etc. [They will be equivalent to taking a μ as we have used so far but with a different a_0 value, since you could write them as $\mu^*(\alpha x)$, with $\mu^*(\bar{x} \ll 1) \approx \bar{x}$.]

All this ambiguity etc. is really the norm in other known instances of transition between paradigms. So the above is not some convenient copout for MOND. It is important to keep that in mind, so I'll mention some examples:

The classical-quantum-mechanics transition is characterized by many "interpolating functions" (which of course do not appear in the basic equations, such as the Schrod. eq.) The black-body function is one; it connects the classical Rayleigh-Jeans expression where h does not appear, with the quantum region where the $exp(-h\nu/kT)$ shows up. The role of a_0 there is played by h. But if you look at the BB function as a function of the frequency or wavelength, it is still not universal but system dependent as it depends on T, which varies. Another quantum/classical IF is the specific heat of solids (say in the Einstein model) as a function of temperature. For high T it has to goes to the classical Dulong-Petit universal constant. But for $T \to 0$ it typically has the exponential decline to 0 like $exp(-\nu_0 h/KT)$, which comes from QM, but it is not universal, and depends on the material through the "gap" frequency ν_0. Other examples would describe transition probabilities across barriers, etc., etc.

In classical/relativity transition too you have various "IFs". For example, the dependence of the kinetic energy of a particle on its momentum: $E_k \equiv (p^2c^2 + m^2c^4)^{1/2} - mc^2$. Here c plays the role of a_0. It interpolates between the highly relativistic limit $c \to 0$, where we have $E_k = pc$, with the NR limit $c \to \infty$, where $E_k = p^2/2m$. But p (analogous to g) and c are not the only parameters; the IP depends also on m.

I have always liked MI better, even though existing full-fledged theories are all MG. Some people do not understand that MI is not at all "a bridge too far". On the contrary, conceptually it makes more sense than MG, especially as effective (emergent) theories, as I think MOND is. (All known theories, such as the standard model of particle physics, and GR itself, are thought to be "only" effective theories in this sense.)

Nature is replete with examples of acquired or modified inertia. What is the whole business with the Higgs mechanism if not "modified inertia". Electrons in solids behave effectively (i.e., as the result of their interaction with the lattice) as free particles but with a completely different mass. In fact there is much talk recently of circumstances in substances such as Graphene, where the electrons behave as relativistic dirac particles (with the speed of light played by the Fermi velocity). So really modification of inertia could very well be in the basis of MOND.

On the other hand, we may not necessarily have to go to MI theories to account for such "anomalies" as you mention. MG still offers a much larger scope than has been studied so far.

In any event the toy MI theories I discussed in that Ustron conference paper are just that. They where only meant to give some idea of the possibilities. The problem is that you can get a wide range of possibilities. For example you can assume that each Fourier component is modified independently according to its own acceleration. This would of course give RCs correctly. It will also give the correct center-of-mass motion (say of stars in a galaxy), but will have no EFE. I also gave more complicated examples that do have the EFE. But really one cannot just rely on any of these.

Clearly, one should keep those potential anomalies in mind, but at present I don't think I would try hard to construct some toy theory that would maybe account for this or that. I think cosmology, for example, is a much bigger issue and gives stronger clues to where to look at for improving on MOND as we know it now.

Stacy, I think we ought to try everything we can and have time for, so what you suggest does make sense (I mean to take hints from the things that seem to you as anomalies). But there is nothing wrong with living for a while with some "anomalies" that we do not exactly know how to account for in the present state of the theory, if we have confidence in that theory from what we already know, even if these weigh on our minds.

It is always like this with any burgeoning revolutionary idea, even tens of years after its advent. Historical examples abound.

Take again quantum mechanics. Planck's "theory" was nothing near what we know about QM today, and the same was true after the extensions that explained and predicted properties of the photoelectric effect, and later the Bohr atom, and later yet specific heat of solids, etc. Even after the Schrod. eq. you could still not describe atoms (except H like), because spin and the Pauli exclusion principle were still missing.

Or take the standard model of particle physics as it is today. For many things it works very well, but it is obviously incomplete: It has 19 free parameters (compared with the one a_0); it does not know how to account for gravity; does not have an explanation for neutrino masses; has the so called strong-CP problem; no explanation for the baryon asymmetry, dark matter, the cosmological constant, the hierarchy problem; etc. People are happy with its doing a lot and try to improve on that.

I feel the same about MOND.

Best, Moti