Assessing an Alternative Cosmology[1]

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The standard theory of cosmology is called the ΛCDM, or 'Lambda-cold dark matter', model. As that name suggests, the theory postulates the existence of dark matter — a mysterious substance that comprises (according to the theorists) the bulk of the matter in the universe.

Every cosmologist working today was educated in the standard-model tradition, and virtually all of them take the existence of dark matter for granted. In the words of Nobel Prize winner P. J. E. Peebles: "The evidence for the dark matter of the hot big bang cosmology is about as good as it gets in natural science."

There is a problem, however. For four decades and counting, scientists have failed to detect the dark matter particles in terrestrial laboratories. You might think this would have generated some doubts about the standard cosmological model, but all indications are to the contrary. To quote from the 2014 edition of the *Review of Particle Physics:* "The concordance model [of cosmology] is now well established, and there seems little room left for any dramatic revision of this paradigm."

But there do exist competing theories, and not all of them contain dark matter. The most successful competitor is called MOND, for 'Modified Newtonian Dynamics'. Observations that are explained under the standard model by invoking dark matter are explained under MOND by postulating a modification to the theory of gravity. If scientists had confirmed the existence of the dark particles, there would be little motivation to explore theories like MOND. But given the absence of any detections, the existence of a viable alternative theory that lacks dark matter invites us to ask: does dark matter really exist?

Philosophers of science are fascinated by situations like this, and it is easy to see why. The traditional way of assessing the truth or falsity of a theory is by testing its predictions. If a prediction is confirmed, we tend to believe the theory; if it is refuted, we tend not to believe it. And so if two theories are equally capable of explaining the observations, there would seem to be no way to decide between them.

What is a poor scientist to do? How can she decide? It turns out that the philosophers have some suggestions. They point out that scientific theories can achieve correspondence with the facts in two very different ways. The 'bad' way is via post-hoc accommodation: the theory is adjusted, or augmented, to bring it in line with each new piece of data as it becomes available. The 'good' way is via prior prediction: the theory correctly predicts facts in advance of their discovery, without — and this is crucial — any adjustments to the theory.

It is probably safe to say that no theory gets everything exactly right on the first try. But philosophers are nearly unanimous in arguing that successful, prior prediction of a fact assigns a greater warrant for belief in the predicting theory than post-hoc accommodation of that fact. For instance, the philosopher of science Peter Lipton wrote:

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> When data need to be accommodated … the scientist knows the answer she must get, and she does whatever it takes to get it ... In the case of prediction, by contrast, there is no motive for fudging, since the scientist does not know the right answer in advance …. As a result, if the prediction turns out to have been correct, it provides stronger reason to believe the theory that generated it.

Some philosophers go so far as to argue that the *only* data that can lend *any* support to a theory are data that were predicted in advance of experimental confirmation; in the words of philosopher Imre Lakatos, "the only relevant evidence is the evidence anticipated by a theory." Since only one (at most) of these two cosmological theories can be correct, you might expect that only one of them (at most) manages to achieve correspondence with the facts in the preferred way. That expectation turns out to be exactly correct. And (spoiler alert!) it is not the standard model that is the favored theory according to the philosophers' criterion. It's MOND.

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Dark matter was a response to an anomaly that arose, in the late 1970s, from observations of spiral galaxies like our Milky Way. The speed at which stars and gas clouds orbit about the center of a galaxy should be predictable given the observed distribution of matter in the galaxy. The assumption here is that the gravitational force from the observed matter (stars, gas) is responsible for maintaining the stars in their circular orbits, just as the Sun's gravity maintains the planets in their orbits. But this prediction was decisively contradicted by the observations. It was found that, sufficiently far from the center of every spiral galaxy, orbital speeds are always higher than predicted. This anomaly needed to be explained.

Cosmologists had a solution. They postulated that every galaxy is embedded in a 'dark matter halo', a roughly spherical cloud composed of some substance that generates just the right amount of extra gravity needed to explain the high orbital speeds. Since we do not observe this matter directly, it must consist of some kind of elementary particle that does not interact with electromagnetic radiation (that includes light, but also radio waves, gamma rays etc.). No particle was known at the time to have the requisite properties, nor have particle physicists yet found evidence in their laboratory experiments for such a particle, in spite of looking very hard since the early 1980s.

In 1983, an alternative explanation for the rotation-curve anomaly was proposed by Mordehai Milgrom, a physicist now at the Weizmann Institute in Israel. Milgrom noticed that the anomalous data had two striking regularities that were not accounted for by the dark matter postulate. First: orbital speeds are not simply larger than predicted. In every galaxy, the orbital speed rises as one moves away from the center and then remains at a high value as far out as observations permit. Astronomers call this property 'asymptotic flatness of the rotation curve.' Second: the anomalously high orbital speeds invariably appear in regions of space where accelerations due to gravity drop below a certain characteristic, and very small, value of about 10^{-10} m s^{-2}. That is: one can predict, in any galaxy, exactly where the motions will begin to deviate from Newtonian dynamics.

This characteristic acceleration value, which Milgrom dubbed a_0, is much lower than the acceleration due to the Sun's gravity anywhere in our solar system. So, by measuring orbital speeds in the outskirts of spiral galaxies, astronomers were carrying out the first tests of gravitational theory in a new regime: the regime of ultra low acceleration. Milgrom knew that there were many instances in the history of science where the need for a new theory only became apparent when an existing theory was tested in a new way. And so he took seriously the possibility that the theory of gravity might simply be wrong.

In three papers published in 1983, Milgrom proposed a simple modification to Isaac Newton's laws that relate gravitational force to acceleration. (Einstein's theory reduces to Newton's simpler theory in the regime of galaxies.) He showed that his modification correctly predicts the asymptotic flatness of orbital rotation curves.

Milgrom was careful to acknowledge that he had *designed* his hypothesis in order to produce that known result. But his theory also predicted that the effective gravitational force was computable given the observed distribution of normal matter alone — not just in the regime of ultra-low acceleration, but everywhere. And when astronomers tested this bold prediction, they found that it was correct. Milgrom's hypothesis correctly *predicts* the rotation curve of every galaxy that has been tested in this way. And it does so without postulating the presence of dark matter.

Note the stark difference between the way in which the two theories explain the anomalous rotation-curve data. The standard-model cosmologist executes an *ad hoc* maneuver: he simply postulates the existence of whatever amount and distribution of dark matter are required to reconcile the observed stellar motions with Newton's laws. Whereas Milgrom's hypothesis correctly *predicts* orbital speeds given the observed distribution of normal matter alone. No standard-model theorist has ever come up with an algorithm that is capable of doing anything as impressive as that.

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Many philosophers would argue that this predictive success of Milgrom's theory gives us a warrant for believing that his theory — as opposed to the standard cosmological model — is correct. But the story does not end there. Milgrom's theory makes a number of other novel predictions that have been confirmed by astronomers. Doing justice to all of these would take a book (and in fact, I've recently written such a book), but I will mention one example here. Milgrom's theory predicts that a galaxy's total mass in normal (non-dark) matter, which astrophysicists like to call the 'baryonic mass', is proportional to the fourth power of the rotation speed measured far from the galaxy's center. And this novel prediction also turned out to be correct. (For obscure historical reasons, Milgrom's predicted relation is nowadays called the 'baryonic Tully-Fisher relation', or BTFR.)

Astrophysics is rife with *correlations* between observed quantities, but *exact relations* like the BTFR are unheard of: they are the sort of thing one associates with a high-level theory (think: the ideal gas law of statistical thermodynamics), not with a messy discipline like astrophysics.

What would a standard-model cosmologist predict for a relation like the BTFR? The simple answer is: nothing. Their theory contains no prescription for relating a galaxy's baryonic mass to its asymptotic rotation speed. But astrophysicists are diligent and clever, and they have come up with a way to try to accommodate relations like the BTFR under the ΛCDM model. Their scheme is to carry out large-scale computer simulations of the formation and evolution of galaxies, starting from uniform initial conditions in the early universe. The simulated galaxies can then be 'observed' and their properties tabulated. The earliest attempts of this sort yielded nothing very similar to Milgrom's predicted relation. But over the years (decades, actually) since then, theorists have come up with more-or-less plausible mechanisms for linking the normal and dark matter in their simulated galaxies, in such a way that they can obtain something approximating the BTFR. The currently favored mechanism, called ‘feedback’, is based on the (reasonable) idea that some of the gas that would otherwise form into stars is pushed out from the dark halo by the stars themselves, via stellar winds or supernova explosions. If the ‘feedback prescription’ is carefully enough chosen, just the right amount of gas can be ejected, from dark halos of each size, to yield the correct relation.

Standard-model theorists have not yet succeeded in reproducing the BTFR via their simulations. But let’s suppose that, one day, they do succeed. Would that success lend support to their cosmological theory, in the same way that the successful *ab initio* prediction of the relation by Milgrom lends support to his hypothesis?

Philosophers of science have an answer, and it is a resounding “no”. For instance, John Worrall writes that “when one theory has accounted for a set of facts by parameter-adjustment, while a rival accounts for the same facts directly and without contrivance, then the rival does, but the first does not, derive support from those facts.” On this view, it doesn’t matter whether the parameters being adjusted are meant to represent actual physical processes (like feedback) or not. The fact that Milgrom’s hypothesis correctly predicts the relation “without contrivance” means that it wins: it is the sole hypothesis that derives support from those data.

Now, the preference on the part of philosophers for scientific theories that predict in advance previously unknown laws or relations is quite in line with the preference that scientists themselves have expressed, over and over again, going back centuries. For instance, Gottfried Leibniz wrote in 1678 “Those hypotheses deserve the highest praise … by whose aid predictions can be made, even about phenomena or observations which have not been tested before.” And so the question naturally arises: Why have most cosmologists been so dismissive of MOND, given that MOND exhibits the very quality that scientists prize so highly?

Up until a few years ago, this disdainful attitude was defensible. One of the most touted successes of the standard cosmological model has been its ability to reproduce (‘accommodate’ would be a better word here) the so-called cosmic microwave background (CMB) spectrum — the statistical properties of temperature fluctuations in the CMB, the universe-filling radiation that was produced soon after the Big Bang. Milgrom’s theory did not originally do this, at least not as well as the standard model. But that situation has now changed. Just over a year ago,[2] two theorists in the Czech Republic, Constantinos Skordis and Tom Złośnik, showed that there exist fully relativistic versions of Milgrom’s hypothesis that are perfectly capable of reproducing the CMB data without dark matter. This relativistic version of MOND, which they call RMOND, incorporates an additional field that mimics the behavior of particle dark matter on the largest cosmological scales, and yields Milgromian dynamics on the scale of galaxies.

Prior to this success, a number of standard-model cosmologists had gone on record to say that fitting the CMB data was the *single thing* that MOND needed to do in order to be taken seriously. For instance, cosmologist Ruth Durrer stated “A theory must do really well to agree with [the CMB] data. This is the bottleneck.” Now that that bottleneck no longer exists, has the standard-model community embraced the RMOND theory as a bona fide competitor to theirs?

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[2] That is, in 2020.

Not so much. An argument that is making the rounds nowadays goes something like this: "Yes, [R]MOND works, but it is so much more *complicated* than our theory, which just invokes one thing — dark matter — to explain the observations."

In my opinion, this criticism misses the mark. The issue is not that RMOND is too complicated; it is that the dark matter of the standard model is too simple. Milgromian theorists have understood for a long time that there is just no way that a formless entity like dark matter can spontaneously rearrange itself — and *keep* rearranging itself — so as to produce the striking regularities that we observe in the kinematics of nearby galaxies. Skordis and Złośnik achieve this by postulating (it seems to me) an almost minimal modification to Einstein's theory. I can hardly imagine that any truly successful theory could be much simpler than theirs.

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Like almost all scientific theories, both MOND and the standard model are faced with anomalies: data that seem difficult to explain. In the case of MOND, I am aware of just one important anomaly; it has to do with the observed dynamics of galaxy clusters. The ΛCDM model is in a seemingly worse state. That theory fails to adequately explain any of MONDs successful new predictions, and in addition, standard-model theorists have identified at least a half-dozen puzzles that are (in my opinion) as least as problematic for their theory as the galaxy cluster anomaly is for MOND. My point here is not that one should judge the two theories by cataloging their failures. Rather, the novel predictive *successes* of MOND give us a warrant for believing (at least provisionally) that that theory is correct, and therefore that it is worth the effort to try and solve the existing puzzles. No such warrant exists under the standard model.

The argument from predictive success is a good reason to favor MOND over the standard cosmological model. But one can hope for more: for what Karl Popper called a "crucial experiment": an experimental or observational result that decisively favors one theory over the other. For instance, it has recently been claimed (based on observations of galaxies) that the so-called strong equivalence principle is violated in regimes of low acceleration. That result, if confirmed, would rule out Einstein's theory of gravity while at the same time confirming a prediction of Milgrom's theory.

A decisive result in favor of the ΛCDM model would be a laboratory detection of dark matter particles. Standard-model cosmologists are quite aware of this, and since the early 1980s a number of sophisticated (and expensive) detectors have been in operation that were specially designed to record the presence of the particles. About a half-dozen such experiments are currently active; the sensitivity of state-of-the-art detectors is about ten million times that of the earliest experiments. But no event has yet been observed that can reasonably be interpreted as the track of a dark-matter particle.

Of course, the failure to detect the dark particles is *expected* under MOND; but does this negative experimental result constitute evidence in *support* of MOND? Most scientists would probably say "no"; to quote the aphorism made famous by Carl Sagan (who was talking about something completely different), "Absence of evidence is not evidence of absence". And in fact standard-model cosmologists routinely argue that the persistent failure to detect the particles can be accounted for by assuming that the particles, even if present, undergo negligibly weak interactions with the normal matter in their detectors.

I think the philosophers might disagree. Paul Feyerabend argued that an ambiguous experimental result could sometimes be interpreted as an effective refutation of the theory being tested, even if scientists were clever enough to 'explain away' the apparent failure. The necessary condition, he said, was the existence of an *alternative* theory that naturally explained the result:

> The reason why a refutation through alternatives is stronger is easily seen. The direct case is "open," in the sense that a different explanation of the apparent failure of the theory … might seem to be possible. The presence of an alternative [theory] makes this attitude much more diffcult, if not impossible, for we possess now not only the *appearance* of failure … but also an explanation, on the basis of a successful theory, of *why failure actually occurred*.

By successful alternative theory, Feyerabend meant a theory that both explained the negative experiemental result, and that also generated new, testable predictions. The confirmation of those new predictions, he argued, constituted an effective refutation of the original theory. MOND amply meets Feyerabend's criteria for the alternate theory, since the same postulate that removes the need for dark matter leads (as we have seen) to a number of novel predictions which have been observationally confirmed.

Feyerabend was arguing here, as he often did, for a methodological rule that is nowadays called the 'principle of proliferation': the thesis that judgments about the performance of a theory are much sounder if there exist alternative theories with which a comparison can be made. If no one had ever hit upon a theory, like MOND, that explains cosmological data without dark matter, the failure of experimental physicists to detect the dark particles could reasonably be ascribed to some combination of poorly-understood phenomena ("Dreckeffekte" — garbage effects — was Feyerabend's sarcastic term). But the existence of a theory like MOND forces scientists to take seriously the possibility that their experimental failure constitutes a falsification of their theory in favor of MOND.

If Feyerabend were alive today, I am certain that he would be delighted at the fact there are two viable contenders for the correct theory of cosmology. I think he would be intrigued to learn that one, and only one, of these two theories has repeatedly been found to 'anticipate the data' – to make surprising predictions that turned out to be correct. And I think he would urge cosmologists to put their effort into identifying crucial experiments that could decisively favor one theory over the other.

At the same time, I am equally certain that Feyerabend would be critical of the manner in which Milgrom's theory has been treated by the larger scientific community. Textbooks and review articles on cosmology rarely mention MOND at all, and when they do, it is almost always in dismissive terms. And while I can not quote statistics, it is pretty clear that winning a research grant, or publishing a scientific paper, or getting tenure, is harder (all else equal) for Milgromian researchers than for standard-model scientists.

I honestly don't know whether this troubling state of affairs reflects a general ignorance about MOND, or whether some darker psychological mechanism is at work. But I hope that scientists and educators can begin creating an environment in which the next generation of cosmologists will feel comfortable exploring alternative theories of cosmology.