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The Dark Energy Problem: Methods and Mindsets

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When I retired more than ten years ago, I moved away from particle theory and toward the daunting problem of dark energy. It is in my mind the most fundamental data-based question in front of us. Over these years, I have indulged in a variety of speculative ventures, as befits the subject. There is precious little data, and this is likely to remain the case for a long time. It could be in fact indefinitely long if dark energy is accurately described in terms of a cosmological constant. If this is the case, something I myself accept as a working hypothesis, then the numbers are in—all one of them. That number in turn is already well enough determined by experiment to satisfy most theorists for a long time.

During this time, thanks to personal experience, I have built up a rather strong opinion on how this dark energy problem should be theoretically addressed. This is the main purpose of this little talk. It will be mostly about the sociology of science and how it applies to this case. And consideration of these sociological issues has pushed me toward thinking about the most basic issues of what science is and how it is conducted.

For dark energy, the odds that really new concepts will need to be introduced before properly comprehending it are especially high. Therefore I think it of primary importance that a highly diversified approach needs to be taken. I personally have studied and sampled the approaches represented by standard Einstein metric gravity, effective field theory, condensed-matter analog gravity, thermogravity, loop quantum gravity, gravity in the first-order Einstein-Cartan form (which admits torsion and CP violation in a natural way), as well as the MacDowell-Mansouri extension of first-order gravity. I also try to follow what goes on in the string theory community, despite my technical incompetence and my aversion to their often haughty, ideological stances. But I do not see much evidence of widespread cross-fertilization between these subfields. I think that is a mistake. There needs to be much more crosstalk and critical dialogue.

In an ideal scientific world, all novel ideas which are introduced into the world of scientific discourse would merit a critical response. In practice, this does not happen for a variety of reasons. It takes effort to critically dissect a crackpot idea, and a more serious off-beat idea requires even more effort. A potential critic must weigh the investment in time in undergoing such a critical analysis, and the result is almost always not to put in the effort. It becomes even more difficult to be a critic if the idea is not expressed in the language in which the critic is fluent. The net result is most often that the only critical review is by the referee of a journal to which the idea is submitted for publication. And we all know that the outcome of that process is far from ideal.

This phenomenon is connected to what I will call here dismissiveness. It is dangerous. The history of science is full of examples where the dismissing of a novel suggestion by the scientific establishment held back the progress of the field for a long time. A favorite example of mine is the rejection of Wegener's idea of continental drift. It has the advantage of being in a different field than ours. And I think there are some lessons to be learned from this example.

Alfred Wegener, a climatologist, proposed his theory in about 1915. He published a whole book putting forward his arguments, which were much more detailed than just taking a casual look at a map of the South Atlantic. And they spanned a large number of subfields, in particular geophysics and paleoclimatology. In the next fifteen years he published three more revised and updated versions of his proposal, prior to his untimely death in 1930. The geology establishment, especially in the US, considered his proposal with overt scorn ("merely poetry") all the way through the 1950's. In his case it was not inattention alone. For example, in the early 1920's, after a considerable period of dismissiveness by the community, one of the leading skeptics suggested getting more decisive facts, by bringing the lead geologist in South Africa, Alexander DuToit, to South America to compare the South American coastal geology with that on the African side. The money was found, and DuToit made the study. He found a closer match continent-to-continent than along the shorelines of each, and became a member of the minority community of experts that supported drift. Especially fascinating to me is the story of how one of the leading geologists of the day, Charles Schuchert of Yale, responded. He paid close attention to this result, and was also in frequent communication with Arthur Holmes of England, another drift advocate, who in fact had already proposed mantle convection as the mechanism behind it. Nevertheless, while suffering over the problem privately (according to correspondence dredged up by historians), Schuchert remained in public strongly anti-drift for the rest of his life. For him it was credible that crust could move around on a scale of hundreds of miles---but not thousands.

What were the nature of the mindsets that led to such dismissiveness? One is very objective: Wegener admittedly had no cogent mechanism to make the continents move. This is regarded by historians as insufficient cause for rejection, because often hypotheses at that level of incompleteness are not dismissed. Another reason is not objective: Wegener was an outsider, who never (at least at the professional level) did any field work. But, as I see it, underneath all of this was simply an inability, at the level of each individual geologist, to shake off long-held personal habits of thought. Continents simply did not move—at least not over large distance scales. It was not thinkable.

At the same time (the late 1920's) that the US community of geologists went out of their way to trash Wegener's heretical ideas, they did the same thing to a young American geologist named J. Harlen Bretz. Bretz, after performing very detailed field work in the scablands of eastern Washington, proposed an explanation-of-last-resort, based on a gigantic ice age flood which inundated half the state. The establishment geologists, despite their lack of field experience in that region, dismissed Bretz in the harshest of terms. It took another ten to twenty years before they began to come around, and accept the existence, now well established, of the Great Missoula Floods. The mindset of geologists of the time, called uniformitarianism, also impeded the acceptance of the asteroid explanation of the KT extinction of the dinosaurs, when proposed by Walter and Luis Alvarez, as well as contributing to the skepticism, typified by the story of Schuchert, regarding Wegener's proposal.

This mindset problem is extremely fundamental and very general. In addition to doing science according to the rules of the scientific method, I think it is fair to say that we are individually driven by much more than that—in particular by personal belief-systems that drive us in certain directions according to our individual tastes and opinions on what is important and what is not. I will here use the word "vision" for such a personal "belief-system". It is for sure an important positive force in doing science; pursuit of a vision makes us work much much harder than we otherwise would do.

Of course, many people will come up with very similar visions, and this can lead to the adoption of the most popular visions at a societal level. It is here where the dangers begin. Institutionalization of a vision will tend to suppress (or even oppress) alternatives. Since by definition these visions go beyond the evidential constraints set by the scientific method, they lead to the creation of non-scientific mindsets.

In the case of continental drift, the Wegener vision was for its time simply too far away from the accepted, collective vision for it to be taken seriously. But there were interesting exceptions, including the aforementioned DuToit and Holmes, as well as the physicist P. M. S. Blackett. I also think it possible that the vision held by Wegener inhibited him from doing even more than he did. According to a colleague, he visualized the continents moving around by analogy to the motion of ice floes over Arctic waters, and driven by exterior forces such as tides, even though he himself knew that the numbers did not support such an interpretation. What he (and most of his supporters) missed was the fact that the seafloors are in general also on the move. This in particular applies to the Atlantic seafloor. Wegener regarded the mid-Atlantic ridge as passive, while in fact it is volcanically active. It is the source of new lithosphere which moves outward from the rift zone. Wegener's biographer Schwarzbach expresses considerable surprise that Wegener did not catch on, given that he often traversed Iceland en route to his research sites in Greenland and witnessed first-hand this volcanism and rift structure. And Wegener certainly understood very well that Greenland was moving away from Europe, and that Iceland lay on the mid-Atlantic ridge.

It is now generally accepted that mantle convection is the driving force behind the motion of continents. As I have already indicated, it was put forward even before Wegener's death in 1930. In addition to Arthur Holmes, the proponents included Robert Schwinner, a geologist at the University of Graz. This is the same institution—even the same building-- in which Wegener himself worked, from 1924 to 1930. But he did not interact with the geologists, choosing instead the cosmic ray physicist Victor Hess. While Wegener's writings exhibit evidence that he knew of Schwinner's idea, it apparently lay too far away from his own vision of how continental drift worked.

So I think the case can be made that even Wegener was to some extent victimized by his own mindsets. Of course he can be forgiven for that, given the magnitude of his basic contributions. But it is a reminder that dismissiveness is no joke, and that we all are at risk of being victimized by it.

Anyway, all this is simply meant to underline the fact that in dealing with dark energy we need to be especially mindful of this problem. I will spare you the enumeration of a list of candidate mindsets. I am sure that you all are fully capable of doing that yourself. Maybe more interesting is the question of whether there is a candidate Wegener in our midst. In my opinion, the one who fits that role the best is

Grigori Volovik, an expert in the physics of superfluid He^3A. Grisha hails from the Landau Institute but is now largely an expatriate, in Helsinki. He has written extensively on the analogues between gravitational phenomena and He^3A (as well as other systems containing Fermi points on Fermi surfaces, such as graphene and high temperature superconductors). He in particular has an argument why the cosmological constant is so small, including why it is nonvanishingly small. This in itself gives the lie to the oft-repeated mantra that "no one has any idea as to why the cosmological constant is so small". Grisha may be wrong, but it is a fact that he is on record with a nontrivial argument to the contrary. I find that his argument is met with dismissiveness by essentially everyone, even including those who actually have heard it through. As for myself, I find it less than totally convincing, but certainly worthy of objective scientific criticism.

Unlike Wegener, Volovik's ideas have not engendered much if any controversy. Instead there is almost total indifference. I am not sure which is worse. And I want to add my own disclaimer: the odds that he is on the mark are probably small. I think there are more missing pieces in the dark energy puzzle than there were for Wegener's problem, and this reduces the odds that Volovik is on the right track. And of all the approaches that I have looked at, analog gravity a la Volovik is the most adventurous and/or speculative. But consideration of his point of view does provide a good way of learning quite a lot of interesting condensed matter physics. And it provides a long catalogue of mindsets violated by his analog-gravity point of view.

Therefore for me it is an especially useful stimulus in thinking about the dark energy problem in unconventional ways. And we should not forget that condensed matter analogies (e.g. operator product expansions and the renormalization group) played a very big role in the development of the standard model.

Before finishing, I will add just a few words on my own personal mindsets, which of course I prefer to label as my personal set of visions. These were assembled soon after I retired. While they have changed a bit since then, they have so far served me quite well. Some of them are conventional, others very idiosyncratic. I entertain multiverses and six large, compact extra dimensions, quite like the string theorists. I expect that present-day theory is effective field theory, and that beyond the Planck scale it will look very very different. I suspect that photons, gravitons, gluons, etc. are no more fundamental that phonons in condensed matter systems. Many sacred symmetries including Lorentz and gauge invariance may not survive at short distances. And I regard supersymmetry as occurring, if at all, only at energy scales at least as large as the Planck scale.

I think that the structure of the QCD vacuum influences gravity and the cosmological vacuum in a serious way, leading—if we are very lucky-- to nontrivial experimental consequences at the most fundamental, dark-energy-related level. I think this could well happen at accessible energy scales, from meV to TeV, and on distance scales no less than a fermi to no more than tens of megaparsecs. And I think that CP violation (and its companion concept torsion) may be a central theme, even at the level of pure gravitation theory.

And I am an optimist, in the sense of 1980's string theory. At that time, the dreams of a final theory centered about uniqueness and simplicity, with a minimal number of input parameters. Those dreams have evolved into the nightmare of the landscape. I think there is a chance that the next step will feature a remarkable simplicity and economy of means, and that such a vision should not be extinguished by the present-day predominance of morbid thinking.

Clearly I am dealing with a very fragile house of cards. Enough said.

Anyway, the bottom line is simple. I urge you to look at the dark energy problem from as many disparate points of view as possible. Do not fear to develop a personal vision, but please for the most part keep it personal. In public discussion, try hard to follow the rules given by the scientific method. And if someone comes by and claims that out there in space, a few tens of megaparsecs away, there are rift zones where new spacetime is being created, and that closer in, like at the edge of neutron stars, there are subduction zones where that created spacetime is being reprocessed, do pause for at least ten seconds before throwing me out of the room.