Pork-Barrel Astronomy

By: Clark M. Thomas

Abstract

Big Astronomy (BA) has a voracious appetite for big cash from public treasuries and other donors. BA promises esoteric insights into the depths of physics and astrophysics, with little more to show than stale paradigms and minimal results. Billions of donor dollars are being sought for pet projects employing large numbers of physics PhDs. Profits from typical BA projects are projected into the fuzzy future, if at all. Still, not all projects are pork, such as the great JWST. Future scenarios suggest that this brief essay could become a much needed, but widely unwelcome, corrective.

In 1610 the great Galileo used in Rome his primitive refractor telescope, with 30x power and a one-inch objective lens, to detect phases of Venus. His *cost-effective experiment* instantly demolished some 1,400 years of bogus astronomy supported by correlating maths. Ptolemy's Earth-centered model, with fancy circles-within-circles had dominated quasi-science and theocracy since late Roman times. For Galileo, his "genius reward" was to barely escape being publicly burned at the stake in Rome by local theocratic zealots, as had already happened in Rome to the young cosmological visionary, Giordano Bruno, in 1600.

Starting with the late 18th century, a succession of professional optical telescopes have been developed, and they yielded results affordably as advertised. Recently, gigantic optical reflectors, and radio telescope groupings, and even unique instruments such as the LIGO project, have discovered fascinating data. LIGO actually recorded de Broglie-Bohm "quantum sea" bow waves, not long gravity spacetime waves. Great experiment, wrong conclusions.

Most impressively, the new age of orbiting telescopes has lifted experimental astronomy above Earth's layers of atmosphere. Multiple BA instruments have been and will be sent to the orbital heavens, usually with great science returned for the money spent. The most amazing optical telescopes of all time are the Hubble and the James Webb space telescopes. Both of these orbiting optical observatories are examples of money well spent.

On the other hand, some cosmological projects are hatching that do not efficiently advance astrophysics or even physics. The worst of them are in partial production, or in the greedy eyes of science dreamers. Considering the world's rapidly emerging existential global dangers, it is prudent to separate the best new scientific performers from pretender projects. Not everything of high value can or will be funded within this century.

How Does Elegant Science Contrast With Pork?

Pure math tries to find coherent solutions for pure problems beyond the realm of physics reality. There are no provable "pure problems" within any self-referencing idealistic maths, only equation solutions that cannot even be solutions according to the great Kurt Gödel's *incompleteness theorems*. Where physics math and pure math overlap there *can* be potential progress.

When either pure or physics math goes beyond what is proper for each, we enter the realm of approaching absurdity and tautology. Although pure math has no limits in fantasy space, physics math *verifiability* is limited by standard deviations, or

sigma. *When sigmas are low, the value of such data is also low. Because we cannot measure data from most dimensions,* ***we cannot know with current theory what we don't know.***

For example, pure math can range from zero to infinity. In contrast, the realm of physics is restricted to three Newtonian dimensions, plus the vector fourth dimension. The vector aspect fully allows for all sorts of motions such as quantum spins. These dimensions are also relative to the observer's frame of reference.

The smallest real-linear 3D physics dimension is at 10^-38m logarithmic, or the size of *individual yin/yang Coulombic spheres* (which have been mischaracterized as sub-Planck quanta). The largest 4D multiversal dimension is not infinity, but approximately 10^28m logarithmic. From this limit to math infinity is absurd.

The largest number of unreal universes is set by M-theory, a variant of 20th-century 2D string theories, at 10^500 full 2D universes. Why so many? That's where the psychedelic math equations work out. There are some mathematicians who believe without proof that math predates and supersedes physics math.

There is a very important concern about just how far levels of experimental data within one actual dimension can be stretched, or extrapolated by induction and/or deduction, without becoming worthless because of low-sigma verifiability. Experimentalists are now barely down to the 10^-18m atto level; and outward only toward, but not at, the edge of our local visible universe.

In other words, *what we know and what we can know, are very much less than what we would need to know to glide from theory to high-sigma universal data.* Many BA experimental physicists ignore or try to minimize this core problem, considering it to be "meaningless" in a verifiable, logical-positivistic way. However, the philosophy of Logical Positivism was popular until the 1930s when Logical Positivism itself was shown to be fatally unverifiable. Physics needs to catch up with what philosophers resolved ninety years ago.

With regard to the wave aspect of Nature, we can only directly detect a relatively small range of frequencies from long waves up to levels of gamma waves. The accessible range of waves seems to us magical, because therein we can envision what we think is the right paradigm for such things as dark matter, and the true nature of sub-atomic quasi-quanta.

*The key to understand*ing all this dimensional mumbo-jumbo is that *proposed mega projects cannot be qualitatively much better tools than what we already have, and therefore are not likely to discover paradigm-shifting data.*

When we invest tens of billion dollars on sketchy experiments with unproven, and partially unprovable, science, then it is time to think seriously. In this increasingly challenging century, money very likely will become less available for fuzzy Big Astronomy. Any future mega project with marginal potential for existentially valuable new science, thus risks souring funding for valuable other projects with much greater potential for quality returns on affordable investments.

The risk/benefit equation is quite different for Big Astronomy pork: Building on the *correlating (but not causative)* fame of Einstein's SR and GR, *breathless hype animates pitches to hook generally clueless investors. Even the hype artists are generally clueless regarding their own absurdities:*

Proposals for spending the big bucks are couched in the search for the alleged 95 percent of the universe that is unknown, but they believe is knowable with the FCC engine. The implied idea is that discovering the essence of Dark Matter and Dark Energy will somehow enlighten us toward understanding "the meaning of life." However, brave projections do not equal knowing the basic elemets of what is chased, because of this thinking about a 21st-century problem/opportunity in terms of antique paradigms.

If any of the hype artists were to understand emerging 21st-century physics, rather than the sketchy 20th-century models

they love, they would already understand what DM and DE are. Multiple foundational science essays within the "Clark's Web Pages" section of astronomy-links.net have already pointed the way to understanding what the FCC machine can never reveal by brute force.

Bedazzled money donors are fed a *semi-religious pitch* about discovering ancient *"life"* far beyond the Earth (which probably has already been found in *primitive forms* on Mars). It is easy to hypothesize from multiple streams of data that basic life is common around the cosmos. *In sharp contrast*, it is very difficult to identify and make a risk assessment about any potential *high-level sentient life*, and where it or they may be.

For thousands of years we humans have properly wondered about advanced alien life, not about microbes. Initially, the aliens were cast as gods or demons. Now the aliens are just highly evolved potential challengers to our emerging military technology. The implied search for advanced consciousness is couched in terms of generic "search for life." It is hypothesized among most cosmologists that life itself is very common, supported by the idea of *Gaia*, where the Earth is a self-regulating organism; along with the idea of *panspermia*, where basic life is essentially all over our visible universe.

The mere presence of probable life on a unicellular level can be found in visible planetary spectra. For instance, the green of our Earth's surface points to chlorophyll. Other atoms and molecules in some planetary spectra can suggest basic life itself. However, what *level of life* "out there" are we discussing? That's the key. Should we even care about cosmic microbes "out there," when the likelihood of us humans very soon going suicidal is growing?

The most ironic scenario is where we might detect with the JWST, or other giant instruments such as radio telescopes, the likelihood of sentient life forms within a spherical radius of two hundred or so light years each way. We next broadcast EM greetings which are in turn detected two hundred years from our

now frame. Aliens could send back electromagnetic signals that arrive two hundred years later, after about 400 Earth years from our initial signal. They also could send out sub-luminal space ships, likely piloted by AI in the form of their version of androids, arriving about 500 years from our initial JWST-inspired discovery.

Then the "joke" is revealed: When the "alien" very advanced life forms (or their AI robots) get here we humans could be nearly all gone, thanks to both human *Gresham's Law* and *Murphy's Law*. Stone-age brain regions and anger centers could have triumphed over wise space-age gray matter, well before our space neighbors arrive.

The bottom line in this imaginary, but still possible, scenario is that the "nearby" advanced civilization populates Earth 2.0 with themselves as the next alpha, hyperkeystone species. By then global hot-nuke radiation will have cooled to allow a quick global repopulation, for them.

Any remnant humans will already have stopped belching CO2 into the Earth's atmosphere and biosphere. The biosphere will more quickly reset over a couple thousand years – as there won't be eight or nine billion entitled, resource-greedy humans relentlessly procreating like bunnies, and carbon polluting.

Those who promote raiding for generations our government treasuries to pay for labor-intensive Astronomy Big Pork (ABP) would never predict anything worse than a very low chance of global thermonuclear suicide. They also will discover more ways to make money from radical climate change. These views are not the best ways to insure ourselves from ourselves.

Already there are weird suggestions from Musk and others about making local Mars into the real Earth 2.0. However, just one nuclear-armed rocket sent toward Martian colonials with a "hostile flag" would end that rich-boy desert dream. Nuclear war on Earth without any missiles sent to Mars would have the same effect, as the colonists would immediately have no new supplies.

Projects of Some Value With Minimal Pork

As great as the dual Keck observatories (and a few others of similar size) have been, there is little more that can be squeezed out of that model of increasingly massive Earth-based reflectors. The next generation of mega-reflectors will feature new adaptive optics; easy ground upgrades and maintenance; and of course much greater light gathering *per unit of time* from much larger reflectors.

Currently three prime locations for observatories of gigantic size are either in early construction, or seriously proposed. Those areas are the Canary Islands off Africa, Hawaii's Mauna Kea, and the Andes high deserts. None of these fine terrestrial sites are anywhere as pristine as the million-miles-distant JWST, putting a lid on their relative scientific potential.

I am not concerned about additional expenses for one or two of the newest gigantic telescopes. It or they will reveal something of value, but hardly more than what the JWST will reveal: The JWST, like the Hubble, can spend days imaging any deep field it wishes. Photography is all about collecting photons; and photons can add up either with larger reflectors, or more exposure time.

Projects of Minimal Value and Maximal Pork

It takes proper knowledge of the smallest electromagnetic entities to understand the largest dimensions. In other words, astrophysics is a subset of physics, and not the other way around. No prettified infrared JWST pictures will provide the fundamental answers for elusive so-called Dark Energy and real Dark Matter. Likewise, there is no way any particle accelerator's incrementally increased power can overcome bad foundational particle theory.

Interestingly, the key to resolving the flaws of great particle accelerators is already known. We don't need to reinvent the wheel, just to use it properly. The very idea behind Special

Relativity begins with relative, accelerating frames of reference. Einstein used special frames of reference properly, but still didn't get the full scope of different acceleration frames around one event. GR goes too far into an alternate reality, which is why GR is challenged by quantum particle physics.

All circular particle accelerators start with fixed guns that chase around the circle tube magnetically-guided elementary particles. The FCC will propel protons more massive than what the LHC now uses. The faster a target, the faster the fixed gun must launch its beam particles. There is a speed limit for both, according to mass and acceleration. In other words, think of Newton's **F=ma**, and Einstein's vectorized Special Relativity.

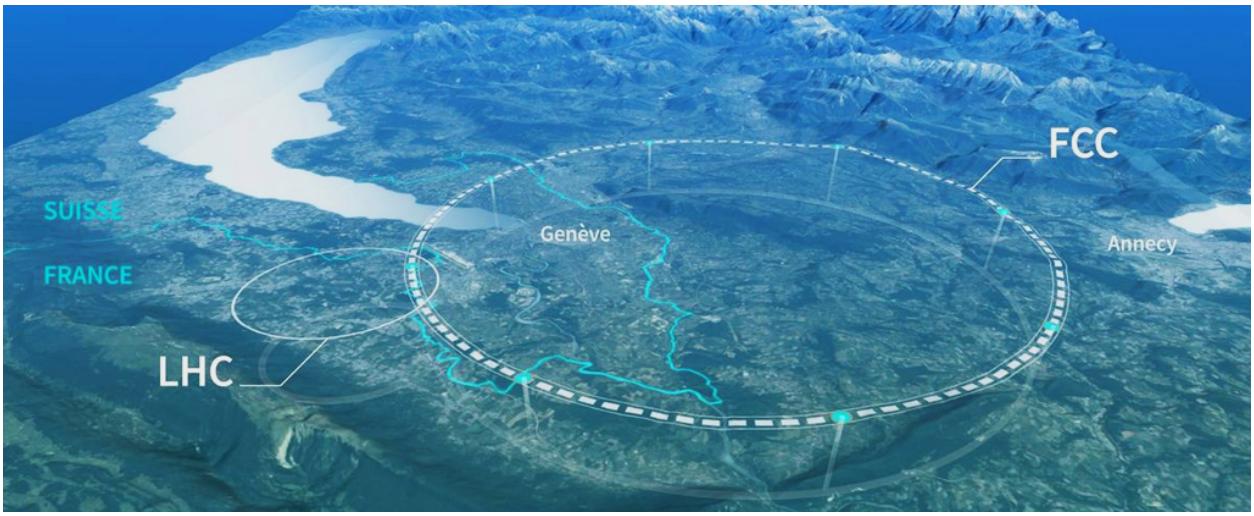
Particle-gun projectiles can catch up with their target particle streams – but by then their relative speeds are not very different. When that happens the impacts are weak, and little of new value emerges, despite all the cash spent to not get there.

The main difference between the Large Hadron Collider and the Future Circular Collider will be in their projectile inertial masses, with protons needing more energy in the FCC. There is no amount of money that can *subluminally* launch a proton into *hyperluminal* speeds with one stationary gun or guns.

The LHC launches sub-atomic small hadron particles around the circle in opposite directions. This is not a way for achieving superluminal energies. It is a way to energize two subluminal terminal speeds to more closely approach the luminal, which is a quantitative improvement not yielding a qualitative change. The LHC has not proven its circular model to be better than a linear model, which some scientists prefer. It is doubtful that the FCC would be much more successful with its deluxe circular paths.

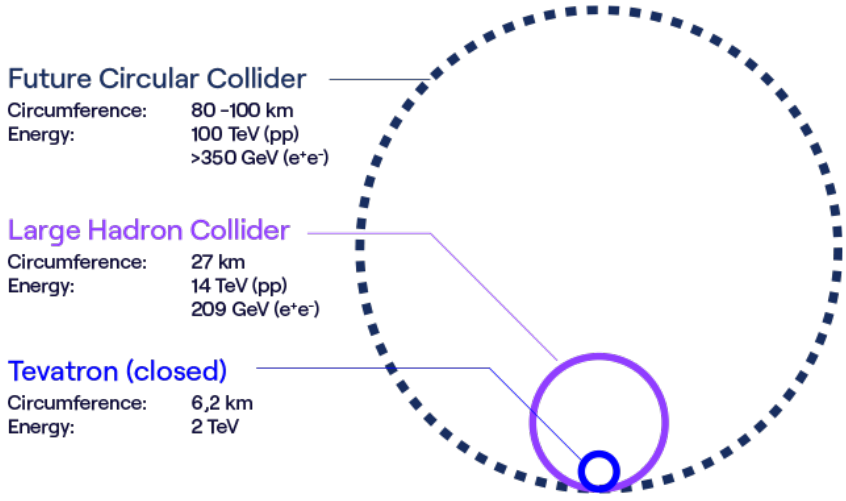
In other words, a few more elusive "large hadron" particles should be caught by a much more powerful accelerator. However, *a change in quantity of romanticized large hadrons does not thereby yield a change in quality of what we learn and weakly*

model. See this essay for another perspective on what hadrons mean to physics



The next deluxe collider is described within the drawing below. It is projected under best-funding guidelines to be operational by the 2040s, and to reach peak power in the 2070s. *Three major problems emerge with this timetable*:

First, there is no hard promise of revolutionary physics to be operationalized anytime within this century.



Second, the now projected cost of about twelve billion dollars will balloon with cost overruns bringing the final total to something from twenty to thirty billion dollars. For what?

Third, for the full Future Circular Collider experiment to reach its projected peak power in the 2070s, *new types of guiding magnets will first need to be invented* to achieve it.

That's right: We could be planning to spend up to thirty billion cost-overrun science dollars for futuristic operational science not yet invented because of theory ignorance, if it ever will be.