Is cosmology just a plausibility argument?\textsuperscript{1}

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Abstract: I review the basis and limitations of plausible inference in cosmology, in particular the limitation that it can only provide fundamentally true inferences when the hypotheses under consideration form a set that is exhaustive. They never do; this recommends abandoning realism. Despite this, we can adopt a scientifically correct pragmatism and understand aspects of the cosmological model with enormous confidence. I illustrate these points with discussion of one certainty—expansion—and two current controversies—the existence of large extra dimensions and the possibility that the matter distribution forms a fractal on large scales. I argue that the existence of large extra dimensions is certainly plausible, but a fractal universe is untenable.

1 Radicals, plausibility, and The Beagle

In my time in the nineteen-nineties working as a student of Roger Blandford, he asked several things of me that have rung in my head. At that time the matter density of the Universe could have been $\Omega = 0.1$ or $\Omega = 1.0$, the Hubble Constant could have been 50 or 100 (in the usual units), hot dark matter was as good as cold, the initial conditions could have been adiabatic or isocurvature or worse, and only a fringe believed there might be cosmic acceleration. Fluctuations were discovered in the cosmic microwave background, the ten-meter telescopes were coming on-line, the highest known galaxy redshifts jumped from 0.4 to 3, and gamma-ray bursts became confidently cosmological. Among other things, Blandford and I hoped to constrain some of the fundamental properties of the Universe with Keck and Palomar observations of hundreds of galaxies in the Hubble Deep Field and various

\textsuperscript{1}A contribution to the meeting Exploring the High Energy Universe in honor of Roger Blandford. Published on the arXiv only. Copyright 2009 David W. Hogg. You may copy and distribute this document provided you make no changes to it whatsoever.
other “selected areas”. Here are three things that Blandford asked me:

(1) “Where are the young radicals?” In this period, the notable objectors to cosmological orthodoxy included Arp, Burbidge, Hoyle, and Segal, proponents of quasars as galaxy ejecta, steady-state models, periodic redshifts, chronometric cosmology, and a range of other relatively untenable ideas. Even among the mainstream, those over the age of 60 were much more likely to fairly discuss a crazy idea about the Universe than anyone under the age of 40. When the youth—those who are the engines of experimental research, data analysis, and new projects—don’t care about outside chances, the outside chances never get properly tested, ruled out, or investigated for the seeds of new and more promising ideas. That’s not healthy.

(2) “Why don’t you gather up all the phenomenology, pack your carpet bag, and sail around the Cape in The Beagle?” Phenomenology was pouring in from telescopes across the spectrum and in such detail that absolutely no theoretical model could be consistent with even a small fraction of it. This continues today: No theoretical model of galaxies simultaneously explains all the rich phenomenology in the scaling relations, mass functions, star-formation rates, chemical abundances, morphologies, and clustering. Why are we taking so much more data when we can’t even make good progress on what we have? Stop gathering and publishing incremental snippets of confusing phenomena (was the implication) and start trying to understand how it all fits together; this will require a long period of concentration! When you get back from the trip, write The Origin of Galaxies and begin a period of maturity for observational cosmology.

(3) “Isn’t that just a plausibility argument?” Is the galaxy autocorrelation function (for example) a power law because the underlying dark-matter structure forms a power-law, or because auxiliary aspects of galaxy formation conspire to make a non-power-law dark-matter autocorrelation function into a power-law galaxy autocorrelation function? Here are two explanations of the same phenomenon. How to decide between them, when (at the time) there is (was) no means to distinguish them directly with an observation? Any argument about this was purely in the realm of plausibility; both sides agree on the phenomena, so the differences of scientific conclusion are just differences in what seems plausible. Now, is everything in

\[\text{We failed, although I, for one, had a great time.}\]

\[\text{This is just an example; of course now we know the answer from weak lensing studies (for example, Sheldon et al. 2009).}\]
astronomy—where we can’t do controlled or repeated experiments, and have (almost) exclusively electromagnetic channels for observation—a plausibility argument? Can we know anything?

I will argue in what follows that these three questions are related. The short answer to the last one is “yes”; observational science is a science of plausibility, and plausibility arguments are an unavoidable part of doing business. Nonetheless, as you know—and as I will try to complexify—we know many things in cosmology with great certainty.

2 Bayesian science

There is an unfortunate set of battles going on in physics about statistics—“frequentism” against “Bayesianism”—that are distorting the meanings of both terms. Although if you read my recent papers you will know who I support in this war, I do not want to discuss this question here. I want to take back the word “Bayesian”; I want to use it to describe a way of reasoning about propositions for which you do not have enough information to decide, absolutely, whether each is true or false.

When you don’t know if a proposition $H$ is true or false, you must assign some degree of plausibility to the proposition. You probably want that degree of plausibility to meet several desiderata, including that (1) degrees of plausibility can be represented by real numbers, that (2) they obey common-sense criteria, like that if the plausibility of $A$ increases, the plausibility of not-$A$ decreases and that plausibility of the joint hypothesis $A$ and $B$, for instance, be related in sensible ways to the plausibilities of $A$ and $B$, and that (3) they obey consistency requirements, like that the plausibility depends only on the evidence, and not the order in which that evidence is considered or other irrelevancies. If you place requirements like these, you are led inexorably and provably to Bayesian inference, with posterior probabilities taking the place of the “degree of plausibility” you seek, and Bayes’s Theorem relating those posterior probabilities to likelihoods and prior probabilities.

Because these are good desiderata, and because (as a community at least) we are relatively rational, the progress of astrophysics really does follow some kind of approximation to Bayesian reasoning. We are Bayesians, even if we don’t want to be; even those arguing that we shouldn’t be doing data

\footnote{This is demonstrated, among other places, in Jaynes (2003), an amusing and problematic text. Jaynes gives credit for the argument to Cox.}
analysis by Bayesian inference are, themselves, in their scientific reasoning, Bayesian. We assign degrees of plausibility to hypotheses by considering our prior knowledge (for example, the consistency of the hypothesis with other data, other things we strongly believe to be true, and the simplicity of the hypothesis for communication to others) and the success of the hypothesis in explaining the observed data (this would be in the form of something like the likelihood), and we combine the prior and the likelihood (approximately) multiplicatively.

As an aside, if I am right, one consequence is that the Popperian view that theories can only be falsified is (itself) false. In any contentious scientific issue, there are competing, mutually exclusive hypotheses. No observation ever completely rules out one hypothesis, because there are finite observational uncertainties, because any hypothesis can always be complexified in relevant ways, and because any observation can be excused from applicability. Any observation that undermines (reduces the posterior probability of) any hypothesis $H_1$ also, in doing so, supports (increases the posterior probability of) at least some mutually exclusive alternative $H_2$, because the probabilities of all the conceivable mutually exclusive hypotheses must sum to unity. Thus, contrary to what we are taught in grade school, observations do just as much “ruling in” as “ruling out”. This realization justifies our feeling—which Popper would not have allowed—that the cosmic microwave background and large-scale structure observations of this past decade strongly endorse or support the ΛCDM model with adiabatic initial conditions. We are right; they do. On the other hand, see my comments on exhaustiveness, below, which may undermine this argument.

As another aside, if I am right, another consequence is that we often find ourselves working with models that are, in detail, a bad fit to the available data. Strictly, if we were not Bayesians, but instead making decisions on the basis of the absolute value of the likelihood (the probability of the data given the model) or some equivalent, we would never continue working with models that aren’t a pretty good fit to the data. And yet, we often perform inference with models that are, technically, not a good fit. This is because inference is not possible without a model, and so we must use the best models we have, whatever their absolute likelihoods.

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5I am exaggerating Popper’s much more subtle view here for emphasis; this should not be taken as a statement about Popper himself.

6If you want examples of all of these, see the literature of the last decade comparing detailed galaxy properties with the ΛCDM model.
3 The impossible demands of realism

Bayesian reasoning is the calculus of plausible inference. What we would all like—and I think what Blandford was getting at with his questions—is an understanding of the fundamental processes that govern the Universe, an understanding that is not just useful for calculation but an understanding that is true in some deeper sense. Typically, a scientist sees the latter point as either obvious and important, or else completely irrelevant. I would like to argue that we don’t have a choice; there is some very clear sense in which truth is not what is returned by any finite scientific investigation; all that is returned is plausibilities (some of which become very very high), and those plausibilities relate not directly to the truth of the hypotheses in question, but rather to their use or value in describing the data.

The fundamental reason scientific investigations can’t obtain literal truth is that no scientific investigator ever has an exhaustive (and mutually exclusive) set of hypotheses. Plausibility calculations are calculations of measure in some space, which for our purposes we can take to be the space formed by the union of every possible set of scientific hypotheses, with their parameters and adjustments set to every possible set of values. Bayes’s theorem tells us how to adjust the relative probabilities of the hypotheses (and, in detail, the relative probabilities of different parameter settings) in the set as each new datum arrives. This procedure is provably correct, which is good, but it only returns the correct hypothesis—the hypothesis that really does generate the physical world we are observing—when the original set of hypotheses is exhaustive. That is, we only get to be realists when we have considered every possible hypothesis. Since the hypothesis space is almost certainly infinite in all relevant senses, realism—the belief that confidently established scientific results are (or can be) literally and fundamentally true statements about the world—requires infinite computing and human resources. If you didn’t perform an infinite amount of computation, you did not find the truth (unless you were unimaginably lucky).

This is not bad news; in fact this liberates us to take a pragmatic view, perform finite inference with finite hypothesis spaces, and choose the best models we have for further study. If we go all the way, we can even call the very best models in this sense “true” and only be misleading to the most

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5For example, Kolmogorov (1965), among others, proved some relevant things. Amazingly, Jaynes (2003) contains many absurd pieces of advice in which these infinities are treated as trivial.
epistemologically rigorous. A good analogy comes from coding theory. By far the best compression algorithms for transmitting data losslessly over a channel involve building generative models of the data, where sender and receiver have both agreed on prior information, or prior probabilities for the settings of those models. As the message is encoded, the model is made better and better, and the transmission of the message is, in some sense, aspects of the posterior probability distribution for the parameters of the model plus residuals. The receiver obtains the posterior parameters, predicts the message, and uses the residuals to adjust it to a lossless copy. At no point does the sender or the receiver ever have to ask whether their model of the message is “true”. They only have to decide whether their model leads to a substantial shortening of the message. In case you think all of this is crazy, this is exactly how the Galileo spacecraft data were encoded and transmitted following the failure of the high-gain antenna. The sender and receiver, in this model, are not seeking the “truth”; they are both pragmatists. They recognize that the exploration of a larger model space would lead to a shorter message, but that exploring a larger model space might violate constraints they have on time-to-encode, time-to-decode, buffer size, or computation. Scientists should take as their role models not priests, who reveal truth, but signal encoders, who improve everyone’s life on a daily basis by pragmatic accomplishment.

4 Expansion

It is important to note that pragmatism and plausibility are not obstacles to extremely confident scientific conclusion. On the contrary, we know many things about the Universe with great certainty. One example is that the Universe is expanding; this has been established beyond any doubt, even

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8McEliece & Swanson (1999).

9There is a perfect analogy between signal encoding and Bayesian inference and the literatures of the two have converged. See, for example, MacKay (2003). That is, plausible inference becomes exactly identical to signal encoding in an odd fantasy in which the goal of a scientist is to losslessly communicate the observations with the shortest possible message. If sender and receiver agree on prior information, that shortest message will consist of the posterior probabilities of models and model parameters, plus residuals. For all this to work, of course, the protocol for communicating the posterior probabilities must be chosen to be one that is optimal for the mutually agreed-upon prior probabilities, where I am using “optimal” here in the sense of Shannon (1948).
when viewed in this pragmatic light. Consider the evidence:

Almost all galaxies are observed to have spectral shifts consistent with redward Doppler Shifts, and more apparently distant galaxies tend to have larger shifts. Now with calibrated supernovae, the distance–redshift relationship is measured with great precision and beautifully consistent with expansion. All measures of intensity seem to vary with redshift consistent with the Tolman relation, as expected if the Universe is governed by Lorentz symmetry (so the shifts can be interpreted as Doppler Shifts). Along the same lines, the Cosmic Microwave Background has not just the spectrum but also the absolute intensity of a blackbody, as expected in an expanding model. All observations of cosmologically distant objects are consistent with the Universe being denser and hotter in the past. All reasonable cosmological solutions to general relativity involve either expansion or contraction. Finally, the only successful physical models of structure formation at present live in an expanding background that is consistent with the observed Doppler Shifts.

The expansion model has an extremely high likelihood—it explains well the data—and an extremely high prior probability—it is consistent with our knowledge from other domains such as the theories of electromagnetism and gravitation. Note that it has no, and has never had any, serious alternative. Oddly that doesn’t reduce our confidence.

Even if we do find a better explanation than expansion—and I seriously doubt that we ever could—it won’t really replace expansion as an explanation, it will complexify, adjust, or complement the expansion explanation. See, for example, the replacement of Newtonian gravity with Einsteinian gravity; Newtonian gravity is not seen by anyone as really wrong, it is just a quantitative limit of the better, more complete theory. This point plays into the realism point as well, but I have to admit I don’t know which way. I think it shows that scientists are not realists at heart, even when they think they are. But it is also related to the fact that inference does often return good results even when models are simplistic; this could—in some sense—be

\footnote{This paragraph ought to be instrumented with thousands of citations, but a random and unfair subsample might include Hubble & Humason (1931); Mather \textit{et al.} (1994); Songaila \textit{et al.} (1994); Riess, Press, & Kirshner (1995); Pahre, Djorgovski, \& de Carvalho (1996); More, Bovy, \& Hogg (2009).}

\footnote{It is obligatory to mention “tired light” at this point, but only to note that it is not a theory advocated by any scientist; it is only a straw man built to illustrate the strength of the expansion hypothesis.}
the meaning of the word “approximation” or “limit”.

5 Fractals

ΛCDM is a ridiculously successful physical model of the Universe on very large scales. It explains, simultaneously, the observations we have that are relevant to the expansion history, the angular spectrum of perturbations in the cosmic microwave background, the growth of structure on large scales, and the abundances of large, collapsed objects. The large-scale structure observations and predictions cover an enormous range in scale, cosmic time, and growth factor. As with expansion, it is difficult to imagine another theory truly replacing ΛCDM: The worst-case scenario for ΛCDM at this point is that it will be seen forever as an exceptionally successful approximation to the true theory; one that permits easy calculation of a multitude of accurately observed phenomena.

There are many issues with ΛCDM at small scales, many of which I work on, in the hope that we will be obliged to complexify the over-simple model and open up new space for fundamental discoveries. But the successes on large scales are such that any modifications to ΛCDM must be made carefully so as not to disturb the large-scale successes. In short, at this point we can see the large-scale success of ΛCDM as establishing good certainty for the model.

Perhaps the most trivial prediction of all for ΛCDM (and its physically motivated competitors, such as the DGP model discussed below) is that the Universe has a mean density—all physical cosmological theories are calculated on a background that is homogeneous on the largest scales. In a recent set of papers, this fundamental prediction of the model has been tested; some investigators argue that this prediction is not consistent with the observations. The Universe does not have to have a mean density, of course, but if it doesn’t, then it is—in some sense—a fractal, or fundamentally inhomogeneous. My view is that homogeneity is well tested and qualitatively and

\footnote{Once again, the reference list could be long here, but an unfair sampling of the very most recent results might include Tegmark et al. (2004), Eisenstein et al. (2005), and Komatsu et al. (2009).}

\footnote{I comment on this motivation in Hogg (2005).}

\footnote{Sylos Labini et al. (2009a), Sylos Labini, Vasilyev, & Baryshev (2009b).}
quantitatively in agreement with ΛCDM\textsuperscript{15}. But let’s imagine, just for the purposes of argument, that the observations did suggest, at some level, that there is no mean density.

Fractals are beautiful and approximations to fractals (fractal-like functions confined to a limited range of scales) are abundant in the natural world. Certainly the massive galaxy–galaxy auto-correlation function is close to a single unbroken power law over all scales on which we can measure it\textsuperscript{16}. So it is tempting to think about inhomogeneous models.

Unfortunately—and importantly—there are no quantitative inhomogeneous models. There are no solutions to general relativity with an inhomogeneous matter distribution. Because of inevitable distortions to the metric and expansion history, observables are impossible to compute (for example, even the distance–redshift relation would become unusable), even if a background were known. The idea that the Universe is inhomogeneous makes no quantitative predictions and explains nothing, so it is not a scientific contender, even if the evidence against a mean density becomes quite strong. Note the blow this strikes against pure realism.

A fractal model can only become a contender in one of two ways: Either the evidence against a mean density must get so strong that it outweighs the success of every other prediction made by the homogeneous ΛCDM model, of which there are many. This condition is so far from being met, I can’t see any way to meet it, even if we measure the redshift of every luminous galaxy inside the horizon. Alternatively, someone can devise a method to compute an inhomogeneous model and predict a number of observables and show that the fractal model does as well as or better than ΛCDM. I don’t know enough to know if this is possible, but I don’t think there are even any strategies for executing this ambitious theoretical program; probably they would have to be numerical. That said, I do not doubt that any success in this field would have a big impact on the study of gravity even if it doesn’t turn out to be a good fit to the data. Here is a subject with which someone ambitious could profitably pack up and sail on The Beagle.

Back to reality: There isn’t at present good evidence against a mean density; homogeneity is well established and in agreement with the ΛCDM

\textsuperscript{15}We performed a straightforward test in Hogg et al.\textsuperscript{(2005)}, designed to be insensitive to unknown issues with calibration and evolution. My view is that a combination of issues with the data and with galaxy evolution create the results of Sylos Labini et al.\textsuperscript{(op. cit.)}; the Hogg et al. test is more robust.

\textsuperscript{16}The largest range of scales is shown in Masjedi et al.\textsuperscript{(2006)}.  

predictions. An inhomogeneous universe is so intractable that there is almost no near-term future in which we are likely to be able to either observe or compute anything interesting in this area. This is an apparent controversy, but in fact great confidence is warranted. The potentially disturbing aspect of the story I have told here is that the confidence comes in part from the intractability itself! But of course a pragmatist is perfectly happy with that.

6 Large extra dimensions

Despite the great success\footnote{Actually, some of my colleagues would say that \( \Lambda \)CDM is a \textit{failure} because it makes use of a cosmological constant that it so far from either a particle-inspired value or zero that it is extremely implausible \textit{a priori}. That is somehow related to all of this.} of the standard \( \Lambda \)CDM model—or perhaps because of it—the world of fundamental cosmology is bristling with new ideas. One of the most interesting new ideas is that 3+1-dimensional gravity is just what we observe of some higher-dimensional model because we are (somehow) confined to a lower-dimensional subspace. The most worked-out example of this is the DGP model, which contains a simple idea but for which it has been challenging to do precise calculations.\footnote{The DGP model was introduced by Dvali, Gabadadze, & Porrati (2000); a cosmological (homogeneous) solution was found by Deffayet (2001); a cosmological (homogeneous) solution was found by Deffayet (2001); and growth of structure was calculated by Hu & Sawicki (2007) and Scoccimarro (2009) among others.}\footnote{This likelihood ratio comes from Fang \textit{et al.} (2008); a very different conclusion is reached by Sollerman \textit{et al.} (2009) with a more careful analysis but of a smaller data set.} Because the current cosmological solution in DGP has a single parameter that determines the properties of the background expansion, it has the same global freedom as the \( \Lambda \)CDM model; that is, the two models have (more-or-less) the same number of parameters. In a straightforward comparison between GDP and \( \Lambda \)CDM and a basket of observations there is \( \Delta \chi^2 \equiv 2 \Delta \ln \mathcal{L} \approx 20 \), in favor of \( \Lambda \)CDM.\footnote{This likelihood ratio comes from Fang \textit{et al.} (2008); a very different conclusion is reached by Sollerman \textit{et al.} (2009) with a more careful analysis but of a smaller data set.} On the face of it, without evaluating the calculation of the likelihood ratio or any controversy related thereto, this is very bad news for DGP. Is the existence of large extra dimensions ruled out at high confidence?

There are considerations here that prevent any trivial answer to this question. The first is that neither model is a good fit to the data. This means either (1) that both models would lose a model comparison with some better, third model, or (2) that both models must be complexified fundamentally with additional physics, or (3) that the observational uncertainties have been under-estimated. In the first case, it is irrelevant that \( \Lambda \)CDM is preferred to
DGP, because ΛCDM is disfavored over all; that is, the test between ΛCDM and DGP does not establish ΛCDM against its truer competitor. In the second case, the addition of new physics will inevitably give both models more freedom; it is not clear which model will more naturally obtain the freedom necessary to obtain a substantially higher likelihood when compared with the observations. The model comparison, at present, is between two models that have had certain relevant physics “switched off” and the amount that affects each model is likely to be different. In the third case, the investigator is encouraged to think generally about what can be wrong with the observational uncertainties: Are there likely to be some data points that are rejectable outliers? Do all data points have underestimated uncertainties? Which data points are qualitatively similar or similar in origin? Once these questions are answered, the uncertainties in the observational uncertainties must be modeled, with parameters fit simultaneously with the fundamental cosmology parameters, and then marginalized out for the model comparison. This procedure, if performed symmetrically for the two models (as it must be, since it relates to the data alone) will inevitably reduce the magnitude of the relative likelihood of the two models.

The second complexity relates to the issue that the DGP model as currently calculated is a very specific and highly non-linear theory, not all the details of which are understood. The calculation of the growth of structure is probably much more general than DGP, in the sense that it is a calculation in an effective theory generated by DGP, that could in principle be generated by many other fundamental theories. But it is also the case that the DGP theory, in a different background or with different brane properties, might be able to support other effective theories for the growth of structure. That is, a test of the growth of structure in the DGP model as currently calculated is not a direct test of the existence of large extra dimensions itself; it is a test of an extra-dimensions-motivated alternative gravitational theory. There is a lot of theory to be done to bring us closer to understanding what fundamental modifications to gravity (subject to Solar System and other constraints) bring about what effective cosmological theories, and what the predictions are for each of those effective cosmological theories. Unfortunately, given the precision of contemporary data, only a small family of effective theories is going to end up being consistent with the data; much of this time-consuming theoretical work will lead to important but null results.
7 The multi-armed bandit

These considerations play into the decision-making of what Blandford called the “young fogeys”: Although putting a sustained effort into calculating and testing alternative gravity theories could result in some beautiful physics, until it looks like it has a good chance of raising the posterior probability of some model above the orthodoxy, that career segment may languish uncited and lacking in influence on the business. The “old turks” have more to spend and less to lose.

There is a class of problems in decision-making known as “multi-armed bandits” in which a gambler is presented with a machine with $K$ levers, analogous to a slot machine with one lever, where each of the $K$ levers has different and unknown probabilities for different payoffs. At each round, the player must decide which lever to pull, where each pull may provide some reward and will definitely provide some information. The details of a player’s strategy can depend strongly on the player’s utility, or what the player is trying to achieve. This will depend, in turn, on things like tolerance for risk, cost of lever pulling, and discount rate for future cash flow.

This toy problem—as odd as it sounds—is an analogy for the performing of scientific investigations, and indeed the problem was initially raised in the context of adaptive experimental design. Every morning, an investigator must decide what project to concentrate on—what “lever to pull”—whether to work on incremental improvements to an orthodox model or develop or test some radical model; the investigator must make this decision without knowing precisely how much he or she will be paid (or charged) for the choice. And, perhaps disturbingly (but related to issues of exhaustiveness above), the number of levers that the investigator can pull is far, far larger than the number of times he or she gets to pull one: There are far more good ideas (at least in cosmology) than there is investigator-time to explore them. The investigator’s utility is the key. In work on experimental design, the utility is usually imagined to be purely related to information or knowledge about the question at hand. But in real decision-making, the utility involves not just questions of knowledge, but also of real-world costs and benefits. In these

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20 This problem was effectively introduced by Robbins (1952); there is now a long literature on strategies.

21 And, of course, the levers change their payoff distributions as context changes, and new levers become available as new scientific opportunities arise, so the pure multi-armed bandit problem is actually far less general than the dilemmas of a scientist.
matters, the utilities are very different for young and old scientists, where younger scientists ought to have less tolerance for risk and a higher discount rate for future payoffs (because they need to get their PhD or postdoc or faculty job or tenure with a short time horizon), and older scientists ought to have more tolerance and a lower discount rate. Hence the relative orthodoxy of the youth.

Despite all this, at present there are in fact a substantial number of the youth working on ideas as speculative as extra dimensions and fractals and even more speculative. Some of my young colleagues are looking for observational signatures of universe–universe collisions! It is possible that our community has recovered somewhat from Blandford’s complaint. There are many respects in which cosmology appears far more exciting now than it did in the nineteen-nineties, despite the fact that the $\Lambda$CDM parameters have all been constrained so well that cosmology is now referred to as a “mature science”. I, for one, have learned that there is life even in middle age.

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