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# Conference Summary: Observational Cosmology

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**Abstract**

I review the major concordances and controversies of the meeting concerning observations. Cosmologists are nearing agreement on the global cosmological parameters of the Universe. A few parameters still have large error bars, notably $\Omega_{\rm m}$, but most of these windows will close soon with upcoming accurate observations of the cosmic microwave background. The major era of chasing the major cosmological parameters is now closing. Understanding galaxy formation in all its messy detail will continue to occupy cosmologists for some time to come. Development of highly accurate standard candles, such as inspiraling black holes, holds out hope for understanding $\Lambda$, whether it is truly constant or evolving. This is the major prospective contribution from cosmology to fundamental physics in the next generation. I close with a discussion of anthropic cosmology. Anthropic reasoning has been shown to be correct at least three times in the history of cosmology. Applying it now leads us to take seriously the prospect of other universes, a notion that should be pursued seriously by theoreticians.

## 1.1 Introduction

If the birth of cosmology can be reckoned from the first in-depth survey of the Universe beyond our Galaxy, that birth dates to the beginning of the 20th century, when the first comprehensive photographic survey of external galaxies was compiled by Keeler at Lick. Dated thus, cosmology has reached its centennial milestone, coinciding providentially with the centennial milestone of the Carnegie Institution of Washington. At this conference we therefore celebrate not one but two birthdays, and it is appropriate to appraise the progress in our field after a century of effort.

The efficiency of the human scientific endeavor is such that one hundred years of work in a field typically yield enormous fruit, and cosmology is no exception. Given the remoteness of the object of our study in both distance and time, what we have learned in 100 years is nothing short of stupendous. The basic outlines of the subject are now known, the fundamental questions have been framed, and many have even been answered. We now have a theory for the global geometry of spacetime, we have a general description of the time evolution of the system from inflation to now, and we are closing in on the values of the dozen or so global parameters that are needed to characterize the observable Universe. We have mapped the heavens and seen structure, and we have a rough theory for why that structure exists and how it developed. In short, the state of our field can accurately be characterized as “mature.”

In this ocean of knowledge, three islands of ignorance still stand out. The first is the messy baryonic physics of galaxy formation, which is challenging but not in any sense fundamental. It will keep us busy for another couple of decades but will ultimately yield to the heavy artillery of future high-powered computing. There is no fundamentally new physics to be discovered here.

The second island is the domain of the question, what came “before” the Big Bang, and indeed, whether we can presently frame any meaningful question at all along these lines. The “cause” of the Big Bang takes us to the very depths of epistemology and natural philosophy. I shall have a few words to say about this at the end of this talk, but the basic conclusion is that, as scientists, we do not appear ready to grapple meaningfully with this mother of all questions at the present time.

The third island, by virtue of its in-between status, is the most exciting for exploration now. This is the realm of the *dark energy*. Dark energy is not so far removed from established principles of particle physics that it is unapproachable by known methods, and it is also accessible to measurement via observations of the distant Universe. Its pursuit promises to enlarge our basic concept of “thing” by providing an entirely new thing to consider in addition to radiation and matter, and in the process it may cause us to examine what other new “things” can in principle exist. It also compels us to question the future evolution of our Universe and whether new physics might appear in the future that will govern its ultimate fate (I am here invoking an analogy to early inflation, whose physics was the mother of our present-day Universe). We are very lucky, I think, to have discovered this third land, as cosmology would otherwise be confined today to the challenging yet basically trivial pursuit of galaxy formation, versus the fundamentally impenetrable and inapproachable realm of the Big Bang. Dark energy provides something meaty yet doable in between and promises to keep cosmology intellectually vibrant for some time.

I was amused at this conference at how some of us evidently feel discomfited by recent success. We are closing in not only on broad concepts but also on specific details, yet some attendees felt (almost reflexively?) compelled to express doubt. Consider, for example, Virginia Trimble, who said pessimistically, “I wouldn’t bet the window on $\Omega_{\rm m}$ to be any smaller than 0.1 to 0.5 with 95% confidence.” Or Malcolm Longair, who anguished whether “we might be extrapolating too far...,” “there might be surprises..,” or “fundamental misconceptions” in the present picture. Or Bernard Sadoulet, who wondered whether the discovery of $\Lambda$ might even signal “the first hint of a failure of gravity.” I began to wonder myself whether WIMPs had actually been detected at this conference for the first time!

But MACHOS were also here in quantity. Mike Turner exuded typical confidence with statements like: “There are no current controversies,” and “Now that we’re closing in.” Andreas Albrecht advised us to “stop whining [over small discrepancies] and get to work!” I personally identify more with the MACHOs—the data are fitting so well together (by virtue of nonzero $\Lambda$) that it seems highly unlikely that the whole edifice could be seriously undermined at this point. Musings about “missing something big” strike me as wishful thinking that the game of chasing cosmological parameters might continue indefinitely, when in fact a major era is closing.

Tactics in this end game are shifting, reflecting the new situation. When a science is young, a single observer with a simple apparatus can make a paradigm-shifting discovery.

Thus, we cosmologists used to go to the telescope with the goal of measuring a single number, like $H_0$ or $q_0$. No more. With the possible exception of $H_0$, hardly any experiments measure a single number any more, but rather increasingly complicated functions of the form $F(h, \Omega_{\rm tot}, \Omega_{\rm m}, \Omega_{\rm b}, \Omega_\Lambda, \sigma_8, b,...)$. Where these surfaces intersect is the sweet spot of the Universe. Each experiment in this new era has only a piece of the truth, and this has led to a new era of coordination, cooperation, and intercomparison, but the old satisfaction of pioneering on alone is somewhat diminished.

## 1.2 Current Status and Prognostications

That said, recent progress has been outstanding, and I list here five major experiments that all of us would agree have provided big breakthroughs.

- *The $H_0$ Key Project* value $72 \pm 8$ (Freedman, Jensen) came just in time to anchor measurements of other cosmic parameters from cosmic microwave background (CMB) experiments and redshift surveys.
- *CMB surveys* (BOOMERANG, MAXIMA, DASI, CBI; Wright) confirmed the flat universe predicted by inflation, and the rest of the CMB spectrum tightly constrained other combinations of important cosmic parameters.
- *Type Ia supernovae* (Filippenko, Perlmutter) directly detected $\Omega_\Lambda \approx 0.7$, exactly the right value to fill the empty gap between $\Omega_{\rm tot} = 1$ from the CMB and $\Omega_{\rm m} \approx 0.3$ from large-scale structure (LSS) and dynamics. Cosmologists might have wrangled endlessly about the reality of an $\Omega_\Lambda$ deduced arithmetically from $\Omega_{\rm tot}$ and $\Omega_{\rm m}$, but the direct detection from Type Ia's has virtually put that controversy to rest.
- *Big Bang nucleosynthesis* (Tytler, Steigman) yielded the first model-independent estimate of $\Omega_{\rm b}$, from primordial deuterium.
- *Massive nearby redshift surveys* (2dF, Colless; SDSS, Bernardi) tightened the noose on large-scale structure, constrained $\sigma_8$ and bias, $b$, and demonstrated the power of huge samples for studying structure formation. Bringing the fire-power of these surveys to bear on galaxy formation will be the logical next step.

From this cascade of recent data, certain fundamental conclusions have emerged:

- The Universe is essentially flat: $\Omega_{\rm tot} = 1$, as predicted by inflation.
- Dark matter exists and is mainly nonbaryonic. Taking $\Omega_{\rm b} = 0.04 h_{70}^{-2}$ and $\Omega_{\rm m} = 0.15$–$0.35$ (see below), we find $\Omega_{\rm tot}/\Omega_{\rm b}$ in the range 4–8, with the value of 1 totally excluded.
- $\Omega_\Lambda$ is nonzero and in the range 0.65–0.85. This is implied indirectly by the previous two bullets but also comes directly, as I have noted, from Type Ia supernovae.
- Primordial fluctuations created in an early epoch of inflation planted the seeds for later formation of galaxies and large-scale structure via gravitational instability.

In contrast, discussion at this conference shows that several important parameters still remain insecure:

(1) The total window on $\Omega_{\rm m}$ quoted at this meeting was 0.15–0.35, which is still quite large. Neta Bahcall marshaled an impressive case for 0.2, but many of the methods she quoted are fairly model dependent. Values of $\Omega_{\rm m}$ from peculiar motions, large-scale structure, and the CMB in contrast tended to hover near or above 0.3 (Dekel, Colless, Bernardi). The best strategy is to wait, as the most accurate value will come from the next round of CMB measurements (*WMAP*).

(2) Similar controversy swirled around $\sigma_8$, which is expected because most methods measure the product $\sigma_8\Omega_m^{0.5-0.6}$, so when one goes up the other goes down. Quoted values of $\sigma_8$ ranged from 0.7 to 1.0, with Bahcall favoring the higher range and Colless the lower. New data from the Cosmic Background Imager (Readhead) on short wavelengths also favor $\sigma_8 \approx 1$. In retrospect, the $\sigma_8$ scale is a bad choice for normalizing the power spectrum because it sits between Ly$\alpha$ and large-scale structure surveys, so that quoted values from these data sets are often entangled with the assumed spectral index, $n$. The final value of $\sigma_8$ will require a joint analysis of the entire body of fluctuation data (CMB, LSS, Ly$\alpha$), which again awaits the next round of CMB data.

(3) We still do not understand the density run of matter in galaxies, and uncertainty here generated a modest rear-guard attack on the Key Project value of $H_0$. Broadly, dark matter halo models (e.g., Navarro, Frenk, & White 1997) predict a fairly shallow dark matter mass profile on the scale of galaxy-galaxy lensing, $\rho \propto r^{-\eta}$, where $\eta$ is about 2. This is consistent with gravitational lensing time delays only if $H_0 = 50$ (Kochanek). For power-law mass models, $H_0 \propto (\eta - 1)$; hence, to permit the Key Project value of $H_0 = 72$ would require $\eta \approx 2.5$, which is already that of the light alone; adding dark matter would only make this shallower, reducing $H_0$ below 72. At smaller radii the picture is also confused. Here, adiabatic contraction of dark matter by baryonic infall should produce rather steep central slopes and densities (Blumenthal et al. 1987), yet a variety of information points to the contrary, including inner galaxy rotation curves (e.g., de Blok & Bosma 2002; Swaters et al. 2003), lensing around central cluster galaxies (Ellis), rotation curve amplitudes (Alam, Bullock, & Weinberg 2002), the amplitude of central bars (Weiner, poster paper), and the scarcity of dwarf galaxies (Dekel).

The disconnect between theory and observations for $\rho(r)$ in halos has led some to posit warm or self-interacting dark matter (reviewed by Silk). Alternatively, halo models may need revision to include stirring of dark matter cusps via bars or mergers (Katz), strong feedback and consequent ejection of baryons (Silk, Dekel), photoionization to retard baryonic infall (Katz), and stripping (Katz). A third possibility, not much discussed here, is a tilt to the fluctuation spectrum, $n$, to reduce the strength of fluctuations on galactic scales, though that might imply late structure formation and consequently galaxy formation too late to match the number counts of Lyman-break galaxies and early QSOs (Somerville, Primack, & Faber 2001). My guess is that the $\rho(r)$ discrepancy will be resolved by a combination of several small things, such as small changes to the lensing time delays (which are accurate to only $\sim$15%; Chartas, Treu), improvements to galaxy collapse models (as noted above), a small reduction in small-scale fluctuation amplitudes via spectral tilt, and a small downward adjustment of $H_0$ to the mid-60's, still within the current Key Project window. Should future data confirm the high value of $H_0 = 72$ from the Key Project, this will exacerbate the tension between model slopes and density data, motivating further scrutiny of the collapse models.

(4) The above discussion makes clear that $H_0$ is not quite as rock-solid as we would wish, and so it is worth reviewing the values of $H_0$ discussed at this conference. In addition to the revised Key Project value of $72 \pm 8$ (Freedman, Jensen), four other independent values were mentioned. The combination of the CMB plus nearby large-scale structure is consistent with $H_0 \approx 70$ (Colless). The CMB itself implies that $H_0 \approx 70$ if the Universe is perfectly flat. The maser galaxy NGC 4258 implies an upward correction of 10% to the Key Project value to 80 or so (Freedman), but the photometry is not *HST*'s best and should be redone. Finally, there are gravitational lenses, which, if $\eta$ really is 2.5, imply a downward correction to $H_0 = 50$ (Kochanek). Several speakers lamented the shaky distance to the Large Magellanic Cloud,

which underpins the Key Project value. Some also mentioned a downward correction as large as 10% to the Key Project value owing to our location in a local low-density bubble. My own guess, $H_0 = 65$, is a compromise reached by stretching the error bars of all methods to achieve overlap. Finally, if cosmologists are willing simply to *posit* that the Universe is exactly flat, CMB data will determine the value of $H_0$ to exquisite accuracy, and that might be in fact be our final adopted value.

(5) David Tytler's review of $\Omega_b$ from Big Bang deuterium was illuminating yet a bit scary. The number of QSOs that has been analyzed is only five, of which only two are really firm. Steigman stressed that there are residual discrepancies for both He and Li obtained when using the $\Omega_b$ measured from deuterium. Given the scatter and small numbers, I am concerned that the data presently do not merit an error bar as small as the 10% that is often quoted. Again, this question will be resolved soon when the second peak in the CMB spectrum is accurately measured.

The following table is a playful attempt to predict how some of these issues will be resolved in future:

2006 $H_0 = 65 \pm 3$ is determined by using a combination of the CMB and LSS.

2008 Dark matter is detected in the lab as the lightest supersymmetric particle; somebody (Bernard Sadoulet?) wins the Nobel prize.

2008 *GAIA* claims to measure $H_0 = 75 \pm 3$ by providing accurate parallaxes for a few Cepheids but cosmologists take no notice; the distance-ladder approach is by then deemed dead.

2010 Moore's law rescues "gastrophysics." The cusp/concentration problem with baryons and dark matter in galaxies is resolved via a bit of everything: fewer baryons falling into galaxies, stirring of central cusps by bars and mergers, expulsion of gas (especially in dwarfs) by winds, etc.

2012 *LISA* measures $w$=$-1.01\pm0.01$, $w' = 0.03\pm0.05$ using the newly developed "ultimate" standard candle, inspiraling central black holes (Phinney). Confronted finally by what appears to be a genuine cosmological constant, string theorists retire in droves.

2015 The fourth and final reanalysis of *Planck* data teases out the tensor $B$ modes (Cooray, Zaldarriaga)...but barely at the 2-$\sigma$ level of significance. Undeterred, NASA trumpets to Congress, "Now we've *really* seen the face of God."

2030 Neal Katz retires, having finally removed all but one free parameter from galaxy formation models. The one remaining remaining free parameter is, of course, the star formation rate.

The fifth entry concerning inspiraling black holes is particularly interesting. If these objects can be made into few-percent standard candles, as Sterl Phinney outlined, they hold the prospect for actually measuring $w'$, and thus testing whether $\Lambda$ is indeed constant or evolving.

## 1.3 Anthropic Cosmology

I close this summary by coming out of the closet as a believer in anthropic cosmology. Much has been written on this topic, both pro and con, some of it needlessly complicated, and I'd like to take this opportunity to state my view, since I envision anthropic reasoning to play a greater role in cosmological discussions in the future. Anthropic arguments are a kind of data, though not of the conventional kind.

To illustrate, consider the plight of an intelligent cosmologist back in the geocentric Aristotelian era. In the then-current world model, the radius of the Earth would have looked like a fundamental constant of the Universe, analogous to the radius of curvature of today's Universe. Thinking like a modern cosmologist, our Greek would have felt compelled to understand where this value, the radius of the Earth, came from. There would have been two choices: search for an argument rooted in physical principles that shows why this radius could have one and only one conceivable value, namely that observed. Or, posit the existence of an infinite (or at least very large) ensemble of spherical bodies with a spectrum of radii, and then argue that the actual Earth must occupy a narrow window within that spectrum that is picked out *a posteriori* by the existence of life as we know it on this Earth (the precise location of the radius within this window would be random.) The first route might be termed the "physics approach." I argue that the situation of the Greek is not fundamentally different than the situation today with modern cosmology, and that the physics route, if followed 2000 years ago, would have been demonstrably sterile. The correct approach for the Greek would in fact be the anthropic approach.

So far, this is familiar, but I now make three points that have not been stressed previously. First, cosmology has been faced with many explanatory challenges in the past similar to the ones we now face, and in all cases the correct approach was in fact anthropic. Classic examples are the nature of the Sun, the nature of the Solar System, and the nature of the Galaxy. We now see that there is nothing unique about any of these objects and that their properties were in fact picked out of a much larger ensemble by anthropic requirements. Thus, anthropic arguments are not speculative, they have been proven correct several times over.

The second point is that an anthropic argument makes sense only if you accept the actual existence of the larger ensemble—*even if you have not yet observed it.* The larger ensemble is not merely hypothetical, it is really out there! This is the real power of an anthropic argument—*not* to explain a particular cosmological parameter but to alert us in to the existence of a much larger (though possibly still unseen) ensemble. Again, this is borne out by history. In the above cases, astronomers actually went on to discover semi-infinite ensembles of suns and galaxies, and we are now (2000 years later) well on our way to verifying a semi-infinite ensemble of planets. In all cases, the singular object that we were once so fixated on was revealed to be merely a member of a much larger sample.

The third point is that, once the anthropic approach is invoked, the thrust of the science shifts from trying to explain the properties of the singular object to understanding the *properties of the ensemble.* Not, why is our Sun the way it is, but rather, what does the total ensemble of star-like objects look like? What are the physics of stars in general, how do they form, what is the range of properties spanned by the class as a whole, and how does one characterize a given star within the class? Again, this has been the standard route of astronomical inquiry, which has borne tremendous fruit.

In our present situation as cosmologists, I argue that taking an anthropic approach to explaining the properties of our Universe is a rational strategy based on historical success. This presumably includes not only the dozen or so macroscopic cosmological parameters cataloged by Freedman in her review (this conference), but also all the messy 40-odd parameters of the particle physics Standard Model (however they might ultimately be revised). Together these parameters are simply the suite of numbers needed to characterize our Universe. Anthropic reasoning then leads us to accept, or at least hypothesize, the existence of the larger ensemble, namely *other* universes. It would be reassuring as we do so to have at least a glimmer of a physical process that might have created that ensemble. Fortunately, the speculative fringes of particle physics and cosmology have come up with a few ideas revolving around chaotic inflation and multiple dimensions. Unfortunately, there does not seem to be the prospect of directly observing these other universes any time soon. Nevertheless, that should not stop us from trying to deduce their properties, any more than the Greek cosmologist should have been deterred from thinking about other planets. His attempts might have failed for lack of proper tools and understanding at the time, but they could not in any sense have been termed "unscientific."

My last point is a slight stepping back from a purely anthropic approach and is occasioned by the fact that at least one cosmological parameter, $\Omega_{\rm tot}$, *has* been explained by appeal to fundamental physical principles. The fundamental process generating $\Omega_{\rm tot}$ is inflation, which in its simplest version produces a very flat universe. Inflation, in turn, is believed to occur under a wide range of conditions, and is thus regarded by particle physicists as at least generic, if not ubiquitous. The case of $\Omega_{\rm tot}$ thus warns us that perhaps not all current cosmological parameters are equal, in the sense that some may one day be derived from others via generic physical arguments. To be more precise, I am reasoning here that the capacity of our Universe to support inflation (and hence have $\Omega_{\rm tot} \approx 1$) was determined by a combination of other, more fundamental cosmological parameters. If this reasoning is true, it suggests that a continuing job of cosmologists will be to discover hidden relationships among the current parameters based on physical principles, and in the process to reduce the total number of independent parameters. This activity will resemble conventional physics, i.e., explaining one thing by another. However, the history of astronomy and cosmology strongly suggests that even diligent application of this method will leave some number of cosmological parameters ultimately unaccounted for. The leftover ones will be the truly fundamental ones and thus, I argue, will have to be accounted for anthropically. The frontier will shift to discovering the properties of the larger ensemble spanned by these remaining fundamental parameters. Not, why is our Universe the way it is, but rather, what does the greater ensemble of universes, the *Meta-universe*, look like?

In closing, I would be the first to admit that the anthropic explanation for our Universe does not provide the ultimate explanation for the Universe that we are looking for. It does not address the origin of the Meta-universe, and hence postpones by only one step the reckoning of ultimate causes—why something exists rather than nothing, and indeed whether that question has meaning. Perhaps the Theory of Everything, if it lives up to its name, will explain the existence and properties of the Meta-universe from first principles. Until then, I find it thrilling to contemplate the possibility of multiple universes in parallel with our own, and count that awareness a major step forward in the growing cosmological sophistication of our species.

There is a convention in astronomy (not strictly observed but still useful) that capitalizes the name of an object when it refers to our local example, as distinct from an object in the larger ensemble. Thus, "earth" becomes "Earth," "sun becomes "Sun," and "galaxy" becomes the "Galaxy." In keeping with this tradition, I suggest that anthropic cosmologists might capitalize the word "Universe" when referring to our own, to express explicitly our willingness to contemplate the existence of the larger ensemble.

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