"It ain't necessarily so"
Sportin' Life - G. Gershwin (1927)

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Summary

The Standard Model for cosmology is fixed in the minds of most particle physicists, and even many astronomers, as the Einstein-de Sitter case, in which there is a negligibly small cosmological constant and negligibly small space curvature. This model does have a lot to recommend it, but it is worrisome that the bulk of the recommendations come from theorists. I propose therefore to indicate here why a more skeptical attitude might not be out of order.

Theory

I will adopt the usual general relativity homogeneous and isotropic cosmological model, in which the expansion rate satisfies

$$\left(\frac{1}{a}\frac{da}{dt}\right)^2 = \frac{8}{3}\pi G\rho \pm \frac{1}{(aR)^2} + \frac{\Lambda}{3}.$$

Here a(t) is the expansion parameter that scales with the mean separation of conserved particles, ρ is the mean mass density, a(t)R is the radius of curvature of surfaces of constant density, and Λ is the cosmological constant. The present value of the expansion rate is Hubble's constant,

$$H = \left(\frac{1}{a}\frac{da}{dt}\right)_0.$$

In the Einstein-de Sitter model the last two terms in the first equation are negligible so the present mass density is

$$\rho_{crit} = 3H^2/(8\pi G).$$

The density parameter is the ratio of the actual present mean density to this value,

$$\Omega = \rho_0/\rho_{crit}.$$

Now let us review the reasons for the lack of popularity of the curvature and Λ terms. Zel'dovich noted that $\Lambda/8\pi G$ has the appropriate properties for a Lorentz covariant vacuum. However, Λ/G is the fourth power of an energy, ε, and if Λ were comparable to H^2, then ε would be ~ 0.01 eV. I have not found a particle physicist who is willing to take this seriously. My private speculation, revealed here for the first time, is that that is because of a reluctance to think that the fate of the universe could hinge on something with the energy scale of chemistry.

In the inflationary picture the curvature term would be negligibly small. To my mind the beauty of this picture is that it offers a mechanism that produces the remarkable homogeneity of the observed part of the universe - expansion during a quasi-de Sitter phase stretches the lengths associated with inhomogeneities to values much larger than our horizon, H^{-1}. It would surely follow that the other relevant length, $a(t_0)R$, is much bigger than H^{-1}, so the curvature term is negligible. I like this argument but feel uneasy about the direction: it uses inflation to justify dropping the curvature term, rather than asking whether this prediction of inflation, which is not all that well tested, really is true.

We have finally the coincidence argument introduced by Bob Dicke. Observations say that $G\rho_0$ is comparable to H^2. We note that ρ scales with expansion at least as fast as $a(t)^{-3}$. It appears that the universe must have expanded by a considerable factor to have been able to make the 2.75 K thermal background. It follows from all this that we can extrapolate the expansion back to a time when $G\rho(t)$ was very much larger than Λ or $(a(t)R)^{-2}$, so that the Einstein-de Sitter model was a good approximation. And we can trace this Einstein-de Sitter phase back in time to some extremely small value of a, at maybe the Planck time, or maybe the end of inflation, at which the initial conditions for classical cosmology would have been set by deeper physics. At this epoch $(a(t)R)^{-2}$ and Λ would have been smaller than $G\rho$ by an enormous number of orders of magnitude, but by just that factor that made one or both of them become comparable to $G\rho$ just as people came on the scene. We avoid this startling coincidence if we assume that we are still in the quasi-Einstein-de Sitter phase.

I admire this argument but note that coincidence can be misleading. A search for a good example, with the help of Scott Tremiane, yielded this one. There is an upper bound on the luminosity, L, of a stable star with given mass, M, from the condition that radiation pressure should not exceed gravity. At distance r from the star the gravitational force on an ion with mass m is

$$F_g = GM\,m/r^2.$$

The flux density of light is $f = L/4\pi r^2$, so the rate of transfer of momentum to an electron is

$$F_\ell = \sigma L/(4\pi\, r^2 c),$$

where σ is the Thomson scattering cross section. If there is one free electron per ion then the stability condition is $F_g > F_\ell$, which we see translates to an upper bound on L/M independent of r, the Eddington limit. This figures heavily in the astrophysics of X-ray sources and of massive optical stars. For example, Allen's Astrophysical Quantities puts the luminosity of a 60 solar mass main sequence star at $L \sim 1 \times 10^6$ solar luminosities, which works out to

$$L/M = 3 \times 10^4 \text{ ergs sec}^{-1}\text{ g}^{-1},$$

close to the Eddington limit. My good friend David Schramm has a mass of 90 kg and a luminosity that the Handbook of Chemisty and Physics puts at

$$L = 4500 \text{ kg cal/day}.$$

The ratio is

$$L/M = 2.4 \times 10^4 \text{ ergs sec}^{-1}\text{ g}^{-1}.$$

Though Schramm's mass is thirty orders of magnitude less than that of the star his mass-to-light ratio differs from the star by only 20%, an amazing coincidence!

Picking out the significant coincidences is a fine art at which Dicke is a master. The Dicke coincidence has been a powerful stimulus to research,

and I am willing to believe that it points in the right direction. But picking out coincidences also is a bit of a gamble, as my example was meant to illustrate, so we might feel even more convinced if the observations pointed in the same direction.

Observations

The parameters Ω and Λ are measured by the classical cosmological tests, which look for relativistic corrections to counts and angular sizes or apparent magnitudes of objects as functions of redshift, z. (1 + z is the ratio of observed wavelength to wavelength at emission.) Progress has been blocked by the fact that objects at high z are observed at great distances and hence as they were in the distant past; it is not unreasonable to suspect that these young objects were systematically different from the old ones seen in our neighborhood. How can we separate evolution from the wanted cosmological effects?

Loh and Spillar[1,2] have developed an elegant procedure. They note that the observed flux density, f, from a galaxy at redshift z is down from its intrinsic luminosity, L, reckoned at the present epoch by the square of the luminosity distance, D(z), and by a factor , E(z), to account for evolution. If E were independent of L then the frequency distribution of f at fixed z would be the distribution of L scaled by D^2E. So Loh and Spillar fit the observed distributions of f in fixed bands of z to an assumed universal function with two free parameters, D^2E and normalization, N(z). Within the assumptions the latter is independent of evolution. Having adjusted cosmological parameters to fit N(z) they can use the cosmological model to determine D(z) and so learn how big is the evolution factor, E(z). If E were large one certainly would worry about the assumption that E is independent of L. As it turns out, their results suggest E at z = 0.6 is ~ 20% larger than at z = 0, which is not so bad. Their fit to N(z) constrains one parameter. If they assume $\Lambda = 0$, they get

$$\Omega = 0.9^{+0.7}_{-0.5},\ |\Lambda| \ll H^2,$$

at 95% confidence. For zero space curvature the limit is

$$\Omega = 0.9^{+0.4}_{-0.2},\ |aR|^{-2} \ll H^2,$$

again at 95% confidence.

This is encouraging for the Standard Model, where $\Omega = 1$. But while the Loh-Spillar method clearly is promising we might do well to reserve judgment on the final answer from such a difficult suite of observations until people have had time to think of what might possibly have gone wrong. Astronomers do worry about the assumption that E is independent of L; Loh and Spillar will have a partial check of this by asking whether the observed joint distribution of f and z can be fit by a universal function with just two free functions, N(z) and D^2E. I will be following the debate with great interest.

Another observational approach uses dynamics to measure the mean mass, m, of a galaxy, and counts of galaxies to find the mean number density, n, so

$$\Omega(d) = mn/\rho_{crit}.$$

The great problem is the familiar fact that the Newtonian gravitational acceleration in a centrally concentrated mass distribution is insensitive to the mass exterior to the point (identically so in the limit of spherical symmetry). Thus if we estimate m from the circular velocity v at the outermost visible part of a galaxy we miss any dark mass present in a more extended halo. We can measure whole galaxy masses by looking at the dynamics of galaxies in groups and clusters. The results, almost without exception, are apparent values of the density parameter in the range 0.1 to 0.3.[3,4] This is some 30 times the mass in the bright central parts of galaxies, but still below the critical value, $\Omega = 1$.

Since there are dark halos around galaxies why not dark halos around groups and clusters of galaxies? In the biased galaxy formation picture one argues that seen galaxies might preferentially form in dense spots, making galaxies more tightly clustered than is mass, so that the mean mass per galaxy in a cluster is biased low. This requires a coincidence, or conspiracy:[5] the formation of galaxies and the formation and evolution of clusters of galaxies would have to have conspired to make the mass bias quite similar in regions as different as our neighborhood, where the mass density is about twice the mean, and rich clusters, where the density is some 100 times higher than that.

There are other conspiracies. Dave Schramm notes that the density parameter in baryons wanted to get reasonable production of light elements in the hot Big Bang is $\Omega(b) \sim 0.1$ (if H is small, to give a reasonable time scale). The conspiracy here is that the matter we are pretty sure is present, baryons, is just about abundant enough to account for dynamics, $\Omega(b) \sim \Omega(d)$; in an Einstein-de Sitter model the mass $1 - \Omega(b)$ which has to be exotic (not baryons), coincidentally is the amount missed by dynamics. Another example is that rotation curves of spiral galaxies (circular velocity v(r) as a function of distance, r, from the center) are pretty close to flat. In Newtonian mechanisms this means the mass within r scales as $m(< r) \propto r$. The mass in the inner parts and to or past the radius at which v(r) levels off is very plausibly assigned to the seen stars; dark mass, where it is needed, is invoked in the outer parts.[5,6] If the dark mass were exotic then the baryonic and exotic components, with such different physical properties, would have to have conspired to arrange themselves so $m(< r)$ scales as r from the baryonic interior to the exotic exterior. This becomes even more problematic under the standard biasing scheme in which the galaxies that form first in dense spots destroy the galaxies that try to form later in less dense spots. If baryons could be stripped from unwanted galaxies why would the ratio of baryons to exotic matter be so tightly controlled in seen galaxies? If the dark matter in the halos of galaxies were baryonic the control would seem less mysterious because galaxy formation would at least start with material with uniform physical properties. In fact, we need dark baryonic matter, because the baryon mass density wanted for nucleosynthesis exceeds what is seen in stars. Do we really want to invoke two types of dark matter, exotic and baryonic?

These problems would be eased if the net mass density were $\Omega \sim 0.1$. This is well outside the Loh-Spillar bounds, so we would have to assume their very careful study missed something; another conspiracy. But we could argue that the mass wanted for nucleosynthesis is the mass seen by dynamics, which would be a considerable triumph.

Conclusions

My impression is that the cosmological model that best describes the present epoch is not yet known beyond reasonable doubt. If your fundamental theory predicts $\Omega = 1 \pm 0.01$ you would be right to regard that as encouraging, but I don't think you could argue that the theory has passed an experimental test. If you are designing detectors for exotic matter I hope you understand that I am not expressing doubt about the importance of the search; it would be hard to overstate the impact of a positive outcome. Of course, what that means is that there is no guarantee the stuff exists.

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