

Λ CDM cosmology: how much suppression of credible evidence, and does the model really lead its competitors, using all evidence?

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ABSTRACT

Astronomy can never be a hard core physics discipline, because the Universe offers no control experiment, i.e. with no independent checks it is bound to be highly ambiguous and degenerate. Thus e.g. while superluminal motion can be explained by Special Relativity. data on the former can never on their own be used to establish the latter. This is why traditionally astrophysicists have been content with (and proud of) their ability to use known physical laws and processes established in the laboratory to explain celestial phenomena. Cosmology is not even astrophysics: all the principal assumptions in this field are unverified (or unverifiable) in the laboratory, and researchers are quite comfortable with inventing unknowns to explain the unknown. How then could, after fifty years of failed attempt in finding dark matter, the fields of dark matter *and now* dark energy have become such lofty priorities in astronomy funding, to the detriment of all other branches of astronomy? I demonstrate in this article that while some of is based upon truth, at least just as much of Λ CDM cosmology has been propped by a paralyzing amount of propaganda which suppress counter evidence and subdue competing models. The recent WMAP3 paper of Spergel et al (2007) will be used as case in point on selective citation. I also show that when all evidence are taken into account, two of the competing models that abolish dark energy and/or dark matter do not trail behind Λ CDM by much. Given all of the above, I believe astronomy is no longer heading towards a healthy future, unless funding agencies re-think their master plans by backing away from such high a emphasis on groping in the dark.

1. Introduction: on the shoulder of giants

The history of science is full of examples of major breakthroughs being made by radical thinkers, those who tend to ask silly questions and ‘chain their coffee mugs to radiator pipes’. No doubt, the rise and fall of great scientific hypotheses are always brought about by the availability of crucial new data, and if credit is truly given to every source where credit is due, then the engineer who serviced one’s departmental Xerox machine should also be included

as part of the ‘team’ which made a miracle possible. But what provides the shoulder upon which a theoretical giant stands, is usually the ideas behind the design of an important experiment, or new and revealing ways of approaching an old dataset. These invariably come from a small number of highly creative minds. I therefore begin this essay by voicing my unreserved support to the courageous gesture of Simon White in his recent article ‘On why dark energy is bad for astronomy’ (White 2007), and by expressing open disagreement with the comments of my great and long time Imperial College friend Matt Mountain, who said (Nature 2007, 447, 122) that cosmology in the present era may no longer be driven by the chosen few. Matt’s implication here, it would seem, is that this field is now so special and privileged that whatever we learned from the history of *any* branch of science has no bearing on it, and can be disrespected.

2. Contemporary cosmology: rising above the shoulder of giants

It is not my intention to go through the past reminiscences of Λ CDM - not even the near-term past. I wish to make just one point, viz. that the version of the model as we often hear about it today has not been around for much longer than a decade. It was only in the early 90’s, when I invited the late Prof. David Schramm (Chicago) to deliver a lecture at UC Berkeley on the age of the Universe, that he showed a plot of the time evolution of the ‘most favored’ value of the cosmological constant. His graph looked like a sine curve with an average of zero. Thus there is not so much scope for arguing that cosmologists expected Λ CDM, less so predicting it.

Has Λ CDM cosmology ‘transcended’ the scientific method? Before we seek to answer the question, I first wish to emphasize that I believe in astronomy we should be particularly proud of our own tradition, and should from time to time ask this same question without feeling insulted and irrespective of our own sub-field of interest, *because* it was the astronomers who brought the world into the era of ‘modern’ or ‘enlightenment’ science, i.e. we should set a *stricter* standard for ourselves. It all began with what happens up there - the heavenly motion - which could suddenly be explained in terms of what happens down here - what keeps our feet on the ground. Newton acquired the status of the ‘Father of modern science’ by his ability to use the ‘known’ to demystify the ‘unknown’ (hence to dispel the ‘fear of the unknown’ that haunted the dark ages with myths and superstition). The history of astronomy since Newton is filled with glorious stories of how the unfamiliar phenomena found in ‘remote’ corners of the Universe could be reduced to something directly or closely related to experiences in our daily environment. In Table 1 I provided a few recent examples of such revelations which most of us feel at home with. These are to be contrasted with recent developments in cosmology, in which *every* key observational result was ‘explained’ (and rewarded with some of the most prestigious prizes of our times) by postulating completely new physics which

received no laboratory verification. Are we effectively endorsing the critics of astronomy funding (our *real* enemies), who advocate that we can afford to play such games because ‘so little is at stake’ in cosmology?

Perhaps one should nevertheless pause and ponder. Charging under the banner of Einstein’s extreme eminence and his forbidding theory of General Relativity, have cosmologists been over-exercising our privileges? Should all of us succumb to Matt Mountain’s Nature testimony that ‘times are changing, abandon traditions and get used to it’? Indeed, even among the examples of Table 1 there are cases in which astronomers helped to bolster (strengthen) the claims and assumptions of physicists, i.e. they serve more than the purpose of merely dispelling ignorant superstitions about unusual celestial manifestations. Thus e.g. a white dwarf star provided the unique battleground, unachievable in the laboratory, in which two separately well tested laboratory phenomena of electron quantum degeneracy and proton classical gravity compete with each other. Given that none of the other elementary particle interactions have been unified with gravity, it is very important and re-assuring to know from this piece of astrophysics that at the average inter-particle distance of white dwarf matter quantum and classical effects live their own existences and can even play tug-of-war. Another example (not in Table 1) which pushes the argument even further concerns solar neutrinos. Here, the discrepancy in numbers between Bahcall’s prediction and observations was at least in part responsible for the eventual discovery of neutrino oscillations in the physics community.

Yet the above interesting cases of ‘feedback’ from astronomy to physics means the only good news for astronomers (including cosmologists) is that we could occasionally be employable to help do some real physics. There is no further room for loftiness: we have *not* seen or heard a single story in which a drastic shift in our understanding of the physical world was initiated and confirmed using astronomical data *alone*, and the reason (which has nothing to do with tradition) is as follows. Whether the subject matter is as fundamental as time and space, or something more mundane, astronomical observations can never by themselves be used prove ‘beyond reasonable doubt’ a physical theory. This is because we live in only one Universe - the indispensable ‘control experiment’ is not available. There is no possibility of ‘flipping this switch’ or ‘turning that knob’. Using SN1a data alone, one will never clinch Λ CDM to the level of rigor of Maxwell’s equations, because there will always be the niggling doubt, however little, of whether SN1a are standard candles. Large samples of data always help, e.g. to probe foreground matter using weak lensing shear distortions, one needs to observe no more than a few background quasars if the intrinsic shape of each is known; but without this ‘control’ we can only rely on the statistical behavior of many quasars. Alternatively, ‘cross checks’ by merging together many diverse and independently acquired datasets, a favorite approach of P.J.E, Peebles, will also serve the purpose. Yet in

cosmology both such efforts inevitably lead to a exponential proliferation of costs, and in the end neither will ever replace the simple action of ‘flipping the switch’. Hence the promise of using the Universe as a physics laboratory from which new incorruptible physical laws may be established without the support of laboratory experiments is preposterous (Matt, perhaps you should get used to *this?*).

3. Cosmic microwave background (CMB): a clean and direct test of its origin?

I now venture the ‘how dare you’ question, on the origin of the CMB (and always bearing in mind that Feynman must find his own way of convincing himself). How do we know that it is the afterglow of the Big Bang? Here are some of the usual responses I heard of, since I was a school boy some 30 years ago.

- *It comes from a redshift of 1,000.* How do you know the redshift of the CMB? We do not have any characteristic emission or absorption line - there is not even a straw to clutch.
- *It comes from every direction in the sky, and is extremely uniform.* Well? I recall the days when BATSE aboard CGRO discovered that gamma-ray bursts (GRBs) are isotropically distributed. Yet, unlike the cosmologists, the GRB community seems to be more careful in going about the conjecture. There was a Great Debate at the Shapley-Curtis level, between Paczynski and Lamb, on how much can one make of the distance scale of GRBs from the isotropy of the source distribution. Cosmologists took a shortcut - the conclusion was drawn without a Great Debate. Since the CMB is more fundamental than GRBs, as I expect my colleagues in the GRB community would probably agree, why a Great Debate never took place on this subject?
- *The CMB spectrum is a perfect black body, pointing clearly and unequivocally back to the era of strong coupling between matter and radiation.* Yet how much does this constitute a *proof*, as opposed to mother nature laughing at us having completely missed some physical process that takes place in e.g. empty space?
- *The CMB temperature was predicted by Big Bang theoreticians.* Well, the prediction by Gamov was off by an order of magnitude. Where do you draw the line? How would you like the temperature of your room be increased ten-fold?
- *There are tiny temperature anisotropies in the CMB that can beautifully be explained in terms of Big Bang cosmology.* This is provided one assumes the anisotropies are also cosmological in origin, and that the early Universe comprised dark matter, dark energy, and underwent a mysterious epoch of ‘inflation’ to secure a delicate balance of proportions between the two ‘dark’ components. Can we frankly say that this is a *clean and straightforward*

Phenomenon	Explanation	Seminal Paper	Based on Laboratory Established Physics?
Planetary orbits	Universal gravitation	Newton	Yes
Tides	Universal gravitation	Newton	Yes
X-ray Bursts	Thermonuclear Flashes	Woosley, Taam	Yes
Her X-1	Accretion	Hayakawa, Matsuoka, Prendergast, Burbidge	Yes/Maybe
Superluminal Motion	Special Relativity	Martin, Rees, Albert Einstein	Yes
White Dwarf Star	Quantum Physics meets Gravity	Chandrasekhar	Known physics individually verified

Table 1: Examples of recent achievements of astrophysicists in re-assuring mankind that unusual phenomena in the sky do not have to mean bad omen: they can be explained in terms of the physical laws here on earth that we are familiar with.

Phenomenon	Explanation	Based on laboratory established physics?	Verifiable in future experiments?
Redhsift	Expansion of Space	No	Unverifiable (Chodorowski 2007)
CMB	Big Bang	No	Far Future
Rotation Curves	Dark Matter	No	Near Future (As always)
Distant Supernovae	Dark Energy	No	Far Future
Flatness and Isotropy	Inflation	No	Remote Future

Table 2: Cosmologists *only know how to use* ‘unknowns’ to explain ‘unknowns’ (and hence are not really astrophysicists). In mainstream physics new postulates are sometimes made to help account for unexpected phenomena found in the laboratory, but the Universe is not a laboratory because one crucial fundamental criterion: the need for control experiments, cannot be met.

proof, when so many other strings and loose ends are attached?

- *How about all of the above? Do you have a better interpretation of the CMB?* In no reasonable court of law will a suspect be convicted of murder simply because there have been no other arrests or suspects.

It is clear that all of the aforementioned arguments, even taken together, only constitute an incoherent collection of circumstantial evidence. To make cosmologists worthy of the billions of governmental support, we actually need to do *much* better than convicting a suspect of murder. At best it would seem that we achieved what a Scottish jury would return as verdict: not proven.

I must then turn to a different question. Is there any direct way of clinching the origin of the CMB at all that we can pursue? Well, it turns out there *is* one good starting point. Traditionally, just about the only clean and indisputable way of charting the scale height of any diffuse radiation in the absence of redshift information is to look for ‘shadow’ effects on the radiation cast by gas clouds at known distances away from us: if a shadow is found, the radiation must have come from *behind* the cloud. Can I provide an example to dramatize this point? The answer is also yes, except the outcome did present a major surprise to the researchers in the field of concern. It has for a long time been generally accepted that the soft X-ray sky background (SXRb) is principally Galactic in origin; moreover it is emitted by a thin hot plasma that fills a void of ~ 100 pc radius centered at the sun - the so-called ‘local bubble’ model. Thus, when the first shadow of the SXRb was discovered by Burrows & Mendenhall 1991, who reported that 60 % of the SXRb was silhouetted by a dense cloud - the Draco Nebula located some 600 pc away in a direction of high Galactic latitude - the ‘local bubble’ model was under serious threat. Today, this great discovery still stands, and completely destroyed any simple way of understanding the SXRb origin. Could there be a lesson here for the unerring community of Λ CDM cosmologists?

For the CMB an equivalent ‘silhouette’ cloud would be a cluster of galaxies, which Thomson scatters CMB photons on the Rayleigh-Jeans part of the CMB spectrum - the Sunyaev-Zel’dovich effect (SZE). A recent study of ~ 100 rich clusters, using WMAP W band data (Bielby & Shanks 2007) confirmed our earlier findings from a smaller sample of 31 rich clusters (Lieu et al 2007) that WMAP detected almost no SZE at all from these clusters, i.e. the CMB appears to have failed the ‘shadow test’. In particular the analysis by Bielby & Shanks of 38 clusters at a mean redshift of $z \approx 0.3$ revealed a level of SZE statistically consistent with a null effect and completely inconsistent with the expectation (they also truncated the predicted SZE profile at a ridiculously small cluster radius, yet the inconsistency remains, so that its origin *cannot* be WMAP’s spatial resolution). When the same sample of 38 clusters was observed by radio interferometric techniques rather than

WMAP, a different verdict was delivered. This method did lead to the discovery of SZE at the expected level (Bonamente et al 2006).

Who is right? It is usual to settle such discrepancies by appealing to the community at large for independent analysis of the same datasets. For WMAP this is possible, because the data are all in the public domain, which is how Bielby & Shanks were able to cross-check Lieu et al (2006). The observations of Bonamente et al (2006) are however not public: it is apparently quite normal for SZE data taken by ground-based telescopes to remain inaccessible by the rest of us for a long time. It is fair to say that the release of data (in as primitive (or unprocessed) a form as possible) of important experiments for everyone to check helps bolster the claims of the original researchers who ‘creamed the crop’, especially if others who did the necessary tests are able to corroborate these claims.

Let us give Λ CDM proponents the full benefit of the doubt. by assuming that the interferometers got it right, viz. the SZE at the fully expected level as reported by Bonamente et al (2006) is correct. This then would mean, unless both Bielby & Shanks and Lieu et al erred, that WMAP got it wrong. For the clusters analyzed by Bielby & Shanks (2006) and Lieu et al (2007), their SZE profiles have typical angular sizes between those of the first and second acoustic peaks, except of course the amplitude of the SZE is deeper than that of the acoustic oscillations. If WMAP could not properly fathom those deeper modulations in the CMB temperature that occur at ~ 0.5 degree angular scales, how shall we satisfy ourselves that it has correctly measured the acoustic peaks?

4. Λ CDM cosmology: some of the long list of counter evidence and how they have been treated

In Table 3 I listed some of the counter evidence of Λ CDM cosmology, all of which were published (or about to be published) in the topmost astronomy journals. The table entries are referred to as ‘neglected evidence’ because I used the latest WMAP3 cosmology paper of Spergel et al (2007) as benchmark concerning the citations of relevant previous work. Not only is this the most important cosmology paper in the contemporary literature (it already received more than 1,300 ADS citations even though it is not yet published), but also it included many sections on CMB external correlations, viz. how the standard Λ CDM model fares against other non-CMB observations, and whether these can help to further constrain the model parameters. Thus the paper *is* meant to cross-compare all the vital evidence, yet with the exception of the ‘Axis of evil’, none of those listed in Table 3 were mentioned. In the last section Spergel et al concluded ‘the standard model of cosmology has survived another rigorous set of tests’. Here I elaborate upon most of items of Table 3 (though not necessarily in the right order) so that each reader can judge if Spergel’s claim is tenable.

- Spergel et al cited the gas fraction analysis by Steve Allen of X-ray (Chandra) observations of rich clusters, which led Allen to conclude upon the correctness of Λ CDM cosmology. Yet it is well known at least within the clusters community that the number density evolution curve of clusters as derived by the XMM Newton Key Project (Vauclair et al 2003) rejected the Λ CDM prediction with 7σ statistical significance, but is consistent with an Einstein de Sitter Universe. This result was not cited by Spergel et al (2007). I understand that there has been a lot of questions about the validity of the conclusion of Vauclair et al (2003), but just as many similar questions have also been directed at the work of Allen. It is for the moment not for any individual to pass ultimate judgements. Even if the WMAP team has their own noble rationale to favor Allen’s work, they should still have cited Vauclair because the claim is so drastic. Quoting the paper in negative light might still be acceptable if at least some reasons are given. Ignoring the paper altogether is unacceptable and unscientific.
- The fact that only $\approx 50\%$ of the baryons predicted by the Λ CDM model to exist at low redshifts has been observed was first noted by Cen & Ostriker (1999). To date this same problem persisted, with the latest paper on the subject being Takei et al (2007). In Figure 2 we show the upper limits to-date on the detection of these missing baryons (graph is courtesy Yoh Takei), resulting partly from the non-detection of the O VII line in clusters with soft X-ray excess, i.e. this excess cannot then be attributed to a massive component of warm baryons at the outskirts of clusters. This to me is a very serious discrepancy, much more so than the debate on dark matter and dark energy, because *baryons are real* and they are still (however one may get fancy) the only thing we can directly measure.
- The soft X-ray excess of clusters has over the past twelve years since its discovery been detected by EUVE, ROSAT, BeppoSAX, XMM, and Suzaku, with the latest paper (on Suzaku’s signal) being Werner et al (2007). There is still no explanation of this excess, which is seen in both the core and outskirts of clusters, in the context of Λ CDM or for that matter any other cosmologies. For those who prefer to sideline this as yet another minor detail, I invite them to take a look at the strength of the soft X-ray signal in the central (but avoiding the complicated innermost) region of Abell 3112, Figure 2.
- On the Hubble constant, Spergel et al (2007) cited the Hubble Key Project paper of Freedman et al (2001) and the X-ray/SZE result of Bonamente et al (2006), but the equally comprehensive treatise of Sandage et al (2006) was ignored. This could simply be due to the very recent appearance of the Sandage, but since in principle there is definitely enough time for Spergel et al to cite Sandage there may be other reasons. While Freedman and Bonamente reported a Hubble constant of $h \approx 0.7$, very close to the value advocated by the WMAP team, Sandage found $h \approx 0.62$, considerably lower perhaps than any team member’s liking. Besides, how could two independent analyses of the HST data (Freedman versus Sandage)

Λ CDM : the neglected evidence	Why it is important	‘Reason’ for neglect
Evolution of cluster counts (Vaucclair et al.)	Curve matches Einstein de Sitter Universe and excludes Λ CDM at 7σ	Cluster ‘detailed physics’ not known (but we understand the Universe)
Missing baryons at low redshift (Cen and Ostriker)	Still not found today as cluster OVII lines remain undetected	Who cares about a few percent of the Universe (except this is the only bit we directly measure, and we are made of it)
Too little Sunyaev - Zel’dovich effect in WMAP (Bielby and Shanks, Lieu et al.)	‘Shadowing’ techniques still the only direct way of charting the CMB scale height	Cluster physics ‘details’. WMAP cannot measure SZE anyway (but can probe the shallower acoustic peaks at similar angular scales)
Matter Budget of galaxy groups (Ramella et al.)	Many groups (like our own Local Group) could easily hold altogether $\Omega \sim 1$ worth of matter	Groups of galaxies are not properly weighed and counted
Axis of evil, correlation with HI clouds (Land, Verschuur)	Significant foreground issues remain down to acoustic peak scales	Statistics unclear (and the same ‘Bayesian prior’ that established Λ CDM can be used to marginalize these)
Hubble constant of Sandage et al	Significantly different from Freedman’s value even though both used HST data	Systematic problems
Soft X-ray excess in clusters (Bonamente, Nevalainen, Kaastra, Fabian, Lieu)	See Fig. 1 (can Λ CDM explain this ?)	Minor obscenity. Phenomenon doesn’t really exist
Dwarf elliptical rotation curves	Data give constant density cores whereas Λ CDM halo profiles have central cusps	Poor spatial resolution of data, and no independent M/L ratio for disc

Table 3: A list of key, independent and respectable evidence not cited in the WMAP3 paper of Spergel et al (2007), where the authors included an extensive section on CMB external correlations to bolster their claim of the standard Λ CDM cosmological model. Their ability to ‘bolster’ is because the external evidence employed were carefully pre-selected.

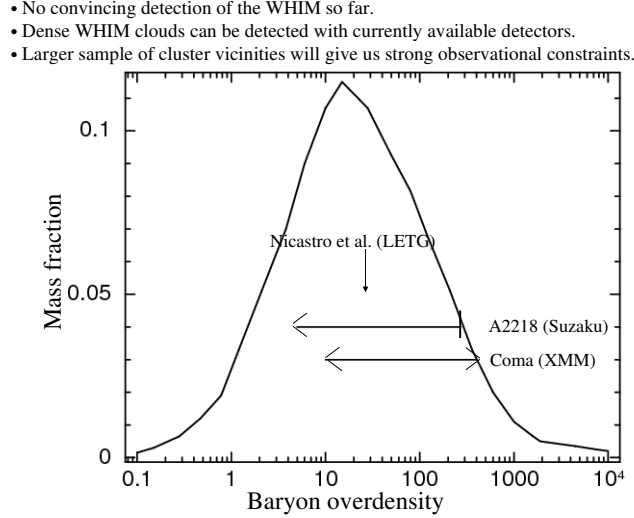


Fig. 1.— Mass fraction of baryons (to total matter) as a function of clump overdensity, with various upper limits and one 3σ detection from Coma cluster. Courtesy of Yoh Takei.

could lead to such a difference in the final answer, especially since I have been hearing so much talk about ‘ H_0 in the era of precision cosmology is nailed to 5 % accuracy’ ?

- A not-too-often mentioned but no less important problem for Λ CDM is the potential for groups of galaxies like our own Local Group to be harbors of much more matter than expected. Thus e.g. from the ESO survey of 1,168 nearby groups (Ramella et al 2002) the mean virial mass per group is $M \approx 1.15 \times 10^{14} M_\odot$ and the number density of groups is $n \approx 1.56 \times 10^{-4} \text{ Mpc}^{-3}$. This already yields a mean mass density nM equivalent to $\Omega_{\text{groups}} \approx \Omega_m/2$, assuming the Λ CDM value of $\Omega_m = 0.3$. However, there is a selection bias, due to many groups having evaded detection. After correcting for this bias in the best possible way, Ramella et al (2002) estimated a number density of $n \approx 4 \times 10^{-3} \text{ Mpc}^{-3}$ for groups. The product nM now corresponds to $\Omega_{\text{groups}} \approx 3.4$, which far exceeds the total mass density of matter in the Λ CDM model. The same pointers to groups of galaxies weighing much more massively than ‘expectation’ (i.e. $\Omega_m \approx 1$) was also found by Myers et al (2003, 2005).

- The very feeble SZE detected by WMAP was already discussed in the previous section, and here I simply mention that David Spergel is fully aware of at least Lieu et al (2007). It was not cited in Spergel et al (2007).

- There has recently been the claim by Verschuur et al (2007) that a significant fraction of

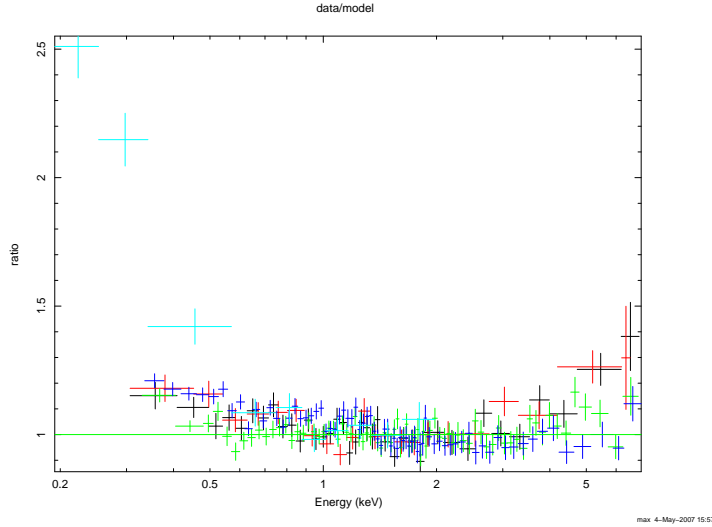


Fig. 2.— Isothermal free-free emission model for the hot virialized plasma in the rich cluster Abell 3112 as fitted to the X-ray spectra of the central 0.5 -1.5 arcmin region of the cluster (the innermost 0.5 arcmin was avoided due to possible complication from a ‘cooling core’ and point source contamination). The graph shown plots the ratio between observed data and the best-fit model, where the different colors correspond to observations by the various X-ray missions: Chandra in black and red, XMM in green and blue, and ROSAT PSPC in Cyan. Note the strong soft X-ray excess at low energies, indicative of a completely new emission component in clusters of galaxies. This ‘cluster soft excess’ phenomenon has been known and ridiculed for twelve years, i.e. it lived a time span as long as Λ CDM.

the degree-scale acoustic peak hot spots in the ILC map of WMAP1 spatially correlates with anomalous velocity HI clouds in the Milky Way - a finding which prompted this author to conclude that a significant fraction of the WMAP anisotropy at the primary acoustic peak is not cosmological. This paper was submitted to ApJ, and I was told that two reasonably disposed referee reports were received. Readers should therefore keep an eye on the development of this front. No complaint is made in the present article of Spergel’s failure to cite Verschuur, as the latter is a very new result.

5. Alternative models: are they really so inferior to Λ CDM?

Given that there are so many bullets of evidence (with varying weights) against Λ CDM cosmology, the question is naturally raised as to how competing models may fare, when the

Observation Verdict	LCDM Model Verdict		Shanks Model Verdict		Blanchard Model Verdict	
	Yes	No	Yes	No	Yes	No
First Acoustic Peak	X		X		X	
Second Acoustic Peak	X			X	X	
Baryon Acoustic Peak	X			X		X
Type 1a Supernova	X			X		X
Light Element Synthesis	X			X		X
Matter Power Spectrum	X			X	X	
Euclidean Geometry	X		X		X	
CMB Global Uniformity	X		X		X	
Hubble constant	X			X		X
Age of stars	X		X		X	
Evolution of Clusters		X	X		X	
Matter Budget of Groups		X	X		X	
Lensing of 1 st CMB peak	X		X		X	
CMB SZ Effect (WMAP)		X	X		X	
Baryons low z		X		X		X
Baryons in Clusters		X		X		X

Λ CDM Model, $\Omega_m = 0.3, \Omega_\Lambda = 0.7, h = 0.7$
Shanks Model, $\Omega_m = \Omega_b = 1, \Omega_\Lambda = 0, h \simeq 0.4$
Sarkar Model, $\Omega_m = 1, \Omega_\Lambda = 0, h \simeq 0.45$

Fig. 3.— How the standard Λ CDM model fares against two competing models, those of Shanks (2007) and Blanchard-Sarkar et al (2003), when all the evidence known to the author are taken into account. Note the slight vertical misalignment of the crosses should be ignored - the verdict from each bullet of evidence is for simplicity only expressed as a binary yes or no.

whole body of evidence is taken into account. I show in Figure 3 just such a metric. The two alternative models I chose are Shanks (2007) and Blanchard et al (2003). Both involve the Einstein de Sitter Universe, with $\Omega_m = 1$. The latter does away with dark energy altogether, and relies on a primordial matter spectrum that is not purely power-law (the level of extra contrivance here is not as severe as postulating dark energy when dark matter has still not be found). The former does away with dark matter as well as dark energy, and uses the gravitational lensing by foreground galaxy groups (see above) to secure agreement between model prediction of the first acoustic peak and WMAP data. Although the 2nd peak is not yet accounted for, the remarkable feature of this Shanks model lies obviously with its economy in extra new postulates, by getting rid of all darkneses.

It can be seen that when all the evidence are placed on the ‘scale pan’ no model is really classifiable as a ‘winner’ or ‘loser’. Perhaps all models are equally poor: the two competitors certainly do not come across as much more inferior than the standard model. What cannot be quantified in terms of figure-of-merit, however, is how much more credibility should one assign to a model that relies on less bizarre postulates.

6. Conclusion

Cosmologists should not pretend to be mainstream physicists, because there is only one irreproducible Universe and control experiments are impossible. The claim to overwhelming evidence in support of dark energy and dark matter is an act of exaggeration which involves heavy selection of evidence and an inconsiderate attitude towards alternative models with fewer (or no) dark components. When all evidence are taken into account, it is by no means clear that Λ CDM wins by such leaps and bounds.

Thus I do not see the wisdom of funding agencies in planning such ambitious and expensive programs to perform dark energy research, to the detriment of other fields of astronomy, as though cosmology has now become a branch of physics, which it will never be. These programs all have the common starting point that dark energy is really out there - no question about it. I hope the present article demonstrated the contrary.

The irony of today's times is that while dark matter is still unidentified despite half a century of search, taxpayers are asked to invest in yet another potential fiasco. Furthermore, the situation as it evolves in time is that the more we do not find dark matter, the less (in relative funding) do we invest in alternative approaches - to the point of totally choking these approaches. Thus we are putting more and more eggs in a less and less likely basket. Could this be the sign of a person (or a camp of people in prestigious institutes) who became angry because they are embarrassed? Even if one were to avoid taking such a view, one should still ask the question 'is this the scientific method' ?

I recommend that major funding agencies seriously consider enlisting to decision making panels a higher (than zero) fraction of those individuals who published equally respectable papers in top journals on the body of counter evidence listed in Table 3. The reason why we are heading in such wrong directions is because while panels rotate they invariably comprise the same camp of researchers, mostly from elite establishments with vested interests. The ultimate selection effect, therefore, might lie with those senior agency administrators responsible for the composition of these panels.

7. References

- Bielby, R.M., & Shanks, T. 2007, MNRAS submitted (astro-ph/0703470).
Blanchard, A. et al 2003, A & A, 412, 35.
Bonamente, M. et al 2007, ApJ, 647, 25.
Burrows, D.N. & Mendenhall, J.A., 1991, Nature, 351, 629.
Cen, R., & Ostriker 1999, ApJ, 514, 1.

- Chodorowski, M. 2007, MNRAS in press (astro-ph/0610590).
- Freedman, W.L. et al 2001, ApJ, 553, 47.
- Lieu, R. et al 2006, ApJ, 648, 176.
- Myers, A.D. et al 2003, MNRAS, 342, 467.
- Myers, A.D. et al 2005, MNRAS, 359, 741.
- Ramella, M. et al 2002, AJ, 123, 2976.
- Sandage, A. et al 2006, ApJ, 653, 843.
- Shanks, T. 2007, MNRAS, 376, 173.
- Spergel, D. et al 2007, ApJ in press (astro-ph/0603449).
- Takei, Y. et al 2007, ApJ, 655, 831.
- Vauclair et al 2003, A & A, 412, L37.
- Verschuur, G. et al 2007, MNRAS submitted (arXiv:0704.1125)
- Werner, N. et al 2007, A & A in press (arXiv:0704.0475).
- White, S.D.M. 2007, Rep Prog Phys, in press (arXiv:0704.2291).