Fundamentalist physics: why Dark Energy is bad for Astronomy

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Astronomers carry out observations to explore the diverse processes and objects which populate our Universe. High-energy physicists carry out experiments to approach the Fundamental Theory underlying space, time and matter. Dark Energy is a unique link between them, reflecting deep aspects of the Fundamental Theory, yet apparently accessible *only* through astronomical observation. Large sections of the two communities have therefore converged in support of astronomical projects to constrain Dark Energy. In this essay I argue that this convergence can be damaging for astronomy. The two communities have different methodologies and different scientific cultures. By uncritically adopting the values of an alien system, astronomers risk undermining the foundations of their own current success and endangering the future vitality of their field. Dark Energy is undeniably an interesting problem to attack through astronomical observation, but it is one of many and not necessarily the one where significant progress is most likely to follow a major investment of resources.

The pursuit of a deeper truth, of a fundamental theory which underlies all others, is a powerful motivator in physics. So too are curiosity and awe at the richness of Nature, at the connectedness which allows disparate and seemingly unrelated processes to produce order, beauty and diversity from apparent chaos. The first motivation is, perhaps, most evident in high-energy physics, where a "theory of everything" has periodically appeared within reach, occupying many of the most talented theoreticians. The second is evident in more interdisciplinary, less "fundamental" fields, solid-state physics, evolutionary biology, or astrophysics. Fundamentalists prize the depth of their research, seeing it as a means to abstract from the complexity of the world a Truth which embodies the ultimate foundation of the physics of particles and fields, thus, by extension, of all physics, chemistry and biology. Generalists, on the other hand, prize breadth and interdisciplinarity which promote the perception and appreciation of the many truths underlying complex phenomena. In their view, the fundamental theory of everything will contribute nothing to our understanding of the origin and nature of life.

The discovery of Dark Energy, a near-uniform field which appears to dominate the energy density of the current Universe and to drive its accelerated expansion, has led astrophysicists and high-energy physicists to make common cause. The apparent properties of the Dark Energy, in particular the extremely low associated energy scale, are entirely unexpected in the standard model of particle physics and extensions such as supersymmetry. This suggests to many that Dark Energy may somehow reflect the unification of gravity with the other fundamental forces, and hence, paradoxically, physics at energies far above those that can be probed directly with accelerators. At present it seems that the properties of Dark Energy can be explored only through astronomical observations, in particular through precise measurements of the recent expansion history of the Universe and of the growth of cosmic structure. Such measurements require observation of large samples of complex astronomical objects such as galaxies, galaxy clusters and supernovae. In consequence, astronomers interested in supernovae and in cosmic structure formation have been working intensively with high-energy theorists and astrophysical cosmologists to design projects which might achieve the required precision. Such collaborations bring risks as well as benefits because of different motivational backgrounds and different methodologies in the two communities. It is on this issue that I wish to focus in this essay.

HST and WMAP

A useful illustration of the contrasts between the motivation and the *modus operandi* of the two communities is provided by two current satellite telescopes: the Hubble Space Telescope (HST) and the Wilkinson Microwave Anisotropy Probe (WMAP). These two projects have very different scale and duration (in particular, HST is well over 10 times as expensive as WMAP) but this is not the aspect of the missions which concerns me here. Both have been extremely successful and have had very substantial impact both in the professional scientific community and among the scientifically minded community at large. They can serve as exemplars of supremely successful projects driven primarily by traditional astrophysicists in the case of HST, and by fundamental physicists and cosmologists in the case of WMAP.

HST, a NASA-led collaboration between the North American and European astronomical communities, was planned over many years as the first true observatory in space. It was anticipated that it would make great advances because its location above the atmosphere allows observations at higher resolution, at shorter wavelengths, and with less contaminating sky emission than is possible with ground-based optical telescopes. Studies made before launch assessed how these capabilities might advance research frontiers as perceived at that time, but a clear expectation was always that opening new windows of observational parameter space would trigger new and unexpected discoveries in areas where little was previously known. By and large this expectation has been borne out. HST is now primarily known for discoveries which were not part of its original science case.

WMAP was proposed by a small and tight-knit group of scientists in response to a NASA call for suggestions for a mid-sized mission. All the proposers had a strong track-record of instrumental development for studies of the Cosmic Microwave Background (CMB), and several of them had worked on NASA's previous CMB satellite COBE. Over the period 1989-1993, COBE had demonstrated the near-perfect black-body nature of the CMB and had detected the weak temperature variations predicted 20 years earlier to remain as visible echoes of the early fluctuations from which all present structure grew. (The PI's of the two relevant instruments were awarded the 2006 Nobel Prize in Physics for these discoveries.) Theoretical predictions for structure in the CMB are robust within the standard paradigm. Detailed measurements should not only test this paradigm, but should also give precise measurements of the geometry of the Universe, of its contents in ordinary matter, dark matter and dark energy, and of some aspects of the process which created all structure, perhaps during a very early epoch of cosmic inflation. The WMAP team designed their experiment specifically and optimally (given available technology) to map fluctuations of the kind predicted by theory and measured on large angular scales by COBE. After several years of operation they have now successfully achieved this goal.

One way to contrast the nature of these two projects is through the images which have become emblematic of their success. In figure 1 I show two of the best known images from HST, the picture of a region of the Eagle Nebula often referred to as the "Pillars of Creation" and the very deep exposure of an apparently blank piece of sky known as the Hubble Deep Field. Both of these pictures have had enormous public and professional impact, achieving iconic status. Both would be considered beautiful by many viewers. To my mind their beauty lies in the complexity of the structures and in the way they resonate with visually familiar images but in a new and striking context. The Pillars of Creation are reminiscent of backlit thunderclouds, evoking the reappearance of sunlight after a storm, yet they depict the hidden birthplaces of stars. The Hubble Deep Field shows us galaxy images almost like those in coffee-table photographic atlases, yet these apparently neighboring systems in fact stretch back through time nine tenths of the way to the Big Bang. In both cases we see rich and complex systems whose structure and evolution are evoked rather than characterised by the images. Quantitative analysis is possible, but it serves to construct approximate phenomenological models rather than to measure well-defined physical parameters

The best known images from WMAP are shown in figure 2. One is a rendition of the CMB sky with all foregrounds removed and with a dramatic colour code to emphasise the tiny temperature fluctuations detected by the satellite. This has no resonance with the familiar world, appearing more like a mathematical construction. To a very good approximation it is a random noise field and the statistical properties of its several hundred thousand pixels are adequately described by a six-parameter model. The second image compares the angular power spectrum of this sky map with a prediction based on an *a priori* physical model where all six parameters have been adjusted to values which are fully compatible with those expected from independent, non-CMB data. The agreement is a triumphant affirmation of the power of physics as a description of our world. Although the present universe may be complex, the early universe was simple, and we can calculate the statistical properties of its structure from first principles. Fitting the observed data puts tight constraints on "fundamental" properties of the universe such as its overall geometry, its contents in dark energy, dark matter and ordinary baryons, and the process from which all structure originated. These properties affect the later growth of galaxies and stars, but the CMB sky offers no insight into the complex regularities which characterise these systems.

The following table gives another view of the contrasting properties of HST and WMAP which illustrates some of the differences in scientific culture which concern me in this essay.

HST	WMAP
An observatory	An experiment
Designed for general tasks	Designed for a specific task
Serving a diverse community	Serving a single, coherent community
Programme built through proposals	Programme set at design
Many teams of all sizes	A single moderately large team
Many results unanticipated	Main results 'planned'
Nourishes astrophysics skills	Nourishes data-processing/ statistics skills
Public support as a facility	Public impact through results

Most of these contrasts are self-explanatory, but the last one may deserve more comment. In the wake of the Columbia disaster the NASA administration decided that the planned shuttle mission to service the HST was too risky and the telescope must therefore be allowed succumb to its natural degradation in orbit and instrumentation. This caused a tremendous outpouring of support, not only from almost the entire astronomical community, but also from the media, from the general public, and from the astronauts themselves. Largely in response to this, the servicing mission is again on the NASA roster. Although the impact of WMAP's results was enormous, it seems unlikely there would have been such an emotional ground-swell of support had NASA decided to discontinue its operations after four years.

This broad public affinity for astronomy reflects widespread interest in deep questions of origin and fate which earlier civilizations addressed through creation myths. Similar emotional undercurrents explain the preponderance of ‘space’ themes in popular science fiction and the remarkable world-wide community of amateur astronomers. The latter unites enthusiasts across generations, across skill levels, across social strata, and across national and cultural boundaries. Amateur astronomers build their own telescopes, use them to do research of significant if not forefront interest, and maintain a lively and high-quality magazine literature featuring substantive reviews of new results from professional research. Astronomy resonates with the popular imagination through its combination of complexity and regularity, of the familiar and the strange, as well as through its extraordinary and seemingly limitless range of subjects for study, from the beginning of time to the birth of stars, from the peculiarities of black holes to those of planets, from the origin of the elements to that of spiral galaxies, from dark energy to the preconditions for life. The fact that it is hard to imagine an enthusiastic amateur community devoted to high-energy physics is another indicator of the cultural differences between the two fields.

The two cultures

Astrophysics and high-energy physics have a number of common features. Neither has any direct application to everyday life, even if their instrumental and computational needs sometimes lead to significant technological spin-offs. Both deal with phenomena on scales which differ vastly from those of normal human experience. Both require very expensive equipment. Despite this, the research communities in the two fields differ notably in their attitudes, in their motivations, in their *modi operandi*, and in the value systems by which they judge their work.

Astrophysics aims to understand the structure and behaviour of inherently complex systems and as a result is interdisciplinary and synthetic in character. An intuitive feeling for the interplay between phenomena from many areas of physics is needed, for example, to model the formation of a galaxy. High-energy physics, in contrast, is reductionist, aiming to break phenomena down into ever more fundamental and more abstract entities, discarding along the way complexities which may mask the underlying Truth. Thus astrophysicists tend to be generalists, prizing breadth of knowledge, while high-energy physicists tend to be specialists, prizing the depth to which they probe the underlying structure of matter. In experimental work astrophysicists seek many truths associated with many phenomena, and the best forefront research is characterised by diversity and opportunism. In particle physics the quest for the fundamental Truth has led to a focus on a much smaller number of 'important' questions (the origin of mass, the unification of quantum mechanics and general relativity...) and to the organisation of industrial-strength teams to address them. New insights in astrophysical research appeal on many levels, intellectual, emotional and aesthetic, but they rarely display the quasi-mathematical rigour of major advances in particle physics such as the understanding of asymptotic freedom or of the Higgs mechanism. Astrophysicists are universalists, democratic in perceiving interest in all aspects of the cosmos, while high-energy physicists are fundamentalists, cleaving to the pursuit of the single Truth.

Many of these differences can be traced to the fact that theory has traditionally been tested against reality through controlled experimentation in high-energy physics, but through observation in astrophysics. The remoteness and scale of astronomical systems preclude control of initial or boundary conditions, while long timescales make evolution unobservable in most individual objects. Astronomers are forced to work with "snap-shots" of non-ideal, strongly interacting and complex systems. This has produced a research strategy quite unlike that in fields where experimentation is possible. When planning major new astronomy facilities, the principal design drivers are usually:

1. to complement and extend previous facilities;
2. to maximise the discovery potential; and
3. to minimise the risk of scientific failure.

The emphasis is on enlarging capabilities by opening previously unexplored regions of observational parameter space (in wavelength, angular resolution, sensitivity...) rather than on targetting a specific scientific issue. The science case, for HST, as for most major observatories, was based on a wide range of problems from many areas of astrophysics. The astronomical community has, nevertheless, always considered HST's principal value to be the availability of most of its observing time for programmes proposed after launch by individual research groups. Most astronomers no longer remember the original science justification for HST or most of the Key Programmes implemented to address it.

To some extent, these considerations also apply to the design of major facilities for high-energy physics, but even a global facility such as the Large Hadron Collider is only able to address a relatively narrow range of problems and to conduct a small number of experiments, each carried out by a large, international team of physicists. These experiments are set up largely according to traditional physics methodology:

1. identify the potential capabilities of new instrumentation;
2. identify issues that these capabilities might address;
3. refine the design
   a) to address the important issues optimally,
   b) to exclude confusing factors.

Team members specialise in optimising particular aspects of the experiment (magnets, detectors, data analysis...) and may work for decades before seeing data. Such long-term efforts require structured and hierarchical management, and few physicists outside the teams are able to work directly with the data. This contrasts with HST where science is primarily carried out through programmes that last a couple of years from proposal to completion and are independent both of the instrument teams and of the science case which justified instrument construction. The HST model offers young scientists a much wider range of opportunities for scientific creativity and visibility than most major accelerator experiments.

Dark Matter and Dark Energy

Over the last two decades a standard paradigm has emerged for the evolution of cosmic structure. One of its most striking aspects is the assertion that the current universe is dominated by two unexpected and apparently independent components, Dark Matter and Dark Energy. The need for unseen matter to explain the dynamics of galaxy clusters was first pointed out by Fritz Zwicky in the 1930's, but only the last 25 years have seen wide acceptance of the idea that cosmic structure growth is driven by a gravitationally dominant population of some new kind of weakly interacting particle. General acceptance of the idea that the current expansion of the universe is accelerated by some form of Dark Energy is even more recent, although the Cosmological Constant was introduced by Einstein as part of his theory of gravitation and is a viable explanation of current observations.

Both Dark Matter and Dark Energy are seen as fundamental by high-energy physicists as well as by astrophysicists. All currently viable elementary particle candidates for dark matter require an extension of the standard model of particle physics with the lightest supersymmetric particle being, perhaps, the current favorite. If this were confirmed, it would prove that the early universe was sufficiently hot that supersymmetry was unbroken. Dark energy seems to require an even more radical extension of current theories, perhaps a unification of quantum mechanics and general relativity in some form of superstring theory.

The current evidence for both dark components is purely astronomical, and it appears that *only* astronomical observations provide a means to constrain properties of Dark Energy. Thus, the experimental testing of the hottest idea in current high energy physics depends to an unprecedented degree on astronomers, and the two communities have collaborated substantially in planning major new initiatives to address the issue.

From an astronomical point of view, however, the Dark Matter and Dark Energy problems differ qualitatively in their richness and in their interaction with the rest of the field. Dark Matter drives the formation of galaxies and galaxy clusters and influences all aspects of their structure. Its distribution can be mapped directly using gravitational lensing, and can be inferred indirectly both from the dynamics of galaxies and intergalactic gas, and from the structure of fluctuations in the microwave background radiation. The current favorite candidate, the lightest supersymmetric partner of the known particles, should produce annihilation radiation which could be imaged by planned gamma-ray telescopes. Dark Matter may soon be observed directly by underground “telescopes” which are rapidly improving their ability to measure the occasional collisions of Dark Matter particles with ordinary matter, and it may be detectable in experiments at the Large Hadron Collider. Dark Matter studies thus impact directly on most aspects of extragalactic astronomy and astrophysical cosmology, as well as stimulating astroparticle experiments and research programmes at accelerators.

In contrast, Dark Energy studies have little or no impact on other areas of astrophysics and experimental high-energy physics. Models have been proposed in which Dark Energy interacts with Dark Matter, resulting in observable effects on structure formation, but in most models the two components are effectively independent of each other. The effects of Dark Energy are then manifest only in the overall expansion history of the universe and in the linear growth rate of irregularities. If Einstein’s theory of gravity holds, one of these functions can be derived from the other and all astronomically accessible information about Dark Energy is then contained in a single observable function, the expansion rate as a function of cosmic time. Current data are all consistent with the expansion history expected if Dark Energy behaves like a Cosmological Constant. Estimates of the current value of the relevant dimensionless parameter are in the range $w \sim -1 \pm 0.1$, where $w = -1$ at all times for a Cosmological Constant. For astronomers, this means that the expansion history is already well enough measured that further refinement will produce at most minor shifts in the inferred history of cosmic structure formation.

Thus, while clarifying the nature of Dark Matter has all the hallmarks of a typical “astrophysicist’s” problem, interacting with many other aspects of the field and accessible by many routes, clarifying the nature of Dark Energy is a “fundamental” problem, apparently accessible only by a route which has little impact on the rest of astrophysics.

This has two consequences which are important for my argument. Further tightening of constraints on the cosmic expansion history and on the growth of fluctuations will not improve our understanding of the formation and evolution of stars, galaxies and larger structures. This is now limited primarily by uncertainties in the many complex and interacting astrophysical processes involved. Conversely, these uncertainties may affect our ability to place tighter constraints on Dark Energy. For example, type Ia supernovae are currently our best probe of the cosmic expansion history, and planned programmes will increase the size and redshift range of well-observed samples to the point where purely statistical errors are small. Unfortunately, the progenitors of higher redshift supernovae formed and exploded in younger galaxies than their lower redshift counterparts, and this could plausibly cause small redshift-dependent shifts in the properties of the supernovae or of their immediate environments. Undetected shifts of this kind could confuse the search for the Dark Energy signal and limit the precision with which it can be measured. Similar systematic errors potentially afflict all other proposed probes, since all are based on complex astrophysical objects. Thus more precise constraints on Dark Energy will not help us understand the evolution of the objects which populate our universe, but our ignorance in this area could frustrate our attempts to constrain Dark Energy.

So why is Dark Energy bad for astronomy?

I come now to the crux of my argument: how the current emphasis on Dark Energy as a principal driver of astrophysical research can undermine not only the methodological basis of astrophysics, but also its attractiveness to its best practitioners, to the most talented of next-generation scientists, and to the public at large. In my view, such negative consequences can result from importing the alien culture of high-energy physics, especially in combination with an independent trend towards “Big Science” which is currently afflicting astronomy.

The dangers I see are of three kinds: inappropriate risk assessment in the design of major programmes; investment of scarce resources in programmes which do not enable new astrophysics or promote advances over a broad front; promotion of a fundamentalist value system and a managed work culture which will make astronomy unattractive to the brightest, most creative and most ambitious young scientists. Let me discuss these in turn.

The remarkable advances made recently through studies of the microwave background have convinced many that astrophysical cosmology provides a new window on fundamental physics. These advances were possible because the observed structure takes the form of linear perturbations of a simple state, an infinite uniform mixture of a small number of components with well understood interactions. The evolution of this system can be treated rigorously and precisely. In addition, foreground effects are providentially weak and so cause only minor complications. Fundamentalist physicists, drawn to cosmology by this success, often fail to appreciate the uniqueness of the circumstances. An interesting comparison is helioseismology, the study of the structure of the Sun based on sound waves propagating through it. Here also the perturbations are linear and propagate in a medium where the relevant physics is fully understood. Here also careful measurement has produced extremely precise results for the properties of a very large number of modes. Conclusions at the level of confidence and precision reached by CMB studies are precluded, however, by the complexity of the underlying system. For example, the initial fraction of heavy elements required for the current standard model of solar evolution to reproduce the structure inferred from helioseismology is almost twice the fraction measured in the Sun's atmosphere by analysis of its spectrum.

Astrophysical routes to a better understanding of Dark Energy all involve complex systems: supernovae to trace the cosmic expansion history; galaxies to outline ripples in the large-scale matter distribution; galaxies as background sources to trace gravitational lensing by the foreground mass distribution; galaxy clusters as markers of the growth of cosmic structure. Astrophysical experience suggests that the ultimate precision reached by such programmes will be set by systematic effects, for example, progenitor or environment evolution for supernovae, nonlinear and non-determinate relations between observables and theoretical quantities for galaxies and galaxy clusters. By their nature such systematics cannot be accurately assessed in advance, and indeed they often remain unrecognised until the programme is complete. Estimates of the final precision of Dark Energy experiments are thus based primarily on purely statistical considerations and should be considered optimistic estimates of the "best possible" result. Dark Energy enthusiasts, emboldened by CMB successes, often fail to appreciate these limitations, believing that sophisticated statistical analysis will enable the best possible result to be approached. This exposes the community to the danger of designing and carrying out a very expensive experiment to measure many thousand supernovae, or to image a very large area of sky, only to find that the resulting measurement of w is only a modest improvement over previous work because of astrophysical systematics. If the experiment is of limited use for other astrophysical purposes, then the funds will, in effect, have been wasted. A problem for which the astrophysicists will surely be blamed!

The potential problem here reflects the combination of inappropriate risk assessment – what is the chance that the complexities of real galaxies, clusters or supernovae will frustrate attempts to measure the cosmic expansion and fluctuation growth histories precisely? – with an inappropriate design strategy – planning an experiment like WMAP rather than an observatory like HST.

This brings me to the second danger: the impoverishment of astrophysics by too heavy an emphasis on Dark Energy when planning the next generation of major facilities. As already discussed, astronomers traditionally limit risk when designing new instruments by concentrating on the expansion of technical capabilities in sensitivity, wavelength coverage, spatial or spectral resolution. This enables progress on a wide variety of problems, particularly since operation in observatory mode allows new projects to be proposed as they are seen to be interesting. Some Dark Energy projects conform to this strategy. For example, wide-angle X-ray and millimeter surveys will not only identify very large samples of distant galaxy clusters, but will also image much of the sky to a sensitivity and resolution which has not previously been achieved at these wavelengths. In addition, these facilities will probably operate at least to a limited extent in observatory mode. Other Dark Energy projects, for example those searching for supernovae or looking to measure baryonic features in the large-scale galaxy and mass distributions, will not extend previous sensitivity, resolution or wavelength limits. Rather they achieve the required precision by observing much larger areas of sky than has previously been possible. Such surveys may not enable significant progress in other areas of astrophysics. For example, deep photometric imaging of 2 square degrees of the sky has already been completed and provides data for hundreds of thousands of faint galaxies. Rather few studies of the formation and evolution of galaxies would benefit from the 1000 times larger but otherwise similar samples provided by Dark Energy surveys. Since existing instrumentation can match the capabilities of these surveys, there is also little incentive to operate them in observatory mode.

The potential danger here is evident. The convergence between astronomers and fundamental physicists produces a powerful lobby in favour of Dark Energy experiments. In the natural competition between proposed large projects this works to the disadvantage of more traditional observatories at X-ray, radio, ultraviolet or infrared wavelengths. These command strong support only from astronomers, and so may be delayed, perhaps indefinitely, by financial constraints resulting from implementation of “higher priority” Dark Energy experiments. Astronomers will spend their time, energy and resources on experiments which have little impact on their main areas of research, while sacrificing the facilities which have traditionally driven creativity, innovation and the advance of knowledge in their field.

This leads to the third, and in my view most serious danger. By accepting the fundamentalist view that Dark Energy is so important that clarifying its nature is the overiding problem for current astrophysics, astrophysicists betray the underlying culture of their field and undermine its attractiveness both to future generations of creative scientists and to the public at large. This is exacerbated by other sociological trends within astrophysics which I now digress briefly to discuss.

In figure 3 I show bibliographic statistics, compiled from NASA's Astrophysics Data System (ADS), to illustrate changes in astrophysics and space science over the last 30 years. In 1975 about 8500 different authors published a total of about 8900 papers in the refereed professional literature. By 2006 the number of authors had quadrupled but the number of papers had only doubled. On the other hand, the mean number of authors per paper also doubled, so that the number of papers signed by a typical astronomer remained constant at about 2 per year. The size of the astronomical community has thus increased dramatically and a drop in the mean productivity of its members has been masked by the tendency for more individuals to sign each paper. In 1975 over 40% of all papers in the major journals had a single author and fewer than 3% had 6 or more authors. In 2006 only 9% of papers had a single author while almost 28% had 6 or more authors. This trend towards team-based projects is undoubtedly real, but it is accentuated by the use of citations as a measure of performance, a practice which may influence another strong trend visible in figure 3: the reference lists of refereed astrophysics papers increased in length by a factor of 3.4 on average between 1975 and 2006. Since the number of citations to individuals for a given year is the product of the number of papers, the mean length of their reference lists, and the mean number of authors for the referenced papers, it was clearly much easier to get cited in 2006 than in 1975 or even in 1995! As an extreme example, the fourth ranked astrophysicist by citations to papers published over the last decade has never written a first-author paper for a refereed journal and has gained almost all his citations through his right to sign official papers by a large collaboration in which he played a purely functional role. The increasing number of such survey collaborations, usually put together to justify large time investments on major facilities, means that more and more astrophysicists work in directed, quasi-functional roles, and that fewer achieve visibility through truly creative science.

The concentration on large long-term projects has long dominated accelerator physics. Dark Energy projects will further accelerate this trend in astrophysics. Only with very large surveys can one hope for a percent level specification of the cosmic expansion and structural growth histories. Achievement of these primary survey goals will have little impact on astrophysics beyond the Dark Energy issue, and most survey researchers will need to concentrate on functional tasks to assure adequate data quality and timely completion of the project. Contrast this with the traditional, opportunistic style of the best astronomical research, where individuals or small groups think up new ideas or build new instruments and apply them to situations where the scientific return seems likely to be greatest. A forward-looking observatory development programme can ensure that there are always new problems to address and new opportunities to extend the scientific frontier. This is an attractive model for young researchers. They can have a major scientific impact already as graduate students and there is a clear path for them to establish themselves rapidly as independent players in an international and exciting field. Such opportunities are rare in big survey science, particularly in many Dark Energy projects.

This then is the third problem. If assembly of the very large surveys needed to constrain Dark Energy comes to dominate astronomical research, then the development of other new capabilities will be slowed, and opportunities to carry out creative individual research in most areas of astrophysics will be reduced. This will make our subject less attractive to the best and most ambitious young scientists, who will look to make their mark in other domains, biophysics or nanotechnology perhaps. Concentration on a single "fundamental" issue rather on the traditional diversity of issues will also make astronomy less attractive to the general public, undermining taxpayer support for the expensive facilities needed to pursue our science. Listening to the siren call of the fundamentalists may lose us both the creative brains and the instruments that are needed to remain vibrant. Dark Energy is the Pied Piper's pipe, luring astronomers away from their home territory to follow high-energy physicists down the path to professional extinction.

What is to be done?

None of the negative consequences I have just outlined need necessarily follow from our current situation. My intention in this essay has been to draw attention to the dangers of uncritically accepting that astronomers should spend much of their energy and resources trying to clarify the nature of Dark Energy, just because it is perceived as a fundamental (perhaps *the* fundamental) problem by high-energy physicists. In my view a hard-nosed cost-benefit analysis is needed, recognising both the inherent limitations of observational astrophysics and the substantial cultural differences between the astronomy and high-energy physics communities.

Dark Energy is a deep and interesting puzzle which can be probed by astronomical observations alone, but it is one interesting puzzle among many and it may be one of the least likely to be “solved”. We do not know if astronomers can deliver measurements of the hoped-for precision, but even if they do, it seems likely that high-energy theorists will construct many Dark Energy models consistent with the observed expansion and structure growth histories. Dark Energy will be constrained, many possibilities will be excluded, but many others will remain. Astronomers must be aware of this and must balance the needs of Dark Energy projects against those of the core areas of our field. New observatories promote exploration throughout astrophysics. They nourish the diversity and provide the opportunities for individual creativity which underlie its current flourishing. We must not be seduced by the fundamental nature of Dark Energy (and by the availability of new funding sources) into sacrificing the foundation of our subject’s strength.

Here are some suggestions for accepting Dark Energy as a prime subject for astronomical study while embracing neither the fundamentalist view that it is the most important problem of our time, nor the industrial work patterns engendered by “Big Science” surveys of the kind required to significantly tighten constraints on its properties.

1. Astrophysicists should recognise the cultural differences between their own field and high-energy physics. They should be willing to argue that astronomical discoveries – that the Universe expands, that the chemical elements were built in stars, that black holes exist and can be far more massive than the Sun, that galaxies continually change form, that other planets orbit other stars – although qualitatively different, are no less significant for humanity than the clarification of the underlying nature of forces and particles. They should resist the fundamentalist argument that searching for the ultimate structure of space, time and matter is deeper and more basic, and thus takes intellectual priority over other ways of extending our knowledge of the physical world.
2. Large astronomical projects, even those for which Dark Energy issues are a prime science driver, should continue to be designed to push back the frontiers in many areas of astrophysics. Supernova surveys should store enough information to explore the supernova mechanism and the relation of supernovae to the stellar populations from which they form, as well to trace out the expansion history of the Universe. Galaxy redshift surveys should take sufficiently good spectra for a sufficiently well-defined set of galaxies that galaxy evolution can be studied, in addition to measuring the characteristic scale of galaxy clustering for use as a standard measuring rod. This is simply good astronomical practice, spreading the risk to compensate for the fact that astronomers cannot ensure “proper” laboratory conditions for their experiments.
3. Prioritisation of projects should be based not only on the case for their prime science goal, but also on the extent to which they will enable future advances in astrophysics

as a whole. In the case of Dark Energy surveys, this means recognising that refinement of the principal quantities to be measured, the cosmic expansion and linear fluctuation growth histories, is unlikely to impact significantly on other areas of physical cosmology. Thus, the enabling aspect of such surveys will come mainly from other science.

4. Large projects require large teams and long time-scales. The negative effects of this on young scientists' opportunities for creativity can be drastic and must be mitigated by promoting a diverse set of science goals for exploration by young team members. Both the Two Degree Field Galaxy Redshift Survey (2dFGRS) and the Sloan Digital Sky Survey (SDSS) were originally set up with a relatively narrow set of primary science goals, but the teams involved were eventually able to address a very broad range of problems with their survey data, and many of these efforts were led by the younger scientists. In the case of SDSS, the release of the full survey data through a powerful, publicly accessible database has allowed astronomers across the world to carry out their own SDSS projects, thereby enhancing the whole community's opportunities for individual creativity.

5. Credit for scientific contributions must be clearly assigned to those responsible for the original insights and for the creative aspects of the enabling work. Hard work alone brings little progress, and appropriate recognition is a prime incentive attracting creative scientists to our field. Current assessment culture in astrophysics is based mainly on total citations to papers signed by a scientist, regardless of whether (s)he is sole author or author number 47 out of 165. This encourages inflated author and reference lists which dilute the visibility of creative work over and above the dilution already caused by the trend towards large teams. This could be off-set in part by greater reliance on first author citations (in astrophysics the first author is usually the person with primary scientific responsibility for a paper) and on normalised citations (where an author is credited with $1/N$ for a citation to a N-author paper). This would remove the temptation to inflate author lists and provide a fairer comparison of the overall impact of individual astrophysicists. Unless we recognise them properly, those capable of original and creative contributions will prefer other fields.

6. Astrophysicists should motivate their activities in their own cultural context, not in that of high-energy physics. This is particularly important when interacting with students and young scientists. Dark Energy is undeniably a fascinating puzzle, but it is a high-energy physics puzzle. The creativity in *understanding* Dark Energy will not come from planned astronomical surveys. They will provide more precise measurements of quantities that are already well enough known for astrophysics. Although reaching such precision is a major challenge, it is a challenge that offers little opportunity for

> scientific creativity unless one is primarily interested in the processing of large datasets or the statistics of data analysis. Bright and ambitious students will decide to become astrophysicists only if they see an opportunity to make high-impact contributions as individuals. Within Dark Energy surveys, such opportunities will come mainly from studies of astronomical objects. In the rush to gain a funding edge by giving projects a Dark Energy label, it is essential to avoid giving the impression that the astronomical science is "secondary", of less significance or interest than improved measurements of the cosmic expansion and structure growth histories. Indeed, it could turn out that Dark Energy is more complex (or different) than most models suppose, and that critical clues to its nature emerge from traditional astronomical exploration of the phenomenology of structure, rather than from these precision measurements.

Astronomy often claims to be the oldest of the physical sciences and it has a broader cultural and intellectual resonance with educated society than any other branch of physics. For this reason many university departments see astronomy as an ambassador for physics, providing the non-scientific public with some understanding of the scientific method and drawing students into physics from a wide catchment area. The attraction lies in astronomy's diversity, in its combination of a lack of direct application to human society with insights into the development not only of our own world, but also of the larger cosmos in which it is embedded. These strengths are different from and complementary to those of fundamental physics. The continued vitality of astrophysics does not depend on its ability to constrain the Deep Truth underlying all reality, but rather on its ability to retain our own and our public's fascination with the many-facetted views it offers of the processes which shaped our Universe and of the objects which populate it.

Acknowledgements

This essay grew out of a talk given in summer 2006 in the director's Blackboard Lunch at the Kavli Institute for Theoretical Physics. I'd like to thank the director, David Gross, for the invitation to give a talk and for his spirited debate of the talk he got.[1] I'd also like to acknowledge the unique atmosphere of this institute, which strives with remarkable success to promote cross-fertilisation between all branches of physics, fundamental and otherwise. Finally, I would like to thank Alberto Accomazzi of the Smithsonian/NASA Astronomical Data System for his help in compiling the publication statistics shown in figure 3.

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[1] This debate can be viewed "live" at http://online.kitp.ucsb.edu/online/bblunch/white1/

Fig. 1.— Two emblematic pictures from the Hubble Space Telescope. On the left is an image of the Eagle Nebula, a set of gas clouds illuminated by young stars and enshrouding a number of stars in formation. On the right is an image of the Hubble Deep Field. At the time it was released in 1996 this was by far the deepest image of the sky ever made, showing galaxies so distant that they are seen when the Universe was a small fraction of its present age.

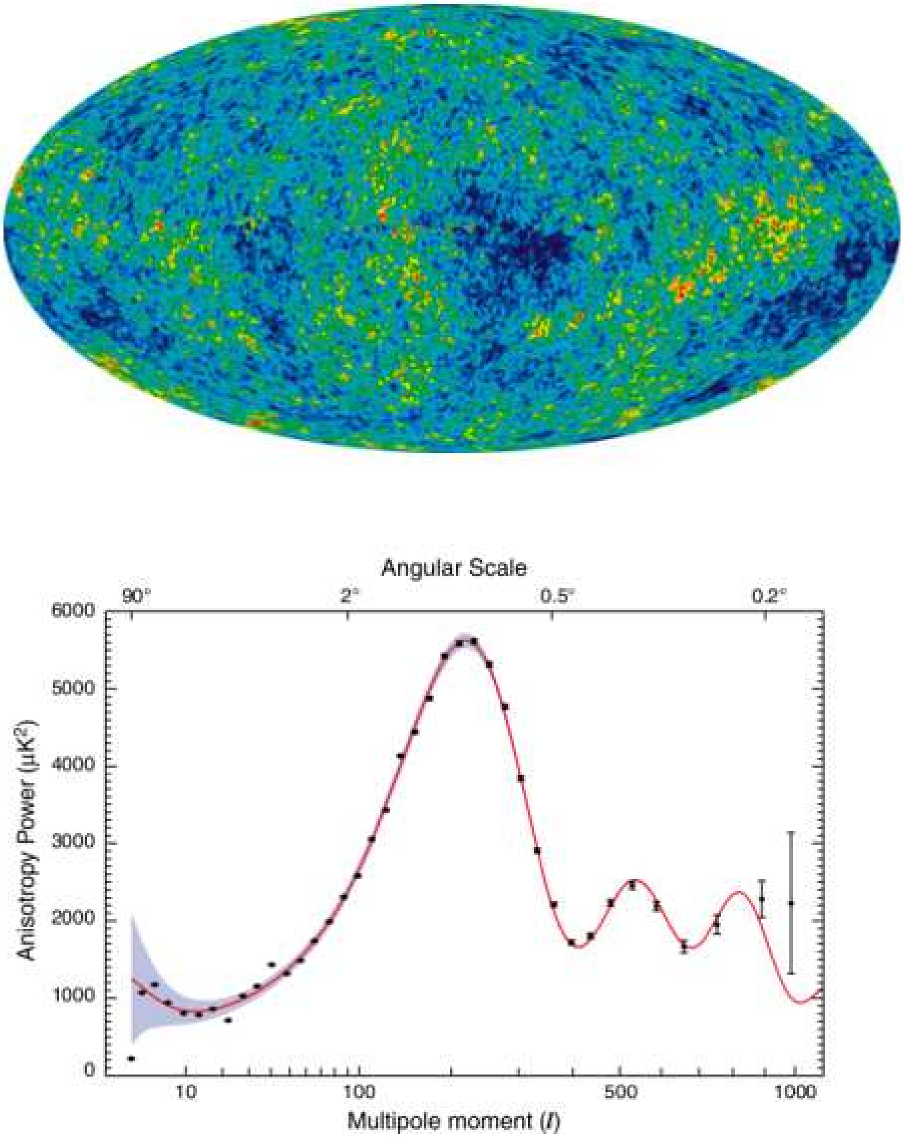

Fig. 2.— Two emblematic pictures from the Wilkinson Microwave Anisotropy Probe. At the top is the WMAP map of temperature fluctuations in the cosmic microwave background radiation. These fluctuations are very weak, with typical amplitudes of a few parts in 100,000. They are a direct image of structure in the Universe when it was only 400,000 years old. Below the map, its power spectrum (the points with error bars) is compared with an *a priori* model (the smooth curve) which assumes that all structure originated as quantum zero-point fluctuations during a very early period of inflationary expansion. The six parameters specifying the model all have physical meanings and they all take values which are quite compatible with those inferred from independent astronomical observations

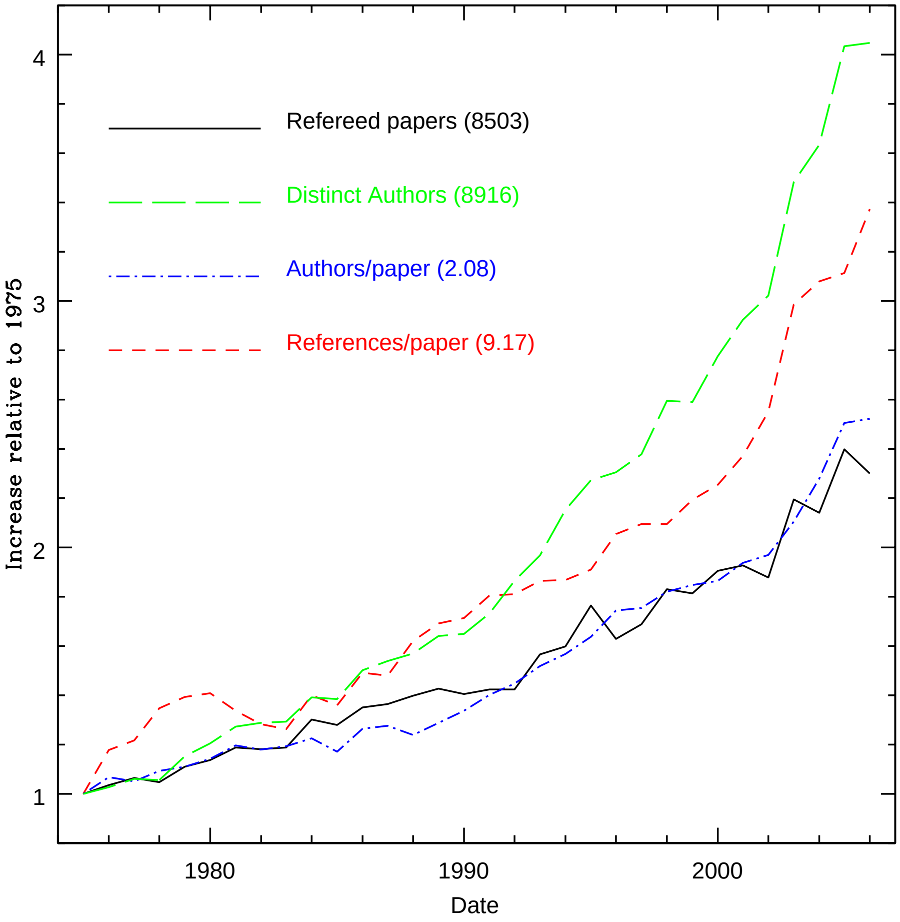

Fig. 3.— Publication statistics based on data from the Smithsonian/NASA Astrophysical Data System showing the growth of activity in astrophysics over the period 1975-2006. The continuous black line refers to the annual count of astrophysical papers in refereed journals, the long-dashed green line to the total number of distinct authors of these papers, the dot-dashed blue line to the average number of authors signing a paper, and the short-dashed red line to the average number of entries in each paper's reference list. All statistics are normalised to be unity in 1975. The actual 1975 values for each are shown in parentheses after the labels in the figure. Since 1975 the number of active astrophysicists has quadrupled. The number of refereed papers and the number of authors per paper has more than doubled. The typical number of papers cited has more than tripled.