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The Growth of Astrophysical Understanding

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THE GROWTH OF ASTROPHYSICAL UNDERSTANDING

The post-war years 1945-1970 saw the discovery of a large number of astrophysical phenomena, few of which had been predicted by theory. They were revealed through the introduction into astronomy of new instrumentation, much of which had been inherited from the military or made available by the communications industry. These new phenomena required alteration of, or assimilation into, a by-then-antiquated astronomical landscape. Although the long-term planning documents issued at ten year intervals in the United States, in a process that came to be known as the Decadal Review, emphasized the promise of further discoveries, the main thrust of astrophysics became an attempt to understand better the phenomena that had already been discovered. How these changes came about, and how they related to the plans advocated by the Decadal Reviews, is the subject of this article, which itself is an outline of a book I am writing. The growth of understanding is discussed in terms of limits on the evolution of the astronomical community and astrophysical thought, and in terms of the social networks that may explain the functioning of the community, its style of work, and the criteria for acceptance or rejection of new explanations. The thrust of the work is to assemble an analysis that could provide guidance useful in the construction of future long-term communal plans.

A Layered Model of Astrophysics

Scientific systems tend to have layers that remain invisible to the working scientist whose main aim is to solve problems in some rational process. The domain in which he solves these problems is the landscape he has constructed. The astronomical landscape is the astronomer’s vision of the Universe we inhabit. In that landscape he sees the interaction of stars with their surroundings, and the interplay of galaxies within their clusters. The galaxies and clusters evolve from an earlier, intensely hot phase of the Universe in which the helium that now pervades the Cosmos was first forged at temperatures of a billion degrees. The aim of the astrophysicist is to tidy this landscape, making new observations to gather new data, and trying to fit these neatly into the landscape so that nothing is missing and everything is explained. It is an arduous project. Each newly gathered piece of evidence clamours for its place in the landscape, but often does not fit. Other pieces have to be moved around to accommodate it, and the face of the landscape changes. These reconfigurations are part of the astrophysicist’s daily work. He gathers new data because fitting them into the landscape enriches it, making it a better representation of the Universe.

In everyday work, the layer that constitutes the landscape is largely shielded from other human activities. But it is not totally shielded, even though the astrophysicist often proceeds as though it were. Occasionally, intense activity in some other domain, some other layer of human activity that normally may be safely ignored, impinges on the astronomical landscape, altering its face bewilderingly. Where there was a plane (Figure 1), there is now a mountain (Figure 2). The new landscape has been unrecognizably disfigured. Astrophysics is in disarray. Social scientists nod wisely and talk of paradigm shifts.

Layered Processes 1

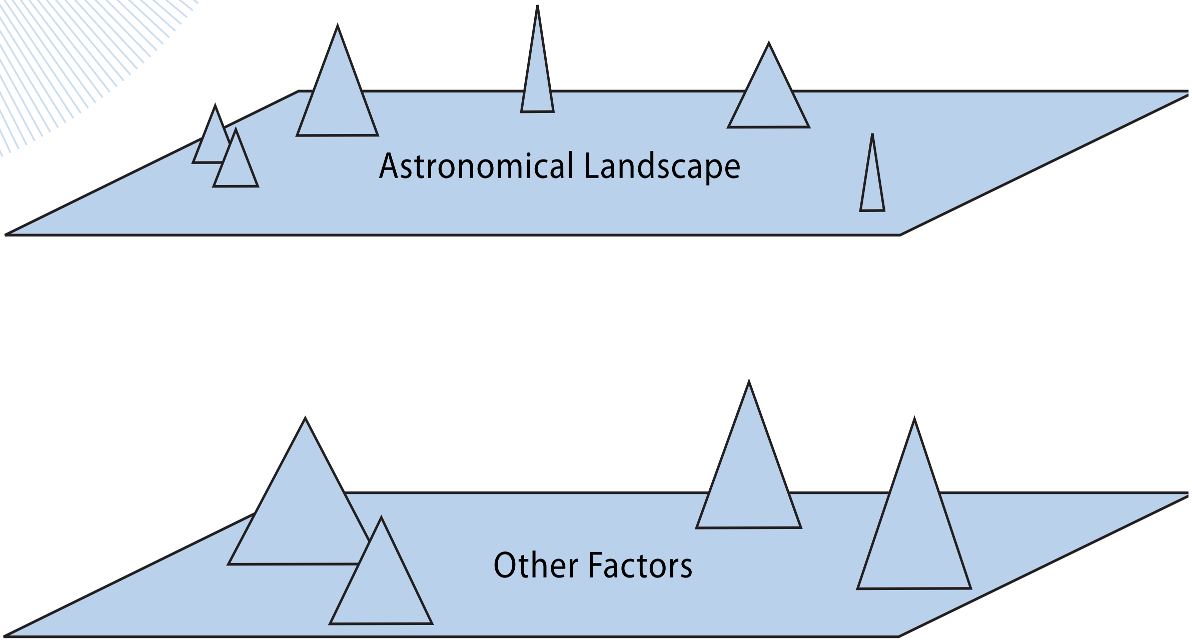

Figure 1: Astrophysics evolves not only in response to changes internal to its own landscape, but also to intrusion from other layers, or realms of activity. These varied layers may, respectively, reflect development of new instrumental techniques, novel tools developed in theoretical astrophysics, new ways of imaging and assimilating information, budgetary constraints, political decision, etc.

Layered Processes 2

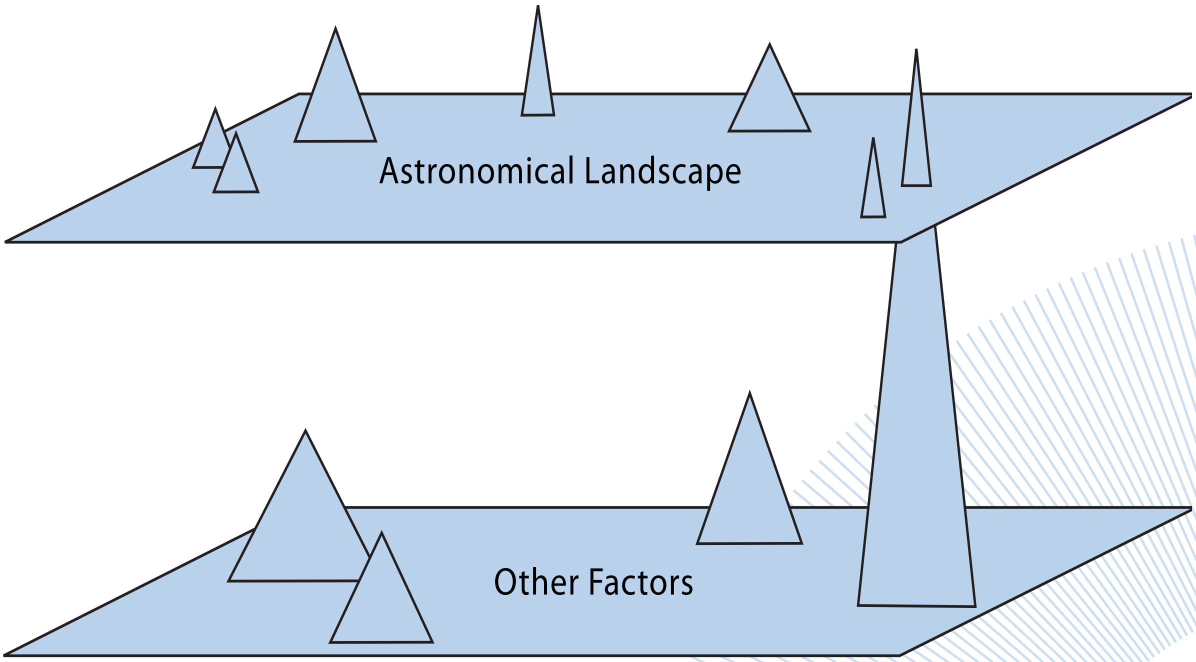

Figure 2: Evolving activities in other layers normally do not intrude on the astronomical landscape. However, when they grow to a critical point, they may suddenly force their way into this landscape, affecting its subsequent appearance and evolution.

In the post-World War II era, a sequence of observational discoveries rattled the field and, time after time, reconfigured the landscape. Often the astronomical community did not even have time to pick up the pieces and reassemble them before the next discovery disrupted the efforts. Each new discovery came as a surprise because the landscape that had been previously sculpted had left no room for the unexpected. Astrophysical theories had been too narrowly defined by observations solely gathered at visible wavelengths in all the years leading up to World War II (Harwit, 1981).

The War and the post-war world changed all this, providing astronomy with new tools, radio telescopes, detectors sensitive to infrared radiation, X- and gamma-rays, and rockets to transport these and ultraviolet detectors to regions well above Earth's atmosphere. Pointed at the celestial sphere, these tools were able to register astrophysical processes that leave no imprint of themselves that visible light could convey. Taken as a group the tools constituted a domain, a layer of objects and activity, from which the astronomical landscape had previously appeared detached and immune, a layer that comprised all the conceivable instruments with which the Universe might be surveyed. These instruments had no assigned place in the astronomical landscape because they were not part of it. They were merely a means for gathering data, whereas the astronomical landscape was pieced together solely from data regardless of the means. Once new pieces of data were accepted as fact, the architects of the landscape had essentially erased all traces of how the data had been obtained. It had not seemed to matter.

Conversely, if you were cognizant of the instrumental landscape, a landscape just as real and perhaps just as complex as the astronomical landscape, you were in possession of information about all the conceivable ways in which the Cosmos might ultimately be studied and had a map which defined precisely where observational astronomy still had failed to take a foothold (Figure 3). These were the areas where new observations would most likely produce further surprises, further paradigm shifts. The Universe is sufficiently complex that theoretical predictions usually fail when extended too far. This is particularly true when extrapolating

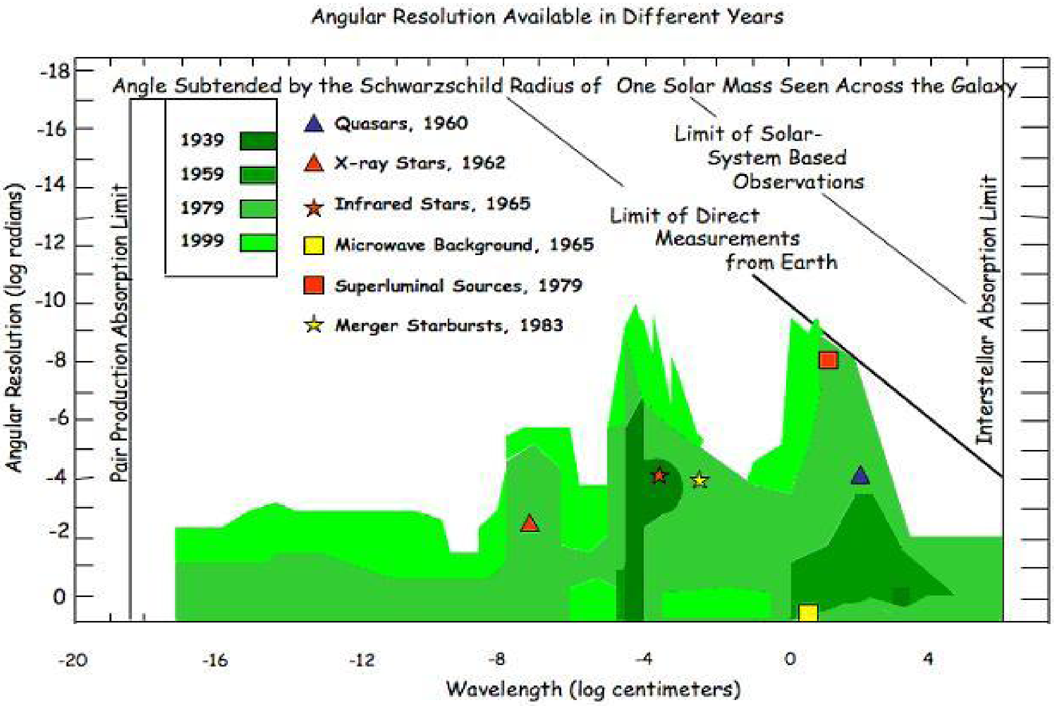

Figure 3: The range of observable wavelengths of electromagnetic radiation, the limits to angular resolving power, i.e., image quality and detail, the fraction of this range accessible to instrumentation developed at different times, and the phenomena discovered as new instrumentation was introduced into astronomy in the course of successive twenty-year intervals.

from realms of the astronomical landscape that are satisfactorily understood — because the instruments required to provide the necessary data have been fully exploited — to realms which have been insufficiently studied for lack of essential instrumentation.

Although the layer of instrumentation may be the most influential extrinsic layer affecting our grasp of the Cosmos, it is not the sole other layer that affects restructuring of the astronomical landscape. In the United States, Presidential decrees commanding new directions of research that the government should fund can overnight decide that work on tidying up the landscape will have to wait for lack of funding or that a new part of the landscape may become accessible that had been too difficult to study under previous funding schemes. In one case the architects or the sculptors of the landscape may have to wait until policies change again before they can effectively resume their work; in the other, new portions of the landscape will emerge that have to be put in order.

It is this complex layered system that I seek to model and explain in my full-length study — the interplay between the landscape the astrophysicist constructs, and the world beyond his control that forever wafts him this way or that, sometimes in ways that make his task easier, at other times with setbacks that test his ingenuity. For, it is not every government that favors science nor every society that provides the open atmosphere for discourse without which science cannot advance.

At first glance it might appear that outside influences that can affect and reshape the astronomical landscape would be innumerable and unbounded. In practice, relatively few such extrinsic forces appear to be at play. For observational astronomy, the factors that leave the most significant imprint on the landscape are developments in other fields that open new windows on the Universe, enabling new kinds of information to reach us to reveal the Cosmos from a new perspective that was unavailable before. When the information that can in principle be transmitted to the observer is bounded, the body of information on which additional studies on the Universe can be contemplated also becomes bounded. We will see later on that this is indeed the case. The innate architecture of the Universe we inhabit is such that only a finite, albeit large, volume of information can reach us (Figure 3).

Observational limitations, however, are not the sole hurdle to overcome in a search for understanding. As long as we firmly believe that a rational description of the Universe exists, the information the observer gathers has to be analyzed and explained in terms of laws of Nature that may not yet be known — although the working astrophysicist first attempts to provide explanations based on known laws of physics. To do this, he often has to introduce into astronomical usage techniques that have been developed for other branches of science — physics, chemistry, mineralogy, biology. These constitute a further extrinsic layer, the influence of which may be profound for developments in our understanding. Computational techniques can also play a major role. Information storage or processing becomes increasingly important as data flows increase. Rapid developments in these two layers, that we might respectively label ‘science theory’ and ‘information processing’, can then have a sudden, previously unanticipated, impact on the growth of understanding as new analytical techniques developed by other sciences, or data handling and processing, become available. The astronomical community is relatively small and therefore benefits greatly if it can import, wholesale, theories or techniques developed for other fields. It is the only way that rapid progress can be maintained on sculpting the landscape to unravel the intrinsic architecture of the Universe.

Wholesale adoption by astronomers of powerful observational and theoretical techniques developed in other disciplines or for everyday societal usage has frequently led to paradigm shifts. Although often portrayed as totally unpredictable, it is more probable that a careful analysis of the evolution of extrinsic layers of activity most likely to have an impact on the astronomical landscape could, in many cases, be anticipated and exploited. Paradigm shifts might then no longer be portrayed in terms of catastrophe theory or caustics, as has sometimes been maintained in other fields that have been shaken by unanticipated events, but would come to be seen as natural consequences of importing powerful new techniques.

Budgetary constraints are the most common steering mechanisms used by governments to inspire scientists to investigate new problems or dissuade them from working on others. Most scientists follow the money; control of the purse channels scientific projects; investigations that looked promising are dropped for lack of funding; new projects are started because they can be financed and because they look at least quite interesting, if not as interesting as those being dropped. Rarely do scientists swim upstream against the budgetary flow.

Although my preoccupation in the present investigation concentrates on the way our understanding of the Universe has evolved, other human enterprises may be subject to similar forces exerted by extrinsic layers of activities or resources on the conduct of the enterprise. In science, political, religious, or budgetary influences play a particularly important role and can rarely be avoided. Some scientific questions are considered too dangerous politically to investigate, either because they could cause sectarian unrest or accidentally spread destruction or diseases. Scientific investigations most affected by such considerations are genetic, biological, or racial, or may involve issues of gender or the use of virulent organisms, radioactive material, or explosives. But even astrophysics does not remain entirely immune to such pressures.

Growth of the Astrophysical Community

The rate at which astrophysical understanding accumulates might be expected to correspond in some fashion to the number of active investigators. But does it? And, if so, what are the limits that determine how many scientists might or should devote themselves to astronomical research?

Astronomy is a luxury that only affluent societies can afford. The field rarely serves society's practical purposes. In earlier times, when human activities were thought to be determined by the configuration of planets, and the occasional passage of a comet or the appearance of a guest star was dreaded by Chinese emperors as a sign of impending disaster, astrologers did have their place as counsellors and guides. But professional astronomers long ago split with and cast doubt on the activities of astrologers, so that astronomers now serve only a far more limited range of practical functions. Today, the community can only serve a genuinely life-saving purpose by studying asteroids that might some day threaten to collide with Earth, wreaking havoc on a scale that appears to have caused the extinction of dinosaurs.

Other, perhaps less obvious, practical functions astrophysicists can serve involve the study of matter at very high densities and temperatures, normally found only at the centers of stars but quite possibly of great value as a source of energy for future generations once coal and oil are no longer viable resources. Through the conversion into helium of the large amounts of hydrogen found in water, we might then tap virtually inexhaustible energy supplies, such as those now known to make the sun shine. In such ways astronomers can make themselves useful. But since most of astronomy's activities are aimed at more esoteric investigations, society pays for the conduct of astronomy not for its utility, but rather because people are curious about their place in the Universe — a mixture of religion and awe. Astronomers are asked to elucidate the relation between mankind and the Cosmos. Because this is a topic that comes to mind only in leisure hours, astronomy flourishes solely in societies that can afford leisure.

Young astronomers brimming with enthusiasm often cannot understand why nations do not spend greater sums on astronomy. The answer is simple. Most people would rather feed their children, assure them of good health, send them to well-run schools, and secure them a predictable future, than be taxed to support astronomy at the expense of their children.

A primary and readily understandable limit on the size of the community of astronomers and astrophysicists, then, has to be funding that society can afford. But other limits exist as well. The theoretical archaeologist Roland Fletcher has identified three such limits (Fletcher, 1995). Although he defines these in the context of the growth of human settlements — villages, towns and cities — they also serve nicely to describe limits to the growth of the astronomical community.

The first of these limits arises where the population density of a community becomes so high that people interfere with each other's activities. When that happens, a community ceases to grow. Its members move elsewhere. Fletcher calls this limit the 'interference limit' and designates it by the letter I. This has its parallels in the sciences, where a young individual may decide to enter a new rather than an established discipline, because the newer realms of science, though far more uncertain in potential yield, might offer more freedom to flourish.

The mutual interference between astronomers is perhaps nowhere as apparent as in what the directors of major astronomical observatories call their oversubscription rates. A high oversubscription is considered a mark of distinction. If four times as many astronomers apply to make use of an observatory as can actually be accommodated, the observatory is deemed to offer services more highly valued than those of an observatory with an oversubscription only of a factor of three. The unfortunate side of this equation is that three out of every four astronomers are turned away by the first of these observatories, while two out of three are denied access by the second.

Such oversubscription rates are not uncommon, either for observatories or for funding agencies supporting astronomy. The result is that astronomers today spend inordinate amounts of time and effort writing proposals, knowing that the chances of submitting one that succeeds might require the submission of another three that, on average, will be turned down. Since the entire system of graduate student apprenticeship depends on raising the resources required to support a student's research, most senior researchers involved in supervising students cannot escape competing for this support. Instead of devoting themselves to research, which is what these scientists do best, they spend their time competing for support to finance their work and that of their students. This is where the I-limit is most visibly approached. When astronomers reach the limit where they spend all of their time writing grant applications and none on research, we will have reached the limit of no further growth.

Venturing into a sparsely populated new field, however, is not without its dangers either. If the field attracts little interest, nobody will care much about what a researcher finds. His publications will not be read and research funding will be hard to attract. No matter how intrinsically interesting the subject, the work is likely to find little appreciation, sometimes until long after the researcher has left the scene when a new generation discovers the field, remains unaware of the earlier work and repeats it without realizing that it had all been done before. Pity the scientist who is ahead of his times. His life is unenviable.

Fletcher defines a threshold limit, designated T, below which a community cannot expect to flourish. A flourishing science requires interaction, discussion, debate, cross-checking, refutation, acceptance, and a thrust toward answering new problems once earlier questions have been satisfactorily addressed. In astrophysics this limit makes itself felt in any new branch of the discipline being created by just a single investigator, or perhaps a very few individuals. Unless they are successful in attracting more colleagues, either through interesting new results they can quickly demonstrate or through a clear demonstration that rewarding results are near at hand, their efforts are doomed to dry on the vine. The larger community will ignore their work; nobody will bother with it. A well-functioning scientific field requires a level of activity above the T-limit and below the I-limit.

Fletcher's third and final limit is the communication limit, designated C. Unless satisfactory lines of communication can be maintained across a field it will not flourish either. The field may then split into smaller sections that hardly maintain contact. This is already characteristic of several large, well-established fields, for example, physics. In recent decades the physics community has splintered into sections that publish in separate journals. At one time the American Physical Society published a single journal for professional physicists, *The Physical Review*. This has now split into several journals. *Physical Review A* publishes research articles concentrating on atomic, molecular and optical physics. *Physical Review B* publishes results specializing in condensed-matter phenomena and materials physics. *Physical Review D* provides an outlet for work on particles, fields, gravitation and cosmology.

These sections rarely interact, but in order not to lose total oversight of what physics tries to accomplish, the Physical Society does publish *Physical Review Letters* which accepts short research accounts that may be of interest to physicists working in sections quite different from their own.

Astronomers and astrophysicists are now approaching a point where a lack of communication no longer permits them to maintain insight on work conducted in areas other than their own. Those trying to understand the atmospheres of Solar System planets are unlikely to read papers on the evolution of the Universe when its age was less than one second — and vice versa.

As Fletcher points out the I- and C-limits are not absolute. They can be overcome by taking suitable measures. In astronomy and astrophysics the tendency to avoid the limit is expressed in an observable growth of joint projects. Instead of competing against colleagues, you join them, often in the name of accomplishing more through joint rather than separate efforts. Large consortia then devote themselves to building new observatories, on the ground or in space, or novel computing facilities that can be made available to others in the community. The joining of forces is also notable in the increasing number of names on the by-lines of articles that, in earlier decades, might have been authored by just one individual or perhaps by a student and his professor. It is not clear whether joint authorship leads to a dilution of critical examination before a scientific paper is submitted for publication.

Over-reliance on co-authors can be tantamount to diminished individual responsibility and perhaps a lowering of standards. Nevertheless, the astronomical community is attempting to cope with the threat of mutual interference, though it may not always succeed.

Overcoming the communications limit may prove much harder. Splintering the field is clearly undesirable. Even investigators solely interested in planetary atmospheres, realize that such quantities as the ratio of hydrogen to helium in those atmospheres was determined at early epochs, when the age of the Cosmos was less than one minute and its temperature was in the billions of degrees. This is because the ratio of neutrons to protons at that early epoch determined the ratio of helium to hydrogen observed today. Something needs to hold astrophysics intact, otherwise we will miss the grander features of how the Universe evolved.

The problem does not reside in the flow of information. Information can be recorded and transmitted at sufficient speeds. Through searches on the web, work on different subjects by any number of different authors is readily recovered. Much of the recent literature in astrophysics, and the entire contents of several of the premier astronomical journals dating back to their inception, are available through the Smithsonian/NASA Astrophysics Data System. A daily download of new articles, many of them being submitted that same day for publication in journals, is available through the Los Alamos/Cornell arXiv system. And, although much of the older literature is missing, most of it in languages other than English, or else is held hostage to payment of copyright fees to publishers, these are not the major problems responsible for the C-limit. Rather, the community of astronomers or, for that matter, the larger community of scientists, has not found ways to organize these archives sufficiently well to find information more readily that is actually needed.

Beyond this, however, an even bigger problem is the assimilation of the vast amounts of information continually generated. The community may have to find ways to utilize information more quickly, either by improving techniques for entering it into our consciousness, or through means to extract key points from a mass of information within which they are embedded. Fortunately, rapid extraction and visualization of information are key areas in which research is being aggressively pursued for a variety of applications, ranging from computer games that are becoming increasingly complex to displays of financial markets where rapid comprehension of a continually changing scene can make the difference between the success or failure of a transaction. The C-limit needs to be overcome by many communities; the community of astronomers is definitely among them.

A Small-World Model

Is the layered structure described above related to the small-world model pioneered by Watts, Strogatz and an increasing number of social scientists and mathematicians in recent years (cf. Watts, 1999, 2002, 2003; Newman 2001, 2004; Ormerod, 2006)? And, if so, how are the two structures related?

To a large extent, the layers I have described above represent the work of different groups of actors in the terminology of the small-world model. Within any one group, whether it be astrophysicists, instrument makers, theoretical physicists, or government officials, the interaction between actors is close and, although only a few of the astrophysicists also have close ties to government officials and somewhat more of them to instrument builders or theoretical physicists, these few bridging links between actors in different groups suffice to bring all of the actors into close contact.

A temptation, then, would be to identify the layers described above with the corresponding groups of actors. This leads to an oversimplification, however. Two problems arise: First, the layers represent the activities generally pursued by the actors in the different groups. But the groups of actors do not map one-to-one onto the different landscapes or layers. The history of astronomy shows that many observational discoveries in the field have been made by instrumentalists who pointed novel observing equipment at the sky and observed a surprising phenomenon. These scientist/engineers had no significant contact with astronomers before their discovery, and only established closer links to astronomers later, in the process of publishing their results. They cannot easily be fitted into the small-world model of actors that belong to two different groups because they joined the group of astronomers, if at all, only after they made their contribution to the field, rather than before as the small-world model would suggest. To some extent this is also true of the theoretical physicists who made fundamental contributions to astronomy and cosmology.

People like Hans Bethe, Albert Einstein and J. Robert Oppenheimer would have thought of themselves purely as theoretical physicists bringing a useful new tool to bear on astronomical questions. They had developed the tool and wished to apply it to whatever problems it might help to solve. An indication of the communities or groups which Einstein, Bethe and Oppenheimer considered themselves to belong to is seen from the journals in which they chose to publish their respective cosmological and astrophysical findings, all of which appeared in journals issued by physical rather than astronomical societies. Any contacts with astronomers they may have had in the process would have served primarily to make sure they were not applying their methodologies to a make-believe world, rather than to the astronomical landscape as understood by professional astronomers of the day. Much of this information may however have been available to them through books and journals, so that personal contacts with astronomers might not even have been their primary source of information.

Second, the primary imports from one layer to another by these members from other groups — marginal workers as Edge and Mulkay termed them because they worked at the margins of the field rather than being central to it — were not people but rather tools (Edge and Mulkay, 1976; Edge, 1977). Einstein did write a few additional papers on cosmology between 1917 when he developed his first world model and 1955 when he died. Most of these were comments on the work of others who had adopted his approach and developed an increasing number of dynamic models of the Cosmos, expanding, contracting, oscillating, and so forth. While literally hundreds of articles adopting general relativity as the basis of cosmological thought appeared in the decades after 1917, it was not until 1945 that Einstein himself and a younger colleague, Ernst G. Straus, wrote another significant cosmological paper, this time to clarify realistically the behaviour of space in terms of general relativity (Einstein and Straus, 1945). They showed why space should be expanding on large scales, although it is static in the immediate surroundings of massive bodies like galaxies. Meanwhile, from 1917 to 1945, Einstein had devoted himself to countless other problems of pure and applied physics that did not involve the Cosmos at all. It is quite unlikely that he considered himself anything like a full member of the group of astronomers. Nor did it matter. His primary contribution to the field was his new tool, general relativity. Whether he himself contributed to the field was largely immaterial. There were dozens of others who were willing to do this, among them Ralph Alpher, Herman Bondi, Willem de Sitter, Alexandr Friedman, Arthur Stanley Eddington, George Gamow, Robert Herman, Fred Hoyle, Pasqual Jordan, Georges Lemaître, J. Robert Oppenheimer, H. P. Robertson, Engelbert Schücking and Karl Schwarzschild, to cite only a few of the leading figures at random.

Hans Bethe, who first introduced nuclear physics in a complete way into astronomy, with his 1939 identification of the energy sources that permit stars to shine for billions of years (Bethe, 1939), wrote three additional papers on related topics in 1940 and 1941, but then did not return to astronomical work for a quarter of a century. In the meantime, his groundbreaking work, based on the theoretical approach he had introduced, had come to dominate totally the field of stellar structure, energy generation, and nucleosynthesis. His personal presence was no longer required. The presence of the tools he had bequeathed the discipline was paramount.

Oppenheimer wrote two papers in 1939 with his students G. M. Volkoff and Hartland Snyder, respectively, on neutron stars and on stellar black holes (Oppenheimer and Volkoff, 1939; Oppenheimer and Snyder, 1939). There he introduced nuclear physics into the context of general relativity and applied these tools to an astrophysical setting. He never returned to astrophysics, except to offer concluding remarks at a 1949 symposium on cosmic rays. Yet the tools he had introduced had significant influence on the subsequent development of astronomy.

From these examples, it seems clear that the groups and actors that the theory of small-worlds includes cannot provide the whole story. Landscapes (i.e., the context within which the groups labour) and objects, in particular tools, play enormously important roles that the small-worlds model does not yet incorporate, but will have to in order to become genuinely useful for discussing the evolution of our understanding of the Universe.

Astrophysical Explanation

The acceptance of astrophysical explanations is not at all predictable. A great deal depends on the sense of the community of researchers at the time. Some theories are accepted at once. They are judged to be that compelling. Others may take years or even decades to gain acceptance.

In the 1960s Fritz Zwicky and Milton Humason at the Mount Palomar Observatory noted that, when they summed the masses of all the galaxies in a large cluster, the total mass obtained seemed remarkably low (Zwicky and Humason, 1964). The galaxies were traversing the cluster at speeds of some hundreds of kilometers per second. At these high velocities they would rapidly escape the cluster, dissolving it altogether unless some much larger additional amount of mass kept the galaxies gravitationally bound. Zwicky's ideas were noted, but not given much attention. Nobody knew where galaxies originate and whether clusters of galaxies are stable and long-lived. Only in the final decades of the century did the puzzle gain further interest when, primarily, Vera Rubin and her co-workers at the Carnegie Institution of Washington found that the velocities of stars within individual galaxies also were surprisingly high, implying that the galaxies similarly contained considerably more mass than the observed stars and gaseous clouds alone would indicate (cf. Sofue and Rubin, 2000). When the observations, both of the masses of galaxies and of galaxy clusters, could no longer be doubted, two possibilities emerged. Either Newton's laws of gravitational attraction no longer held on scales of galaxies or clusters, or else a new form of undetected matter, which came to be dubbed 'dark matter', existed and gravitationally bound the contents of galaxies and clusters to keep them intact.

At the moment, this second point of view dominates the thinking of the community. Physicists are hard at work trying to understand how a new form of matter might fit into theories of fundamental particles, and experimentalists are building instrumentation that might detect the more plausible types of novel matter.

Smaller groups of researchers are also pursuing the ideas of Mordehai Milgrom (1983), who first proposed a scheme in which gravitational attraction increases over distances comparable to the dimensions of galaxies. A reluctance to come to terms with this second point of view is that it would mean giving up, or at least modifying, general relativity, the theory that Einstein devised in 1915, which has served astrophysical theory well in describing the expansion of the Universe and the relative abundance of hydrogen, helium and other light elements derived from the fiery birth of the Universe.

Given such ongoing debates, is there a rational basis on which they may be ultimately resolved? What are the kinds of arguments that eventually lead to general agreement and acceptance of an explanation independently of whether it is right or wrong? In short, how do scientists reach at least provisory agreement on an issue?

One class of scientific explanation entails symmetry arguments. Consider a throw of dice. If you were to ask what the chances are that a single die will land with the pattern of five dots on the top surface, symmetry tells you that this will happen in one out of six throws. To reach this conclusion, we argue that the likelihood of the die landing with any face pointing up is the same for all faces. Since there are six faces, the chance of seeing the face with five dots is one in six (Figure 4).

A different class of explanation arises when a sceptic mistrusts this explanation and insists that its hypothesis be tested experimentally. He does this by throwing dice hundreds or thousands of times, only to find that the face with five dots lands facing upward, on average,

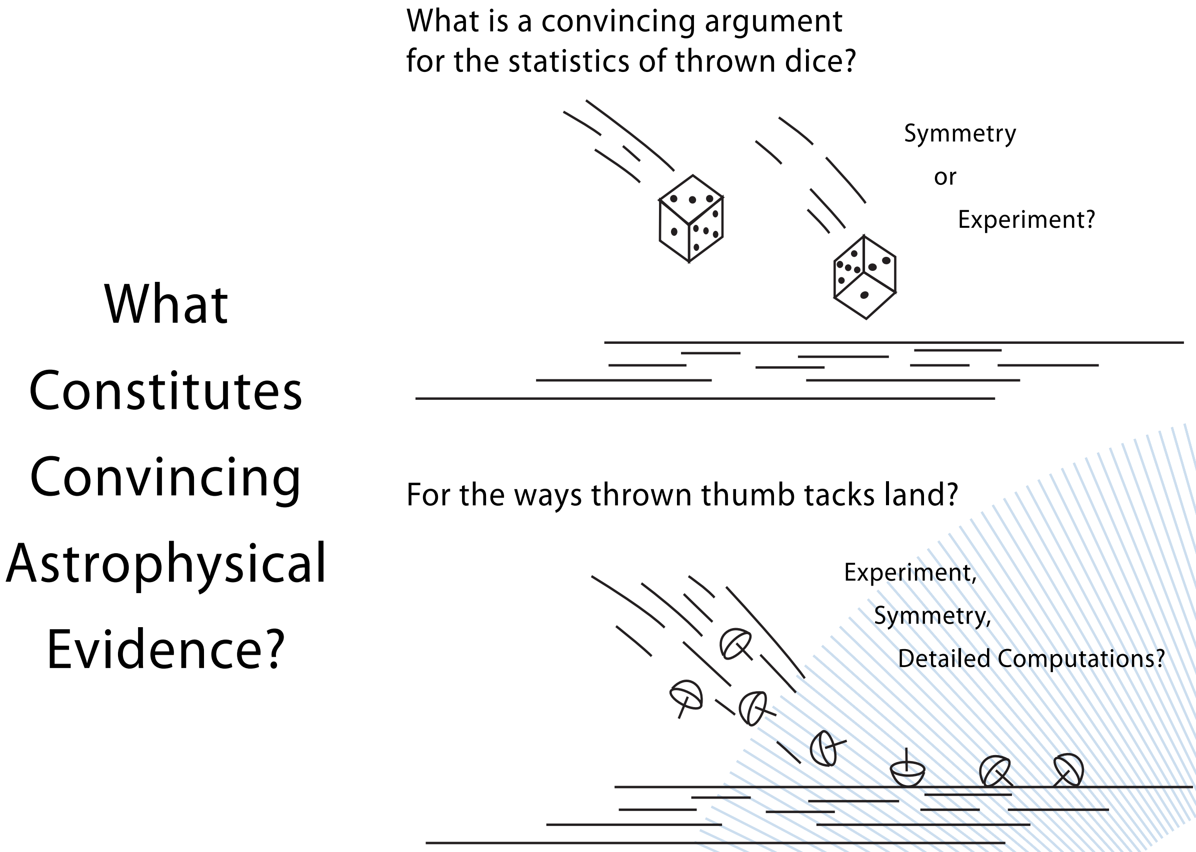

Figure 4: Symmetry arguments can be powerful and persuasive in astrophysics. As long as an astrophysical setting is not too complex, symmetry arguments tend to carry considerable weight. Once complexity sets in, most scientists will only be persuaded by detailed calculations or experiment. Even the likely final resting position of thrown thumbtacks tends to be too complex to be predicted without detailed calculations or to be resolved by an experimental run.

let us say, once in four times — not once in six. Evidently the dice are loaded. But even when nothing untoward is going on, the symmetry argument tends to gain credence only with simple systems. If we replace the tossing of dice onto a table with a scatter of thumbtacks randomly dropped from a height, some of the thumbtacks will land on their heads with the pin pointing upward and others will come to rest with both the rim of the head and the point of the pin touching the table top. The symmetry of the thumbtacks, though not terribly complex, already makes a prediction of these landing patterns much more difficult and certainly less credible.

A different theoretical approach might gain more general acceptance. It would involve a careful analysis of the shape of the thumbtack's head, whether completely flat or more rounded like a carpet tack. It would also take into account the mass of the head, compared to the mass of the pin. A comprehensive calculation based on the shape of the tacks and their mass distributions might then lead to reliable predictions about the probability distribution of how they will come to rest. A well-rounded tack with a massive head inevitably would come to rest on its head, with the pin pointing upward, acting somewhat like a stand-up doll. An experimentalist still might distrust even these calculations and insist on throwing the thumbtacks to convince himself of their behaviour. This would leave little doubt about the behaviour of the particular type of tack he was using, but might tell little about how other tacks, with different shapes and different mass distributions, would come to rest on the table.

A recent variant of theoretical explanations is computer modelling. The distinction between modelling and theoretical calculations may be considered to be merely one of scale, but when that difference of scale becomes substantive, the need for distinction becomes increasingly urgent. The reason is that massive astrophysical computer models generally invoke a large number of successive steps. The modeller may start out with a set of more-or-less randomly distributed particles, all of which exert gravitational forces on one another. A first step is to see how the forces between these particles induce instantaneous accelerations and small changes in their relative positions after a brief time interval. This information is fed back into the model and a new set of forces resulting from the induced displacements is calculated together with further displacements these forces will produce. The computer model may include millions of particles all acting on each other in this fashion and may follow their displacements in the course of a million successive small steps. At the end of this massive computation, the assembly of particles may have assembled into what appears to be a galaxy, a cluster of galaxies, or a massive black hole, depending on the initial conditions assumed for the assembly.

Computations of this type have become increasingly common in the modelling of processes believed to have taken place early in the evolution of the Universe, once it had cooled sufficiently for most of the ionized matter to combine into neutral atoms. With some delay for additional cooling of the Universe through expansion, star and galaxy formation is thought to have occurred. The computer models seek to show how this may have happened, whether stars formed before galaxies, or vice versa, and whether quasars formed before or after the first stars, reflecting the extent to which massive black holes now generally found at the centers of galaxies came into being at this epoch.

The difficulty with modelling on such scales is that the large number of iterative steps the computer models involve permit what may at first sight appear to be insignificant anomalies to gain ascendancy. Because an innumerable number of iterations, each introducing the same small anomaly, may play a role, the modeller needs to be extremely careful in appropriately taking such anomalies into account. Even then, there is no guarantee that this has been done satisfactorily. For this reason a model gains credence only if it can simulate a large number of different processes, all of which are well understood both experimentally, observationally, or from less complex calculations. It also helps to have a number of differently structured computer models reaching consonant conclusions, preferably if the models have been produced by independently working groups of researchers working with different mathematical techniques.

Each of these different types of evidence, and the understanding that can be derived from them, provides new insights but leaves further questions unanswered. Astrophysical conclusions try to take each of these types of evidence into account, where available — symmetry arguments, observational determinations, traditional calculations, and computer modelling.

Because astronomy is largely an observational science, and many astrophysical phenomena such as the explosions of supernovae are quite rare, the community often has to depend heavily on theoretical explanations to guide understanding — if for no other reason than to help define critical measurements to be carried out when the next opportunity for further observations presents itself. That way the community may eventually come to distinguish between alternative explanations of, and models for, supernova explosions or any other rare phenomenon.

Thus far, I have described the system of justification and acceptance of new ideas in science as though science develops along logical lines. This, however, is only one of many aspects of an incomplete depiction.

In his seminal book *Entstehung und Entwicklung einer wissenschaftlichen Tatsache,* Ludwik Fleck (1935) first showed at great length, the extent to which a scientific community resists new ideas and concepts, often wedded to extant thought long after it has outlived its usefulness. This weakness can afflict even some of the leading minds.

The great Cambridge astrophysicist Arthur Stanley Eddington gained prominence, in 1919, right after World War I, for measuring the gravitational bending of light around the sun and finding the angle of deviation to agree precisely with Einstein's theory of General Relativity that had been published only four years earlier. But when a young graduate student from India, Subrahmanyan Chandrasekhar, arrived in Cambridge proposing a new theory explaining the physics of white dwarfs (Chandrasekhar, 1931), Eddington ruthlessly ridiculed it. Eventually, Chandrasekhar left Cambridge to work in the United States. In 1983 Chandrasekhar was awarded the Nobel prize for his contributions to astrophysics, mostly in his Cambridge years 1931–1936. Figures of authority can be helpful but also highly destructive. In the long term, their influence fortunately tends to wane, but that does not diminish the confusion and pain they may spread along the way.

Acceptance of New Approaches and Theories

Given these considerations, how are we to model new ideas, new ways of thinking, that do occur from time to time?

The small-world theory appears to provide a prescription of how old ideas can be toppled and give way to the acceptance of new directions to follow in astronomy. It does this not through a depiction of the psychological factors at play, but rather as a schema involving linkages between workers in the field, influenced purely by reliance on acceptance of the new idea or thought convention by a trusted colleague.

New approaches to research in any subfield of astrophysics tend to be spearheaded by groups that mutually agree on pursuing new lines of attack. This collaborative approach is common also in other disciplines, as Fleck (1935) repeatedly emphasized. The groups that take these approaches then fend off any alternative lines of attack on the investigation they are conducting, often to the bewilderment and consternation of others who may have equally valid or possibly even more interesting approaches and who cannot comprehend the narrow focus, bordering on mania, of members of the leading group. Only one benefit accrues from this single-minded approach. Large numbers of scientists all working on the same problem can quickly determine whether the approach has merit or leads to one more dead end. If it does terminate in disappointment, a short respite results, but not for long. Soon an idea for a new direction surfaces and a new lead-group emerges, sometimes with the same membership or some slightly re-arranged version of it, less often with a new membership altogether — and the headlong chase is on again.

What determines the acceptance of these new directions? How solidly are the criteria for the new direction in hand? How quickly does the new group gather its membership? How long does each chase persist before running its course? These are questions that seem to have answers in the explanation of social cascades incorporated in the small-world theory.

A particularly conspicuous example of it is the acceptance of the inflationary theory of cosmology, a scenario of an evolving universe which only works because, remarkably, it incorporates as its basis a critical inflationary phase, identical in many ways to the steady state model of cosmology that supporters of the evolutionary theory had scornfully demolished only a decade or two earlier.

The Inflationary Universe was first proposed by Alan Guth (1981) and then was, almost immediately, modified by Andrei Linde (1983) to remove some deficiencies in the original version. This theory was rapidly accepted because it explained why the Cosmos, i.e., space around us, appears to be flat rather than curved, how we might account for the finding that the Universe seems to look identical to us no matter which direction we look — the so-called homogeneity and isotropy problems — and why the Universe seems devoid of magnetic monopoles.

This last criterion for acceptance was the most surprising. Only very few astrophysicists could have even known that a monopole problem might exist, or that monopoles were an integral part of fundamental particle theory at the extremely high temperatures that Guth proposed to have existed at primordial epochs. Since magnetic monopoles have never been observed either in accelerator experiments or in nature, why was this a point in favour of the new theory?

It was not as though there were no alternatives to the inflationary scenario in the 1980s. A variety of oscillating models of the Universe appeared to satisfy the isotropy, homogeneity, and flatness criteria.

So, if virtually no astronomer and only a few astrophysicists sufficiently familiar with the details of high-energy-particle physics could understand the theory, why was it so universally accepted?

The cascade theory of the small-world model provides a plausible explanation for the acceptance of novel hypotheses and for the near-total exclusion of others (Watts, 1999, 2002, 2003). The basic idea is that if a theory finds acceptance by a nucleus of the community — i.e., the group, in the nomenclature of networks — it will have the opportunity to trigger a cascade. To do this a proponent of a theory must have (i) a sufficient number of links to other members of the community, and (ii) sufficient credibility to have his opinions carry weight — equivalent to those links being open, permitting the new idea to percolate through. If a sufficient fraction of those contacted are willing to give the theory at least provisory acceptance, and the idea gains sufficient acceptance at successive transmissions from astronomer to astronomer, the number of adherents will expand exponentially.

The actual cascade theory of Watts is more comprehensive than this simple depiction, taking probabilistic ramifications into account, but the essence of the idea is that acceptance of a theory grows not because every member of the community understands it, but because a few members whom others trust have done so, particularly by expressing their serious commitment by publishing papers that accept the new approach as a reasonable, if provisory, way to go forward.

Whether or not the theory of network cascades can explain the observed acceptance rates will need to be checked. Paul Ormerod (2006) has shown that considerable insight into the types of networks that may be at work in different kinds of processes can be gleaned from relatively sparse data. Data on the astrophysical community have been gathered and analyzed by M. E. J. Newman (2001, 2004), while other data dealing with publishing patterns and citations are available through the on-line Smithsonian/NASA Astrophysics Data System. In my full-length study I plan to examine these to see whether a closer analysis will yield informative insights.

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