• *ABSTRACT*

Since 1980, catastrophist theories of extinction have experienced a dramatic renaissance, based on the claim that important biological changes have extraterrestrial causes – that asteroids or comets were responsible for the extinction of the dinosaurs, as well as a host of lesser-known creatures. Formerly relegated to the periphery of evolutionary biology and paleontology, the search for evidence of extraterrestrially instigated extinctions has become a well-funded and rapidly developing area of research, involving scientists from a wide variety of disciplines. In large part, the rapid establishment and diffusion of the 'impact' or 'asteroid' hypothesis may be attributed to the ways in which the style of the argument, as well as the institutional location of its proponents, has interacted with the system of scientific publication, both professional and popular. Through an analysis of the organization of publication and its relationship to various ideals concerning evidence, argument and 'good science', some of the consequences of the organization of communication for the development of scientific thought are suggested.

Of Asteroids and Dinosaurs: The Role of the Press in the Shaping of Scientific Debate

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Among scholars who study, but do not practice, the natural sciences, special importance has long been attached to the differentiation of science from 'nonscience' — a residual category encompassing anything from magic to sociology. But with the recognition of incommensurability within the realm of science, both across time and between theories, even philosophers have devoted increasing attention to specific disciplines and research networks. Despite major epistemological disagreements, many theorists have agreed in identifying some form of discrete collectivity, whether intellectual or organizational, as the foundation of the distinctive status of scientific knowledge.[1] One boundary has been replaced by many.

The adequacy of this theoretical solution is brought into question by the existence of 'interdisciplinary science' — a puzzling term that denotes discourse among what are often defined as distinct realms of discourse. This formulation is problematic, not only in a definitional

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sense; few specialists ever renounce their identity as scientists. Discipline-specific practices co-exist with claims to participation in a common intellectual endeavour. Particularly within the context of interdisciplinary debate, establishing norms of general scientific argument that support discipline-specific practices is an important component of the process of making truth claims.

The tendency to base theories of scientific practice on a notion of discrete disciplines or communities has obscured the character of the relationships among various publics involved in, and attentive to, scientific debates. Despite the lack of attention from social scientists and philosophers, interdisciplinary science does occur and often has important consequences for the established fields which become involved. One such case, involving particle physicists, paleontologists, evolutionary biologists and geochemists, is currently receiving a great deal of attention in both professional and popular circles. The 'asteroid', or Alvarez, hypothesis proposes that the Cretaceous–Tertiary (K–T) boundary, usually defined by the extinction of dinosaurs and other animals and plants approximately 65 million years (myr) ago, can be interpreted as evidence of the catastrophic extinction of Cretaceous life forms by the impact of an extraterrestrial object. In less technical terms, an asteroid is given credit for having done in the dinosaurs and some of their contemporaries.

Starting from the detection of iridium 'spikes', using a technique known as neutron activation analysis, the Alvarez team developed an elegant scenario in which a discrete cause — the impacting asteroid — produced sudden mass extinctions while leaving distinctive physical evidence in the form of elemental enrichments and a variety of mineralogical formations. Since the argument was first published,[2] this claim and the research that it has inspired have been debated in the pages of the major multidisciplinary journals (*Science*, *Nature*, and *Scientific American* among others) and have received considerable coverage in the growing number of popular scientific publications (*Discover*, *Science Digest* and *Natural History*, as well as the *New York Times* and the science sections of other newspapers). Many conferences have also taken place in both the United States and Europe.[3]

Further research has been undertaken to identify new evidence confirming the impact argument, as well as to detect similar incidents at other major transitions in the history of life. On the basis of a statistical analysis of the fossil record suggesting that prominent extinction events occurred at periodic intervals of approximately 26 myr, scientists have developed general theories of extinction, the most notable of which are the 'death star' and 'invisible solar companion' arguments put forward in April 1984.[4] Since that time, the debate has continued, with varying degrees of intensity and rancour, as new evidence is sought and participants reassess the criteria upon which their initial judgements were based.

From a comparative perspective, the rapidity with which the asteroid hypothesis has been established as a major element of long-standing debates over the history of life is striking. Yet the argument is by no means unprecedented. In fact, it has a long history of being ignored within scientifc circles. Viewed against this background of repeated failures to gain professional acceptance, it may be possible to identify some of the factors that have contributed recently to the rapid diffusion of this controversial argument. Without claiming to offer a comprehensive analysis, this paper explores one aspect of the contested terrain — the organization of communication, specifically the professional and popular scientific press — in order to demonstrate how it contributes to the shaping and tempo of scientific debate.[5]

For the purposes of this paper, the issue is not to predict the character of the eventual consensus, if any. Nor, despite the inevitable prejudices of an investigator, is it intended to discredit scientific arguments as impure or tainted by social factors, but, instead, to contribute to 'an appreciation of the contingent circumstances affecting the production and evaluation of scientific accounts'.[6] The professional and popular press are important members of this set of 'contingent connections'. Publication may be thought of as a filter that selects and transforms much of what passes through the system of scientific communication, a system that interacts with other factors central to sociological accounts of scientific practice.

Following a brief theoretical discussion, a close examination of the current controversy will be used to identify certain significant characteristics of the forms of argumentation employed by participants. The final section will discuss some of the ways in which these analytic or rhetorical elements are embedded in an organized system of scientific communication.

Communication across Boundaries

The participants in this debate have been brought together by their enthusiasm for, or opposition to, a scientific theory, rather than as members of a single specialty. Few can be assumed to know enough about all the relevant fields to assess the linkages proposed between them. Under these conditions, the processes by which problems are identified and solutions evaluated are far from obvious. This situation may be contrasted with that described by Thomas Kuhn in *The Structure of Scientific Revolutions*:

> this strong network of commitments — conceptual, theoretical, instrumental and methodological — . . . provides the rules that tell the practitioner of a mature specialty what both the world and his science are like.[7]

In the controversy over catastrophic extinction, both the nature of the questions to be explained and the character of good explanations have been hotly contested. Clearly, an analysis that emphasized professional socialization, and consequently the training, textbooks or ideology of a given discipline, will be insufficient to explain the character of the debate inspired by the impact hypothesis.

Thus, it is necessary to divest disciplines of some of their theoretical centrality and to recognize the criteria of scientific specialization as problematic and often contested. The character of this sort of research is suggested by the work of organizational theorists who often treat boundaries not as barriers that define and isolate the phenomenon to be studied, but as interfaces, either between organizations or between organizations and their environment.[8] Consequently, greater attention is paid to interactions between organizations, the identity of gatekeepers, and the character of access structures. Investigation of the cognitive and institutional frameworks of scientific practice is combined with an appreciation of the possibility of strategic behaviour which, in pursuit of intellectual consensus, is often manifested through rhetoric. From this perspective, the published record of scientific debate may be appreciated as a rich source of evidence concerning scientific practice without necessarily assuming that it is an accurate representation of 'how science really works'.[9]

As Dorothy Smith has noted, 'in the study of communication, the dominant model has been that of face-to-face communication' although 'our lives, to a more extensive degree than we care to think of, are infused with a process of inscription, producing printed or written traces or working from them'.[10] Scientific articles and articles about science are part of the context within which disciplines and research networks operate. But with the exception of citation analysis, social scientists have paid little attention to scientific publishing as a set of institutions that span disciplinary boundaries, or as an arena in which authors attempt to establish the relation of their specialty to some more general notion of science.[11] Such a prominent and pervasive form of (mis)representation should not be ignored.

While profiting from many of their insights, this analysis should be distinguished from the work of a growing number of sociologists who, inspired by the work of cultural anthropologists such as Mary Douglas,[12] have begun 'to recognize science as a form of culture like any other, and hence to realize that sociological and anthropological methods could be legitimately employed in its study'.[13] In part, the attention paid to the study of single specialties stems from a methodological commitment to a micro-sociological, ethnographic approach.[14] But theoretical concerns reinforce the framing of problems in terms of individual disciplines or specialties.

> When anthropologists and historians of science describe the workings of these processes in detail, it is apparent that they are more subtle than the stereotypes of reasoning that have been abstracted and codified in deductive and inductive logic. The link between a cue and the meaning attached to it is certainly something other than an inductively supported generalisation. This is because criteria are taken, for all practical purposes, to convey certainty. They are authoritative.[15]

Social processes of learning establish authoritative links between cues and meanings: 'Training and instinct . . . provide the starting point of all our explanations, and the terminus of all our justifications'.[16] Despite their attention to agreed-upon meaning, rather than Truth, the adoption of this essentially tribal metaphor reinforces the practice of explaining science by studying discrete groups of scientists.

Moreover, meanings are not always agreed upon. As Pierre Bourdieu has argued, in scientific debate as in class conflict, 'no arbitrating authority exists to legitimate legitimacy-giving authorities; . . . inasmuch as the definition of the criteria of judgment and the principles of hierarchization is itself at issue in a struggle, there are no judges, because there is no judge who is not also a party to the dispute'.[17] Thus, when debates are not linked to disciplines, when ideals of scientific practice are questioned, an understanding of the terrain upon which the contest occurs is particularly important.

Broad-ranging interdisciplinary science strains the ability of personal and professional ties to promote consensus over the definition of problems and the criteria for proof. Under such conditions, considerable light may be shed on procedures and criteria that usually go unexamined. After sketching the debate inspired by the asteroid hypothesis and identifying the most prominent forms or strategies of argument, the influence of publication and the press in the shaping of scientific debate will be considered in greater detail.

The Problem of Extinction

There is nothing novel about the suggestion that extinct life forms met with an extraterrestrially instigated fate. During the sixteenth and seventeenth centuries, similar suggestions were voiced in the context of a general interest in the biological consequences of the Noachian flood. Newton considered comets to be an important factor in the periodic 'replenishment' of the earth.[18] Immanuel Velikovsky's argument that many religious beliefs and myths are reports of celestial catastrophes generated interest although little research,[19] but cool responses have not been limited to professional outsiders. In his 1969 Presidential Address to the Paleontological Society, Digby McClaren argued that mass extinctions might be attributable to the impact of extraterrestrial bodies, but as Walter Alvarez has noted, 'the idea was almost completely ignored'.[20] Still more striking is the relative lack of interest that greeted an article in 1973 by Harold Urey, the 1934 Nobel laureate in chemistry and a scientist with an established record in the earth sciences, that suggested that the approach of a comet might have sideswiped the earth's atmosphere with predictably deleterious consequences for its inhabitants.[21] Based on a statistical analysis reminiscent of the later work of Raup and Sepkoski, the article evoked little response and has been resurrected only in the context of the more recent controversy.

This consistent lack of response, despite the credentials of some of the scientists involved, forms the background against which the widespread attention paid to the Alvarez theory must be understood. In the early 1970s, there was a burst of interest in possible linkages between extraterrestrial phenomena and the history of terrestrial life. Both *Science* and *Nature* published short pieces proposing cosmic radiation from supernovae as a mechanism that could cause 'the extinction of many exposed animals without the simultaneous extinction of plant life'.[22] The theory received some attention in the popular press and its advocates were a receptive audience for the Alvarez findings a decade later, but did not inspire a major research effort at the time.

Both formulations shared the assumption that the Cretaceous–Tertiary transition marked 'a brief interval during which many organisms underwent a severe attenuation in diversity or went extinct'.[23] Similarly, the discussions of the probabilities of nearby supernovae over the course of earth history foreshadowed the strategies used to extend the temporal scope of the original Alvarez hypothesis. At the time, however, these proposals were attacked for assumptions concerning the diffusion of cosmic radiation.[24] But as has been noted by recent commentators, 'the difficulty with this hypothesis has been the problem of obtaining credible evidence'.[25]

Changes in intellectual climate are also relevant to an explanation of the rapid diffusion of the asteroid hypothesis, particularly the status of 'gradualism' in the field of geology. This view of earth history has been described as 'geological dogma' or 'gradualistic prejudice':[26] 'many geologists have felt that a belief in slow and steady changes defines the necessary practice of their success'.[27] In the last few decades, a similar clash between gradualist and anti-gradualist views developed among biologists over the punctuated equilibrium theory of evolutionary processes.[28] In part, the lack of response to earlier catastrophic theories of extinction might be attributed to the pervasiveness of gradualistic orientations among geologists, biologists and paleontologists. Yet it is equally true that the initial popularity of the Alvarez hypothesis may stem from current intellectual fashion: 'The *zeitgeist* is now in for an overhaul. Punctuational change is attracting attention and excitement in a host of diverse disciplines.' This selfsame *zeitgeist* manifests itself in the attention paid to paradigm shifts and the emergence of new specialties by the chroniclers of science.[29]

Framing the Problem

Despite a long history of speculation and research on the topic, much of the current interest in the latest Cretaceous extinctions is attributable to a single article and its consequences: 'Extraterrestrial Cause for the Cretaceous–Tertiary Boundary: Experimental Results and Theoretical Interpretation'. Twice as long as the standard lead article, this paper appeared in the 6 June 1980 issue of *Science*.[30] A recent survey of paleontologists and geoscientists in five countries found that 30 per cent first became aware of the argument through this article (50 per cent for the sample of *Paleobiology* subscribers and geophysicists in the US), whereas only 14 per cent were familiar with the hypothesis before then.[31] Close analysis of this article, particularly when compared to similar arguments that were published at approximately the same time, suggests some of the central elements of effective argumentation within the medium of formal scientific communication.

The 1980 article reported work done by a team from the University of California at Berkeley: Luis W. Alvarez (physicist, awarded the Nobel prize in 1968 for his work in particle physics), Walter Alvarez (professor of geology), Frank Asaro and Helen V. Michel (staff scientists at Lawrence Berkeley Laboratory). In the course of their efforts to use the rate of infall of micrometeoritic material as a measure of the time elapsed during the deposition of pelagic or deep-sea sediments, Luis and Walter Alvarez had discovered unusually high concentrations of iridium in one layer of clay in the Gubbio deposits in Italy. These findings provoked the suggestion that 'at the time of the mass extinction of certain marine micro-organisms (and by inference at the time many reptiles disappeared) some 500 billion tons of extraterrestrial material had been abruptly deposited on the surface of the earth'.[32] They concluded:

> There is prima facie evidence for an abnormal influx in the observations that the excess iridium occurs exactly at the time of one of the extinctions; that the extinctions were extraordinary events, which may well indicate an extraordinary cause; that the extinctions were clearly worldwide . . .[33]

The recognition and structuring of problems is one of the functions commonly associated with paradigms, communities or research programmes, but many accounts suggest that disciplines do not always frame their own problems. An alternative route to the same problem is described in an article by R. Ganapathy, who published a geochemical analysis of K–T boundary clays shortly after the 1980 Alvarez article: 'I became interested in this subject after reading in a local newspaper and a popular scientific weekly about a revival of the supernova hypothesis to explain findings by the Alvarez group . . .'.[34] The interest in catastrophic extinction displayed by a variety of scientists appears to stem, at least in part, from a widely-shared popular interest in prehistoric life.

> . . . a few years ago, the four of us suddenly realized that we combined in one group a wide range of scientific capabilities and that we could use these to shed some light on what was really one of the greatest mysteries in science — the sudden extinction of the dinosaurs.[35]

But there is more than one way to characterize this mystery. Previously, the disappearance of the dinosaurs had been addressed in the context of differential extinction and survivorship (particularly the emergence of mammals as the dominant fauna), or of reassessment of general evolutionary theory.[36] Others investigated these issues with reference to major ecological changes, notably sea-level regression and evidence of increased vulcanism. Therefore, it is necessary to assess some of the reasons why the conceptualization of the K–T boundary advanced by the Alvarez group — a discrete event with a discrete cause — was so rapidly established as a major position in the ongoing debate.

Reasons for the enthusiastic reception given to the Alvarez hypothesis are suggested by a comparison with earlier catastrophist arguments, two of which appeared in the 22 May 1980 issue of *Nature*.[37] The first discusses platinum group metals at the K–T boundary at a site in Spain, concluding that there is evidence of the impact of an extraterrestrial object. The abundance of these metals (including iridium, osmium, rhenium and ruthenium) has been believed to differ markedly in materials of crustal, mantle or cosmic origin. Therefore, the abundance patterns of these elements have been used as a 'signature' of the material's origin. But unlike the Alvarez paper, the discussion of biological changes in the first paper is limited to marine microfauna. The second, while addressing a broad range of faunas, proposes a mechanism of combined atmospheric heating and the spread of cyanides in the ocean; extinction rates would vary with locale for marine fauna and body size among terrestrial species. Both accounts, either implicitly or explicitly, allow for variation in the pattern, and possibly timing, of extinction in different locales.

As has been mentioned, other scientists had proposed 'an extraterrestrial catastrophic event — probably a burst of hard radiation from a nearby supernova — at the end of the Maastrichtian [the final portion of the Cretaceous]',[38] but the idea made relatively little headway. By comparison, the precise, technically sophisticated evidence described in the Alvarez paper was accepted rapidly by a wide variety of scientists. This response was aided by presenting their argument as an example of the application of a general scientific method. Observations were made, an argument was developed, hypotheses were tested:

> In the 570-million-year period for which abundant fossil remains are available, there have been five great biological crises, during which many groups of organisms died out. The most recent of the great extinctions is used to define the boundary between the Cretaceous and Tertiary periods, about 65 million years ago . . .
>
> A major obstacle to determining the cause of the extinction is that virtually all the available evidence on events at the time of the crisis deals with biological changes seen in the paleontological record and is therefore inherently indirect. Little physical evidence is available, and it is also indirect . . . but we have developed a hypothesis that appears to offer a satisfactory explanation for nearly all the available paleontological and physical evidence.[39]

The K–T boundary is defined by the extinctions, and the extinctions are assumed to be catastrophic ('five great biological crises', 'the time of the crisis'). Despite technical difficulties stemming from the dependence of geological time scales on paleontological evidence and the partial nature of the fossil record, the K–T boundary is presented, without qualification, as a discrete event in need of explanation.

Citations concerning the character of extinction are minimal, as in the discussion of the link between impact and extinction (a three-year period of darkness at noon caused by dust ejected into the stratosphere). The discussion of these biological effects merits a half page (out of a total of thirteen), and supporting evidence for the proposed mechanism is limited to a reference to the explosion of Krakatoa and some results of recent nuclear bomb tests.[40]

Despite its many similarities with the first of these two papers, most notably in the presentation of platinum group metals as evidence of an impact at the end of the Cretaceous, the Alvarez team's paper had considerably greater consequences and is consistently cited as the foundational reference for research in this area. In part, the persuasive power of 'direct' evidence as presented in this article stems from its reference to a particular model of scientific practice. A related emphasis on theoretical modelling and sophisticated measurement is apparent in the use made of work done on ballistic orbits by scientists in Pasadena and Los Alamos. The Alvarez group proposed the 'convective vertical winds in the fireball' as a mechanism for a global distribution of dust resulting in catastrophic biological effects.

> The so-called hydrodynamic computer programs used in these computer simulations are like the ones used to design nuclear weapons; they involve temperatures, pressures and material velocities much higher than those found under normal conditions, and they are known to do their tasks with great precision. A typical computer run involves many billions of numerical calculations. So far as I know, such great computing power has never before been brought to bear on problems of interest to paleontologists.[41]

Using these sophisticated models, 'killing scenarios' involving fluctuations in temperature or potentially toxic chemicals were developed. Such models, it must be remembered, are intended to suggest links between two assumed events: one extraterrestrial impact and one rapid global extinction. Whereas the persuasiveness of the initial framing of the problem rested on the juxtaposition of discrete bits of physical evidence — the iridium and a geological marker — this interpretation is buttressed by the asserted validity of a sophisticated mathematical model. In a sense, the problem of the K–T boundary was framed so as to be amenable to the methods of particle physics.

Initial Objections

Among paleontologists, the initial response seems to have been an expectation that this would pass, as had so many other more or less bizarre theories. These have included heat-induced sterility, lightinduced blindness, drug-induced frenzies, egg-eating mammals and, on the basis of even less evidence, *paleoweltschmerz*, death by boredom.[42] In understanding the dynamics of this debate, it is important to appreciate the inclination to dismiss yet another neat explanation of the Cretaceous extinctions. In addition, however, early critics identified two fundamental grounds for concern: the accuracy of the problem as framed by the Alvarez group and the ability of the fossil record, as well as of a variety of measurement techniques, to detect the geologically brief time spans assumed by the impact hypothesis.

The first concern was expressed with respect to the complexity of biological changes during the K–T transition. Referring to Danish sections, paleontologists argued that 'the Cretaceous–Tertiary boundary "event", rather than being a single catastrophy [sic], appears to represent the net effect of a series of possible independent events, each commanding a varying ecological influence'.[43] This objection was echoed by a group of American paleontologists who asserted not only that fossil evidence suggests the extinction was gradual (or, at least, not instantaneous), but that proposed explanations should also account for the patterns of survivorship. Among the smaller mammals, multituberculates, marsupials and placentals experienced different survival rates at the level of genus; different patterns of faunal change are found in the flood plain and river valley deposits of late Cretaceous North America; and there were higher rates of marine extinction in tropical climates and 'a highly variable pattern of plant extinctions throughout the world'.[44] From this perspective, the argument that the asteroid caused the extinction of terrestrial species whose adult members weighed 25 kg or more[45] constituted an extreme and incorrect simplification of the evidence; insofar as there is a fine structure to the Cretaceous extinctions, any proposed explanation is expected to account for it. Thus, the presentation of the problem as complex is joined with an assertion that strong theories should be capable of accounting for complexity.

Discussion of the K–T 'event' also addressed the ability of current measurement techniques to detect geologically instantaneous, globally simultaneous changes and, hence, to discriminate between rapid (10,000 to 100,000 yrs) and catastrophic changes. As Clemens and his colleagues noted, 'biostratigraphic, magnetostratigraphic and radiometric' methods do not allow one to differentiate between events in intervals as short as 10,000 or as long as half a million years;[46] the geological record is not capable of providing firm evidence of 'instantaneous' events.

The tension between biological complexity and geological imprecision on the one hand, and astrophysical simplicity on the other, has been avoided rather than resolved. Rather than debating theories of extraterrestrially caused biological change, the physicists and astronomers, geochemists and geologists, paleontologists and evolutionary biologists have tended to assess those parts of the theory most relevant to arguments within their own fields. This division of labour has been facilitated by the fact that the initial Alvarez argument was composed of a number of distinct assessments of probability: that there is geochemical evidence of an *extraterrestrial* event; that there is geochemical evidence of a *worldwide, simultaneous* event; and that there is evidence of its global, biologically catastrophic consequences. These hypotheses provide a useful framework for analysing the interplay of the general problem with specialized research.

Material Evidence

Among the most significant elements of scientific specialties are their criteria for recognition. Along with the exemplars and anomalies emphasized by many sociologists of science, the inheritance of each field includes some counterpart to the biblical assurance that 'this shall be a sign unto thee'. As has been suggested by the criticisms discussed in the preceding section, some forms of evidence seem better able to serve as definitive markers than do others. For the purposes of this controversy, assumptions concerning the character of iridium were among the most important sets of expectations to be activated: 'Platinum metals are depleted in the earth's crust relative to their cosmic abundance; concentration of these elements in deep-sea sediments may thus indicate influxes of extraterrestrial material.'[47] Relative abundances of these elements in materials of cosmic, crustal and mantle origin were thus used to define 'diagnostic isotopic signatures'.[48]

Consequently, much of the controversy has concerned the conventional knowledge that identifies iridium as evidence of asteroidal impacts. In their 1980 article, the Alvarez group made their case for an extraterrestrial impact by considering alternative explanations for the observed concentration of iridium (simultaneous local weathering of iridium-rich ores, or an impact). These explanations were tested against alternatives, usually involving straightforward extrapolation from contemporary processes.[49] Further support for an extraterrestrial source for the iridium anomalies was provided by an analysis of osmium isotopes reported by two geochemists.[50] However, they also concluded that a single impact could not account for variations in the isotope ratio for different sites, suggesting that the models linking impact and extinction were not entirely accurate. One of the consequences of further research has been to break down the straightforward relationship between iridium anomalies and impact events orginally used to frame the inquiry; observed variations in the amount and distribution of iridium (as well as of other elements) have shifted the terms of the debate.[51]

Consensus concerning the adequacy of the geochemical evidence has not been reached. A 1983 eruption of Kilauea in Hawaii weakened arguments that high iridium abundances must have extraterrestrial sources: 'Strikingly large concentrations of iridium were also observed, the ratio of iridium to aluminum being 17,000 times its value in Hawaiian basalt'.[52] As research inspired by the controversy produces new findings that undermine the reliability of the original platinum group 'markers', the strictly geochemical contribution to the debate increasingly has consisted of descriptive evidence of platinum group enrichments, rather than causal arguments.[53]

The argument over extraterrestrial origins has shifted to a consideration of mineralogical evidence of catastrophic impacts: altered impact droplets; shocked quartz grains; platinum group nuggets; sand-sized, glassy objects known as microtektites, and various other ejecta that would be implied by the Alvarez scenario.[54] The discovery of shock quartz at a variety of sites had particular significance for geologists. During the late 1960s, the concept of meteorite-induced shock metamorphism supplanted attribution of these mineralogical phenomena to volcanic causes; 'geologists have witnessed the transformation of this idea from a science-fiction theme into a subject for serious geological debate and discussion'.[55] By identifying a distinctive marker for the proposed event, theories of shock metamorphism facilitated a similar transformation of the scientific status of extraterrestrially-instigated extinction. As with iridium, however, greatly increased levels of research on the K–T boundary have begun to suggest that, while characteristic of impacts, shock quartz may not be absolutely diagnostic.[56] Similarly, the debates have underscored the imprecision of relative dating based on the fossil record, notably changes in the characteristic faunas and pollen kingdoms.[57]

Questions of Timing and History

Geochemical evidence has also been significant for assessing the worldwide and simultaneous character of the proposed event. In order to test this claim, two Dartmouth geologists combined magnetic dating techniques with geochemical, microfaunal and stratigraphic analysis in order to compare the duration and absolute dating of the K–T boundary at various sites around the world, including deep sea cores. They concluded that 'the fossil sequences show a range of transition times and transition time intervals depending on the fossil indicators and the location of the site . . . [which] may represent regional rather than worldwide factors'.[58] These findings were a direct challenge to the existence of a 'problem' of worldwide, simultaneous extinction.

The Alvarez group responded that the significant (10,000 to 100,000 years) and varying times noted for the transition between predominantly Cretaceous and predominantly Tertiary faunal groups do not constitute a falsification of their theory: 'we would agree . . . that a major impact would produce important environmental changes, and that instantaneous extinction in all groups everywhere is not a necessary corollary of the impact theory'. They then argue that changes triggered by an impacting, extraterrestrial object could cause extinctions for a period of 10,000 to 100,000 years: 'This consideration invalidates Officer and Drake's use of d(t) [the duration of transition between faunas] as a test of the impact theory'.[59] This criticism was possible, if not entirely justified, only because repeated confrontations between the impact theory and historical evidence had made the significance of time and duration all too apparent.

The initial characterization of the problem ('that the excess iridium occurs exactly at the time of one of the extinctions') has been gradually redefined. 'Exactly' now meant 'on the order of ±30,000 years'; the response to Officer and Drake demonstrated that an impact scenario can be made compatible with biological changes extending over a period of 10,000 to 100,000 years.[60] This marks a considerable shift from the extreme catastrophism present in their early papers: 'Dinosaurs did last for nearly 140 million years . . . and we believe that had it not been for the asteroid impact, they would still be the dominant creatures on earth'.[61] In later papers, evidence of declining diversity, both among dinosaurs and marine faunas, is not refuted but incorporated: 'The paleontological record thus bears witness to terminal-Cretaceous extinctions on two time scales: a slow decline unrelated to the impact and a sharp truncation synchronous with and probably caused by the impact'.[62] Faced with the complexity of the fossil record, the impact scenario was reframed in ways which precluded refutation by stratigraphic or paleontological evidence.

When presented in another form, the fossil record has been a valuable asset for the impact hypothesis. In the 19 March 1982 issue of *Science*, there appeared a two-page report of a statistical analysis of a compilation of fossil data: 'Mass Extinctions in the Marine Fossil Record', by David M. Raup and J. John Sepkoski of the University of Chicago. The effect of their work has been to broaden the potential scope of the debate over mass extinctions:

> [O]ur analysis shows that major mass extinctions are far more distinct from background extinction than has been indicated by previous analyses of other data sets . . . The data do not tell us, of course, what stresses caused the mass extinctions. The extinctions were short-lived events in geological time, but the data do not have the resolving power to show whether the events were also short-lived in human or ecological time.[63]

As the authors note, attention to other paleontological boundaries was not without precedent; twelve years previously 'on the basis of other information, McClaren suggested a meteorite impact as one possible explanation for the Frasnian extinctions [approximately 400 myr ago]'.[64] The influence of Raup and Sepkoski's work stemmed from further results of their statistical analyses, eventually published in February 1984. They identified twelve extinction events exhibiting a statistically significant periodicity; 26 myr was the mean interval between events. Noting that two of these events coincide with extinctions tentatively linked to meteorite impacts, they conclude 'although the causes of the periodicity are unknown, it is possible that they are related to extraterrestrial forces (solar, solar system, or galactic)'.[65]

When reduced through statistical analysis, the complexity of the fossil record was a fruitful addition to the development of scientific models. The increasing role of statistical analyses in the debate over extinction is somewhat at odds with the emphasis placed on discrete facts and direct evidence employed in the rhetoric of falsification. For example, a three metre gap between the highest dinosaur fossil and the iridium anomaly in the Hell Creek formation of Montana — the location of some of the best evidence of terrestrial faunas of the period — is dismissed as nonsignificant on the basis of statistical analyses, 'computer-generated plots of randomly occuring "fossils",' Monte Carlo methods, and so forth. Yet fossil deposition is neither regular nor random, as assumed by these tests.[66] Echoing the use of 'hydrodynamic computer programs', technical sophistication is employed to dismiss specific findings. This argument, presented as refuting specific empirical evidence, is another case of model-building both in relative independence from the fossil record, and as an attempt to minimize its significance as a record of the history of life — and, therefore, as a source of falsifying evidence.

Similarly, the importance of elegance and parsimony, often presented as general criteria for the evaluation of theories,[67] was apparent in an issue of *Nature* which included five astrophysical explanations of the 26 myr periodicity reported by Raup and Sepkoski. These explanations invoked either the oscillation of the sun about the galactic plane[68] or the effects of an unseen, distant companion star (the 'death star') in this solar system.[69] In the latter argument, disturbances of 'the "Oort cloud" of comets which surrounds the Sun' result in larger numbers of comets within the inner solar system, hence a greater frequency of impacts with biological consequences. This scenario is supported by a statistical analysis of a small set of dated impact craters which resulted in a periodicity of 28.4 myr.[70]

The death star incident is an appropriate point at which to conclude detailed consideration of this debate. Although research continues and disagreements persist, proponents and opponents of the theory have returned to a more detailed consideration of its component arguments. Both sides continue to study K–T boundaries at a variety of sites, hoping to find favourable evidence. Efforts have been made to improve the estimation of periodicity (with regard to both impact cratering and extinction). Statisticians have criticized the findings of periodicity,[71] while paleontologists and biometricians scrutinize the assertion that 'the extinctions were extraordinary events, which may well indicate an extraordinary cause'.[72] Two major collections of papers were published in 1984, and many scientists asserted that a consensus had been reached. Unfortunately, there is no consensus concerning the character of the alleged consensus.[73]

Despite these claims of closure, one reviewer concluded 'that selective use of the available evidence can prove neither gradualism or catastrophism, and that neither kind of evidence seriously affects the other'.[74] The original Alvarez argument suggested that alternative processes for the concentration of platinum group metals, evidence against simultaneity, and evidence contrary to their impact mechanism of extinction, would all constitute falsification. Reflecting a major current in twentieth-century philosophy of science, the scientist is presented as one who 'constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment'.[75] The geochemical research appears to have been influenced by the same image of scientific practice; taking hypotheses derived from the Alvarez argument, attempts have been made to falsify these arguments with empirical evidence. Yet in none of these cases have the criteria of evaluation proven clearcut or the conclusions universally convincing. To the extent that a single conclusion can be drawn, it is that strong evidence of a discrete event has proven elusive, although a great deal of evidence of something has been found. The power of the criteria which are invoked appears to lie in rhetorical considerations as much as in objective standards. But, given the ambiguity of the evidence, the question remains as to which styles of evidence and argument have been most effective.

The Shaping of Scientific Debate

> To connect the dinosaurs, creatures of interest to everyone but the veriest dullard, with a spectacular extraterrestrial event like the deluge of meteors . . . seems a little like one of those plots that a clever publisher might concoct to guarantee enormous sales. All the Alvarez–Raup theories lack is some sex and the involvement of the Royal family and the whole world would be paying attention to them.
>
> Ian Warden, *The Canberra Times* (20 May 1984).

A concern for the distinctiveness of scientific knowledge has motivated the theorizing of philosophers, sociologists, historians and natural scientists themselves. As Robert Merton observed, 'the question is, of course, whether these diverse kinds of "knowledge" stand in the same relationship to their sociological basis . . .'.[76] The 'of course' reflects a conflation of distinctiveness and epistemological validity. Long concerned primarily with the latter issue, sociologists and philosophers of science have emphasized the ways in which the organization of scientific practice insulates its intellectual products from 'impure' influences. Consequently, rather less attention has been paid to the ways that social organization influences what is learned and how it is presented. As has been suggested, recognition of patterns in the framing of problems and the evaluation of arguments can provide considerable insight into the interplay of organizational and theoretical factors in the production of scientific knowledge. The intellectual resources of specific disciplines — various models, markers and techniques — are deployed in conjunction with idealized versions of appropriate scientific practice.

As is true of many endeavours in the modern world, the organization of science is not composed solely of relationships among persons or among ideas; these relationships are mediated, at times established, by objects and institutions.[77] Not the least of these is the press, the provider and distributor of the printed texts that pervade academic life. By way of conclusion, the role of publication in shaping scientific debate will be considered from three perspectives: the constraints of the genre; the organization of publishing; and the interrelationships of publications with their publics, both professional and popular. Analysis of the texts, in terms of both the form and content of the arguments, reveals the range of issues at stake in this debate, as well as suggesting the ways in which the constraints of this highly stylized genre influences the conception and presentation of arguments. But neither the professional nor the popular press simply reports the statements and beliefs of practising scientists; publication plays a central role in the diffusion of ideas, and hence in the development of scientific thought. By considering arguments within the context of the system of publication, it is possible to isolate some of the ways in which the social organization of science has affected the content and character of the controversy provoked by the asteroid hypothesis.

Forms of Argument and Exposition

The truth claims made in the name of modern science presume a direct relationship between scientists and their subject, unmediated by ideology or personal loyalty: 'a physicist can react instantaneously when you give him some evidence that destroys a theory'.[78] This self-image of science, as has been noted by G. Nigel Gilbert and Michael Mulkay, informs the rhetoric of much scientific publication.

> Although the content of experimental papers clearly depends on the experimenters' actions and judgements, such papers are overwhelmingly written in an impersonal style, with overt references to the authors' actions and judgements kept to a minimum. By adopting these kinds of linguistic features, authors construct texts in which the physical world seems regularly to speak, and sometimes to act, for itself. Empiricist discourse is organised in a manner which denies that its author's actions are relevant to its content.[79]

This sort of denial is not always effective. At times, the use of such an impersonal, objective style is suspended in favour of arguments that are fundamentally normative, although presented as general truths rather than personal statements. Frequently, the subject of such nonempiricist discourse is the nature of science itself, the model or ideal that informs the ways in which the physical world will be seen to speak for itself. After sketching the role of this normative discourse in the controversy over extinction, the interaction of these issues with the specific characteristics of scientific publishing will be considered.

In the controversy over extinction, it has been difficult to exclude metatheoretical and normative assertions.[80] Specific findings, often presented in the terminology of a specialty, are accompanied by claims to authority within a general scientific conversation. Such arguments merge a discipline-centred understanding of the sciences with assertions, reminiscent of the Vienna School, concerning *the* scientific method.[81] As was noted above, analysis of interdisciplinary science suggests the problematic nature of sociological or philosophical arguments that appeal to the organization or norms of a scientific community, or to multiple discipline-based communities. In the debate over catastrophic extinction, these tensions have been expressed through disagreements over appropriate models of scientific practice and argument, models that often favour the practices of specific specialties.

Many qualities have been attributed to 'good science': falsifiability, technical sophistication, ability to account for a wide range of evidence, elegance, parsimony, novelty, to mention a few. There is no reason to assume that these values are inevitably compatible, and it appears that the power to designate such criteria is an important goal of scientific contestation. Reflecting on the debate, Robert Jastrow, a physicist/astronomer/geologist, commented on this rhetoric:

> When leading fossil experts persisted nonetheless in disagreeing with Alvarez, he said, with some exasperation, 'I'm really quite puzzled [that] knowledgeable paleontologists would show such a lack of appreciation for the scientific method', and, giving his side of a dispute with two paleontologists over the time when the last dinosaurs vanished, 'I'm really sorry to have spent so much time on something the physicists in the audience will say is obvious'.
>
> Professor Alvarez was pulling rank on the paleontologists. Physicists sometimes do that; they feel they have a monopoly on clear thinking. There is a power in their use of math and the precision of their measurements that transcends the power of the softer sciences . . . .[82]

Using the concept of scientific community or paradigm, one might argue that different criteria of 'good science' are held by members of different disciplines. Yet this view fails to appreciate the divisions within fields, the ambivalence of individual scientists, and the belief held by many that they are not only specialists but scientists in a much broader sense. In particular, scientists in fields at the bottom of this hierarchy of sciences are often influenced by ideals of scientific practice prevalent in the harder sciences, ideals reinforced by many of the most prominent philosophers of science.[83]

Quantitative data, preferably with a basis in experimental work, are central to this ideal. The roots of this preference for the quantified lie beyond the scope of this debate, but the bias disadvantages certain types of findings, particularly ecological and historical evidence. Paleontological field work involves establishing the relative position of faunal and floral remains in the context of a geological structure which is often complex. In a sense, this type of analysis is more akin to the interpretation of a text (with nine out of ten pages missing) than to the ideal of a repeatable laboratory experiment. This data is not easily appropriated by nonspecialists. As was noted by Officer and Drake, geologists critical of the asteroid hypothesis, this problem is exacerbated by the tendency to assume precision rather than uncertainty or ignorance.

> A nonstratigrapher may well assume that period or epoch boundaries are well defined isochrons just as a stratigrapher might assume that geochemists know the bulk composition of the earth or that the seismologists know the make-up of the core. Stratigraphers, who have to wrestle with the problem, know the pitfalls involved in the establishment of a time stratigraphic boundary or in ensuring that it represents the same time at different places.[84]

Differences concerning evidence or proof would be of minor consequence if individual scientists adhering to different positions had nothing to do with one another. But this is not the case. Nor, more importantly, is it necessarily believed that this should be the case:

> Luis Alvarez, the great Berkeley physicist and cofounder of the asteroidal theory, has advocated from the start a scenario for extinction based upon a massive dust cloud thrown aloft by the impact . . . He also explicitly recognized the parallels between asteroidal zap and nuclear war . . . Sagan and his colleagues read the message and applied it directly. *Good science also displays its continuity across apparently unrelated disciplines.*[85]

Models of science, often drawn from philosophy and sociology, are not only descriptions but elements of scientific practice. This interrelation is particularly evident in the rhetoric and practice of falsification; few concepts from the philosophy of science have been borrowed with greater frequency. For example, in an article entitled 'Experimental Evidence that an Asteroid Impact Led to the Extinction of Many Species 65 Million Years Ago', Luis Alvarez argued for the strength of his evidence on the basis of its compatibility with a particular model of scientific practice:

> In physics, theories are declared to be strong theories if they explain a lot of previously unexplained observations and, even more importantly, if they make lots of predictions that are verified and if they meet all the tough scientific challenges that are advanced to disprove them. In that process, they emerge stronger than before.[86]

The response to the Alvarez hypothesis indicates that this is a common view of the way science, not only physics, works. While virtually ignoring one of the research suggestions included in the Alvarez team's initial article (the search for 'the crater produced by the impacting object') and coming to the second (the relation of impacts to earlier extinctions) by way of paleontological evidence, researchers have concentrated on testing the predictions stemming directly from the theory: extraterrestrial event, simultaneity, single impact, and so on. Yet the resulting modifications and criticisms have led to little in the way of falsification.[87] Such findings have been ignored, dismissed with charges of technical error,[88] or incorporated in a way which does not fundamentally alter, much less strengthen, the impact theory.[89] With the moderation of the original predictions, the theory has become increasingly insulated from falsification.

These practices are reinforced by the constraints of scientific publishing; often problems are defined and arguments developed in fewer than five magazine pages. The characteristics of the genre affect both the form and content of specific arguments. The viability of this highly abbreviated form of communication rests on the assumption that the audience possesses sufficient knowledge for its interpretation; consequently there is an incentive to simplify, particularly with respect to the framing of questions and discussion of conclusions. In this way, the form of discourse reinforces the importance of 'markers' or 'signatures', such as the iridium or shock quartz, while raising an additional barrier to criticisms based on an appeal to the complexity of a problem or area of research.

In many cases, this simplification also extends to the presentation of data. Bolstering his argument with charts drawn from various articles, paleontologist Erle Kauffmann argued that 'the way in which taxa, especially higher taxa, are depicted graphically — as unvarying straight lines extending from their points of origin to their points of extinction — produces misconceptions about the nature of the extinction event'.[90] Thus graphical conventions reinforce the perception of a sharp transition between geological periods. Again, the argument put forward by critics of the impact hypothesis is that the perception of complexity is central to the accurate identification of the problem, not symptomatic of 'unscientific' thinking. The character of the genre, however, works against the effective expression of this type of argument; one can easily imagine the difficulty of conducting a debate on the causes of World War One, or on the origins of capitalism, within this sort of arena. Furthermore, the maintenance of a direct link between biological changes and the proposed impact — with the ensuing simplification of the former —was a central component in the continuing interest in the extinction controversy on the part of publics both within the scientific profession and without.

The Published Record

As the numbers of scientists and scientific findings increase, the role of professional publications has been redefined. Rather than claiming to reflect the evaluations or status orderings of some scientific community, these journals have presented themselves as one means for countering the intellectual fragmentation associated with progressive specialization. When introducing a new feature, 'This Week in *Science*', the magazine's editor noted that:

> ... adventurous readers who venture outside their areas of expertise soon run into the language barrier. The arcane terminology of a different field is denounced, whereas the jargon in one's own field is defended as the only way to express complicated concepts succinctly .... One institution that can contribute to a translating service is a multidisciplinary journal like *Science*.[91]

This feature was less an innovation than an addition to a large battery of techniques available to editors for packaging research findings, techniques often obscured by the neutral verbs 'present' or 'translate'. At a very basic level, there is the ability to order and to categorize contributions: which piece is first in an issue? which are research reports rather than technical comments? which articles are to be solicited? The fact that the 1980 paper by the Alvarez team was not only first in the issue, but twice as long as the standard lead article, automatically flagged it as an important contribution. In addition, the role of the editorial board in selecting referees, and thereby influencing the entire process of peer review, is a source of control.[92]

The importance of the press as a professional gatekeeper is confirmed by the self-reports and opinions of those involved in, or observing, the controversy over catastrophic extinction. As Nitecki and Hoffman report, almost 30 per cent of their sample of paleontologists and geoscientists in five countries initially encountered the asteroid hypothesis by way of 'scientific commentaries'; this is in addition to the slightly larger proportion who cited the 1980 Alvarez paper.[93] They also noted considerable differences in the ways in which scientists first learned of the hypothesis; Germans were unusually dependent on meetings and informal discussions, while Poles relied heavily on popular media. In addition, their findings lend support for a close consideration of specific journals:

> About 95% of our North American respondents, 75% of British paleontologists, 65% of German paleontologists, and a mere 38% of Polish geoscientists know the 1980 article by Alvarez *et al.* in *Science*. There is no such trend for *Nature*. The asteroid hypothesis of terminal Cretaceous extinctions was published simultaneously and on the basis of essentially similar evidence by Alvarez *et al.* in *Science* and by Smit and Hertogen and Hsü in *Nature*. The paper by Smit and Hertogen, however, is familiar to a mere 32%-38% of respondents in all the groups except for Polish geoscientists; in the latter group, only 21% know this article. This comparison clearly indicates how important is the role of *Science* in placing a scientific problem in the focus of interest of the North American scientific community, though considerably less so in Europe.[94]

As was suggested previously, formal characteristics of these arguments may also have affected their reception. Nevertheless, these conclusions concerning the role of major journals have been reinforced by a growing ethical debate accompanying discussions of the impact hypothesis. At the 1985 convention of the Society of Vertebrate Paleontology, the editor of *Science* faced charges that 'some scientific journals, notably *Science*, have tended to favor the impact hypothesis in their selection of papers for publication'.[95] Proponents of the impact hypothesis, on the other hand, have received a series of rebukes on the editorial pages of the *New York Times*, bearing titles such as 'Miscasting the Dinosaur's Horoscope', and 'The Nemesis of Nemesis'. The former concluded that 'terrestrial events, like volcanic activity or changes in climate or sea level, are the most immediate possible causes of mass extinctions. Astronomers should leave to astrologers the task of seeking the cause of earthly events in the stars'.[96] More recently, the controversy was transformed into an editorial battle between *Science* and *Nature*, with staff writers of, and contributors to, the latter attacked by a *Science* staff writer for their critical stance toward the impact hypothesis.[97]

Changes in the format of scientific publications reflect their increasing role in summarizing and 'translating' research findings. Over the past seventy years, *Science* has been transformed from a journal managed by an academic editorial board and primarily concerned with in-house news from the profession — reviews, reports of expeditions, biographical notes and so forth. By the 1920s and 1930s, the journal included a greater proportion of research reports. Major changes began to take place in the 1950s and 1960s as the articles and research reports were supplemented by a growing number of news features and research summaries: 'Science in the News', 'News and Comments', 'Research News', and, most recently, 'This Week in *Science*', a page of brief summaries intended to 'allow the mathematician to understand the purpose and basic content of an article in medicine or a sociologist to understand an article in solidstate physics'.[98] Initially emphasizing social and political issues, an increasing proportion of staff contributions to each issue have been directed at inter- or intra-disciplinary communication. Concurrently, the editorial staff has grown from an editor and two assistant editors in 1960 to an editor, three department editors, seven reporters each for 'News and Comments' and 'Research News', and a host of associate and assistant editors.

These observations might be taken as a call for the sociology of science to retreat to its role as 'a sociology of error and distortion'.[99] However, it is more constructive to recall insights drawn from both psychology and organization theory: by affecting both the identification of issues and the range of perceived solutions, the mix of information available in the environment influences the decision-making behaviour of individuals and organizations alike.[100] In addition to providing information, the media may serve either to reinforce or to undermine beliefs and concerns developed through both formal training and personal interactions.

Sociologists and scientists alike have tended to limit their concerns to the ways in which the professional journals inform professional scientists and the mass media educates the masses:

> Journalists, and the public, thrive on controversial and stunning statements . . . If the growing corps of popular science writers would focus on *how* scientists develop and defend those fascinating claims, they would make their greatest possible contribution to public understanding.[101]

Yet, to the extent that broadly shared perceptions influence scientists, the popular press contributes to professional beliefs and ambitions.

Major journals influence the intellectual environment not only by accepting or rejecting articles for publication. The editorial staff serve as gatekeepers for a much broader system of communication; research findings may be discussed in articles by staff writers, in turn be reported in newspapers with their own science writers and, finally, appear in newspapers throughout the world by way of imitation or wire services. To the extent that scientific communication takes place in the guise of news, it becomes subject to a set of genre constraints somewhat different from those relevant to research papers themselves. Timeliness, novelty, human interest and ease of explanation are all factors.[102] As will be argued in the following section, interest in the extinction controversy has been maintained by the continual linking of research concerning impacts to issues with a much broader appeal — notably dinosaurs and, by way of a consideration of nuclear holocaust, the fate of mankind. For whatever reason, the demise of a wide variety of marine microfauna and the differential survival of mammals, placentals and multituberculates hold far less appeal for most of the scientific profession, as well as for the general public.

As is revealed in almost any comparison of research papers with the review articles that they inspire, science journalists in both the professional and popular press seek to interest their audience. Like the masses, they may prefer 'controversial and stunning statements'. Thus, scientific commentary has tended to emphasize either major points of opposition or claims of scientific consensus. In turn, the suspicion of editorial preferences jeopardizes the role of the journals as neutral arbiters of debate. But rather than reading this as an accusation of sensationalism on the part of science writers, it should be taken as indicative of the extent to which values such as novelty, human interest and political relevance inform the directions taken by scientific research.

The importance of this type of communication stems from the fact that scientists are not experts in every aspect of their life, or even of their vocation. Under these conditions, journalists may exert a degree of influence which has so far gone relatively unnoticed — perhaps because it fits so poorly with the ideal of the independent observer. In part, the growing importance of science writers may be seen as a function of the volume of scientific production; as the latter increases, shortcuts become unavoidable. A vacuum may also exist when the authority of prominent individuals is contested, or the definition of the controversy is unclear. At least in the case of the extinction controversy, these conditions have been involved in the emergence of a sharply divided, interdisciplinary debate.

Professions and Publics

Philosophical notions such as falsification, reduction and logical entailment capture something essential about the activities and ideals of individual scientists.[103] However, an emphasis on reduction and causal dependence obscures the importance of other potential relationships among theories. Two other modes of interrelatedness are evident in the controversy over the asteroid hypothesis: relations based on personal ties, and constructed analogies. In both cases, the press has been a significant resource in forging connections among individuals as well as ideas.

The interaction of personal networks and professional publication is illustrated by a controversy that followed the 1982 article by Raup and Sepkoski reporting a 26 myr periodicity in the fossil record. This article became the centrepiece of a dispute over scientific conduct, as well as the inspiration for new lines of inquiry. The ethical issues came to the fore in an essay by the editor of *Nature*, John Maddox, who expressed concern over 'the process of professional communication' — in particular, 'the widespread circulation of preprints.' As Maddox argued:

> The most obvious complaint against the system is that it is discriminatory, excluding from the circle of those in the know people who happen not to be on the authors' mailing list. On this occasion, the first accounts of models to explain periodic extinction were so swiftly on their way to *Nature* that, if revision of their content had not been recommended, it would have been possible for them to appear in print before Raup and Sepkoski.[104]

In effect, Maddox is asserting that the role of his journal is to serve as an open arena for scientific discussion, rather than a chronicle of the accomplishments of tightly-knit networks of researchers. Attacking the image of a closed circle, Raup and Sepkoski responded by noting that a summary of their research had been presented the previous year at the 'Dynamics of Extinction' conference in Arizona:

> The meeting was attended by several science writers and subsequently *Science*, *Science News*, and several other publications carried accounts of the meetings that included substantial treatment of our extinction analysis. It was mostly from these accounts that physicists, astrophysicists and geologists heard of our work, and this led to many requests for more information and for preprints.[105]

In this sense, the scientific press itself may contribute to the formation of interpersonal research networks by publicizing the existence of like-minded individuals to one another. Similarly, publicity itself creates a certain incentive to become involved in a research area that the media has presented as being of considerable consequence.

Scientific theories develop not only on the basis of logical entailment and personal ties; a new idea may gain popularity by drawing on individual expertise or by appearing to support a position taken with respect to an entirely different debate. It is not surprising that a paleontologist concerned with the last dinosaurs' evolutionary advantages would be sympathetic to a scenario that portrayed these beasts as cut off in their prime rather than as incapable of adaptation,[106] although the two issues are logically separable. Similar relationships between the asteroid hypothesis and debates within evolutionary biology have been influential. For biologists and paleontologists, if not necessarily for physicists, one of the major issues informing positions on catastrophic extinction has been the status of contemporary Darwinian theory, currently challenged on a number of fronts (punctuated equilibrium, cladism, and so on).

In part, the incentive to establish such analogies may stem from the desire to demonstrate the power and validity of particular techniques or theories, a concern that often inspires some sort of 'intellectual imperialism':

> professional vested interests may form the middle link which connects . . . controversies about the nature of phenomena and . . . conflict over the availability of resources or the securing of credibility for scientists' work.[107]

Earlier conflict between naturalists and experimentalists, as well as between taxonomists and biosystematists, foreshadow the current tensions between field paleontologists, geologists and evolutionary biologists. These struggles need not be restricted to professional scientists; indeed, many of the most significant resources, notably funding, celebrity and the trappings of power, are to be found in the public domain.[108]

Consequently, these analogical relations are not confined to intellectual projects, but may encompass political and social issues as well. Such connections are, obviously, potentially damaging to the reputation for objectivity claimed by most natural scientists, as well as many editors and writers. In the case of the asteroid hypothesis, political considerations have become increasingly apparent, as the fate of the dinosaurs is linked with the possibility of nuclear winter:

> As just one example of the unexpected, distant cross-fertilization that good science engenders, the Alvarez hypothesis made a major contribution to a theme that has riveted public attention in the past few months — so-called nuclear winter . . . .This theme of impact leading to massive dust clouds and falling temperatures was an important factor in the decision of Carl Sagan and a group of colleagues to model the climatic consequences of nuclear holocaust. . . . Full nuclear exchange would probably generate the same kind of dust cloud and darkening that may have wiped out the dinosaurs.[109]

The scenario proposed in the initial Alvarez article drew on models developed to predict the atmospheric effects of weapons testing. In turn, this scenario was an impetus to the further development of these models, and their application to new problems, most prominently the concept of 'nuclear winter'. The strength of this relationship derives not only from the personal ties and methodological preferences shared by the physicists, but from the commonplace use of the fate of the dinosaurs as a metaphor for the fate of mankind. It is the political — almost existential — connection which has been portrayed as a threat to the objectivity of scientific discourse. These discussions prompted the editor of *Nature* to argue that 'there is the strongest case for asking that the prospect of a nuclear winter should not be made into a more substantial bogeyman than it is by those who earnestly wish to avert the prospect of nuclear war as such'.[110] Similarly, the linkage of the two issues has allowed positions on the validity of the impact hypothesis to be mistaken for statements of personal politics.

The use of analogies or personal networks to shape the perception of phenomena is neither without precedent nor unique to the physical sciences. In the current debate, however, an exceptionally wide range of resources has been exploited: Luis Alvarez's Nobel prize, with its cachet for the mass media and funding agencies; the involvement of a number of 'visible scientists', including Stephen Jay Gould and Carl Sagan, as contributors and commentators;[111] the long-standing connections with funding agencies developed by scientists in capital-intensive specialties; impressive statistics; big computers; and, above all, the prominence of dinosaurian extinction as a scientific mystery.[112]

Conclusion

Art historians speak of the 'period eye', the distinctive set of expectations and interpretive skills possessed by the artists and audiences of a given age.[113] Similarly in science, fellow specialists, other scientists and the general public form different but overlapping audiences with different, but related, understandings of the appropriate styles and important problems for scientific debate. The ability to meet these expectations and interests is one factor influencing the reception given to specific arguments. Thus, in tracing the development of a debate, it is important to consider the ways in which various positions accord with the notions of good science and interesting questions held by these publics. In this respect, the impact hypothesis had a definite advantage over arguments that stressed the complexity of both the question and the appropriate sort of answer; the Alvarez theory offered an elegant and parsimonious solution to a question firmly embedded in popular culture, at least in the United States.

Like the 'period eye', however, concepts of good science and interesting issues are not established for all time — nor are they universally held at any one time. Although the attribution of dinosaurian extinction to extraterrestrial objects had a long history, only with the work of the Alvarez group did it become a reputable argument. Contrasting this episode with the supernovae theories of the previous decades, the consequences of the formal character of the argument, as well as of the institutional location and personal influence of its proponents, are clear. In both respects, publications served as a central medium for the establishment and diffusion of impact-related research. For within a debate, differing access to resources can create significant asymmetries in the flow of information. Relevant resources include not only rhetorical appeals to shared ideals, but institutional location, material wealth, and the ability to mobilize popular or professional sentiments in a way that enhances one's position within a debate or a status hierarchy.

This line of inquiry reveals some of the organizational changes behind the growing concern for, and criticism of, the authoritative status of scientific knowledge. Sociologists should pay increasing attention to changes in the internal organization of disciplines and the extent of their insulation from other social organizations. A research project in its own right, this line of inquiry could enrich any study of the structure of scientific communication, both professional and popular, by addressing the varying ability of different kinds of researchers and research programmes to become news. Access to funding and other resources affects the ability to do interesting research, as well as the ability to sponsor the meetings and speeches which promote the status of that research as newsworthy — not only for the public, but for other scientists. The press, however professional, both influences and is influenced by theoretical, political and cultural interests. The isolation of academic science as an object of study has obscured the importance — one might argue increasing importance — of these relationships.

• NOTES

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1. In the work of Lakatos, Kuhn and Merton, the autonomy of scientific practice is intimately linked to the validity of scientific knowledge. The collective structure of the field — whether rooted in logical unity or social organization — is placed between scientific practice and social or self-interested influences. See Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: The University of Chicago Press, 2nd edn, 1970), and *The Essential Tension* (Chicago: The University of Chicago Press, 1977); Imre Lakatos, *The Methodology of Scientific Research Programmes* (Cambridge: Cambridge University Press, 1978); Larry Laudan, *Progress and its Problems: Towards a Theory of Scientific Growth* (Berkeley, Calif.: University of California Press, 1978); Robert K. Merton, *The Sociology of Science* (Chicago: The University of Chicago Press, 1973), 267–78, at 270. For an interesting exception, see T.F. Gieryn, 'Boundary-

Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists', *American Sociological Review*, Vol. 48 (1983), 781-95.

2. Luis Alvarez, Walter Alvarez, Frank Asaro and Helen V. Michel, 'Extraterrestrial Cause for the Cretaceous-Tertiary Extinction: Experimental Results and Theoretical Interpretation', *Science*, Vol. 208 (6 June 1980), 1095-108.

3. Karl W. Flessa. 'Extinction is Here to Stay', *Paleobiology*, Vol. 9, No. 4 (1983), 315-21, at 315.

4. M.R. Rampino and R.B. Stothers, 'Terrestrial Mass Extinctions, Cometary Impacts and the Sun's Motion Perpendicular to the Galactic Plane', *Nature*, Vol. 308 (19 April 1984), 709-12; R.D. Schwartz and P.B. James, 'Periodic Mass Extinctions and the Sun's Oscillation about the Galactic Plane', ibid., 712-13; D.P. Whitmire and A.A. Jackson, 'Are Periodic Mass Extinctions Driven by a Distant Solar Companion?' ibid., 713-15; M. Davis, P. Hut and R.A. Muller, 'Extinction of Species by Periodic Comet Showers', ibid., 715-17.

5. To quote Michel Cloître and Terry Shinn, this is an example of how 'expository practices play an active role in the knowledge production process': 'Expository Practice: Social, Cognitive and Epistemological Linkage', in Terry Shinn and Richard Whitley (eds), *Expository Science: Forms and Functions of Popularisation* (Dordrecht: D. Reidel, 1985), 31-60, at 32.

6. Steven Shapin, 'History of Science and its Sociological Reconstructions', *History of Science*, Vol. 20 (1982), 157-211, at 159.

7. Kuhn, op.cit. note 1, 42.

8. Paul M. Hirsch, 'Processing Fads and Fashions: An Organization-Set Analysis of Cultural Industry Systems', *American Journal of Sociology*, Vol. 77 (1972), 639-59. On the use of environmental concepts in the history of science, see R.J. Richards, 'Natural Selection and Other Models in the Historiography of Science', in M. Brewer and B. Collins (eds), *Scientific Inquiry and the Social Sciences: A Volume in Honor of Donald T. Campbell* (San Francisco, Calif.: Jossey-Bass, 1981), 37-76.

9. For a discussion of the contrasts between scientific publications and less formal scientific conversation, see G.N. Gilbert and M. Mulkay, *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse* (Cambridge: Cambridge University Press, 1984), Chapter 3.

10. Dorothy E. Smith, 'Textually Mediated Social Organization', *International Social Science Journal*, Vol. 36 (1984), 59-75, at 59.

11. Timothy Lenoir, 'Quantitative Foundations for the Sociology of Science: On Linking Blockmodelling with Co-Citation Analysis', *Social Studies of Science*, Vol. 9 (1979), 455-80.

12. David Bloor, *Wittgenstein: A Social Theory of Knowledge* (London: Macmillan; New York: Columbia University Press, 1983), 139-41.

13. Barry Barnes and David Edge (eds), *Science in Context: Readings in the Sociology of Science* (Milton Keynes, Bucks: Open University Press; Cambridge, Mass.: MIT Press, 1982), 66.

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15. Bloor, op. cit. note 12, 42.

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Concept of Ideology (Athens, Georgia: The University of Georgia Press, 1979). When this reluctance has been overcome, typically it has been for the purpose of denouncing modern science as thoroughly ideological.

17. Pierre Bourdieu, 'The Specificity of the Scientific Field', in Charles Lemert (ed.), *French Sociology: From Rupture to Renewal* (New York: Columbia University Press, 1981), 257–92, at 264.

18. Sara Genuth, 'Comets, Teleology and the Relationship of Chemistry to Cosmology in Newton's Thought', *Annali dell' Istituto e Museo di Storia della Scienza di Firenze*, Vol. 10, Part 2 (1985), 31–65.

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29. Gould, op. cit. note 26, 29. The works of Thomas Kuhn and Michel Foucault are prominent examples of this trend in the sociology of knowledge.

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81. See Frederick Suppe, *The Structure of Scientific Theories* (Urbana, Ill.: University of Illinois Press, 1977), 53–56. A recent article suggests further reasons for scepticism concerning the role of falsification in contemporary science. In a study which explored scientists' understanding of the propositional logic which underlies many of the historical and philosophical accounts discussed in this paper, the authors report that:

> nearly half of the scientists failed to recognize the logical validity of *modus tollens*, an inferential rule of propositional logic which, from a strictly normative standpoint, has been depicted as the only form of valid conclusive inference in theory and hypothesis testing. The pivotal role of formal logic in philosophical analyses of scientific inference is questioned on empirical grounds.

L.H. Kern, H.L. Mirels and V.G. Hinshaw, 'Scientists' Understanding of Propositional Logic: An Experimental Investigation', *Social Studies of Science*, Vol. 13 (1983), 131–46, at 131.

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92. An analysis of the considerable impact of referee selection on the NSF's process of making research grants is found in Stephen Cole, Jonathan R. Cole and Gary A. Simon, 'Chance and Consensus in Peer Review', *Science*, Vol. 214 (20 November 1981), 881–86. For a discussion of the problems of journal review with respect to the impact hypothesis, see Malcolm W. Browne, 'Dinosaur Experts Resist Meteor Extinction Idea: Paleontologists say Dissenters Risk Harm to Their Careers', *The New York Times* (29 October 1985), 21–22. During the period explored in this paper, Luis Alvarez joined the editorial board of *Science*.

93. Hoffman and Nitecki, op. cit. note 31, 885.

94. Ibid., 886.

95. Browne, op. cit. note 92, 22.

96. 'Miscasting the Dinosaur's Horoscope', *The New York Times* (2 April 1985), A26; 'The Nemesis of Nemesis', ibid. (7 July 1985), 16. Earlier in the year, however, the newspaper had provided a prominent platform for advocates of the impact hypothesis when the Sunday magazine published 'An Adventure in Science: The Pleasures of Being an Astrophysicist', by Richard A. Muller, one of the authors of the death star theory. Accounting for his interest in the problem, he referred to a childhood book, *The Dinosaur Hunters*:

> The book showed paleontologists digging up rocks, carefully scraping away the mineral around the fossilized bone, taking weeks to remove a single specimen. I told my parents that I wanted to be a paleontologist... But deep down I didn't believe it. Real paleontology looked terribly boring. My most valued possessions were a telescope and a microscope. The world I saw through these seemed particularly beautiful, crystal clear and sharp. From that point on I associated science with beauty.

The New York Times Magazine (24 March 1985), 34–46+, at 34. Many participants in this controversy have recounted similar childhood encounters with the mystery of the extinction of the dinosaurs. Thus a common puzzle exists, not by virtue of shared training, but through exposure to museums (whose collections reflect the nineteenth-century emphasis on procuring large dinosaurian skeletons) as well as books and other products of popular culture.

97. Antoni Hoffman, 'Patterns of Family Extinction Depend on Definition and Geological Timescale', *Nature*, Vol. 315 (20 June 1985), 659–62; John Maddox, 'Periodic Extinctions Undermined', ibid., 627; Roger Lewin, 'Catastrophism Not Yet Dead', *Science*, Vol. 229 (16 August 1985), 640; letter from Hoffman and reply from Lewin, *Science*, Vol. 230 (4 October 1985), 8. Maddox used the medium of a staff review article to highlight a research report, and make it the basis for an assessment of the entire debate. The same strategy has been used to emphasize findings supporting the opposite position: see, for example, Roger Lewin, 'Extinctions and the History of Life', *Science*, Vol. 221 (2 September 1983), 935–37. Although the text states that 'if anything, the meeting was biased in favor of those who lean toward earthbound, as against extraterrestrial, agents as a cause of, specifically, the late Cretaceous extinction', the subtitle of the article is: 'Now that, for many at least, asteroid impact has been accepted as a causative agent in mass extinction, attention turns to the wider view'.

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99. Joseph Ben-David, cited in Shapin, op. cit. note 6, 157.

100. Graham Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston, Mass.: Little, Brown, 1971); James G. Marsh and Johan P. Olsen, *Ambiguity and Choice in Organizations* (Bergen: Universitetsforlaget, 1976); John Steinbruner, *The Cybernetic Theory of Decision* (Princeton, NJ: Princeton University Press, 1974).

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102. Gastel, op. cit. note 101, 30–32.

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104. J. Maddox, 'Extinctions by Catastrophe?', *Nature*, Vol. 308 (19 April 1984), 685.

105. D.M. Raup and J.J. Sepkoski, Letter, *Nature*, Vol. 309 (24 May 1984), 300.

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107. Shapin, op. cit. note 6, 165.

108. Goodell, op. cit. note 101, 206.

109. Gould, op. cit. note 42, 72; R.P. Turco, O.B. Toon, T.P. Ackerman, J.B. Pollack and C. Sagan, 'Nuclear Winter: Global Consequences of Multiple Nuclear Explosions', *Science*, Vol. 222 (23 December 1983), 1283–92.

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111. Goodell, op. cit. note 101, 36–32; 29–31.

112. Accounts given by participants of their involvement in the debate tend to mention 'one of the greatest mysteries in science — the sudden extinction of the dinosaurs', rather than theoretical logic: Alvarez, op. cit. note 35, 632. Children's toys, museum sales, and coverage in the mass media all testify to, as well as perpetuate, a widespread popular interest in the subject. For a Freudian analysis of children's interest in dinosaurs, see John E. Schowalter, 'Children's Fascination with Dinosaurs', *Children Today*, Vol. 8 (May–June 1979), 2–5.

113. Michael Baxandall, *Painting and Experience in Fifteenth Century Italy* (Oxford: Oxford University Press, 1972).

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[36] **Review: Catastrophes, Expectations, and the Evidence**
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Leigh M. Van Valen
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