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## Contents




• *ABSTRACT*

*Protecting endangered species is a very complicated scientific task, requiring ingenuity and imagination. Yet the organizations to which we have given this task may be little suited to it. Examining the ecology of black-footed ferrets and their near-extinction under government management, the authors question the adequacy of traditional wildlife management approaches whose calculative rationality fails to respond to the often rapidly changing and precarious situation of an endangered species. They argue that a different, generatively rational approach is needed if species like the ferret are to survive. Such an approach would use available cognitive resources better than does the highly centralized approach which wildlife bureaucracies seem to favour. Such organizational checks and balances would provide less danger for blind spots in our perception of the environment.*


# Paradigms and Ferrets


## Tim Clark and Ron Westrum


**In 1803, after the** impressive fall at l'Aigle, most of the European scientific community accepted the reality of meteorites. Many years later their discoverer, Ernst Friedrich Flourens Chladni, reflected upon the earlier time when these strange objects had struggled for acceptance. It was a time, he said, 'when people thought it necessary to throw away or explain as error everything that did not conform to a self-constructed model'.[1] In many respects the problem we shall discuss here falls along these same lines. It too concerns the refusal, in the face of contrary evidence, to give up one's conceptions of the way things are.

Human action responds to a self-constructed world, a world seen through the filter of concepts we use to interpret it.[2] This conceptual grid shapes our perceptions and actions. Its sophistication and accuracy have important consequences for the success of the projects we undertake, and its inaccuracies cause us serious problems. It might seem, therefore, that we should have a strong interest in making it as adequate as we can. Yet there are always other factors which shape it. As Freud observed for individuals and Janis for groups, emotional as well as


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cognitive pressures influence our map of the environment.[3] Wishes, fears, defence mechanisms and tendencies towards 'groupthink' are examples of these emotional forces. Concern for external accuracy must compete with concern for internal coherence, stability and calm. At the level of larger social units and complex organizations, these same conflicts reappear in different forms. Established organizations struggle to maintain a previously negotiated reality against new organizations that seek to erect new systems of belief. New evidence must fight for acceptance against such institutionalized 'truth'.

This struggle has particular salience for resource management organizations which depend heavily on scientific personnel. The very process of constructing organizations to carry out such responsibilities may mean the development of powerful intellectual and organizational interests which will jealously defend the points of view they develop. In designing these social 'inquiring systems', we should take care to make them strong enough to resist inappropriate pressure, but flexible enough to be self-correcting.[4] Strength and flexibility are, however, somewhat contradictory requirements, and we can expect few scientific organizations to meet both of them. As Robert Michels found with political parties, the decision to organize for an activity always carries a danger: that the organization may become more concerned about its own survival than about the goals for which it was set up.[5] In science, as with other human activities, the organization's welfare can become more important than fulfilment of its mission.

Protection must thus be dual, for scientific organizations are vulnerable to misdirection because of both external or internal forces acting upon them. External pressures can come in the form of political influences, as in the case of 'Aryan science' or the Lysenko Affair; or in the form of financial incentives, typical of industrial research organizations and grant-supported research.[6] The mere fact of external funding for science is an important social influence which needs a more comprehensive examination.[7] On the other hand, the organization's own internal processes also pose a danger to its mission. There is a danger that opinion will become dogma. Once the organization has developed a workable conceptual grid, strong pressures against changing it build up. The organization's doctrines, through a variety of processes, become resistant to new facts or theories, even where these may be appropriate. The organization may come to view threats to its doctrines as threats to its own existence. Resistance is likely to be particularly fierce when the organization has come under attack, or when its doctrines have become involved in its relations with other organizations. The classic example





of such intransigence is the Army Corps of Engineers' defence of Humphreys's views on the hydraulics of the Mississippi, long after they had lost credibility outside.[8]

Thus the very organizations which are responsible for maintaining the conceptual grid may develop strong parochial interests in shaping it in particular ways. And society, operating on the world seen through this grid, will be influenced to act in certain ways and be subject to certain blind spots as a natural consequence. One immediate implication of such blind spots is that society may be unable to see parts of the natural environment which require action. What it cannot see, it cannot protect.

This paper will examine the social relations of one such blind spot: the situation of very rare endangered species. The case we will use as a focal point is that of the black-footed ferret (*Mustela nigripes*), a medium-sized weasel-like mammal found west of the Mississippi in the United States. A ferret population in Wyoming has been under study now for five years, but this paper will largely concern itself with events prior to the discovery of this population, reserving until the end observations on the current situation.

## The Ecology of Learned Ignorance

The use of scientific methods depends upon accurate observation. Yet one of the major thrusts of recent sociology of science is that perception is directed and shaped by the expectations of the observer. Such expectations may come from theories, from broader disciplinary matrices (paradigms), or from specific suggestions by other observers as to what ought to be seen. One of the most articulate exponents of this view was the late Ludwik Fleck, whose study of the changing perceptions of syphilis is now recognized as a classic.[9] Thomas Kuhn's arguments about the influence of disciplinary training on what is looked for and what is ignored are even better known.[10] It is evident from the work of these and other scholars that what is seen at one time as 'noise' or anomaly can well become the 'basic data' for later theories.

The history of science is filled with examples of the influence of expectation on perception. A sampling of these will illustrate:

1. *What is expected is what is looked for.* The geologist Werner espoused a 'neptunian' theory of the origin of strata so convincing that his pupils travelled near and far to show how universal it was. Unhappily, although intent upon proving their master's theory, many

of them experienced such distress in trying to fit the observations to it that they switched and became staunch 'plutonians'.[11]

2. *What is looked for is what is seen.* Some of the most embarrassing episodes of science involve seeing something that is not there because one expects to see it. N-rays, polywater and erroneous observations of the rotations of Venus all show the influence of the role of expectations in causing faulty observation.[12]

3. *What is unexpected is unobserved.* A common problem in medical diagnosis, this tendency is well displayed by the history of the battered child syndrome.[13] In view of the huge number of cases that must have passed under the eyes of physicians, the virtual lack of mention of this problem in the medical literature prior to 1946 is astonishing. Nor was this simply a case of reluctance to report. One pediatrician stated that if he thought a parent could abuse a child in this manner, he would leave pediatrics immediately.[14] Such attitudes do not make for accurate observation.

4. *What is unexpected is unreported.* Ball lightning was formerly considered a 'rare' phenomenon in meteorology textbooks. Surveys, however, showed that it was much more common than had been guessed.[15] Yet even professional lightning observers hesitated to report it, since it was scientifically unacceptable.[16] Here, of course, the individual may see the anomaly, but it is 'invisible' to the social system.

Each of these examples shows the potent effect of expectations, but since in each case the perceptions in question have later been 'corrected' in the light of more recent observations, it is also evident that expectation is not everything. A mismatch between stimulus and expectation can result either in reformation of the perception or of the expectation.[17] The influence of the conceptual grid is strong, but not absolute. The time frames involved in some of the cases, however, should give us pause: 'corrections' do not always come speedily.

Expectations have a certain psychological force, but they also have a social force, for beliefs are developed and maintained by groups of people.[18] As the ball lightning example shows, an organized collectivity (for example, the meteorology community) develops doctrines about what is real and what is not. The individual ignores community norms only at considerable risk to peace of mind and reputation.[19] One of the valuable discoveries of the sociology of science is the extent to which these group norms affect scientists' opinions.[20] The image of the scientist as a disinterested observer of the natural world has been seriously dented, if not actually demolished.





The influence of organizational interests on scientists' opinions is well illustrated by the accidental pollution of the food chain in Michigan by the chemical polybrominated biphenyl (PBB). Although the mishap originated with a private chemical firm, the involvement of scientists from state agencies consistently resulted in conservative estimates of the threat. As Joyce Egginton convincingly shows in her book *The Poisoning of Michigan*, the Michigan Department of Agriculture, the state Department of Public Health, Michigan State University and even the federal Food and Drug Administration acted as if the problem could safely be ignored.[21] Each of these organizations had connections to the agricultural community, and each was forced to weigh political and economic consequences as well as ecological ones. Outside experts had to be brought in by the farmers affected to provide a thorough examination of the level of contamination. One would have to possess a very robust faith in the autonomy of science to believe that the PBB case is an isolated example of such organizational influences.[22]

The influence of such factors on scientific opinion is strong. The creation of an organization is the creation of a living system whose survival will naturally be sought by its members. While in theory the organization's scientists should form opinions without considering their consequences for the organization, this is unlikely in real life. It is only human for an organization's scientists to be more favourable to facts and theories which present it in a positive light. The organization's position on some issues may provide an 'anchoring point' for future thought and opinions. The development of special cognitive norms and rationales thus seems inescapable, and their influence over individual scientists assured. The value and necessity for using organizations to carry out scientific tasks necessarily entails also the risk of this kind of institutional dogmatism.

While considerable attention has been given to the intellectual effects of disciplinary paradigms, not enough has been given to their organizational basis. The vital tension in science between tradition and innovation frequently translates on a societal level to a struggle between organizations. Success in science leads to recognition, and recognition to power. Once a set of ideas has laboriously been developed, fought against opposition, and its holders established in university chairs, research institutes and government bureaux, it is likely that they will try to reinforce their position in various ways.[23] Organizations offer a variety of means for such reinforcement: control of access to research sites, control of funding, gate-keeping of publications and, ultimately, the ability to determine what is to be considered scientifically 'competent'. The temptation to use these

means is strong. Providing organizational power to the holders of any set of ideas risks the use of this power against new or critical ideas. Only when this power is suitably limited can intellectual discourse be protected from inappropriate closure.

Furthermore, the problem may not be a single organization but an establishment – that is, a set of organizations whose interactive processes maintain a mutually-agreed-upon definition of reality. Conformity is achieved through training institutions, professional societies, peer review and transfers of personnel. In time what was merely a consensus begins to appear as objective fact. Critiques of the established view are received with surprise, incomprehension and ridicule. Just such a reaction greeted Rachel Carson's *Silent Spring* in 1962. It was not surprising to find this book, which first suggested widespread dangers of pesticide use, strongly attacked by the chemical industry. What was more surprising was the number of attacks on it by university scientists. Many of these, it turned out, had funding associated with pesticide use or friendly relations with the agricultural chemical industry.[24] In many respects the early reactions to *Silent Spring* provided a melancholy illustration of Marx's charge that the intellectual superstructure is rooted in economic relations.

The establishment's key resource is legitimacy. A constant propaganda presents the institutions involved as expert, objective and comprehensive, their opinion as the opinion of professional science. As the establishment becomes larger and more dominant, it can present its critics as misguided, badly informed or even dishonest. Its own growing monopoly of knowledge resources may give some truth to the 'badly informed' claim. And its power to reward its friends may bring allies from outside the scientific community proper, such as the press. Legitimacy is a powerful resource in controlling critics and threats to an establishment's domain of action.

A scientific establishment, then, represents not only a concentration of opinion but also a concentration of power.[25] In itself such power is a natural consequence of the delegation of important missions (such as resource management) to scientific organizations. If such power is unopposed, however, it may interfere with the goal's accomplishment. These organizations should provide society with the ability to 'see' the environment by maintaining an adequate conceptual grid. Yet a healthy maintenance programme requires constructive criticism, the free examination of ideas, and above all an organized opposition.[26] To depend, on the other hand, on a single inquiring system is to make the blind spots of the establishment those of the society itself. The cost of these blind spots may be high. In the case of the black-footed ferret, one cost may be extinction.



## Looking for Ferrets

The black-footed ferret, a member of the weasel family, is one of the rarest and most endangered mammals in North America. To some biologists, in fact, it appeared to be extinct after 1974. It has a low density, is secretive, nocturnal and mainly subterranean. It depends on prairie dogs (*Cynomys* sp.) as prey and their burrows for shelter. Prairie dogs are thus critical to the survival of ferrets. Ranchers and farmers, however, often consider prairie dogs pests and thus shoot or poison them en masse. The drastic loss of habitat involved in the thinning out of prairie dogs is the main reason for the ferrets' endangerment today.[27] First described to science in 1851 by John James Audubon and John Bachman, the ferret has always been considered rare by most natural historians and wildlife managers. This perceived rarity needs to be kept in mind when considering government activities for ferret management and preservation.

Research on the first ferret population ever studied began in South Dakota in 1964, 113 years after the species was first described – and continued through 1974, after which no animals were found in the area under study. From 1974 to 1981, 'normal science' in a Kuhnian sense could no longer be practised since ferrets could not be located consistently, and even appeared functionally extinct to some. During this period, however, other people worked on locating another population. A new population was discovered in Wyoming in 1981, came under study, and normal science is again being employed.

This case study will concentrate on the years 1974–81, and on the set of organizations involved in ferret management during these years: the United States Fish and Wildlife Service (USFWS), the US Bureau of Land Management (BLM), and the Wyoming Game and Fish Department (WGF) – referred to hereafter as 'the agencies'. Other organizations were also searching for ferrets during this period. These non-governmental organizations (NGOs) came to play a major role in ferret conservation. One of our major objectives will be to sort out the respective roles of the agencies and the NGOs. While, on the surface, the agencies' dominant approach during these years appeared to be in the best tradition of modern wildlife biology, a more detailed examination of their activities suggests the opposite. In fact, the behaviour of the agencies raises a serious question: *can agencies established to manage wildlife, using traditional paradigms and management practices, protect very rare species like the ferret?*

Traditional methods of species conservation work fairly well with large and visible populations of animals, when such surveys are economical and

when the animals have high symbolic value (for example, the American bison, *Bison bison*). These methods do not work well with small, difficult-to-detect populations such as ferrets. Erickson noted that the ferret was one of the least well known of all the endangered US mammals, despite more than ten years of research.[28] From 1964 to 1974, furthermore, the research programme in South Dakota declined markedly because the rate of fact-finding (measured in return per hour spent by researchers) had itself decreased. Until the new population was found in 1981, biologists were unable to locate any ferrets, and thus could not identify the species's *critical habitat* for protection. The factors which limited ferret populations and habitat could not be identified, much less mitigated or eliminated. In the years after 1974 it became increasingly apparent that 'research as usual' could not be continued.

Since the ferret had always been assumed to be rare, the die-out of the population in South Dakota led to a reduced effort to preserve ferrets by the agencies. In the South Dakota study ferrets had been shown to occur at low densities, and had proven difficult to locate and study. When the South Dakota population became extinct, many land managers assumed that ferrets themselves had become extinct, or hung on in such low numbers in remote, isolated pockets that it was useless to attempt finding them.[29] Consequently, agencies made little effort during the 1974–81 period to improve ferret surveys, or to direct surveys to areas that had the greatest likelihood of harbouring ferrets, practices justified in turn by lack of new ferret finds. This took place in spite of evidence developed by NGOs that traditional approaches to ferret surveys were inadequate.[30] The agencies finally classified the ferret as extinct or 'unrecoverable' over large portions of its former range. They were in the process of extending this classification to Wyoming when the new population was discovered there.

At this point it is worth noting that a change in the biological situation – the extinction of the South Dakota population and the failure to find more ferrets – might have been seen to demand a change in research strategy, especially after the 'normal science' methods for locating and studying ferrets were shown to be less and less effective. At this point there might have been a reflective pause and reconsideration.[31] This change did not, however, take place in the agencies.

The real danger was that the agencies might fall into a routine. The agencies' survey methods were based on the South Dakota experience and on assumptions appropriate to the study of larger and more visible population ('if ferrets are present we can find them'). These methods might not necessarily work elsewhere. Yet the agencies continued to treat



ferrets according to such assumptions as the years went by and no ferrets were located. It was then assumed that the species was extinct or at best unrecoverable. The disappearance of the ferret from the agencies' conceptual grid was caused in part by the failure of traditional methods, in part by the agencies' underlying assumptions, and by pressure on scarce agency resources. Bureaucrats had to justify time, money and effort looking for ferrets. When ferret search efforts produced few results, the natural response was to reduce inquiry.[32]

Life-cycle characteristics of the ferret pose severe problems for its preservation given the calculative rationality (see below) employed by the agencies. We know that for rare species observability decreases as population size decreases. Thus a disproportionate increase in search effort is required to find individuals as the species becomes more rare. By definition, a species is rare when experts have difficulty finding it. But at what level of unsuccessful search effort can a species be considered extinct? And also, what kind of observations are considered to provide valid evidence? Here the 'operational definition' of ferret presence is critical.

Ironically, ferrets were missing from the agencies' conceptual grid in precisely the region of Wyoming in which the new population was discovered in 1981. In the twelve months preceding the ferret population discovery, several oil/gas wells were drilled immediately over a large prairie dog colony later shown to be occupied by ferrets. Apparently due to lack of communication between agencies, the USFWS and WGF did not know about the wells, although the BLM did, and did not require a ferret survey prior to the drilling. Even if the possibility of ferrets had been considered, however, it is doubtful whether surveys using then-current USFWS guidelines could have detected them. Surveys using guidelines proposed by some NGOs might have detected the ferrets, but they were not sanctioned by the state and federal regulatory agencies at the time.[33]

How does one know if there are ferrets present or not? There is a wide range of evidence of ferret presence, including: (1) corpses; (2) photographs; (3) sightings; (4) skeletal remains; (5) scats (faeces); and (6) distinctive diggings. The traditional view taken by the agencies has been that only three kinds of evidence count as *proof* of the ferret's presence in an area. Preservation actions would take place only if one or more of three types of evidence were located: (1) a corpse (for example, a road kill); (2) a photograph with sufficient context so that the site could be identified; or (3) a group of 'credible' biologists who simultaneously observed a ferret. These criteria express an attitude that 'science' requires verifiable 'hard' evidence and inter-observer checks. It must be noted

that such criteria, since largely unwritten, often have to be inferred from the attitudes and actions of agency personnel. Before considering another class of 'softer' evidence, let us examine the probabilities that these three types of evidence could be obtained for a very rare species like the ferret.

## The Nature of 'Hard' Evidence

Since hard evidence is usually considered to consist only of corpses, photographs and group sightings, let us consider each of these types in turn.

1. *Corpses*. Ferret corpses, even when ferrets are present, are not likely to be abundant. Corpses from natural causes are not likely to be found on the surface, and if they are, may be removed by scavengers. Those dead from human causes are not likely to be reported, either because they are not identified or because there is little motivation to report them. Attitudes towards government wildlife agencies are often negative, and few farmers want their land declared a sanctuary for a protected species. One of the authors (TC) was told about several ranchers who claimed to have ferrets on their land but refused to divulge information directly to wildlife biologists or managers, because of feelings that all wildlife conservationists are affiliated with the government. Landowners fear that federal land takeovers will follow if an endangered species is found on their land. Furthermore, accidental killing of ferrets could mean legal problems, providing more negative incentive to report sightings or corpses to the authorities.

2. *Photographs*. While a really persuasive photograph of a ferret would constitute hard evidence, such photographs are unlikely to result from casual encounters. Few ranchers, biologists or others regularly carry a telephoto and flash-equipped camera, which would be essential for photographs of a small, secretive, nocturnal animal. In one instance the regional representative of a national conservation organization, a Wyoming resident, wrote to the USFWS for a permit to photograph a ferret. This would have served as evidence of the ferrets' existence in south-western Wyoming in 1978. In reply, the petitioner was told that no such permit would be issued and that if he turned up with a photograph, he would be liable under Section 9 of the Endangered Species Act for harassment of an endangered species![34] There is little question that such incidents discourage people from photographing ferrets.

3. *Group sightings*. If a group of 'credible' biologists (minimum of three?) saw a ferret, this might be hard evidence of ferret occupancy in an



area. But no matter how many individuals see a ferret simultaneously, there is necessarily a subjective component, and thus the validity of the sighting is open to question. This raises the question of how often one would find a group with the proper composition (that is, of credible biologists) assembled at the right place at the right time. How often has such a group stayed on a prairie dog colony long enough at night even to see a ferret?

So each of these three types of hard evidence is difficult to obtain, even when ferrets are known to be present, as experience in South Dakota showed. By requiring a class of evidence very difficult to obtain, the agencies have stacked the deck against the detection of ferret presence. At the same time, however, the agencies present themselves as operating in the best tradition of wildlife science: they are calculatively rational. The scarcity of hard evidence does not mean, however, that there has not been other evidence of a less 'hard' nature. Other traces, in the form of skeletal remains, scats, distinctive diggings and single-person sightings have been found or reported in potential habitat – prairie dog colonies.

## The 'Soft' Evidence

In Wyoming, for example, eight skulls (three in one grouping three miles apart, and four found in another grouping within twenty-five miles), faecal remains similar to known ferret scats, and diggings similar to those described in South Dakota were found in several locations around the state.[35] This evidence can be combined with continuing reports by single observers, though in small numbers, some of which have been in the general area of the skull finds amidst abundant prairie dogs. This combination of evidence was not considered sufficiently 'hard', however, for the agencies to mount systematic searches or to recognize formally the high probability of ferret presence, and thus to designate the area with a special status. The agencies argued that the skulls could not be distinctly dated and therefore could be very old; that scats – even though found in semi-desert areas – could not be positively differentiated from the semi-aquatic mink (*M. vison*), and that diggings of whatever size and conformation could have been made by other animals. Single observer sightings, regardless of the background of the individual, were considered to carry too large an element of subjectivity for credence. And limited follow-up surveys in the area of a reported ferret sighting failed to find ferrets. We will discuss the adequacy of such surveys below.

What constitutes a valid sighting is a serious issue, since numerous single observers – in some cases highly regarded biologists – reported seeing ferrets in western states.[36] Several people reported seeing ferrets prior to 1981 in the immediate region of the 1981 Wyoming ferret discovery. These reports were seldom confirmed through field investigations by agency biologists. Indeed, the report of a live ferret by a cowboy three weeks after the discovery in Wyoming of a dog-killed ferret in the same area, was not believed until agency biologists were taken to the area by the cowboy and saw a ferret themselves. Without the additional confirmation provided by the corpse, it is not clear even that this investigation would have taken place. Discovery of the corpse itself was a stroke of good luck. The taxidermist who first identified it had been visited a week earlier by an NGO biologist who noticed the shop while driving by. He told the taxidermist about ferrets, their rarity and the NGOs interest in locating and recovering them. Without this background, it is possible that the taxidermist might never have identified the specimen or turned it in.

In a few other cases in which agency biologists did follow-ups, their own assessments (independent of NGOs) seemed to confirm the authenticity of certain single observer reports. Nevertheless, no such observation between 1974 and 1981 carried enough weight for management agencies to declare formally that an area was of possible high value to ferrets, and to decide to manage it to protect ferrets; this despite other corroborative 'hard' evidence. One might infer from lack of formal preservation actions that the agencies believed that no one had actually seen a ferret in the areas in question.

This situation was made worse by the failure of follow-up surveys in areas where ostensible ferret sightings have taken place in Wyoming. In the light of the ferret's scarcity and relatively unique life-history, the failure of such surveys is not surprising. In fact, the Wyoming area containing the large ferret population was mapped for prairie dog distributions by BLM biologists the year before ferrets were found: no evidence of ferrets was seen. Failures to find ferret evidence thus convinced the agencies of the correctness of their belief that ferrets were, 'for all practical purposes', extinct in these areas.

To summarize, the 'hard' evidence which was considered essential for government action was largely unavailable for the ferret, at least until the new population was found in 1981. So 'management actions' which might have served to protect the ferret in areas where it might be existing were limited or not taken at all. That they were not taken is clearly seen in the subsequent permission given to allow drilling of oil and gas wells





in the Wyoming ferret area. One can appreciate the need for government organizations such as the BLM to have firm evidence on which to act.[37] Since most land is likely to have alternative uses, areas which could be set aside for ferrets could be used effectively in other ways if there are no ferrets on them. It is not unlikely, furthermore, that the proponents of other uses will exert pressure on government agencies. Hence there may be a bias toward assuming that ferrets are not present unless there is strong evidence to the contrary. One possible consequence of this bias toward non-existence may be that rare species like the ferret will disappear. Since these species are difficult to detect, routine methods may not be sufficient to detect them. And the bureaucracy's decision needs may militate against using more adequate science which could detect them. At some point there has to be a decision that further search will be foregone. At this point it would be honest to say simply that there are no resources for further search. Yet the agency still has to present a rationale for stopping searches which will satisfy wildlife lobbies and the public. Hence the politically astute agency will fortify what is essentially a pragmatic decision with an elaborate scientific rhetoric.

## The Rhetoric of Denial

When an organization wants to deny the existence of a rare or anomalous phenomenon, it is likely to deploy a *rhetoric of denial*.[38] This rhetoric is a set of arguments to show that current lack of conclusive evidence of a phenomenon's existence is proof of its non-existence. Absence of evidence thus becomes evidence of absence. The rhetoric of denial does two things: (1) it transforms a situation which is ambiguous into one which is definite and about which, therefore, defensible decisions can be made; and (2) it deflects attention from the phenomenon in question by offering a seemingly conclusive set of arguments that it does not exist. A typical set of such arguments would go something like this:

1. By its very nature, the alleged phenomenon could not take place.
2. The people who report the phenomenon cannot be trusted because they are incompetent or dishonest.
3. Observations will be reported, and since the number of reports is small, this shows that there are few observations.
4. The phenomenon would be observed, if it were real, as a by-product of routine scientific or other activities.

5. If physical evidence of the phenomenon were available, it would be unambiguous.
6. Specialists in the relevant area of expertise would have received useful reports, if there were any, of the phenomenon.

While only some of these arguments are likely to be used in a given case, one must recognize that, *if true*, the sum of these arguments would make a very damaging case against the existence of a given hypothetical phenomenon. And these arguments often have strong face validity. But are they valid?

These arguments, with the exception of the first, are sociological. That is, they constitute assertions about what people would do in a given circumstance in terms of behaviour or cognition. Since they constitute sociological assertions, we must check their validity through sociological study. Previous studies of these *social intelligence* processes by one of the authors (RW) have in fact shown many of the assertions to be false in relation to anomalous events, both in the case of hypothetical anomalies and those generally accepted by the scientific community. Contrary to the rhetoric of denial, these studies have shown:

1. Often observers of the phenomenon are competent, but either they do not report or their reports are not transmitted further.[39]
2. The number of reported cases may bear little or no relation to the number of observations; this is particularly true for the scientific literature.[40]
3. The intersection of the phenomenon with potential observers may be overestimated, thus providing much less coverage than believed, and fewer potential observations.[41]
4. The phenomenon may well be observed during routine scientific or other activities. Such observations are not necessarily reported, however.[42]
5. The interpretation of physical evidence is often either faulty or indeterminate, and sometimes both. Only after the phenomenon is admitted to exist, and its contours clearly defined, are such examinations likely to be fully competent.[43]
6. The existence of useful observations or even reports in the scientific literature does not necessarily mean that the relevant experts – or, even more to the point, those momentarily interested – will be able to retrieve them. The very process of reporting and publication may encourage authors to disguise the anomalous nature of the reports.[44]





These observations give one only the most general idea of the relation between human behaviour as represented in the rhetoric of denial and human behaviour in real life. This difference is important, since in any actual situation people are likely to behave in socially strategic ways rather than acting as the ideally dispassionate information-processors which the rhetoric of denial presents them to be. The practical result is that *the amount and quality of spontaneously transmitted information about rare or anomalous events may bear little relation to the prevalence of such events.*[45]

A major influence on the reporting process is the credibility of the events in question. If the event is considered anomalous, reporting it may require an act of considerable courage, and such reports are usually received with less than enthusiasm. Fear of ridicule is a powerful force, and should not be underestimated, despite public relations statements to the contrary.[46] Members of the scientific community are very concerned about ridicule. Reports of rare events, though less heavily sanctioned than reports of implausible anomalies (such as UFOs), are nevertheless subject to similar difficulties. Above all, the consequence of making a report for the individual should be taken into consideration in weighing the relative abundance of reports. Potential reporters are more likely to be concerned about the personal consequences of a report than about the needs of science. Negative attitudes by official agencies about the existence of certain phenomena may thus become a self-fulfilling prophecy.

We have seen how such attitudes can affect evaluation of ferret presence. The agencies argue that ferret presence would definitely be shown by certain kinds of indicators. But as we have seen, the absence of these indicators is not necessarily proof of ferret absence. Only through the rhetoric of denial are such missing indicators turned into positive evidence that no ferrets are present.[47] Lack of physical hard evidence, although not definitive, becomes a major argument against ferret presence. The existence of soft evidence is rationalized as untrustworthy. Pre-determined conclusions are thus bolstered by inadequate testing.

Thus far we have seen how the rhetoric of denial can be used to neutralize anecdotal or episodic ferret evidence. But the same arguments can be creatively extended to cover more systematic data-gathering efforts by NGOs. If anecdotal evidence can be ignored because it can be made to seem faulty or insignificant, similar strategies can be employed to degrade the legitimacy of sources of information uncontrolled by the establishment. If these strategies are successful, the only legitimate sources of information about ferrets will then be under governmental control.

The government, however, is not merely a gatherer of information. It must also make decisions about the use of land, which include consideration of other uses besides rare species preservation. Since commercial uses for land are likely to be represented by strong pressure groups, the roles of fact-finder and land manager may conflict. The potential for scientific opinion to be slanted in the direction of commercial objectives is likely to be strong. Hence the need for pluralistic sources of information to provide checks and balances. The need for such pluralistic sources of information, and the agencies' attempts to eliminate them, are most evident in the question of what constitutes an adequate ferret survey.

## The Politics of Scientific Competence

A most important issue in any scientific discipline is what constitutes 'competent' research. This is very much the case with ferret surveys. The issue of competence involves: (1) who is allowed or sponsored to do ferret research and (2) what methods are considered adequate for demonstrating ferret presence or absence on a prairie dog colony. One of the most interesting developments in ferret conservation during the years of this study was a strong tendency towards government-only surveys using 'officially approved' methods whose effectiveness some private groups questioned.[48] Some of the officially approved methods have since been shown to be ineffective in locating ferrets.[49]

Consider the question of staffing. Who is to carry out surveys on ferrets? In 1978, 1979, 1980 and 1981 the government contracted with itself to carry out ferret surveys.[50] The BLM contracted, during these years, for USFWS to carry out ferret surveys on seven large proposed coal lease areas in Wyoming. Such intra-governmental contracting is strongly discouraged by federal law (for example, the 1932 Economy Act) and is supposed to be used *only* in case of dire national need or insufficient alternatives. Neither situation pertained in this instance. Yet Memoranda of Understanding between the two federal agencies established such a contract for four consecutive years, providing hundreds of thousands of dollars to the USFWS, in spite of the existence of NGOs willing and able to undertake the task using ostensibly more rigorous survey methods at lower costs. Part of the rhetoric which established the legitimacy of this agency procedure included suggestions that NGOs were not competent to carry out the research, since they were 'experimenting with new unsupportable survey techniques and are inexperienced in applying acceptable survey standards'.[51] This meant, of course, that





government agencies had no alternative but to carry out the research themselves. One suspects that the premise ('no one else is competent to do it') may well, however, have been guided by the conclusion ('the government must do it'). It is worth noting that both other government agencies and private clients, such as pipeline companies, were thus discouraged from considering NGO surveys. Legal approval of NGO surveys would have to come ultimately from the same agencies which are claiming that *they* had the sole competence to carry them out. Such agency strategies would seem to be directed at securing control of their operating domain. This situation existed in spite of evidence that NGOs were cost competitive and as efficient as government surveys, and possessed or exceeded the qualification necessary for the job.

The economics of NGOs are fragile. The contracting job for an annual ferret survey is worth over $100,000 in Wyoming. Government pre-emption of the survey role meant that NGOs were forced to turn to private funding, which they were fortunate enough to get. Decisions about 'competence' by an agency which controls much of the funding may thus represent not only a land management decision – which is perfectly legitimate – but also a decision as to who can practise certain kinds of science. Without the cultivation of outside expertise, governmental science decisions in this area could be inappropriately shaped by pressures for certain land management policies, since the same agency would be responsible for both. It is not healthy for science to be so closely tied into other vested interests.

The issue sharpens when the methods approved for ferret surveys by the agencies are considered in detail, since there is some evidence that they are not the best available. Government survey methods were developed from experience with ferrets living on black-tailed prairie dog (*C. ludovicianus*) colonies in South Dakota where the mean colony size was twenty-one acres in high-visibility shortgrass prairie. Even in this situation, ferrets were hard to spot. The Hillman-Linder team reported that surveillance of one thirty-five acre colony, known to harbour a ferret, yielded only two observations of the animal in twenty-three days. Furthermore, studies in South Dakota showed that ferrets might use a prairie dog colony one year and skip it the next.[52] In seventeen colonies on which ferrets were observed from 1964 to 1974 (eleven years, or 187 'prairie dog colony years'), seventy-seven ferrets, including nine litters, were seen in only thirty (16 percent) of these colony years.[53]

By comparison, the Wyoming situation was even worse. The average prairie dog colony size was 200 acres, eleven times greater than for

any of the colonies in Mellette County, South Dakota. Visibility in Wyoming colonies is often restricted by dense sagebrush and broken terrain. It is unknown whether the government-recommended minimum search was adequate to reveal a litter in Wyoming, let alone a single ferret.[54]

At night, ferrets are located by 'spotlighting' them and using the light reflected from their eyes to provide targets for visual observation and photographs. The USFWS recommended spotlighting only colonies with signs suggestive of ferret presence (for example, those exhibiting large numbers of plugged prairie dog burrows or typical diggings). Using this advice would significantly reduce chances of finding a ferret. Characteristic ferret sign is nearly non-existent at certain seasons and is sometimes difficult to recognize. What very few signs ferrets leave in black-tailed colonies are often destroyed by the prairie dogs within two hours after sunrise. Until study of the Wyoming ferret population, it was not known whether ferrets living on white-tailed prairie dog (*C. leucurus*) colonies made any signs at all. Findings from a study of the Wyoming population now show that ferret sign is virtually non-existent in ferret-occupied areas in the summer and early fall – the survey season called for by the USFWS regulations!

The USFWS recommended stationary nocturnal surveys with the spotlight on 50 percent of the time and a total nightly effort of only one and one-half hours per person. This appeared to work in South Dakota, where the prairie dog colonies were small. But on large, visibly obstructed colonies, such as many of those in western Wyoming, moving surveys with the spotlight on 100 percent of the time provides the greatest temporal and spatial coverage. These latter methods required two people for a minimum of five hours each night. Such techniques, developed by the NGOs, were precisely the 'new and unsupportable' methods to which the agencies objected! After the Wyoming ferrets were discovered in 1981, the NGO survey methods were overwhelmingly proven superior (via normal science) to those recommended by the USFWS.[55] Here the government experience developed from one situation was arbitrarily and rigidly applied to a new one, despite growing evidence produced by NGO research that its efficiency was low. Yet departure from these official guidelines was not permitted by the agencies.

The official guidelines of the USFWS suggested that a brief survey would be sufficient to determine ferret presence. Yet both the South Dakota experience and a computer-based energy balance model of ferret prey requirements indicated that a once-over survey would very likely miss ferrets occurring in low density colonies.[56] This in turn would





contribute to the rhetoric of denial: 'Searches do not detect ferrets. Therefore there are no ferrets.' A situation like this, of course, is a typical problem in statistical decision theory, with possibilities for both false negatives and false positives. The danger of a false negative is likely to be enhanced, however, when those carrying out the survey have conflicting interests, such as putting the land to other uses for oil, gas or coal production.

## Calculative and Generative Rationalities

The Endangered Species Act, under whose authority the US Fish and Wildlife Service manages ferrets and other threatened species, envisions the use of scientific methods for wildlife management.[57] Yet how such methods are to be applied is left to the agency's discretion. We are faced here with a grim reality: a species can be 'managed' into extinction simply by the use of routine methods. From their use of a rhetoric of denial, it would seem that USFWS, BLM and WGF desire closure of inquiry on the continued existence of ferrets. Yet, in another sense, USFWS is acting to 'rationalize' the problem – that is, to make it certain and calculable. But is this the only kind of 'rationality' that can be applied to the problem? We think not. The question here hinges on what we consider 'rational'. Let us examine the question in a bit more detail.

There are two ways of defining 'rational' behaviour for organizations.[58] On the one hand, action that is rational could be defined as that whose consequences achieve the results one wants in a predictable way. Such action would be easy to understand or defend. For Max Weber rational organizations were calculable in just this manner. This *calculative* rationality, Weber felt, was the defining characteristic of modern organization, which for him meant bureaucracy.[59] In a system like this, past experience and experiment would provide a basis for actions in the present. The certainty and calculability come from reliance on these tried and true principles. Ideally, the organization would act with such certainty and regularity that it would resemble an automaton. Weber felt that this kind of calculative rationality was what made Western society scientific as well.[60] Yet it is obvious that such a view of science reflects nineteenth-century German reality better than it does the history of science generally. Today, even conservative philosophers of science stress science's calculative rationality less than its ability to correct itself.[61] The ethic of progress in contemporary philosophy of science was no part of Weber's conception of rationality. Therefore, it is obvious

that if we are to see science as a rational enterprise, we must be able to envision a different kind of rationality. This other kind of rationality, furthermore, must be based on different principles.

We call this second kind of rationality '*generative*', for its value rests with its ability to generate critique and innovation – in short, to provide for self-renewal of the system. Its distinguishing feature is that it makes efficient use of the cognitive resources of the system. Just as a bureaucracy represents the epitome of the calculatively rational system, the epitome of the generatively rational system would be an intellectual marketplace. Such a marketplace would encourage a variety of different approaches from which increasingly better solutions would evolve. Good ideas would be recognized without regard to their place of origin in the system.[62] The system's rationality, then, would depend on its ability to generate new ideas and choose sensibly between them.

We could characterize the major concerns of these two systems as 'optimization' and 'progress' respectively. A calculative system is trying to do the best it can with the information in hand. A generative system is trying to develop information so that it can make a better decision than any that are currently possible.

Each form of rationality, in turn, is likely to shape a characteristic organizational structure. Calculative organizations will pick structures which are clear-cut, 'mechanistic' and tend towards bureaucracy. Generative organizations will develop in a looser and more pluralistic way. Such designs are often called 'organic' or 'parallel', but whatever one calls them, they have been in the management literature for a long time.[63]

What are the implications of these kinds of rationality for the design of scientific organizations? We think these implications are profound. If science is to remain generative, this must be reflected in the design of either individual organizations or the organizational field in which scientific institutions operate. To turn the operation of science over to a single bureaucracy is virtually to guarantee that it will move towards calculative rationality, and towards a style of inquiry that is less creative.

In regard to the field of science as a whole, such a situation seems unlikely. There are too many scientific institutions and the pressures of competition between different scientific institutions is too great. Yet in specific disciplines, individual institutions and government agencies, such creeping bureaucratization does occur. In any field of endeavour, those who have been successful are likely to be placed in situations of power. When these positions become relatively stable and highly linked, then we talk about the existence of an 'establishment'.[64] An establishment



can also come about through the development of government agencies which move towards control of a given domain of inquiry. Any establishment is likely to justify its power by reference to calculative rationality – the results of past effort, experience and, above all, success.

Government agencies are especially likely to develop a calculative approach because of the political environment in which they operate. Bureaucracies are strongly aware of the need to defend their actions before legislative committees or outside interest groups. Calculative rationality is by nature a defensive rhetoric, oriented towards the past. Actions taken in accord with past principles are much more defensible than those based on principles which are novel or experimental. We can expect bureaucracies to present science as a relatively clear-cut activity, whose judgements follow proven procedures. For every question there is an answer, for every problem a solution. Such calculatively rational organizations are easy to defend to legislative committees, since everything is done methodically, according to the book, and ideally ambiguity or uncertainty nowhere exist in the system.[65]

Calculative rationality solves another pressing problem for the bureaucratic organization: lack of resources. Few bureaucratic organizations are given enough resources to carry out their jobs well. There is usually a mismatch between what they are expected to do and what they actually can do. Hence they are always potentially in a situation where they can be criticized for failure to do what they are supposed to do. One response to this situation is to complain about the resources they have been given to do the job. And in fact agencies frequently respond to criticism in just this manner. Yet this excuse is usually fortified by additional rationalizations. The bureaucrat is always tempted to give some other reason why he or she did only $X$ instead of $X+Y$, even though the actual reason may have been that there were enough resources only to do $X$.[66] One motivation for the more devious response may be avoiding the appearance of incompetence and related threats of other agencies invading one's domain; to confess inability to do a job is to encourage others to take it over. A second response to inadequate resources is to prioritize tasks, doing first those tasks for which an algorithm exists. The more complicated tasks are left for later. Species *recovery*, unlike species *protection* is a hard task to programme. Thus USFWS was more likely to make sure that no one wiped out an evident ferret population than it was to help the population to recover from declining numbers.[67]

We might observe here another aspect of calculative systems: their reaction to failure. Since calculative systems are likely to be centralized,

and failures might call into question the establishment's actions, it is important for these systems to deflect awareness that the structure is not functioning well. Systematic dishonesty may well result from such failures, leading to bizarre situations where 'we were successful even though the patient died'.[68] Generative systems, by contrast, are more likely to use failure as an opportunity for self-examination.

Just as calculative rationality comes naturally to those defending an establishment – like Mannheim's 'ideology' – generative rationality is a natural rhetoric for its attackers.[69] If one can show an accumulation of defects in the current system; that new approaches offer promise; that creativity and valuable ideas are being stifled by those in power; that decisions are being made by too few people, then one creates a case for limiting the power of the establishment. Challengers of an establishment must usually base their claims on approaches that are still in the experimental stage, although to them personal experience may be indisputable proof. Challengers base their approach on what it is possible for the system to do, not what has been done.[70] Yet these are always possibilities, not yet realities. To believe in them requires imagination and faith. The challengers have this imagination and faith, this ability to project into the future to see the results of their ideas.

Another likely feature of organizational challengers is stress on pluralistic decision-making. In contrast to the official agencies, who are trying to consolidate decisions into a few hands, challengers are likely to want to open up the process by which decisions are made to include other groups, such as themselves. Instead of the predominantly calculative emphasis of the establishment, challengers are likely to place a premium on discussion, debate, consideration of alternatives and moderated conflict. This process, they believe, will help generate the facts and ideas on which better policies can be based.[71] But it is also likely to make more complicated the job of agencies which are seeking closure.

What happened to ferrets from 1974 to 1981, then, was not just a conflict over facts, but one between contrasting logics, oriented respectively towards calculative or generative approaches. The agencies naturally adopted a calculative approach, since it stresses that one can apply routine tests to make decisions. There is always, according to this philosophy, a straightforward answer which proceeds from use of the standard procedures. One rounds up the usual suspects and they supply the answers. The NGOs, however, argued for a more generative approach, one that would not depend so closely on standard methods. Their approach would depend on ingenuity in developing novel methods *that would protect the species if it was present*. There is never a guarantee,





of course, that use of ingenuity will produce such methods. But if standard methods were not sufficient to detect ferrets, it is only through generative approaches that ferrets can be detected and protected.

**Recent Developments**

Since discovery of the critically endangered ferrets in north-western Wyoming in late 1981, the coalition of organizations involved in monitoring ferrets has steadily become more dominated by governmental bureaucracies. The tactics used by official agencies in ferret protection and recovery have slowly decreased the circle of organizations which can legitimately take action in their domain. Among these tactics are increasing regulation and attacks on non-official approaches as illegitimate. Since the agencies' missions require intensive use of scientific talent, these actions can be described in technical language which disguises their essentially political intent. Individuals and organizations favouring different approaches are slowly being eliminated or co-opted, their ideas and methods characterized as 'illegitimate' or 'inappropriate' by the agencies. As the domain becomes largely dominated by government agencies the chance for intelligent outside critique or innovation is reduced. This elimination of critics and variant approaches results in a net loss of generativity for the field as a whole. As the species vanishes, the bureaucracy itself becomes more rigid and unyielding. The agencies' actions have significantly limited the problem-solving approaches of the coalition to a calculative rationality directed by the government. The impact of this restriction of capability has been serious. Important signals have been missed and opportunities have passed without being acted upon. Among the effects:

1. In autumn 1984, 129 ferrets were found of which 75 percent were expected to die off over the winter. This provided an opportunity to transplant at least ten to twelve ferrets to other favourable habitat sites via a captive breeding programme. Nothing was done to act on this opportunity.[72]
2. There was a consistent rejection of data from NGO field teams during the summer of 1985. These data documented the decline of the ferret population in the wild, thus providing the only feedback on the government's calculatively-oriented programme. The emerging population collapse (due to canine distemper and Sylvatic plague) was identified early in the summer and duly reported to the government

agencies by the field team. These findings were met with the rhetoric of denial – in essence it was argued that there were no problems with ferrets in the wild, only with the field team's methods.[73] It is not clear whether refusal to accept these data was due to simple disbelief or more complex motives. When data have become a political issue, it becomes more difficult to disentangle actions and motives. In any case, the rhetoric prevented any action to save the declining ferret population until by October it dropped to about fifteen individuals. By then nearly all options to save ferrets in the wild had vanished. 3. In the late summer and early fall of 1985, Wyoming Game and Fish captured six ferrets; they were stored in the same room. Two were infected with canine distemper. The rest soon caught it and died. In November six more were captured and are now being kept in strict isolation.[74] In the summer of 1986, four adult wild ferrets remained, and these included two females which produced ten young. Eleven of these wild ferrets have been added to the captive population and only one ferret is known to have survived in the wild. The future of the species is in question.

One could wish that the ferret case was unique, but it is not. Similar problems have brought the grizzly bear, one of America's most popular mammals, near extinction in Yellowstone Park.[75] As in the ferret case the government continued doggedly to implement and defend approaches that were demonstrably not working. At the same time it spent large amounts of energy neutralizing its critics. The California condor is also nearly extinct, partly as a result of government policies.[76]

## Conclusion

Preserving endangered species is an important social task. It is a task that requires a maximum of creativity and skilful management, given the rapidly changing conditions and the precarious situations of the species themselves. Yet we have given this task to organizations whose very nature tends to militate against the exercise of such creativity and skill. The nature of bureaucracies is to develop a highly predictable environment, and this calculatively rational approach often stifles novel approaches. For many government bureaucracies, calculative rationality has become such a reflex that generative approaches actually seem dangerous. Rather than carefully examining the possibilities generated by groups external to the bureaucracy, it seems easier, safer and cheaper





simply to dismiss them with the use of a rhetoric of denial. The same tactics are used to dismiss observations or data which conflict with the organizations' dominant approach.

At a time when the Endangered Species Act is itself in danger,[77] the very grave results of calculative rationality in the ferret case are worth pondering. They illustrate how bureaucratic organizations, acting in an ostensibly 'rational' way, may procrastinate or prevent effective action on important environmental issues. This incapacity, furthermore, appears to be due to factors internal to the organization. This case study suggests serious problems with our society's dependence on such bureaucracies to manage endangered wildlife and natural resources. If the last thirteen years of US governmental actions regarding ferrets is typical, then it would appear that such agencies have severe limitations in their mission to save endangered species.[78]

Any establishment, if unchecked, tends to become dogmatic. Since our society is virtually dependent on scientific organizations to monitor and manage our environment, we cannot be indifferent to such faults. It would be most desirable to get such organizations to be generative internally, but this is probably asking too much of bureaucracies. An important second best alternative is trying to ensure that they operate in a generative environment, and are subjected to external checks and balances.[79] Only if we ensure adequate external critique and innovation are they likely to remain alert and able. Otherwise the conceptual grid through which we view the environment may well become a set of blinders.

## • NOTES

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1. The quotation is from E. F. F. Chladni, *Ueber Feuermeteore* (Vienna: J. B. Heubner, 1819), 4. More generally, see R. Westrum, 'Science and Social Intelligence About Anomalies: The Case of Meteorites', *Social Studies of Science*, Vol. 8 (1978), 461–93.

2. The very useful concept of the 'enacted environment' is included in K. Weick, *The Social Psychology of Organizing* (Reading, Mass.: Addison-Wesley, 2nd edn, 1979), 147–70.

3. The influence of wishes on perception is a theme that runs through Freud's writings, but the selection we have found most helpful is 'The Question of a Weltenschauung', in his *Complete Introductory Lectures in Psychoanalysis* (New York: W. W. Norton, 1966), 622–46. See also I. Janis, *Victims of Groupthink* (Boston, Mass.: Houghton Mifflin, 1972).

4. The idea of an 'inquiring system' comes from C. W. Churchman, *The Design of Inquiring Systems* (New York: Basic Books, 1971).

5. R. Michels, *Political Parties* (New York: Collier Books, 1962). See also J. P. Roche and S. Sachs, 'The Bureaucrat and the Enthusiast: An Exploration of the Leadership of Social Movements', *Western Political Quarterly*, Vol. 8, No. 2 (June 1965), 248–61.

6. This kind of 'inappropriate influence' was a major concern of the earlier American sociologists of science, such as Robert Merton and Bernard Barber. A recent review is B. K. and M. T. Kilbourne (eds), *The Dark Side of Science* (San Francisco, Calif.: Pacific Division of the American Association for the Advancement of Science, 1983). See also D. Robbins and R. Johnston, 'The Role of Cognitive and Occupational Differentiation in Scientific Controversies', *Social Studies of Science*, Vol. 6, Nos 3 & 4 (September 1976), 349–68; and B. Gillespie, D. Eva and Johnston, 'Carcinogenic Risk Assessment in the United States and Great Britain: The Case of Aldrin/Dieldrin', ibid., Vol. 9, No. 3 (August 1979), 265–301.

7. See the special number of this journal, edited by Susan Cozzens, devoted to 'Funding and Knowledge Growth': *Social Studies of Science*, Vol. 16, No. 1 (February 1986).

8. M. Ruess, 'Andrew Humphreys and the Development of Hydraulic Engineering: Politics and Technology in the Army Corps of Engineers, 1850–1950', *Technology and Culture*, Vol. 26, No. 1 (January 1985) 1–33; see also A. Morgan, *Dams and Other Disasters: A Century of the Army Corps of Engineers in Civil Works* (Boston, Mass.: Porter Sargent, 1971).

9. L. Fleck, *Genesis and Development of a Scientific Fact*, translated by F. Bradley and T. J. Trenn (Chicago: The University of Chicago Press, 1979).

10. T. Kuhn, *The Structure of Scientific Revolutions* (Chicago: The University of Chicago Press, 1962).

11. F. D. Adams, *The Birth and Development of the Geological Sciences* (New York: Dover, 1954), 210–49.

12. M. J. Nye, 'N-Rays: An Episode in the History and Psychology of Science', *Historical Studies in the Physical Sciences*, Vol. 11, No. 1 (1980), 125–56; F. Franks, *Polywater* (Cambridge, Mass.: MIT Press, 1981); P. Guerin, *Planetes et Satellites* (Paris: Larousse, 1967), 168.

13. R. Westrum, 'Social Intelligence About Hidden Events: Its Significance for Scientific Research and Social Policy', *Knowledge: Creation, Diffusion and Utilization*, Vol. 3, No. 3 (March 1982), 381–400.

14. Personal communication to R. Westrum from E. Elmer, 30 October 1981. The author is working on an expanded treatment of the history of the 'battered child syndrome' concept.

15. W. N. Charman, 'Ball Lightning', *Physics Reports*, Vol. 54 (1979), 261–306.

16. See Westrum, op. cit. note 13, 389–90.

17. It is extremely probable, in fact, that the pathologies cited here are the exception rather than the rule, although the processes involved are merely extensions of ordinary reasoning and perceptual processes.

18. See for instance S. Toulmin, *Human Understanding* (Princeton, NJ: Princeton University Press, Vol. 1, 1972).

19. One discussion of these issues is R. Westrum, 'Crypto-Science and Social Intelligence About Anomalies', *Zetetic Scholar*, No. 10 (1982), 89–102.



20. Unfortunately, no one has brought together the various studies dealing with these norms and their enforcement; but see R. Westrum, 'The Psychology of Interested Science', *Social Psychology of Science* (March 1985), 4–5.

21. Joyce Egginton, *The Poisoning of Michigan* (New York: W. W. Norton, 1980).

22. See other instances in Kilbourne and Kilbourne, op. cit. note 6.

23. On 'dynamic conservatism', see D. Schon, *Beyond the Stable State* (New York: W. W. Norton, 1973).

24. F. Graham, *Since Silent Spring* (Greenwich, Conn.: Fawcett, 1970), 59–76.

25. The theory of intellectual establishments is undeveloped, and might well become an important task for the sociology of science. In the meantime, one can consult N. Elias, H. Martins and R. Whitley (eds), *Scientific Establishments and Hierarchies* (Dordrecht: D. Reidel, 1982).See also J. Ben-David and A. Zloczower, 'Universities and Academic Systems in Modern Societies', *European Journal of Sociology*, Vol. 3, No. 1 (1962), 45–84.

26. A stalemate can result, however, if the opposition and the establishment cannot cooperate. See, for instance, M. Crawford, 'Condor Recovery Effort Hurt by Strategy Debate', *Science*, Vol. 231 (17 January 1986), 213–14.

27. See R. R. Henderson, P. F. Springer and R. Adrian, *The Black-Footed Ferret in South Dakota*, South Dakota Game, Fish, and Parks Technical Bulletin No. 4, 1–37; and T. Clark, 'The Black-Footed Ferret', *Oryx*, Vol. 13 (1976), 275–80.

28. See R. C. Erickson, 'Some Black-Footed Ferret Research Needs', in C. N. Hillman and R. L. Linder (eds), *Black-Footed Ferret and Prairie Dog Workshop* (Brookings, SD: South Dakota State University, 1973), 153–63; and T. W. Clark, *Black-Footed Ferret Studies in Wyoming* (National Geographic Research Reports, 1977 Projects, 1985), 223–31.

29. This attitude was evident in many talks with land management personnel at the time (TWC).

30. For example, one of the authors (TWC) in 'Current Status of the Black-Footed Ferret in Wyoming', *Journal of Wildlife Management*, Vol. 42 (1978), 128–34, stated (at 132): 'New techniques must be developed to locate and study ferrets.' He went on to list avenues for research: 'These might include scent lures, live trapping, night photography stations, and the use of dogs.'

31. This kind of reassessment is discussed in D. Schon, *The Reflective Practitioner: How Professionals Think in Action* (New York: Basic Books, 1982).

32. W. G. Jobman and M. E. Anderson, 'Potential Present Range of the Black-Footed Ferret as of January 1, 1981' (Pierre, SD: unpublished report of the US Fish and Wildlife Service, 1981), 65pp.

33. Black-footed ferret search procedures used by non-governmental organizations were outlined in T. W. Clark and T. M. Campbell, III, 'Suggested Guidelines for Black-Footed Ferret Surveys' (Jackson, Wyoming: Biota Research and Consulting, 1980), 6pp.

34. R. Randall, personal communication (1978) to T. W. Clark.

35. T. W. Clark and T. M. Campbell, III, 'Additional Black-Footed Ferret (*Mustela nigripes*) Reports from Wyoming', *Great Basin Naturalist*, Vol. 41 (1981), 360–61.

36. For example, Dr M. Hornocker reported observing a ferret in Wyoming in 1974. This report is cited in T. W. Clark, 'A Listing of Reports of the Black-Footed Ferret in Wyoming (1851–1977)', *Northwest Science*, Vol. 54 (1980), 47–54.

37. See S. J. Martin and M. H. Schroeder, *Black-Footed Ferret Surveys on Seven Coal Occurrence Areas in Southwestern and South Central Wyoming, June 8–September 25, 1978*, Final Report to the Bureau of Land Management, Wyoming State Office (1979); and S. J. Martin and M. H. Schroeder, *Black-Footed Ferret Surveys on Seven Coal Occurrence Areas in Wyoming, February–September 1979*, Final Report to Bureau of Land Management, Wyoming State Office (1980).

38. This concept will be more fully developed in a book being written by R. Westrum, *Hidden Events: Social Intelligence About Anomalies*.

39. Westrum, op. cit. note 13.

40. Ibid.

41. As is the case, for instance, with small nocturnal mammals generally. The Cyclone trap and its successors enormously increased our knowledge of these shy animals. See A. G. Wetmore, G. S. Miller and J. W. Gridley, *Warm-Blooded Vertebrates* (New York: Smithsonian Institution Series, 1934), 237–40.

42. Westrum, op. cit. note 13.

43. Westrum, op. cit. note 1, at 464.

44. This was the case with the first twentieth-century article on what later became known as the 'battered child syndrome'. The paper, written by John Caffey, was couched in such ambiguous terms that many of Caffey's readers failed to read between the lines: R. Westrum, 'The Battered Child Syndrome: the Social Relations of a Medical Concept', typescript. See also Westrum, op. cit. note 13, at 386. Both of the authors of this paper can testify from personal experience about the difficulty of getting material on anomalous observations published in the scientific literature.

45. The hesitancy of public and professionals to report possible sightings of endangered species is limited neither to the ferret nor the United States. Accounts of the efforts to find the thylacine 'tiger' in Australia demonstrate a similar set of reporting problems. See, for instance, S. J. Smith, 'The Tasmanian Tiger – 1980', unpublished manuscript prepared for the National Parks and Wildlife Service, Tasmania (PO Box 210, Sandy Bar, Tasmania); D. E. Rounsevell and S. J. Smith, 'Recent Sightings of the Thylacine (Marsupialia, Thylacinidae) in Tasmania', in M. Archer (ed.), *Carnivorous Marsupials* (Sydney, NSW: Royal Zoological Society of New South Wales, 1982), 233–36; and M. Smith, 'Review of the Thylacine (Marsupialia, Thylacinidae)', in Archer, op. cit., 237–53.

46. R. Westrum, op. cit. note 19, footnote 3.

47. As we have observed, the major function of a rhetoric of denial is to deflect attention from a subject in question. Some earlier observations on the use of such rhetorics were made by Charles Fort in his works on anomalous natural phenomena. See *The Books of Charles Fort* (New York: Henry Holt, 1941). It might be observed that in the process of deploying such arguments, often very questionable assertions are made which do not bear detailed scrutiny.

48. US Fish and Wildlife Service, *Black-Footed Ferret Survey Guidelines* (Denver, Colorado: mimeographed draft, 1981).

49. T. W. Clark, L. Richardson, D. Casey, T. M. Campbell, III and S. C. Forrest, 'Seasonality of Black-Footed Ferret Diggings and Prairie Dog Burrow Plugging', *Journal of Wildlife Management*, Vol. 48 (1984), 1441–44. The article concludes

> Draft ferret survey guidelines [cited in note 37 above]... adopted by federal and state regulatory agencies have required that surveys be conducted during summer and early fall when ferret sign was virtually nonexistent in our study area. Ferrets could be readily spotlighted on our study area during this period, but the guidelines allowed spotlighting only when ferret sign was located first. The locus of past ferret sign searches has been diggings of the 'classic' burrow type and burrow pluggings. Our results showed that 'Classic' diggings composed only 5% of the ferret diggings in our 2-year sample on white-tailed prairie dog colonies. Furthermore, we were able to recognize many partially eroded diggings that would have been unidentifiable only because we watched them deteriorate over time. Low burrow plugging rates in response to ferret presence on white-tailed prairie dog colonies showed burrow plugging to be of limited value in determining ferret presence. (1443)

Another article, T. M. Campbell, III, D. Biggins, S. C. Forrest and T. W. Clark, 'Spotlighting as a Method to Locate and Study Black-Footed Ferrets' (in *Proceedings of the Black-Footed Ferret Workshop*, Laramie, Wyoming, 18–19 September 1984), concluded that the US Fish and Wildlife Service survey guidelines 'intentionally or not... downplayed the significance of spotlighting as a ferret survey method by recommending that [it be used on] 3 consecutive nights on colonies *only* [emphasis in original] when ferret sign was found.'

50. See note 37.

51. 'Memorandum of Understanding Between the Bureau of Land Management and Fish and Wildlife Service BLM No. WY-26M' (12 April 1978), 1. The Memorandum also states that 'nearly all those biologists who are qualified [to do ferret surveys] are within the FWS' (2).

52. C. N. Hillman and R. L. Linder, 'The Black-Footed Ferret', in Hillman and Linder, *Black-Footed Ferret and Prairie Dog Workshop* (Brookings, SD: South Dakota State University, 1973), 10–23.

53. C. N. Hillman, personal communication (1978) to T. W. Clark.

54. Clark, op. cit. note 30.

55. T. W. Clark, T. M. Campbell, III, M. H. Shroeder and L. Richardson, *Handbook of Methods For Locating Black-Footed Ferrets*, Wyoming Technical Bulletin No. 1 (Cheyenne, Wyoming: Wyoming Bureau of Land Management, 1984), 55pp.

56. M. R. Stromberg, R. L. Rayburn and T. W. Clark, 'Black-Footed Ferret Prey Requirements: An Energy Balance Estimate', *Journal of Wildlife Management*, Vol. 47 (1981), 67–73; and R. A. Powell, T. W. Clark, L. Richardson and S. C. Forrest, 'Black-Footed Ferret (*Mustela nigripes*) Energy Expenditures and Prey Requirements', *Biological Conservation*, in press.

57. S. L. Yaffee, *Prohibitive Policy: Implementing the Federal Endangered Species Act* (Cambridge, Mass.: MIT Press, 1982). Yaffee's book documents the extent to which conservatism of the kind discussed here results from classic bureaucratic pressures.

58. This distinction between calculative and generative rationalities will be pursued at greater length in a book upon which one author (RW) is at work. It was suggested by an article written by J. C. Pitt, 'Conceptual Change and Conceptual Tension', *Methodology and Science*, Vol. 14, No. 2 (1981), 132–38. In the article, Pitt speaks of 'reactionary' and 'progressive' rationalities. Similar distinctions are present in the Model I versus Model II problem-solving systems of C. Argyris and D. A. Schon in their *Organizational Learning* (Reading, Mass.: Addison-Wesley, 1978).

59. See the famous essay on 'Bureaucracy' in H. Gerth and C. W. Mills (eds), *From Max Weber: Essays in Sociology* (Oxford: Oxford University Press, 1958), 196–244; also his 'Politics as a Vocation' in the same volume.

60. See the 'Author's Introduction' in M. Weber, *The Protestant Ethic and the Spirit of Capitalism* (New York: Scribner's, 1958).

61. See, for instance, J. R. Ravetz, *Scientific Knowledge and Its Social Problems* (Harmondsworth, Middx: Penguin, 1973), 181–208; Toulmin, op. cit. note 18, 204–21.

62. There is a good description of such an organizational culture, identified as 'Action', in E. Schein's *Organizational Culture and Leadership* (San Francisco, Calif.: Jossey-Bass, 1985).

63. For contemporary ideas, see T. J. Peters and R. H. Waterman, *In Search of Excellence* (New York: Warner Books, 1984), 200–34; R. M. Kanter, *The Change Masters* (New York: Simon and Schuster, 1983). For earlier versions, see, for instance, J. Leitch, *Man to Man: The Story of Industrial Democracy* (New York: B. C. Forbes, 1919).

64. See note 25.

65. Yaffee, op. cit. note 57, 104–31; L. C. Gawthrop, *Bureaucratic Behavior in the Executive Branch* (New York: Free Press, 1969), 189–213, esp. 191.

66. For instance, the NASA bureaucrats, before the Challenger space shuttle disaster, could have complained that they were given inadequate resources to provide both safety and a frequent launch schedule. Rather than admit that they were given adequate resources, however, they simply cut safety and continued with the launch schedule: P. Boffey, 'Analyst Who Gave Shuttle Warning Faults Gung-Ho, Can-Do Attitude', *New York Times* (14 February 1986), 11. See also R. Pear, 'Senator Says NASA Cut 70% of Staff Checking Quality', *New York Times* (8 May 1986), 1.

67. One wildlife official commented that 'FWS/BLM are primarily interested in Section 7 compliance. We don't go looking for ferrets, we *try* to do something [so that a] development won't *take* BFF or stifle chances for recovery. Recovery seems to be someone else's job, someone with time and money to do it. FWS is too busy with Section 7 compliance to get involved in recovery . . . FWS has to do recovery plans. But, once completed, implementation seems to be lacking motivation and courage.'

68. Thus, in October 1985, Harry Harju of Wyoming Game and Fish, in the face of a disastrous decline of the ferret population: '. . . while refusing to give a reason for the apparent population decline, Harju said, "I know exactly what's going on, and they'll announce it Thursday."' *Billings, Montana Gazette* (23 October 1985).

69. K. Mannheim, *Ideology and Utopia* (New York: Harcourt, Brace and World, n.d.).

70. For one account of a challenger's struggles and strategy, see W. B. McLean, 'The Sidewinder Missile Program', in F. Kast and J. E. Rosenzweig (eds), *Science, Management, and Technology* (New York: McGraw-Hill, 1962), 166–76. He shows his struggle against the rhetoric of denial and how he turned the 'impossibilities' into the design objectives for the system he intended to build. See also J. Fialka, 'Weapon of Choice', *Wall Street Journal* (15 February 1985), 1, 30.

71. Much of the work of Irving Janis is relevant here; op. cit. note 3. See also I. I. Mitroff and R. O. Mason, *Creating a Dialectical Social Science* (Dordrecht: D. Reidel, 1981).

72. The estimate of the situation in 1984 was made in S. C. Forrest, T. W. Clark, L. Richardson and T. M. Campbell, III, in 'Litter Survey and Mark/Recapture of Black-Footed Ferrets at Meeteetse, July–October 1984', unpublished report to Wyoming Game and Fish, etc., 7pp. WGF objections to transporting captive ferrets to other states for breeding are discussed in A. Carr, III, 'Introduction' to special issue of *Great Basin Naturalist Memoirs*, No. 8 (1986) on 'The Blackfooted Ferret', Further reference to this problem and some of its politics is made in 'Ferret Breeding Program Looks Doubtful', *Caspar Star Tribune* (6 May 1985), based on an Associated Press interview with Harry Harju of Wyoming Game and Fish.

73. It is interesting to contrast the stark picture shown by field reports with government suggestions that nothing was wrong. For instance, in a newspaper article dated 14 August, Harry Harju, supervisor of the Wyoming Game and Fish biological services, is quoted as saying: 'It was like the Chicken Little Syndrome . . . Everyone started to panic. But there is no cause for alarm. No one first talked to the [US] fish and wildlife services or the [Wyoming] game and fish . . . We still cannot see these ferrets as being in serious danger.'





M. Ontiveroz, 'State Says Ferret Scare Unnecessary' [newsclipping source unrecorded], 14 August 1985.

74. I. Peterson, 'Six Endangered Ferrets Are Under Strict Guard', *New York Times* (15 December 1985).

75. A. Chase, 'The Grizzly and the Juggernaut: How Enlightened Environmental Theory Has Destroyed the Great Bears of Yellowstone', *Outside*, Vol. 11, No. 1 (January 1986), 29–65. Mr Chase apparently overstepped himself in this article, exaggerating in various places, according to R. Barbee, Superintendent of Yellowstone Park. An annotated copy of Chase's article is available from Mr Barbee at National Park Service, Yellowstone National Park, Wyoming 82190. See also J. Adler, M. Hager and J. Copeland, 'The Fall of the Wild', *Newsweek* (28 July 1986), 52–54.

76. On the condor's survival problems, see M. Crawford, 'The Last Days of the Wild Condor?', *Science*, Vol. 229 (30 August 1985), 844–45.

77. I. Peterson, 'How Much Life is Left in the Endangered Species Act?', *New York Times* (5 January 1986), Section IV.

78. R. M. May, 'The Cautionary Tale of the Black-Footed Ferret', *Nature*, Vol. 320 (6 March 1966), 13–14.

79. Only after we finished the paper did we become aware of the work of the National Research Council's Committee on the Applications of Ecological Theory to Environmental Problems. This committee suggested that ecological interventions ought to be regarded as experiments, and that an overall learning system to track such experiments was needed. See R. Lewin, 'Why Dynamiting Vampire Bats Is Wrong', *Science*, Vol. 232 (4 April 1986), 24–46.

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