A Rejoinder to Perrow

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S cholars respond to others' work in terms of the ways it stimulates their own and, on rarer occasions, how faithful their own work is characterized as a basis for stimulation and/or critique. Charles Perrow is both stimulated and gratified by Sagan's treatment of his work; we are not in regard to ours. Neither Perrow's defence of Sagan's formulations nor the result of its stimulation dissuades us of the views summarized in La Porte's review in this Symposium. Indeed, Perrow's treatment of our work demonstrates a misreading in characterizing it; a misreading he shares with Sagan. Both of them have difficulty in taking as genuine our attempts to specify our intentions.

Perrow supports Sagan's dismissal of the complementarity between the two approaches in the search for contrast, applauding Sagan's contrasting of theory as a test of organizational strategy. But to warrant applause, the comparison should be of theories or hypotheses that claim to be explaining the same or similar phenomena. Working from secondary analysis of organizational structure, behaviour and culture, Sagan (and by implication Perrow) seeks to estimate from them the relative likelihood of 'serious' accidents. He then goes on to attribute to these organizations properties that they claim are representative of HROs, and then infers that the Berkeley group would have drawn contradictory conclusions.

In contrast, the HRO group takes as its starting point the perception by the organization that it is inherently susceptible to accidents, including very serious ones. We then attempted to find out what strategies have been developed by organizations that have demonstrated high levels of performance in the face of the demanding conditions summarized in part by theories such as Normal Accidents Theory (NAT). As we have said repeatedly, that is why we take our work to be complementary to, or an extension of, NAT.

Yet, Perrow recreates in his response the same putative dichotomy between his NAT and what he persists in identifying as 'High Reliability Theory' (HRT); something the Berkeley group has strongly criticised in Sagan's work. Perrow treats our work as if a) it were an alternative, contrary model to the NAT work, and b) it aspires to become a theory of accidents. But despite Perrow's insistent claim to the contrary, the HRO project is pursuing a theory of organizational behaviour under very trying conditions; it is neither a theory of accidents nor an attempt to construct a theory of why or how they occur.

Perrow also criticises HRT for our choice of organizations and settings. But in so doing, he appears to have trapped himself in a contradiction. His original complaints that we are constructing an alternate theory of accidents and draw back from the theoretical implications depends on our organizations having the character of those to which NAT applies. If, as he asserts here, the organizations we study are loosely coupled, not very complex, with little catastrophic potential, and therefore not very interesting, if our research is hardly conclusive with regard to the risky systems NAT worries about, then our work has nothing to do with the systems to which he wishes to apply NAT. If that is the case, the HRO work is not even complementary to NAT, let alone the competing theory that he and Sagan claim it is. Perrow cannot have it both ways.

If this were all that Perrow had done, this would be the sum of our response. It would take much more time and space here to respond to all of his points. We refer the reader to our body of published and forthcoming work. What follows responds to a few of the more salient points of criticism.

In defending his claim, Perrow challenges the HRO work in terms of a scatter of critiques familiar to students of organization, especially those interested in the firm. He obfuscates our work with the invidious term 'B-school', by which he implies that we adhere to a traditional, overly empirical, and largely a theoretical behaviourist approach. But our work is 'evolutionary' rather than behaviourist. Unlike either traditional empirical induction or Perrow's structuralist deductivism, our analysis is expressly dynamic (La Porte and Consolini, 1991); it seeks to explore the evolving relation between

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behaviour and structure as an interactional process (Gaddis, 1992/1993).

Perrow further criticises HRO research for only having examined systems that have had very few (or no) failures, of selecting only on the 'dependent variable' and thus of being 'not sufficiently alert' to these problems of analysis. We have always been sensitive to the point that we have only studied organizations that are not widely judged to be failures.[1] But given finite resources and a demanding agenda of field work, every research group has to pick and choose. Indeed, when Perrow was developing NAT he himself argued for research and conducted studies only on organizations that had failed.

We chose to study organizations that have apparently 'succeeded' on the grounds that (a) there now have been ample studies of organizations that have failed, and (b) almost all of Organization Theory is based on organizations depending largely on trial-and-error learning, which ours clearly wish to limit. While we did not set out to 'test' the standard literature, we took it as a fair representation of organizations that either avoided the challenge of managing under NAT conditions or failed under them.

Perrow further adds to the confusion created by Sagan when he links the work of Wildavsky (1988) with that of the Berkeley group. The HRO studies take as an essential element of the organizations of interest that they live in a task environment of tight technical and/or social coupling and interactive complexity and an organizational environment in which costs of failure are likely to be very high. There are of course many organizations in society that do perform well and reliably in a simple and loosely coupled task environment, for whom the costs of failure are relatively minor. From our theoretical view, these are simply a good bit less interesting. It never was our intent to try to compare systematically trial-and-error systems that have failed with 'HRO' systems that have not. Organization Theory already summarizes much about the patterns associated with trial-and-error and failure. In a rough sense, that work has been pursued for sometime. But we have explicitly stated that one of the problems facing the organizations we have studied is that they avoid trials in fear of errors; indeed, that is one of their more problematic characteristics.

Perrow also seems concerned that we do not explicitly recognize his point that compensating systems may add to the problem by making the system more complex and increasing the opportunities for error. This is a naive reading of our work. What he faults us for is failing to explicitly support his

formulations and to add evidence to his argument, as Sagan attempts to do. We have implicitly (and at times explicitly) stated that we accept its major points; our agenda is to go beyond them, to search for some of the implications for organizational behaviour and evolution.

What the Berkeley group has sought is to provide analytical descriptions about sustained (and successful) efforts to assure consistent performance in systems that include technologies which are hazardous in their design. To put the question again, if it is intrinsically impossible eventually to avoid major failures or significant errors within organizations, what patterns of behaviours appear to substantially limit the experience or incidents of failures *and* simultaneously result in high levels of expected performance?

Had we been chasing a theory of accidents, our questions would have been posed differently, the literature necessarily being more wide ranging. We would have taken into account the extensive work from human factors engineering on the sources of human error as well as on regulated industries (La Porte and Thomas, 1994) that suggests regulatory pressures result in operating compromises and problematic attention to safety. We also would have included other theories of accidents that are grounded more in psychological and human factors analysis than organizational sociology, and have pointed to a somewhat different list of organizational characteristics than those of Perrow (Rasmussen, Duncan and Leplat, 1987; Reason, 1989; Rasmussen, 1990). If comparative analysis is what Sagan was pursuing, he would have done well to examine his organizations not only for the 'resident pathogens' discussed by Reason (1989), but also for potential for negentropic generation of disasters by human actors out of otherwise relatively safe situations (Turner, 1978).

We turn now to several of Perrow's characterizations of our concept of HRO and the way we prosecuted our work, then take up the question of theory test and development. Among items too numerous to fully discuss here, Perrow argues that the Berkeley group: i) holds that operational perfection is attainable; ii) claims HROs are driven primarily by ultra-safety concerns; iii) ignores the complexification in the organization studied that stems from the structural and procedural developments designed to avoid failures and/or limit their consequences; iv) described phenomena that are not really that unusual after all; and v) has naively 'gone native' in its work thereby sullying the results. Along the way, he asserts

that we could not have found what we claim to have observed.

We have addressed the first point in La Porte's contribution to this Symposium, though apparently Perrow has not found our assertions to the contrary persuasive. He continues to insist that we really do believe perfect systems can be accomplished. He offers as evidence La Porte's claim that the HRO group has been learning about 'the degree and character of effort necessary to overcome the inherent limitation of securing consistent, failure free operations'. Perrow (and Sagan) apparently suppose that we think the effort is not so great. But taken in the context of the quote (in La Porte above and Sagan quotes La Porte), it is just the contrary. It is because we have seen how great the effort must be that our pessimism arises.

On point two, Perrow argues that the HRO perspective rests on the assumption that such complex organizations cleave singularly to the goals of safety with only limited attention to productivity. This is consistent with his assertion that we are pursuing a theory of accidents, for that literature puts scant emphasis on it. But this devotion singularly to safety is clearly not the case in any organization known to Perrow, and cannot be imagined theoretically. Therefore, the HRO perspective as theory is significantly wanting. We would certainly have to agree had this been a faithful characterization of our work. But our interests include reliability in readiness, as well as assured production and service in the face of technical change and environmental turbulence. Contrary to Perrow's argument that we believe HROs hold safety as singularly paramount, we have remarked repeatedly on the equal status of both assuring performance in the face of demanding conditions (some of them summarized by NAT) and avoiding operating failures in the process as motivating the organizations we studied.

Perrow goes on to argue (point three) that we find the behaviours and structural patterns we describe as responses to the demands for both reliable performance and safety as 'unproblematic, do not involve more complexity or new irrationalities ... solely positive', that they themselves become sources of complexities that increase the difficulty of attaining high reliability as argued in NAT. It is true we do not explicitly take as our objective the verification or application of NAT so to say. We were not, as was Sagan, interested in demonstrating what to us seems quite obvious and agreed to at the outset. We did not need to prove to ourselves, nor demonstrate, what was apparent in the field and is implicit in a good deal of our organization description. Rather, what is interesting is the willingness of some organizations to take on what could be considered a greater than linear increase in effort to carry out processes compensating both for the intrinsic hazard of their technical systems *and* the subsequent increase in 'secondary complications'.

Perrow also challenges the salience of our observations in characterizing the organizations we studied (point four). He joins Sagan in his scepticism of the outcomes as we described them, or else tosses them aside as not unusual. Sagan (1993: 259) similarly notes that the navy keeps the records and, implicitly, suggests they probably have cheated. How could one believe them? (We often saw the raw data.) Secondly, he is not sure whether aircraft accidents is the appropriate measure of safety and reliability. He also suggests that handling of nuclear weapons would be an 'obvious alternative' to study, even though no outsider could perform the kind of intensive, interactive field work that defines our study. From a theoretical perspective remote from the field, and the field work, Perrow and Sagan are not only unhappy about our choice of what to study (flight operations!) because it is not what they want to study, but also challenge our ability and credibility as field workers by dismissing our observations as contrary to what they would have expected to see.

In characterizing air traffic control, grid management and even flight operations as basically linear, Perrow confuses technical complexity with social and operational complexity; but by that standard, the nuclear operations studied by Sagan are just as linear. Moreover, he once again uses the assumption that we aspire to a theory of accidents in arguing that the aircraft carrier can be easily stopped and is without catastrophic potential. While it is true that the damage from the loss of an aircraft, or even a major deck fire, is not a *public* catastrophe, it is *to the organization;* and the maintenance of flight tempo is its major measurable output and therefore the surrogate for economic performance.[2] By insisting that we measure performance by his superimposed (external) indicators, Perrow subordinates the study of organizations to the study of accidents, denying the value for research of measuring an organization's performance in terms of its own goals and resources, and its errors and failures by its own vulnerability.

These are not trivial differences. Perrow is interested in air traffic control (ATC) because it could cause considerable public harm and has argued that its effect on elites is an important factor in its remaining safe — and then eliminates the seeming contradiction with

NAT (since the reaction should increase coupling and superimpose more complexity) by arguing that it is not complexly interactive. We are interested in ATC because by direct observation it *is* an interactively complex and tightly coupled system with very demanding internal goals. That organizational goals match external ones surprises none of us; they are tightly coupled to their environment. That the controllers have effectively internalized them is something else altogether — something that Perrow does not treat and, apparently, does not believe.

Perrow also restates as fundamental to NAT, the assertion that an organization operating under the stated conditions requires both operational decentralization and central coordination and that these are mutually exclusive. Evoking a familiar organizational aphorism, Perrow suggests that there are no conditions that allow for mixing of alternate authority relationships. The limitations of Perrow's premise of incompatibility have already been explored elsewhere from a social-psychological perspective that invokes a principle of 'collective mind' that lies outside the domain of Perrow's static framework (Weick and Roberts, 1993). We confine ourselves here noting that the evolutionary approach allows a complementary extension within an organizational theory perspective.

Naval flight operations are the most critical test case because military hierarchies are so strong and central, yet the decentralization we have observed in crisis is not only allowed, but encouraged (Roberts, Rousseau and La Porte, 1993). If these two modes of authority persisted stably over time, as is assumed in static organizational analysis, they would be incompatible (or, at the very least, cause a great deal of friction). But they are not (and do not.) The decentralized structures we observe in the midst of centralization are tolerated because they are either transient or latent (Rochlin, La Porte and Roberts, 1987), called into being only on demand. Even so, the organizational costs would not be accepted were they not judged to be worth the results. It is in this sense that we have argued that our work builds on rather than competes with NAT.

We emphasize again that the costs to the individuals and to the organizations are far from negligible. Perrow is at best disingenuous in characterizing what we have observed as no more than 'trying harder'. The intensity and engagement we have observed is not so much earnest effort (as Perrow implies) as it is an effort to get beyond their own tasks for the sake of the organization; this includes a variety of activities — seeking for early signs of impending problems, spending off-time hours gaming or rehearsing possible crises, and assuming responsibility on the spot, even at the risk of going 'illegal' in the process or being tagged as the responsible party if they fail. Moreover, the effort and intensity of purpose required to build what we sometimes characterize as the 'bubble', the state of cognitive integration and collective mind that allows the integration of tightly-coupled interactive complexity as a dynamic operational process, is enormous (Roberts and Rousseau, 1989; La Porte and Consolini, 1991; Weick and Roberts, 1993). If we were sometimes awed by the level of performance we observed, and at the human and organizational costs entailed, that is at least as genuine a response to the dynamic we observe as Perrow's 'awe' at the size and power of the technical artefacts, and possibly a bit more pertinent.

This leads us to the fifth and final point. Perrow's most serious concern is that the HRO group has somehow naively 'gone native' and thereby compromised its work. This is the most difficult to address because it is not only the most important but the most general. It introduces a matter of concern to all those who attempt work that is akin to organizational ethnographies: the methodology inevitably causes the observer to take up some of the perspectives of the observed as a function of the time spent on site. That being the case, to what degree will one's observations of behavioural patterns be distorted or obfuscated?

Perrow knows we share serious concerns with him about this, for he was party to early stages of our planning and preliminary de-briefings. This was of continuous concern to us in our field work; in a number of instances we deliberately cut short or limited the length of intense field observation. We were also quite self-conscious about explicitly ventilating the analytical frameworks we were using to frame initial questions and exploratory strategies. If we 'went native' it was not for want of caution. Moreover, Perrow also knows that we encouraged others to help us de-brief after long field trips to help us generate the necessary discount factor.

Perrow's assertion raises in another context the familiar tension between 'going native' enough to see the world of the observed (in this case, as in many in classical anthropology, a world never before reported in such detail) from their perspective and 'staying so alien' in the interest of adhering to a prior framework that surprising phenomena are masked (Clifford and Marcus, 1986; Atkinson, 1988). He ignores the complementary critique that

those who remain 'alien' reify in their work the fear that getting to know the established 'other' will lead to lowering our guard — perhaps, even, to discovering that they are 'something like us'.[3] But exploring this difficult problem does not seem to be Perrow's primary concern.

Our research has never been directed at seeking causes for perpetual alarm (as aliens sometimes do); but neither has it ever fallen into the category of arguing that there is *no* cause for alarm (as has some of the work thrown haphazardly into the HRT pot). We are interested in understanding how organizations respond to an environment of continuous serious hazard. There is need for alarm, certainly, and many have argued this position. Few have attempted to examine the dynamics and costs associated with dependence on systems that warrant it.

After his catalogue of concerns, Perrow goes on to note what he has learned — about theories of accidents — from Sagan and this exchange. He became aware that his early work did not include the importance of power, interest groups and environmental turbulence as potential contributors to the erosion of safety conditions. In his gratitude to Sagan for these insights, Perrow is speaking as an accident theorist, not as an organization theorist. As a leading student of organizations, neither the discovery of special interests groups nor environmental turbulence as factors in organizational behaviour would be of the slightest surprise. That it seems to be points up the variations in these complementary approaches that have been incorrectly cast in opposition to one another.

NAT is treated as the beginnings of an all encompassing theory of accidents within which organization materials can be included as providing original insights and surprises.[4] When hazardous systems are seen this way, what Perrow has 'learned' inductively from Sagan about accident theory flows straight forwardly from deductive organization theory insight. From this view, pressure groups and environmental factors should be expected rather than a source of conceptual surprise. Indeed, these are intrinsic to HRO work in our initial characterization of the environments of each organization, including regulatory pressures, and in our attention to the importance of 'watchers' as a key reliability-assuring component (La Porte, 1988; La Porte and Thomas, 1994). Part of the problem for management in HROs, as other complex organizations, is somehow to absorb both proximate and environmental demands. The presence and persistence of 'watcher' groups is central to maintaining HRO-attaining conditions.

We end by returning to the original subject. Perrow implicitly accepts the assertion that the military organizations studied by Sagan can be used to test the findings of the HRO work. If, as Sagan asserts from his case work, safety was not the prime goal and learning did not take place, then the organizations he studies are, by our definition, not HROs, nor do they aspire towards becoming them (Rochlin, 1993). The book is full of interesting, retrospective case work. But in what way does that test a theory of what makes HROs highly reliable? It only becomes a (manufactured) test if one asserts that we would claim that these organizations were reliable, or, as Perrow implies, because we claim that all organizations have the potential to become highly reliable.

This we flatly deny. What we have said is that we have observed organizations that do seem to be managing complex, sophisticated, dangerous, tightly-coupled technologies with an error rate low enough as to be quite remarkable in the framework of NAT and that we have uncovered a set of common organizational and structural factors sufficiently robust to provoke theorizing. This seems particularly notable, given the diversity of technology. We then suggested that if our research proved robust there was something to be learned from these organizations about the organizational and socio-cultural conditions that were *necessary* for relatively safe and productive management of technologies in the 'NAT' domain. Nowhere has the Berkeley HRO group ever stated, argued, or implied that these conditions were *sufficient*. And neither, despite their somewhat more positivistic stance, have Morone and Woodhouse (1986).

On this point, we differ greatly from Perrow and Sagan, or with the hypothetical NAT theorist who would not be impressed by 'nearly error-free operations'. The scale by which the organizations we study are measured is not set by the thrill of major failures, or the existential angst of near-misses, but by the more prosaic activities by which organizations do manage to operate successfully safety-consequential systems of high interactive complexity and coupling, even in a setting of high public and regulatory attention. This does not mean they will never fail; regrettably, it also does not mean that they will not be castigated, or perhaps even vilified, by analysts as well as the press, when they do.

Indeed, the entire question of whether any organization that manages complex, safety-critical technologies is an 'accident waiting to happen' or is a remarkable organization for

having avoided one thus far, is a matter not only of perspective, but of scale. A glass that is 99 per cent full may well suffice for aircraft carrier flight operations. It might suffice for a nuclear accident that causes no public harm (although, given the experience with Three Mile Island, it most probably would not). It almost certainly would not suffice where public safety is involved and perfection expected, as in air traffic control. And it certainly does not suffice for something on the scale of Chernobyl or Bhopal.

But where on this scale do we put an accident that would cause the global destruction of a thermonuclear war — or even the 'lesser' consequences envisioned in Burdick and Wheeler's (1964) *Fail Safe?*, where 'only' Moscow and New York are destroyed. For what is at stake is not just the possible accidental explosion or destruction of an nuclear warhead, but the possibility of triggering a sequence of events that would produce a catastrophe that is neither acceptable nor tolerable at any level of probability, whatsoever. This is what gives Sagan's work its importance.

It is the lack of attention to the unique dimensions of Sagan's case studies that we finally find most perplexing. None of the organizations studied by HRO researchers, nor even by those working within NAT, has anything resembling the terrible burden imposed upon the nuclear forces. Whatever our personal opinions on this, it is widely accepted within the community of nuclear strategists that a deterrent is of no use unless it is completely credible. This means not only that nuclear weapon organizations must avoid failing to fight when ordered, but that they must avoid even the appearance that they might fail. On the other hand, to fail in the other direction, to cause a nuclear war without cause, would be unprecedentedly horrible as well as catastrophic. Nuclear warning systems face a similar burden.

No other organization in our society is asked to live with such ferocious boundary conditions on error — and certainly not the 'economic' organizations such as nuclear power plants and chemical factories studied by theorists and students of accidents. Even for the most demanding case in our HRO work, that of air traffic control, what is traded off against pushing the safety envelope too hard is still primarily economic loss or inconvenience, and not a complementary disaster. To attempt to frame the nuclear weapons dilemma within the bounds of organization theory — whether NAT or any other approach — would have been truly exciting. The failure even to recognize, let

alone to systematically analyze, how and why the nuclear-weapon-managing organizations to which Sagan had such unique access are different from, or similar too, other organizations in civil society is perhaps the single major missed opportunity in Sagan's study. To do so would have been far more of a contribution to the field than the effort to improvise a sterile dialectical tension between the more conventionally-oriented NAT and a body of HRO work that seems to have been postulated as a foil.

Notes

1 Although some members of the Berkeley group have examined some cases of failures as instructive counterpoints (Rochlin, 1991; Roberts and Moore, 1993; Roberts, 1994), a matter ignored by both Sagan and Perrow.
2 By the same token, the accident at Three-Mile Island is still taken by Perrow to have been catastrophic, even though there were no serious injuries at the plant and most analysts now agree that the physical harm done to public was not very great.
3 Ironically, this implies that in this case our ethnographic methodology is closer to being 'natural and open' than Perrow's (or Sagan's). Imposing a prior framework and testing for compliance with it is a hallmark characteristic of a 'closed rational system'.
4 One is reminded of Steinbruner's (1974), *Cybernetic Theory of Decision*, which introduced insights into decision making from social psychology into political science and national security affairs.

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