CHAPTER 6

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EXPERTISE AND POLITICAL RESPONSIBILITY: THE *COLUMBIA* SHUTTLE CATASTROPHE

One of the major conflicts between the principles of democratic politics and the practical reality of expertise in public decision making takes place in connection with responsibility. The basic principles of democracy include some notion of political responsibility, usually understood to take a bi-directional form, in which the relation between ruler and ruled takes the form of representation and the relation in which the ruled control the ruler takes the form of accountability. The means by which the people assure that the persons who politically represent them are responsible to them vary among democratic regimes. Even within the general framework of representative liberal democracies the character of political responsibility varies considerably, both within a particular regime and between political regimes. Moreover, typically a modern state employs many devices apart from simple parliamentary representation, and very often these devices, such as juries, commissions, independent judges, law lords, and the like predate (or are based on models that predate) parliamentary government. Yet they involve their own notions of responsibility that resolve, ultimately, into a form of political responsibility.

Expert opinion has traditionally played an ambiguous role in basic constitutional arrangements, falling neither into the ruler nor the people side of this structure. Mill, in his classic constitutional text *Representative Government* (1861), was exceptional in that he was careful to discuss what he called council as a part of representative government. But he was vague about how it relates to responsibility, and for good reason. There are some very striking differences between the kind of responsibility that can be assigned to decision makers and the kind that can be assigned to experts, either in making decisions which are the products of expertise or expert opinion or simply expressing expert opinion in the context of a process leading to decisions. In this chapter I will explore these ambiguities in terms of a case study: the *Columbia* Shuttle catastrophe of 2003.

RESPONSIBILITY: THE *COLUMBIA* SHUTTLE

In its sharpest form, the contrast between the personal character of decision-making, which leads to the notion of personal political responsibility, and the liability for the decisions made,

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is represented in Max Weber's famous colloquy with Erich Ludendorff, in which the people are given the ultimate power to judge the actions of their leader on their behalf and to send him to the gallows if he fails.

> Max Weber: In a democracy the people choose a leader whom they trust. Then the chosen man says, "Now shut your mouths and obey me. The people and the parties are no longer free to interfere in the leader's business."
> Ludendorff: "I could like such a democracy!"
> Max Weber: "Later the people can sit in judgment. If the leader has made mistakes, to the gallows with him" ... (Marianne Weber 1975: 653).

Expert opinion, by definition, can never be judged in this way. Characteristically, expert opinion is not even formulated in the first person. Nor are individuals thought to be personally liable for opinions competently formulated within the limits of professional standards of expert knowledge. Put more simply, expert knowledge is impersonal knowledge expressed or formulated by individuals, while political responsibility is personal and judged by collective processes.

The reality beyond these abstractions, however, is more complex. The individuals who express technical opinions do so as persons, and the expression itself is governed by a kind of ethic. Speaking for science has a form that resembles representation, though the representation is of the science community and its opinions (see Turner 2002). But a personal element remains. The somewhat paradoxical notion of unbiased technical opinion, which is to say a technical opinion that is genuinely impersonal and therefore genuinely purely technical, is exemplary of the problem: if an opinion were purely technical, it is difficult to see what sort of question of bias could arise. The term itself implies that technical opinions are inherently impure, a mix of the personal and the technical, and that one can be held responsible for technical errors and biases, but not for one's unbiased opinions (see Thorpe 2002). Unbiasing ones *expressions* of opinion, however, serves to obscure or eliminate one's personal responsibility for them, and thus shifts responsibility to decision-makers.

When something goes wrong in a situation that involves matters that are primarily technical and includes political responsibility, these abstract paradoxes about the relation between technical expertise and responsibility turn into real administrative and political issues, and bureaucratic distinctions between council or expert opinion and participation in decision-making break down. The *Columbia* shuttle catastrophe of 2003 is an example of this transformation and concretization of the problem of responsibility. The catastrophe itself immediately led to the creation of a classic pre-democratic council mechanism for delegating technical questions to experts, namely a commission. The commission was given a task which drew on a specific and highly developed political tradition of boards of inquiry designed to assign responsibility for particular outcomes in complex events. What is striking about the *Columbia* case, which embodies the paradoxes of political impersonality, is that technical advice itself appears to have been responsible for the decision, so that the task of the board was to evaluate expert opinion and decision-making made on the basis of it. I will suggest that the phenomenon of expert knowledge here, as elsewhere, presents pervasive challenges to the notions of political responsibilities that underlie democratic theory, which can be highlighted by asking the Schmittian questions of "Who decides?" and "Who exercises unquestionable discretion?" (Schmitt 1985: 32–3). The

answer here appears to be that no one can be assigned responsibility – an anomalous result for democratic theory.

In general, the production of expert knowledge is controlled through the indirect means of professional certification and recognition, the concentration and control of opinion through methods of peer review and evaluation that typically do not directly assess the truth or validity of expert claims but only their minimal professional adequacy, but which nevertheless, through a series of complex redundant and overlapping mechanisms, generate consistent expert conclusions that fall within a small range. Thus responsibility for expert opinion is diffused by the very mechanisms which create the phenomenon itself, though as we will see there are some important exceptions to this. For the most part, however, the political power experts exercise over one another is limited, episodic, and indirect. Journal editors, board certifiers, and degree granters exercise discretion, but their powers are indirect: they do not determine truth, but evaluate competence, admissibility of claims, and the application of standards. This is the personal substructure of the impersonality of expertise. The wide distribution, indirectness, and uncontroversial character of personal decision-making allow for the illusion of impersonality. Political decision-making, in contrast, involves direct mechanisms of command and control as well as direct mechanisms of punishment for failure that enforce responsibility. The impersonality of expert claims, in contrast, removes the possibility of assessing responsibility, and even expert error itself becomes something that only experts themselves are in a position to assess. One intriguing aspect of the *Columbia* case is that the commission was used precisely for this purpose: as a means of using expert opinion to evaluate the actions of an expert bureaucracy, NASA.

The *Columbia* case provides a quite stark example of the transformation and evanescence of the concepts of representation and accountability in the face of expertise. In the immediate aftermath of the catastrophe, in numerous newspaper editorials and statements by congressmen there was a call for blame to be assessed and the blameworthy punished. For the public, holding individuals responsible was an essential aim, and their representatives took this as part of their own political responsibility. The expressions of the desire to hold individuals responsible were typified by those of Senator Ernest Hollings, who observed that this was "an avoidable accident" (a condition of responsibility), and noted that "in similar circumstances a Navy ship captain would be 'cashiered'" (*St. Petersburg Times*, September 4, 2003).

In response to the widespread public desire for accountability, the administration of NASA, in anticipation of an external investigation, immediately appointed an internal review committee which was assigned the task of investigation. The committee was promptly dismissed in the press as an inside job, in spite of the care that had been taken to include persons who were not directly involved in the decision-making. An additional board of outsiders and former NASA officials was appointed under the title of the *Columbia Accident Investigation Board* (CAIB) and began to work (*Orlando Sentinel*, February 13, 2003). The CAIB chose to mimic aircraft accident investigation boards. In this case, 200 investigators were put to work producing a report which cost nearly a half billion dollars.

The CAIB was advised not only by technical experts but also by top line management theorists and social studies of science scholars. It came to conclusions con-

trary to the conclusions of interested outsiders and citizens as well as Congressmen, many of whom rejected implicitly the CAIB account in favor of one that emphasized responsibility. The Board and the managers responsible for the shuttle also took opposed views of the problem of responsibility. The differences between them point to fundamental issues over the aggregation of expert opinions and over the assignment of responsibility for errors and incompetence that bear directly on the fundamentals of liberal democratic theory. In what follows, I will show why these are intrinsic to the problem of expertise.

Ending the Problem of Responsibility: NASA and the CAIB

Among the first acts of the commission was to choose a model for their inquiry, namely aircraft accident boards, that enabled them to avoid the task of assigning personal responsibility. The ostensible reason for this was that it would be impossible to expect candid testimony to a commission which was inquisitorial and judicial at the same time. But the choice also reflected generic and important difficulties with applying judicial and quasi-judicial concepts to expert inquiries. One problem was this: the Board was compelled to rely for its judgements not only on the testimony of the potentially guilty, which would be an ordinary situation in criminal cases, but on their claims as experts – in which bias would inevitably be an issue. Although the reasoning behind particular decisions, the differences of opinion about these technical decisions, and so forth, was more or less accessible to the members of the commission, who functioned as near-experts, and was accessible to the advisers to the commission as even nearer experts, neither the commission nor its advisers could reasonably hope to develop a completely independent understanding of these issues on their own. So the testimony was a curious combination of exculpation and expert witnessing of a kind that no ordinary court would have permitted as testimony.

Public discussion of the inquiry continued to use the phrase "smoking gun," which is to say the language of criminal accountability, and, despite the Board's self-limitations, the reality that the findings of the Board would be translated into some sort of punitive action was never far from either the public discussion or the thinking of the participants in the process, both on the Board and among the NASA employees. Because technical causes implied prior administrative choices, there was good reason to believe that some sort of personal accountability would result.

Public speculation about causes, and particularly about damage produced by the large chunks of foam that were seen to have been shed from the fuel tanks at liftoff, began immediately after the event. The release of internal NASA e-mails involving a frantic discussion among engineers of the possible consequences of foam shedding added to the speculation. The e-mails included a characterization of a potentially catastrophic scenario which proved to be an eerily accurate prediction. The puzzle then became the question of why nothing was done, even after a plausible professional opinion within the NASA team had made this prediction.

The initial reaction of the NASA bureaucracy to foam speculation was to attack its credibility. The NASA director Sean O'Keefe, a manager with science studies management background rather than a physicist, disparaged what he called "foam-ologists" and characterized the effect of the foam on the *Columbia* as similar to the

effect of a styrofoam container on hitting the front of a pickup truck. To call the adherents to the foam explanation "foamologists" implied that there was no form of technical expertise at its base – foamology is a parodic name for a nonexistent technical discipline. Presumably these comments reflected what O'Keefe had been told by senior technical managers. As it turned out, he was relying on technical opinions that turned out to be grossly in error.

The magnitude of the error became apparent in the middle of the inquiry when an experiment was conducted by the CAIB simulating the kind of high speed foam hits that the orbiter had actually been subject to. It was clear that the foam was more than sufficient to do the kind of damage that would have doomed the orbiter – and that the foamologists were right. But there was a crucial ambiguity about the results that bore on responsibility: the discussions within NASA during the flight itself involved the heat tiles that protected the orbiter during re-entry, which was assumed to be the weakest link, while the actual damage, as the CAIB reconstructed the evidence, was to the leading edge of the wing, made of reinforced carbon-carbon (RCC) (CAIB/NASA 2/18/2004; see Sec. 5: 1–60; Sec. 12: 1)

THE SHUTTLE MANAGER'S DEFENSE

In the middle of the investigation, after it had begun to be clear that the technical cause of the catastrophe was foam, the mission managers, those responsible for the decision about what to do about possible foam damage, held an extraordinary press conference to get their side of the story into the media. This side of the story was part of the evolving NASA response to the events which is itself revealing as a confirmation of the claims about the nontechnical causes of the event that form a large part of the CAIB report. In this interview the NASA managers centrally involved in the decision-making retreated to a second line of defense, which for our purposes poses the critical issue because it so neatly encapsulates the core problem of expertise in relation to fundamental concepts of responsibility and representation.

The actions that came into question were taken by Linda Ham, mission manager, her deputy, and their senior technical advisor, in response to an informal request by the debris team for satellite imagery of the bottom of the orbiter to see if it was possible to identify holes or damage to the tiles of the kind that would have resulted from a foam strike. This request was made shortly after the launch itself, after a frantic e-mail discussion within the team about the videotape of the launch, where extensive foam shedding had been observed. There is some question about the exact nature of the discussions between the various advisors concerned to have an image of the tiles and the flight management team, but the response of the management team was extremely interesting. The management team not only declined to entertain their request, but declined to do so on procedural grounds, the ground that it had not come through proper channels, or to put it in the peculiar bureaucratic language of Ham herself:

> We have read news reports that the mission management team had declined a request for outside assistance and if you read through the transcripts, you'll note that the mission management team never addressed a request for outside assistance because it never came up in any of the meetings. It never came up to me personally (Harwood 2003).

There was some confusion over what amounted to a proper channel for such a request, but the request was discussed at length not only with Ham's assistant but with her technical advisor, and Ham herself intervened and "killed the request," according to reports (O*rlando Sentinel*, July 1st, 2003) because the "possible request" had failed to come through proper channels, and thus was not a bureaucratically genuine request, or in the language of NASA, a "requirement" (Langewiesche 2003: 81).[1] But to track down the source of the request, as she put it:

> I began to research who was asking, and what I wanted to do was find out who that person was and what exactly they wanted to look at, so that we could get the proper people from the ops team together with this people or group of people, sit down and make sure that when we made the request, we really knew what we were trying to get out of it (Harwood 2003).

This statement was widely interpreted as indicating that she was trying to identify the source of the request in order to punish and intimidate the requester, a desire that becomes more relevant in the context of some other remarks by Ham about the model of decision-making for the process.

Ham, in the interview, argued explicitly against holding anyone responsible, on the grounds that "we do operate and we communicate, and everything that we do, we do it as a team," commenting that "if the system fell down, we will fix the system, but it is really difficult for me to attribute blame to individual personalities or people" (Harwood 2003). Ham had good reason to attempt to deflect responsibility, as she had made the critical problematic decision. In her capacity as manager, she had decided that the problem was something that the team could not and would not have done anything about until after the mission anyway. But she did so with the public assent of relevant technical officers. Formally, what she said was true: This was a collective decision, though a collective decision made through a process that, as she herself observed, might have been flawed.

Part of the problem of Ham's responsibility arose through issues of intent. She appeared to have been motivated by managerial considerations (that in retrospect seemed trivial and inappropriate) about the cost of getting the images and about the inconvenience and cost to other elements of the mission of maneuvering the orbiter into a position such that photographs could be taken. Moreover, Ham raised questions about the adequacy of the paperwork and rationale for dismissing foam events on previous flights, and indeed, because there had been such a large number of these events, the problem was one of which she was at least minimally aware. But here again her motivation appeared to be simply to construct an adequate rationale for dismissing the event which could be based on the precedent of previous rationales for dismissing it; 'appeared to be' because if she had taken the possibility of damage seriously she would have opened a full scale inquiry while the flight was still going on. Her concern with the paperwork on previous flights seems to have included no interest in or willingness to actually do anything with this paperwork, such as reading it or having it re-examined for its relevance. And her bureaucratic approach to the problem of foam shedding seemed, in retrospect, also to be inappropriate and callous. Her interest in citing it appeared to be to use it as bureaucratic authority for her own actions. Her comment that she hoped the paperwork was good, in the context of her failure to address the question of whether it was, indicated that she had no interest in

the question, despite the fact that she had to be aware, as her comments indicate, that these rationales were often not especially good.

Because it would have at least been possible to consider strategies to save the orbiter at this point, this decision would have been in a similar context, such as military context, culpable, and intent would have been relevant. But she employed an Eichmann-like defense of 'following procedures.' And the top administrators of NASA seemed not only willing but eager to treat this as not only acceptable but competent managerial action. As O'Keefe later said, she was an excellent manager and other divisions of NASA would be eagerly competing for her managerial services. In the military context, she would likely be court-martialed, as Senator Hollings observed, though in those contexts as well the reliance on procedure would have been at least partly exculpatory.

Why did this apparently strange acceptance of the 'procedural' defense seem normal within NASA? One answer is this: NASA was tracking 5,396 individual hazards that could cause major system failures in flight, and about 4,222 of those could either threaten mission success or cause the loss of the crew. Waivers had been granted on 3,223 of those known problems (Halvorsen 2003). Thus, there were many possibly catastrophic unsolved problems with the shuttle with similar paperwork histories. Foam shedding was a case in which the paperwork was extensive, the problem had been experienced repeatedly before, progress had been made in dealing with it, and in no case had the damage been severe enough to cause the loss of the orbiter. So there was a substantial rational probabilistic ground for thinking that the problem would not arise in the case of this foam strike either, and Ham herself noted in the key meeting that "the flight rationale [for going ahead with launching after the foam strike in October was] that the material properties and density of the foam wouldn't do any damage," and suggested looking at the data from a 1997 flight where there had been debris damage (Sawyer 2003). In the face of 'paperwork' of this kind, the reliance on procedure becomes less puzzling. No manager could hope to assess 5,000 hazards on their merits: that was the task of engineering teams that had formal responsibility to speak up, through channels, if they believed the issue needed to be addressed. Ham relied on those procedures because there was no practical alternative to doing so.

The second line of defense opened by Ham pointed to some reasons why this would have been a defensible action purely on grounds of the NASA model of handling expert opinion. As one NASA official was quoted, "she did the best she could do given the information she had. She talked to people she trusted, she listened to the analysis" (Harwood 2003). This in itself is a peculiarly ambiguous formulation, but it is nevertheless, from the point of view of responsibility, an interesting one.

As it happens, the technical advice which seems to have decisively sealed the fate of the requests for imagery was from Dan McCormack, a senior structural engineer, who told the team that the analysis of possible tile damage by the contractually responsible Boeing team showed no serious threat, and that the RCC wing edge might show some coating damage, but no "issue" (Sawyer 2003). At this meeting another engineer spoke up to agree with him. The Houston-based NASA engineer, Alan Rodney Rocha, who had expressed strong concern, backed down after this analysis.

What should we make of these errors? In a model of decision-making in which there is a well-developed division of labor such that particular technical decisions and particular competencies match perfectly, there is no problem in assigning responsibility. If a particular cable breaks, the person whose job it is to make decisions about the cable and to be expert about everything relating to the cable is responsible. But a complex system typically cannot be understood in this way and new systems contain novel unknowns that cannot be understood in this way.

The technological division of labor (and expert knowledge) that works for automobiles or aircraft carriers, which are well-understood technologies, will not work for a poorly understood technology because the relations between the parts that correspond to the division of labor may be different than expected. In this case at a certain very simple level there was a part with a problem tile, and therefore a corresponding body of 'expertise.' This primary level of expertise did not fail: no one was proved wrong with respect to anything that they properly gave a technical opinion on. The tiles, as far as the CAIB could tell, were *not* the cause of the catastrophe (James et al. 2004: 9). The error was in taking this legitimate piece of expertise and interpreting it to have significance it could not bear. This is a problem of aggregating technical opinion, or deciding what to make of it.

The NASA method for aggregating expert opinion involved two key elements: to rely on an elaborated division of labor involving teams responsible for specific engineering systems used in the orbiter and the launch system, as well as teams for specific problems, such as debris. Each of these teams reported to higher level mission teams which operated in a similar way: they had a managerial hierarchy, with persons who were formally responsible, but operated, with respect to technical issues and to a large extent also with respect to managerial issues, as a team which required consensus. Technical disagreements were taken seriously, and a strong rule of procedure was to insist on data-based discussion. Both of these elements of procedure worked against minority opinion, a point to which we will return, but also created a situation which made the zone of relevance of particular kinds of expertise ambiguous, so that a person participating in a consensus decision who was not genuinely expert in some area could overreach and deliver a strong message that could affect the consensus.

Among other things that need to be said about consensus in the context of large bureaucracies is that the consensus that occurs in the face of unequal, and indeed hierarchical relations between those participating in decisions, is not necessarily 'representative' of expertise in the same sense as consensus that emerges among equals. Even in the case of a consensus among formal equals, it is typically necessary to reach agreement through the artificial means of specifying what counts as agreement, such as through voting, because a genuine consensus is so difficult to actually achieve. In the case of hierarchical bureaucracy, 'consensus' is easier to achieve because it is typically only consent, not overwhelming agreement between independent experts. People on the bottom tend to go along with strong judgements made at the top out of fear for their careers; there is a strong selective bias in any bureaucratic career structure that punishes those who reject prevalent ideas.

But even in the case of genuine consensuses of independent experts, there is a question of domains of expert competence. And this is a conundrum that is central to

the problem of the political significance of expert knowledge. Ham was caught in the following paradox of authority. She was not herself an expert on the issue in question, and was compelled to rely on expert judgement. But because she was not an expert, she could not independently determine who was genuinely expert. To put this in another way, there is a paradox of managerial omniscience: if the manager is sufficiently omniscient to determine who is in fact expert, they will be virtually as expert as the expert herself, and indeed, this would be the limiting case; so the closer one approaches to the limiting case, the less there is any need for expertise, counsel, and expert advice in the first place. Not surprisingly, she relied on 'the system.' In this case she relied on senior technical advisors, who screened and evaluated technical advice and comment from engineering teams, and on data-driven presentations at meetings in which quite large numbers of managers and engineers, representing different teams, participated.

AGGREGATING EXPERT OPINION: THE ROLE OF META-EXPERTISE

Some method of aggregating expert opinion was necessary, simply as a result of the complexity of the system. But a subtle and important change in the character of the expertise in question occurs as the technical issues move up to the highest level of decision-making, and a related change occurs in the meaning of consensus. If we ask who was responsible for the fact that the team went wrong in arriving at a consensus, the greatest responsibility falls on the people who influenced the consensus. The culpable figure in the story now becomes the expert who overreached his competence with bad but persuasive advice. One could imagine constructing from this an appropriate, enforceable ethic of expert opinion that made this kind of error culpable. But this cannot be a workable procedure, as it would have the effect of chilling discussion, which should consist in sharing information and mutual persuasion. Ham was accused, perhaps correctly, of creating an atmosphere in which engineers were intimidated and afraid to speak out. But Ham relied on the fact that the relevant technical advisors had a formal responsibility to raise concerns. It was Ham's zeal to hold them responsible for raising questions that chilled the atmosphere and prevented discussion. So a method of limiting this kind of discussion by making people responsible for the consequences of presenting an opinion to a group would have a predictable effect of restricting the content of the discussion further.

It is evident that assigning responsibility for *expressions* of technical opinion is a peculiarly difficult matter.[2] The engineers who invented the scenario which correctly predicted the course of events once the orbiter was damaged made a great point to the newspapers that their predictions were only speculative, absolving themselves from responsibility for not pushing their arguments harder, which was their clear formal responsibility, on the solid grounds that their scenario *was* only speculative, that is, not a technical opinion in the engineering sense, grounded on known principles and data. One of the key figures, Bob Daugherty, who did the damage assessment that indicated "what would happen to the shuttle's left tires if extra heat got inside the landing gear department because tiles were damaged" and sent a series of e-mails about this, indicating his frustration with the lack of response, was later to insist that

"his messages have been grossly misinterpreted and were neither warnings nor concerns, but "just engineers talking," an interpretation that the investigators rejected.

The distinction between "just talking" and something else is nevertheless interesting, for the something else would have to be 'taking managerial responsibility.' The 'team' structure of NASA's consensus system for employing expert opinion requires engineers to take this dual role, and thus, in effect, to police themselves with respect to the expression of their opinions, which has the effect of requiring them to distinguish between offstage technical speculation and opinions for which they are accountable as members of a management team. One might say that it was this part of the 'system' that failed, because a whole series of engineers who expressed doubts in the course of 'just talking' failed to take the next step of invoking formal procedures that would have made those doubts into managerial actions (*Orlando Sentinel*, March 23, 2003).

Their actions were thus defensible: they lacked confidence in their own predictions. So, however, were Ham's. Ham pointed to some reasons why this would have been a defensible action purely on grounds of the NASA model of handling expert opinion. Ham argued that she relied on the best technical advice available. The advice that she received clearly was not correct, nor was it in fact the advice that the most appropriate and competent experts even in NASA itself would have given her. Nevertheless, in another significant sense, it was the best advice. It represented the assessment made by her own senior technical advisors of the technical advice given by the debris team and the advice given by other technical specialists, through a formal process involving responsibility, however burdensome, in the case of the lower level engineers, those responsibilities were. Only in retrospect could she have known that this heavily vetted advice was wrong.

What these various 'bests' in the category of advice indicate is what is perhaps the fundamental problem in understanding technical advice in terms of these familiar bureaucratic and democratic concepts: accountability is difficult. The potential role of an aggressive advocate of a technical opinion raises some other interesting questions. Terms like 'consensus' and 'team' have a formal meaning as well as an informal reality. The formal reality is that particular group dynamics, group-think, submission to a dominant or stubborn minority, a general reluctance to make waves, and the sense of safety of apparent majority opinion, may be the operative determinants of outcomes. The formalistic idea that expertise can be pooled through discussion ignores these processes. It also conceals an important change in the nature of the expertise that is supposed to be collectively produced. We might call the expertise of the competent practitioner operating in an understood domain of practice 'primary' expertise. What these bodies are designed to produce, however, is expertise about expertise: meta-expertise. The errors of the *Columbia* management team were failures to be expert about expertise – errors in judging the relevance, probative character, and implications of 'primary' expertise claims about tile damage and about debris damage that arguably was literally correct. The Boeing analysis, for example, included caveats that had to be ignored or judged irrelevant to make the report useable in the decision process. With this we enter into the interesting zone of ambiguity.

The Ethics of Speaking as an Expert

The effect of these considerations is not to eliminate notions of responsibility so much as shift them to experts functioning as councillors. But the problem of 'responsibility' for council is a difficult one, especially since the distinction between council and representation itself seems to imply that the responsibility falls on the recipient of the council who holds actual decision-making responsibility. Nevertheless, with respect to the concepts of scientific expertise and engineering expertise the traditions contain very strong notions of responsible expression of opinion, and yet another, even more revealing model, is to be found in medical expertise. In the case of scientific expertise, it takes the form of an expectation to observe the distinction between what is known to science, that is, what is accepted as knowledge that is genuinely scientific by at least a virtual or presumptive consensus of scientists, and 'speculation' or opinion. Scientists learn to speak in both ways and to distinguish the two and routinely criticize one another for crossing the line between the two. In engineering, the tradition is to draw a distinction between that which can be fully and predictably calculated according to known formulas and which results in predictable outcomes, and that which is not. One of the interesting examples of drawing this line occurs in connection with the concept of 'software engineering.' The claim has been made that software engineering is a misnomer because the sheer complexities of software result in bugs and problems that cannot be fully predicted or reduced to formula, and therefore cannot be, in the full sense of word, 'engineered.' Medical expertise is governed by a different set of imperatives in which 'speaking for' the medical consensus is not as central, because a balance needs to be struck between the need to respond, even in the absence of complete knowledge, to suffering and the Hippocratic admonition of 'do no harm.' This leads toward a greater emphasis on responsible speculation. Some years ago the chemist Linus Pauling suggested that vitamin C might be a response to cancer. Within the limits of chemists' notions of responsibility for utterances, he could reasonably say that this was a plausible hypothesis. But from the point of view of medical expertise, it was insufficiently plausible to justify the potential suffering it could cause for those who, predictably, over-optimistically took this advice and failed to avail themselves of more effective treatments, and was, therefore, denounced as irresponsible. Yet physicians themselves are commonly willing to try treatments that are not sanctioned by any medical consensus if they work for the individual or appear to work in other empirical cases even though the underlying mechanisms are not known. For many drugs, for example, most of the prescriptions written are 'off-label,' meaning they are written for conditions other than those they were tested to treat. But this is justified by the physician's own assessments, for which she can be held responsible, of the potential benefits and risks of the uses, which typically are based on experience – 'empirical' in medical parlance – rather than a full understanding of the relevant mechanisms and disease processes.

This more conservative ethic bears directly on the *Challenger* and *Columbia* cases, because each situation involved the assessment of data which either could not be, or had not yet been, reduced to the level at which it could be 'engineered,' but which nevertheless could justify a certain judgement about the relevance of the facts and their importance and epistemic weight. The problem with *Challenger* was a seal

on a joint that was known from prior post flight examination to have behaved in an anomalous way, but which had also successfully functioned under exactly the stresses that the joint was being engineered to function under. Here, understanding and empirical knowledge did not match, just as empirical knowledge and medical science often do not match. In the face of uncertainties of this kind, it is common to employ heuristics based on the 'empirical' successes of the past. The fact that some theoretical consideration had failed to accurately predict a problem in the past was a reasonable ground for ignoring it, or if not for completely ignoring it, giving it a smaller weight in decision-making. In the case of *Challenger*, some engineers *were* concerned about the problem, but in the end a specific kind of consensus process within a managerial system, which we will discuss in the next section, overrode those concerns, and even those who voiced the concerns went along with the consensus. It is this process that many commentators pointed to as eerily reminiscent of the *Columbia* catastrophe and indeed it involved the same kinds of discrepancies between data and actual past experience: foam had not, despite hitting the orbiter 57 times, done sufficient damage to threaten a flight (*Florida Today*, August 26, 2003).

In the case of *Columbia*, there was an additional problem involving the data and its presentation that resulted from the way in which NASA routinely responded to this kind of problem. As I have mentioned, NASA discourse operated on a principle of accepting only data-based claims, yet was reluctant to spend the money and effort required to collect the relevant data when, empirically, things were working. The difficulty with data thus took on the following structure. A concerned engineer or engineering group would seek authorization to conduct the relevant engineering study, have this request denied, and then be told that its concerns were not backed by data and therefore could not be considered. Typically this response came from two sides of the communication system. On the management side, decisions were made to prioritize the relevant requests for research; on the expert advice side, claims made by lower level engineers were routinely dismissed for not being sufficiently data driven. Not surprisingly, in this information-poor environment, it was difficult to persuade either consensus groups or higher-ups of positions involving any concern, and the overriding desire of groups to be seen to be problem-free discouraged arriving at a consensus that would have placed them in conflict with announced goals of both mission management and the agency itself. This meant that, in addition to the usual mechanisms which served to punish employees for failing to act in accordance with consensus, there was a mechanism that had the effect of threatening to punish the groups who formed the consensuses themselves.

Should experts restrict their claims to those matters about which they are truly expert? There is a complex issue here. At the close of the World War II, James Bryant Conant, reflecting on his experiences, was very direct about his sense that experts, particularly scientists, routinely had hobby horses; this implied that they routinely overreached their technical competence in advancing their own views. He also recognized that the consensuses of scientists were themselves often in error, even with respect to their most fundamental beliefs. It was this aspect of Conant's thought that is amplified in his protégé Thomas Kuhn's *Structure of Scientific Revolutions* ([1962] 1996), where conformity to the paradigm was taken to be characteristic of normal science. Conant's suspicion of the overreaching expert is particularly relevant

here. Who is the best judge of the limits of expertise? The question seems to lead us to exactly the problem of expert omniscience with which we ended our discussion of managerial responsibility.

If experts are genuinely the best judge of the limits of their own expertise, it would seem that expertise in effect had no limits, or to put it differently, that they were not only experts in the primary sense but meta-experts, at least in relation to everything involving their primary expertise. They would have to have the kind of birds-eye view of the domains of expertise that their own expertise related to that permitted them to say what they were expert about, and what they were not. In practice, experts often must apply the tools of their own expertise to situations that are outside of the core of their expertise, and for the scientist, this is simply the means of producing scientific results and scientific progress. For the engineer, it may be the necessary element in engineering creativity, which obviously cannot consist merely in the application of known formulas to previously solved problems, but may involve the discovery of new applications that can be then reduced to formula, or the making of novel applications. In the case of the expert who dismissed the foam problem and those experts who were above him and took his opinion as their own, backed by the 'consensus' of experts at the lower level, one wonders whether this was merely, so to speak, a 'normal error' of expertise rather than something genuinely culpable. Without the kind of expert knowledge possessed by others, would this expert have known that his particular extension of his knowledge was simply erroneous? Without what we might call omniscience about expertise, omniscience about what the limits of expertise are in particular cases of expert knowledge, we would not be able to make these judgements. And there is no reason to think that this particular expert possessed this particular kind of second order omniscience.

There is also a problem relating to the use of consensus and the conflict between a refined division of labor and the pooling of expertise to produce a consensus. If the point of consensus is not merely to diffuse responsibility and produce what I have elsewhere called fact-surrogates (Turner 2003: 41–3) for managers to operate with, but is to actually facilitate and improve expertise or improve consensuses which are in some sense better than the limited expert knowledge within particular expert domains in the division of expert labor, there must be some discursive process that allows for the dialectical evolution of this improved consensus. And whatever else the discursive ethic of this form of discussion might be thought to consist in, it does seem that the familiar considerations of Mill's *On Liberty* ([1859] 1975), of collective fallibilism and willingness to tolerate dissent and indeed even to encourage it, must be part of the ethic.

The problem, however, is that this particular model, and even its elaborated and modernized version as promoted by Habermas under the title of 'the ideal-speech situation,' in which second-order considerations about the assumptions and interests of the parties involved is made open to discussion in order facilitate the rationality of the discussion, is undermined and fundamentally transformed by the asymmetries of knowledge that are intrinsic to the situation of expert knowledge itself. The participants are not equal and all the relevant issues are not fully accessible to all the participants. Typically they are fully accessible to *none* of the participants. As an expert one participates in these discussions with a command of a particular limited range of

techniques of analysis, experience, tacit knowledge, practical understanding, and so forth, and the considerations which one can bring to bear on the questions to which the consensus is addressed are necessarily limited to those considerations which one not only understands best but understands the grounds for best. In the setting of discussions with other experts with different domains of expertise, it is impossible to completely eliminate these asymmetries through discussion – Habermas' model. Discussions, however prolonged, will not produce the same tacit knowledge, practical experience, theoretical understanding, and so forth in all of the participants in the discussion. So the discourse must be of a kind that relies on trust in the expert claims of others, and also in at least some of their meta-expert claims. And this means that participants must rely on, or at least make judgements of, the self-discipline of the other participants – particularly of their observance of the distinction between expertise proper and meta-expertise, questions involving the limits, relevance, and character of the participants' own expertise.

This means that the kinds of considerations that motivated Mill, namely that free and open discussion would allow the formation of a consensus in which the participants understood one another cannot fully occur. There is an ineliminable role for meta-expertise: judgements about the competence of others to make the claims they make, and this adds an ineliminable limit to any given participant's capacity to inquire into the grounds for claims made by others. Put differently, the condition of omniscience about expertise – perfect meta-expertise – cannot obtain. Necessarily then, this means that the notion of responsibility for assent to a consensus is no longer readily applicable. One necessarily assents to things that one does not fully understand. But one cannot be held responsible for that which is beyond one's powers. One can scarcely be said to be responsible for an outcome about which one necessarily relies on others and on one's inevitably limited non-omniscient judgement of their capacity to make those judgments. But since every participant in the dialog is in this situation, in effect no one is responsible. Moreover, any stringent ethic of self-limitation in discussion would defeat the purpose of open discussion entirely and simply amount to delegating the decision or elements of the decision to those people who claimed to have relevant expertise or were assumed by the division of labor of expertise to possess that expertise. This suggests that we really have no usable model of consensus here and that these consensus processes are a kind of black box, which we can relate to as consumers in terms of their reliability, but not in any reasonably full sense understand, much less assign individual responsibility for.

NASA, MANAGERIAL RESPONSIBILITY, AND CONSENSUS

I have introduced this rather odd concept of meta-expertise, expertise about expertise, not because I think there could be expertise about expertise, but because there would need to be such a thing in order for many of the common claims about experts to make sense. It was an interesting feature of the *Columbia* inquiry that there was a significant amount of speculation, for example web pages, about the competence of the NASA managers, especially Linda Ham. The suggestion was made that anyone with elementary knowledge of physics would have realized that foam could have caused significant damage, and it was implied that her lack of this knowledge ex-

plained her mistaken actions. A more sophisticated variant of this argument was made by Hugh Pennington, who argued that

> when the known weakness in the design of the solid rocket boosters was discussed at the *Challenger* pre-launch conference, one senior manager was unhappy. He was told to "take off his engineering hat and put on his management hat." He did, and the launch proceeded – to catastrophe. With *Columbia*, the team examining the effects of the insulating foam that had peeled off the enormous external fuel tank and hit the shuttle at 500mph (creating a hole in the wing that led to the craft's destruction on re-entry) made numerous requests for imagery to be obtained to check for damage. Managers were not interested, such strikes had happened many times before and were classified as not posing a critical threat. Imagery requests were denied. When asked why the team had not pressed their requests harder, the engineers "opined that by raising contrary points of view about shuttle mission safety, they would be singled out for possible ridicule by their peers and managers" (Pennington 2003).

Put into practical organizational terms, this reasoning would suggest that there should be a complete separation between the expression of expert opinion and decision-making that no expert *should* be forced to put on a "management hat." It further suggests that sanctions for expressions of opinion – even informal sanctions such as "possible ridicule" – should be forbidden. This gives us a theoretical model for the aggregating of expert opinion for decision-making: experts express opinions for which they are not accountable, even informally, to their peers; decision-makers decide what to do with these opinions, and they are the only participants to have any responsibility. Needless to say, this is a piece of utopianism.

Having the expert put on the managerial hat makes them accountable for their opinions. It explicitly makes them accountable not merely for their technical opinions, but for something more – weighing the importance, evidential base, risks, and consequences of having their opinion taken seriously, which is to say it holds them responsible for meta-expertise about their own expertise. Is this also utopian? One view might be that the answer to the question of who is a better meta-expert about a given area of technical knowledge than the expert in this area is 'no one,' and that ordinarily, experts should be managed by experts of the same kind, and define their own scope of decision-making competence as well as their expertise. This was certainly the position of the atomic scientists in the West during the two decades after the Bomb. They believed, because they were technical experts about nuclear weapons, that they were uniquely and even solely qualified to make judgements about nuclear disarmament, about which weapons programs should be pursued, and about nuclear strategy. Eventually, however, a vast array of other kinds of experts staked a claim to expertise on these topics, and a body of informal meta-expertise guided both decision-making and the use and assessment of expert claims.

The atomic scientists, however, *sought* managerial power and responsibility, and complained about the inability of the decision-makers to understand them or respect their claim to special expertise with respect to these meta-expert topics. This model *is* another piece of utopianism, at least when it conflates the distinctions between technical knowledge and what I have been calling meta-expertise of how to assess the claims of experts in decision-making. It implies that experts should simply be delegated managerial authority in those domains about which they are expert, and that their own claims about the significance and relevance of their expertise should be

accepted, even where the political consequences, about which they are not expert, are enormously significant.

The NASA system was an attempt to deal with expertise in a practical way by holding those who expressed opinions responsible for their opinions, as they bore upon decisions. "Just engineers talking" was not discouraged, and obviously, in this case, occurred. But there clearly was a problem of aggregating opinions in public discussion, which was shown in the many comments that participants in the decision-making process made about the risks of expressing concerns. There was a gap in perceptions between the underlings and the top mission managers about the quality of the atmosphere for discussion. From the point of view of the underlings, there was a sense that anything that was raised with managers had to be sugar-coated, that messengers with bad news would be punished, and so forth. The managers, in contrast, placed faith in the formal procedures, which obliged many people to raise these concerns if, in their professional opinion, the concerns were valid. And they also placed faith in reliance on hard data while failing to provide adequately for the collection of relevant data. And they seem to have been exceptionally blind to the ways in which pressures to perform and to conform with the consensus not only imposed responsibility for expressions of opinions but effectively prevented opinions from being aired. The CAIB investigators interviewing the mission managers identified this as a serious failing. In the course of the CAIB inquiry, Ham was asked

> "As a manager, how do you seek out dissenting opinions?" According to him [the investigator], she answered, "Well when I hear about them . . ." He interrupted. "Linda, by their very nature you may not hear about them." "Well, when somebody comes forward and tells me about them." "But Linda, what techniques do you use to *get* them?" She had no answer (Langewiesche 2003: 82).

One answer she could have given was that it was the formal responsibility of her subordinates to raise these questions. She chose not to blame others, however, and appealed to the idea that, because the mission workers were a team, no one should be held responsible. But the failure to 'speak to the opposition' and otherwise maintain an appropriately open kind of discussion points, as the CAIB investigator intuited, to a deeper issue with meta-expertise. Part of the point of the discursive processes of a large technical project of this kind, without the kind of stable division of knowledge spheres characteristic of well-developed technologies, such as the automobile, is to provide a collective surrogate for the necessary meta-expertise. Discussion, sharing perspectives and concerns, is the means of constructing the meta-expertise necessary for decision-making, but also, in a situation in which experts are responsible for meta-expertise about their own areas, provides the expanded understanding necessary for participants to grasp how their own domains of responsibility relate to that of others.

As important as this domain is, it is also, necessarily, the least accountable of all. The point of discussion is to arrive at a meta-expert climate of opinion. But the content of this climate of opinion itself is no one's responsibility, nor is the content a matter of expertise proper. At most, issues of *process*, such as those the CAIB investigator raised with Ham, can be treated as matters of responsibility. So the inclusion of managerial responsibility in this fashion has an ironic consequence. It makes the manager-councillors more circumspect about their claims, but at the same time frees

them from responsibility for outcomes. The outcomes are the products of consensus for which no one is formally responsible.

CONCLUSION

What happened to the call to hold NASA officials responsible? There are four answers to this, and a coda which suggests that all of the discussions were misguided. The first is the answer given by the CAIB, which, in addition to its discussion of the technical causes, blamed the NASA 'culture.' The second is the response of NASA employees and former employees, which was curiously mixed. A number of different persons were identified as culpable, but perhaps the strongest reaction was that Linda Ham was a scapegoat. The third is the response of the politicians, who eventually gave up on the question of responsibility. The fourth is that of NASA management, which professed acceptance of the conclusions of the CAIB, but did what was in effect, the opposite. The coda is this: the newest understanding of the cause of the foam shedding and the foam hit itself suggests that this particular foam-shedding event was significantly outside any past experience.

One of the oddities of the CAIB report and the administration of NASA is that the board relied very heavily on social science knowledge, including the work of Karl Weick, the revered organizational social psychologist and former editor of *Administrative Science Quarterly*, and Diane Vaughan, the author of an influential book on the *Challenger* (1996). The report appealed, as Vaughan did in her book, to the organizational sociologist Charles Perrow's classic book on normal accidents (1984). The advisors to the board included Harry Lambright, who was a longtime professor in the Syracuse Science and Technology Administration program. Sean O'Keefe, the head of NASA, was both a student and a professor in the same program. So the participants were steeped not only in social science lore but in the STS tradition as it pertained to these kinds of decision-making processes.

What these participants shared was a commitment to a body of organizational behavior theory that itself served to shift issues of responsibility from individual managers to managerial processes and structures. The origins of modern managerial thinking in 'scientific management' had been based on an attack on 'the pressure system' and argued for the theory that there was a 'one best way' for performing tasks that workers themselves could not discover and hence should not be held accountable for. The Human Relations approach that followed shifted responsibility for such things as workers' feelings to managers, who came to be thought of almost as therapists. So the appeal to an organizational level of analysis already implies the diminution of the notion of responsibility and perhaps even its relegation to the medieval torture chambers of premodern organization practice.

When the CAIB dealt with the problem of responsibility, it did two things. It pointed its finger at fundamental problems in the way in which NASA was managed that were not problems that could be assigned to any single past or present administrator or administrative action. The issues were matters of process, and then 'culture,' a more recent organizational behavior concept. The notion of culture served as a useful stand-in for the guilty party, but since cultures cannot be held responsible, it had the effect of eliminating the notion of responsibility entirely.

So it is perhaps useful to close with a brief discussion of the culture argument and its implications and validity. One of the peculiarities of Vaughan's book on the *Challenger* was that the culture concept was applied to a body of decision-making practices that on another approach would have simply seemed rational. Engineers working with advanced and complex technologies, especially with technologies which are not mass produced, are routinely faced with the problem of understanding how parts fail while operating with relatively limited data. The best analogy here, perhaps, is the work of the engineers and mechanics of race cars. This is a technology in which one routinely tries to get more out of the machinery than other people have, and thus is always straining at the limits of the machinery, which routinely breaks. The failures provide data for strengthening, fixing, and redesigning parts. Safety is of course a concern, but technical decision-making relies, necessarily, on heuristics applied to actual experience. Every decision Vaughan attributed to 'culture' in the book would just as plausibly be attributed to the reliance on these heuristics,[3] and, as I have argued here, reliance on meta-expert judgements is ineliminable. The use of the heuristics or pragmatic justifications is only a part of meta-expertise, but, in any complex system with uncertainties, it is a necessary part.

The 'culture' explanation had practical implications, and here the NASA response becomes relevant. On the one hand, O'Keefe took public responsibility for the task of changing the culture. On the other, he rejected the means of changing the culture that best fit the situation. Some background here is relevant. In the early 1990s, when culture became a managerial concept, there was a spectacular and largely successful attempt to change the manufacturing culture at Ford Motor Company, in order to get managers to emphasize quality over productivity, as manufacturing quality had been a long-standing Ford problem. Some managers, particularly the head of a major New Jersey manufacturing plant, resisted or ignored the newly imposed 'culture.' As an important part of the strategy of cultural change, he was fired, and the reasons for his firing were widely circulated. Public execution, in short, is a major device for culture change. The other devices, such as charismatic 'change-agent' leadership, are more problematic, and less relevant to the NASA situation. O'Keefe, however, insisted that there was no need for a "public execution" (Cabbage and Shaw 2003), and did precisely the opposite of what Ford did: he reassigned staff members implicated in the report, allowed others to retire, but praised others, including Ham, and never circulated or even publically acknowledged the reasons for these personnel actions. Even the CAIB members accepted this. As one of them put it, "Do you want their heads on a fence someplace?" and added, "Rather than listen to what he says, watch what he does" (Leusner et al. 2004).

The predictable result of this, as Aneil Mishra, a Wake Forest professor who studies organizational change put it, would be this:

> People will read between the lines and make up their own stories and sentences and paragraphs. And they will either be wrong, distorted or they may be right. He went on to say, Sean O'Keefe needs to be telling people why those 14 or 15 people were replaced, who replaced them and why. He predicted that "if he doesn't start doing that, the culture will change, but it will be for the worse" (Leusner et al. 2004).

Barry Render, a NASA consultant and business school professor, made a similar point.

> The message [from O'Keefe] is that the old system is still in place. Clearly, someone had to take blame, but they just got a lateral transfer [or] were due for retirement anyway, so that's not a big deal (Leusner et al. 2004).

When Admiral Harold Gehman, the head of the CAIB, responded to Senators pressing him on the question of accountability, he had said that, "if someone – the administrator of NASA or the head of this committee, wants to find out whose performance was not up to standard, it's all in the report." O'Keefe himself said there would be "no ambiguity on the question of accountability at all" (Cabbage and Shaw 2003). But NASA employed a routine organizational ritual that deflected the problem of responsibility further. As Robert Hotz, who had been on the *Challenger* review board put it, "you hang it on the procedures and the organization. The manager is automatically removed" (Leusner 2003). Because this would have happened anyway, as a sacrificial propitiation, it meant nothing.

Eventually a large body of opinion formed in support of the idea, as Barry Render put it, "There isn't one person to blame" (Leusner 2003). Ultimately even the politicians, who were most adamant about holding individuals responsible, such as Representative Dana Rohrabacher of California, a member of the Congressional supervising committee, in the end were persuaded that this was extremely difficult, perhaps impossible, to do. Those who persisted found the 'culture' analysis of the CAIB to be an obstacle. "... I'm trying to get past this 'culture' finding and fix responsibility," Senator Hollings said (Pianin 2003). But he could not.

The debate about responsibility continues on-line as I write this, a year after the event. The discussion is strikingly complex and inconclusive. A significant body of NASA opinion has concluded that Ham was a scapegoat, but also accepted that formal responsibility had to be taken by decision-makers to satisfy the demand to blame and punish someone, however ritualistically. A new finding by NASA explains why this foam incident was in fact unlike previous foam incidents, in which the foam had "shed" or peeled off in small pieces and fallen down along the tank and largely missed the shuttle. This time, according to the new analysis, a suitcase-sized chunk, propelled with explosive force by liquified air that collected beneath the foam, struck the orbiter, apparently causing a large gash in the wing. If this analysis is correct, the event was genuinely anomalous, and even more firmly beyond the assignment of responsibility.

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NOTES

[1] I omit any discussion here of the communications problems that resulted from those back channel dealings in which Ham's response may have been misunderstood by lower level engineers to mean that the issue had been taken care of. This is discussed in detail in Langewiesche (2003: 81–2).

[2] A parallel case is presented by Charles Thorpe (2003: 539–46) in a discussion of the culpability of J. Robert Oppenheimer for defective advice with respect to the hydrogen bomb, which he was suspected

of having opposed for political reasons. This long-discussed, never resolved, case shows the difficulty of assigning responsibility for technical opinion.

[3] Phil Engelauf, the formally responsible senior engineer, explained the response to the foam problem in this way: "We've had incidences of foam coming off the tank throughout the history of the program and the same management processes that I think got us comfortable that that was not really a safety of flight issue have been allowed to continue, rightly or wrongly." The problem had been analyzed by Boeing, but, as he put it, "we got the wrong answer on the analysis"(Harwood 2003). The analysis, of course, came with many caveats and was not literally "wrong". The error was a meta-expert error, taking the available facts to be sufficient reason to ignore the foam problem.

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