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The Construction of Knowledge in Organizations: Asking the Right Questions about the Challenger

Dorothy A. Winsor

GMI Engineering & Management Institute

Previous research on the communication failures contributing to the Challenger's explosion tends to ask why it happened that various people in the organizations involved knew about the faulty O-rings but failed to pass on the information to decision makers. This is a faulty question, revealing assumptions many of us unconsciously share even when we consciously reject these assumptions. This question implies a simplistic notion of knowledge and a conduit model of communication. Insights from the sociology of technology and the new rhetoricians can help us to form better questions about rhetoric in organizations.

The hearings of the Presidential Commission investigating the Challenger's explosion produced revelations that were, for many of us, almost as shocking as the explosion itself. As I listened to those hearings, I found myself asking the question "Why did it happen that various people in the organizations involved knew about the faulty O-rings that caused the Challenger to explode but failed to pass on the information to decision makers?" The hearings made it possible to put together a narrative which gave one kind of answer to this question (Winsor), but it was hard to explain the actions the narrative described. It seemed that personnel from NASA and Morton Thiokol, Inc. (MTI)—one of NASA's contractors—either knew about the problem and failed to act or should have known but ignored plain evidence. If one believes that the people responsible for the Challenger's launch knew it was likely to fail and took no steps to prevent launch, then the only explanation seems to be that these people were unethical (Dombrowski). If one believes that the people responsible did not know, although evidence was available which indicated the likelihood of failure, then the only explanation seems to be that these people were incompetent (Gouran, Hirokawa, and Martz). In either case, future failures could be prevented only by removing unethical or incompetent employees—in effect the action which NASA and MTI both eventually took (Seeger 155). Few of us would be entirely comfortable, however, claiming that with the removal of the people responsible for the Challenger's launch and with the careful hiring of replacements, NASA and MTI automatically became better at constructing and communicating knowledge.

Many of the articles concerning the events leading up to the Challenger's explosion contain valuable insights, but researchers seem to have trouble concluding much beyond the assertion that people knew, or should have known, but failed to act. Goldzwig and

Dionisopoulos, for instance, state that “the space tragedy did seem inevitable, not because of some technical flaw but because of continued and prolonged malfeasance and mismanagement . . .” (187). Likewise, Rowland says, “It is not merely that NASA ignored warnings about the O rings, but that the warnings were not even reported to the top decision makers responsible for the launch decision” (138). Explanations for why people would ignore or fail to report such vital information are weak or nonexistent. In this article, I would like to propose that our explanations are weak because our questions are weak, with my own question, as cited previously, being a prime example of a weak question. The object of my discussion is less to throw light on the Challenger’s explosion as such than to demonstrate the difficulty we have in bringing our theoretical understanding of the uncertain, socially conditioned nature of discourse to bear on concrete incidents.

At least two parts of my question are faulty—the words “knew” and “pass on the information.” This faultiness is not random or accidental in origin. Rather, it reveals assumptions about epistemology and communication that many of us unconsciously share even when we consciously reject these assumptions, and it is these assumptions that have led us astray. Our unconscious fidelity to such assumptions impedes our understanding of events like the Challenger’s explosion and leaves us with vaguely unsatisfactory articles in our journals and hopelessly shallow advice to offer those who hope to prevent a similar occurrence. With these problems in mind, I will analyze the difficulties with *knowing* and *passing on the information*. In doing so, I will employ the work of a group of European sociologists who have examined the social construction of scientific and technical knowledge. Their work can help us potentially to clarify our thoughts about how people in groups come to know something. These sociologists’ work and my analysis of this work carry implications for the ways we think about communication and the construction of knowledge in organizations.

What Does It Mean to Know Something?

What does it mean to say that the engineers at MTI *knew* about the O-rings? As I will show, researchers on the Challenger’s explosion seem to suggest that the engineers had become certain of the O-rings’ flaws because these engineers had seen evidence which clearly demonstrated the flaws. For my argument, two elements here are especially important. First, knowledge seems to be based on evidence, in contrast to such necessarily misleading factors as what co-workers thought or what was politically expedient. Second, knowledge is regarded as certain; if someone is still unsure of an idea, we don’t usually call the idea knowledge. This epistemology of logical positivism is critiqued by Karen LeFevre in relation to the creation of ideas and by Carolyn Miller in relation to the communication of ideas. These two studies criticize the belief that an individual achieves knowledge through the simple contemplation of evidence. In addition, these

studies cast even stronger doubt on the belief that organizations achieve shared knowledge through the simple contemplation and communication of evidence.

In order to evaluate the epistemology of logical positivism as it relates to the construction of knowledge about the Challenger's explosion, I would like to detour briefly through work done by sociologists Nigel Gilbert and Michael Mulkay. This work examines scientists' explanations of how knowledge is achieved in a community of experts. In *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse*, Gilbert and Mulkay interviewed prominent biochemists involved in a controversy and asked them why other biochemists disagreed with them (63-89). In effect, Gilbert and Mulkay were asking these biochemists to account for the distribution of what the biochemists regarded as knowledge and error. The biochemists' answers formed a pattern: These scientists said that their own "correct" beliefs were formed by looking at evidence, while the "erroneous" beliefs of other scientists could be blamed on factors extraneous to evidence. For example, the scientists with "erroneous" beliefs may study at laboratories where the people share a restricted point of view; or they may lack access to the latest instrumentation or the best materials or methodology; or they may need to acquire specific results in order to satisfy certain grant terms. This pattern of judging that one's own views are based solely on evidence while judging that opposing views are improperly influenced by social factors appeared throughout the interviews Gilbert and Mulkay conducted. The fact that studying at another laboratory or having access to state-of-the-art instruments or conforming to grant terms constitutes extra-evidential factors also for the scientists with "correct" beliefs does not seem to have occurred to this group of scientists.

As is readily seen, the views of these biochemists parallel the reasoning many researchers use in talking about the Challenger's explosion. We tend to believe that the engineers who suspected problems with the O-rings based their views on evidence and that those managers who advocated launch were reacting only to factors such as pressures to maintain tight launch schedules and worries about possible congressional cuts in funding. Gouran, Hirokawa, and Martz, for instance, say that on the night before the Challenger's launch, MTI's managers decided to approve the launch because NASA had questioned their original recommendation for delay: "The perception of external pressure from [NASA] eventually led to a state in which the Thiokol managers succumbed to a self-induced pressure" (123). Seeger judges that NASA's managers knew about the dangers but ignored that knowledge: "The explosion of Challenger was all the more disturbing in light of the fact that administrators were aware of the flaws yet pushed for launch" because these administrators had established programs which were "overly ambitious and created high levels of performance pressure for NASA" (152). These researchers, then, see the problem as having been caused by managers who based their actions on factors other than evidence about the flawed O-rings.

While organizational pressures probably did influence the managers at both NASA and MTI, this does not mean these managers made decisions contrary to evidence. The managers, like the engineers, had some evidence on their side, although—as Herndl, Fennell, and Miller demonstrate through their use of research in discourse theory—what counted as evidence differed for the two groups: The managers looked at past performance and the engineers looked at test results. Thus, in assessing the effect of cold weather on the O-rings, the managers looked to see what the air temperatures were for previous launches which resulted in O-ring damage. They found damage at temperatures which seemed scattered randomly from 53° F to 75° F and concluded that different temperatures were therefore not the deciding factor (Presidential Commission 1: 146). As Toulmin points out, a mistaken prediction is not necessarily an improper one (183). The managers' prediction was mistaken but not unfounded on evidence. (See Gouran, Hirokawa, and Martz 125-29 for a critical examination of the reasoning of Challenger's managers.)

Moreover, MTI's engineers, who we now think came to the right conclusion, were also influenced by factors other than evidence. For example, evidence that the O-rings might fail to seal crucial joints began to appear about two years before the Challenger's launch. In response to this evidence, MTI eventually formed a task force whose charge was to find and correct any problems associated with the O-rings. While the formation of the task force was triggered by evidence, however, this formation was crucial in shaping the task-force members' views. Once the task force had been formed in the summer of 1985, its members increasingly began to see their work as crucial to the safety of the shuttle. The task-force members' sense of urgency plainly appears, for instance, in the following activity report written by an MTI engineer approximately four months before the Challenger's explosion. This engineer is frustrated by what he sees as complacency throughout the rest of MTI:

The [task force] generally has been experiencing trouble from the business as usual attitude. . . . Part of this is due to lack of understanding of how important this task team activity is and the rest is due to pure operating inertia which prevents timely results to a specific request.

The team met with Joe Kilminster on 10/3/85 to discuss this problem. . . . The problem was . . . explained to require almost full time nursing of each task to insure it is taken to completion by a support group. Joe simply agreed and said we should then nurse every task we have.

He plain [sic] doesn't understand that there are not enough people to do that kind of nursing of each task, but he doesn't seem to mind directing that the task never-the-less gets done. . . .

The vendors we are working with on seals and spacer rings have responded to our requests in a timely manner yet we (MTI) cannot get a purchase order to them in a timely manner. . . .

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I might add that even NASA perceives that the team is being blocked in its engineering efforts to accomplish its tasks. NASA is sending an engi-

neering representative to stay with us starting Oct 14th. We feel that this is the direct result of their feeling that we (MTI) are not responding quickly enough on the [O-rings'] seal problem. . . .

Finally, the basic problem boils down to the fact that . . . upper management apparently feels that the [shuttle] program is ours for sure and [NASA] be damned. (Presidential Commission 1: 254-55)

This highly contentious document confirms that there was a split between the managers and the engineers in thinking as well as in organizational roles. Notice, for instance, the report writer's use of the pronoun "we." This is an internal MTI document. Yet in the fourth and fifth paragraphs, the writer identifies "we" in parentheses as "MTI," apparently to distinguish it from the report's other uses of "we" to mean the task force. The writer seems scarcely able to talk about the task force as part of MTI.

LeFevre has examined the degree to which the invention of ideas is affected by group membership. "Invention," she writes, "is a dialectical process in that the inventing individual(s) and the socioculture are co-existing and mutually defining" (35). The engineers who made up the task force affected the nature of the task force, but this task force in turn affected the thinking of the engineers. All of the MTI documents which strongly questioned the O-rings came from task-force members. Did this situation mean that anyone who was worried got assigned to the task force? Or did it mean that membership in the task force—which was, after all, charged to find and correct problems—made it much more acceptable to be worried? One of the premises of social constructionism is that an idea counts as knowledge only when it is socially validated (Latour 1-17). It is, of course, possible to say that the task force was formed because the doubts about the O-rings' reliability were getting stronger, but it is also possible to say that the doubts got stronger because the task force was formed. The task-force members' support of one another may have served to convince them that their doubts were justified. To know, then, is not simply to attend solely to evidence while ignoring social influences, for the very perception of evidence is always shaped by social factors.

Considering these epistemological difficulties, how are we to achieve certainty that what we *know* actually constitutes knowledge, in light of the competing "knowledges" that different social groups create? In their research with scientists, Gilbert and Mulkay asked the scientists they interviewed how certainty about knowledge could ever be achieved. In their answers, the scientists used what Gilbert and Mulkay called the "Truth Will Out Device" (TWOD), meaning that the truth would outlast all competing ideas and thus would eventually be recognized (90-111). These scientists believe that knowledge is clearly recognizable when consensus is eventually reached, especially as that consensus becomes formulated in research publications. The TWOD enables these scientists to be certain about knowledge, but only retrospectively: One waits and sees what ideas survive and then declares that these ideas were knowledge all along,

