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Theorizing disaster

Analogy, historical ethnography, and the *Challenger* accident

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A B S T R A C T ■ Building explanations from data is an important but usually invisible process behind all published research. Here I reconstruct my theorizing for an historical ethnography of the 1986 Space Shuttle *Challenger* disaster and the NASA (National Aeronautical and Space Administration) decisions that produced that accident. I show how analogical theorizing, a method that compares similar events or activities across different social settings, leads to more refined and generalizable theoretical explanations. Revealing the utility of mistakes in theorizing, I show how my mistakes uncovered mistakes in the documentary record, converting my analysis to a revisionist account that contradicted the conventional explanation accepted at the time. Retracing how I developed the concepts and theory that explained the case demonstrates the connection between historic political and economic forces, organization structure and processes, and cultural understandings and actions at NASA. Finally, these analytic reflections show how analysis, writing, and theorizing are integrated throughout the research process.

K E Y   W O R D S ■ disaster, space shuttle, deviance, technology, culture, *habitus*, ethnography, analogy, institutional theory

When NASA's Space Shuttle Challenger disintegrated in a ball of fire 73 seconds after launch on 28 January 1986, the world learned that NASA was not the pristine citadel of scientific power it had seemed. The Presidential Commission appointed to investigate the disaster quickly uncovered the cause of the technical failure: the O-rings that seal the Solid Rocket Booster joints failed to seal, allowing hot gases at ignition to erode the O-rings, penetrate the wall of the booster, and destroy *Challenger* and its crew. But the Commission also discovered a NASA organization failure of surprising proportion. In a midnight hour teleconference on the eve of the *Challenger* launch, NASA managers had proceeded with launch despite the objections of contractor engineers who were concerned about the effect of predicted cold temperatures on the rubber-like O-rings. Further, the investigation indicated that NASA managers had suppressed information about the teleconference controversy, violating rules about passing information to their superiors. Worse, NASA had been incurring O-ring damage on shuttle missions for years. Citing 'flawed decision making' as a contributing cause of the accident, the Commission's Report (Presidential Commission on the Space Shuttle *Challenger* Accident, 1986) revealed a space agency gone wrong, forced by budget cuts to operate like a cost-efficient business. Apparently, NASA managers, experiencing extraordinary schedule pressures, knowingly took a chance, moving forward with a launch they were warned was risky, wilfully violating internal rules in the process, in order to launch on time. The constellation of factors identified in the Report – production pressures, rule violations, cover-up – indicated amorally calculating managers were behind the accident. The press fueled the controversy, converting the official explanation into historically accepted conventional wisdom.

These revelations attracted my attention. Always fascinated by the dark side of organizations, in 1986 I began to investigate the political, economic, and organizational causes of the disaster. This research culminated in a book, *The Challenger Launch Decision: Risky Technology, Culture, and Deviance at NASA* (Vaughan, 1996). Contradicting the Report in both fact and interpretation, I concluded the accident resulted from mistake, not misconduct. In 'Revisits', Burawoy (2003) writes about the ethnographic revisit, in which the researcher returns to the field site for another look. It could be the next day or ten years hence – or possibly another researcher visits the same site, seeking to depose the first. Exploring the variety of revisits, Burawoy identifies the archeological revisit: the ethnographic practice of digging into the past, deliberately reconstructing history in order to identify and then track the processes connecting past and present. Distanced from action by time and space, the ethnographer working in this mode relies, to a greater or lesser extent, on documentary records. My NASA research was an archeological revisit – an historical ethnography – but this article engages me in a different kind of a dig. I return not to my research site, but to my research experience to think reflexively about my interpretive practices as I theorized disaster in a revisionist account published in 1996.[1]

Theorizing is the process of explaining our data; theory is the result. In this article, I focus on theorizing, retracing how I developed the concepts and theory that accounted for this event, showing the utility of analogical comparison, mistakes, and documentary and historical evidence in my theorizing. Too often we read only the finished product of research, the theory fully formed and perfectly polished, while the cognitive manoeuvres behind that theoretical explanation remain invisible.[2] Perhaps it is because we are not trained to think about how we theorize as we arrive at certain interpretations and theoretical conclusions.[3] Perhaps it is just difficult to articulate an intuitive cognitive process that is tacit knowledge. Perhaps it is because the path to developing theory is through making mistakes and that publicly admitting our mistakes is not easy.[4] Ironically, the documentary record that made my research possible also led to my mistakes. Significantly, my mistakes were about social factors that were central to my explanation. So it is useful for the methods of ethnographers engaged with history to think reflexively about the construction of the documentary sources I used, how I read culture, structure, and history in that archival record, and the mistakes, contradictions, and differences that drove my frequently shifting explanation.

These analytic reflections have relevance for all ethnographers, however. They reveal analogical comparison to be a useful method for elaborating theory.[5] To the extent that all ethnography can be conceptualized as ethnography-as-revisit, analogical comparison and theorizing is foundational to the enterprise. Second, although certain problems I faced are distinctive because of the peculiarities of the organization and event I was studying, the social factors that were important to my analysis are found in all social settings. Following the trail of my mistakes shows how the same social factors that explain our research questions can be obstacles to our analysis. Yet we benefit from recognizing the sources of misunderstanding: mistakes are the turning points in the research process that open up cultural meaning making and symbolic understandings, driving the development of theory.

Analogical theorizing, mistakes, and historical ethnography

In a late night epiphany in 1981 as I reworked my dissertation on organizational misconduct for publication, I discovered that my own process of theorizing was analogical. I was revising three not-very-good, disconnected literature chapters when I saw that my case study data resonated with Merton's Anomie Theory (1968), which he developed to explain rates of individual deviance. With growing excitement I dived into Merton's writing, weighing every aspect of his scheme against the details of my case and the published research on corporate crime, ultimately reorganizing and converting my three lacklustre, stand-alone chapters into three interrelated parts of a causal theory (Vaughan, 1983: 54–104). Not only did the major components of Merton's theory fit the data on organizations, but the comparison showed differences that allowed me to critique and reconceptualize his theory, which, as it turned out, better explained the deviance of organizations than that of individuals. I realized that what I had done was switch units of analysis, taking a societal level theory designed to explain individual deviance and applying it to organizations. It worked! But why?

As a graduate student, I was strongly influenced by Simmel's argument that the role of the sociologist is to extract essential social forms from content, as he so brilliantly did in his writing, in particular with 'dyads and triads' (Wolff, 1950). Returning to Simmel, I noted that his position legitimized developing theory by comparing analogous events, activities, or incidents in different social settings! Theorizing by analogical comparison also made sense to me because forms of social organization have characteristics in common, like conflict, hierarchy, division of labor, culture, power and structured inequalities, socialization, etc., making them comparable in structure and process. I concluded that it was sociologically logical to, for example, develop a theory of organizational dissent, defined as one person speaking out against authority, from such seemingly disparate cases as the corporate whistle-blower, the prison snitch, sexual harassment, and domestic violence (Vaughan, n.d.). Searching for precedent, I found a neglected passage in Glaser and Strauss (1967) that suggested comparing similar activities in different social settings as a way of formulating general theory. With few exceptions, however, grounded theory had evolved in practice to explain a single case, or multiple incidents within a case, the comparison being limited to the back-and-forth interplay between data and the case explanation rather than developing general theory. I had theorized from the ground up, as their model suggested, but it did not fully explain what I had done. Grounded theory tied scholarship to the local, with no directions about pursuing the structural or political/economic contexts of action. Also, Glaser and Strauss suggested that having a theory in mind invalidated the procedure. Finally, their inductive method gave no insights about the cognitive principles involved in theorizing itself.

Fascinated to discover how other people theorized, I turned to the classics, finding that analogical theorizing across cases was frequent but unacknowledged by those who used it (e.g. Blau, 1964; Coser, 1974; Goffman, 1952, 1961, 1969; Hirschman, 1970; Hughes, 1984). Stinchcombe (1978) discussed the search for analogy and difference as a method for social history, but for units of analysis belonging to the same class of objects (e.g. all nation states). My own experience convinced me that not only was analogical case comparison useful for theorizing across different cases, but also that analogy drove our more spontaneous tacit theorizing:

linking a known theory or concept to patterns in our data, deploying examples, even the simple act of citation. I was taught in graduate school to theorize by comparing all hospitals, or all nation states, or all families. I was taught that in case analysis, you start 'theory free'. I was taught that you can not generalize from a case study. I was no longer convinced. I believed that if analogical comparison, which I and other scholars were intuitively using to theorize, could be made explicit and systematic, the cognitive processes underlying it could be identified and taught.

So my experiment in analogical theorizing began. By the time of the 1986 *Challenger* accident, it had progressed to a book-in-progress that compared corporate crime, police misconduct, and domestic violence as a step toward developing a general theory of organizational deviance and misconduct. From experience with the three cases, I had arrived at the following working principles (for elaboration, see Vaughan, 1992). A case is chosen because an event or activity appears to have characteristics in common with other cases, but also because the social setting varies in size, complexity, and function. The individual case must be explained first, however, for it may not turn out to be an example of what we thought. Thick description produces the detail that guarantees discovering differences, thus guarding against forcing the case to fit a theory or a previous case. The cross-case comparison is done after the case analysis, but the way is paved at the outset by loosely sorting data for the new case into categories known to be associated with the comparison cases, thus drawing attention to analogies and differences as the analysis progresses.

Moreover, each case is analyzed using a combination of diverse qualitative methods known to illuminate differences as well as similarities: a) analytic induction (Robinson, 1951; Znaniecki, 1934), b) Blumer's (1969) sensitizing concept, and c) Glaser and Strauss's (1967) grounded theory, the latter amended to acknowledge that we always have some theories, models, or concepts in mind; by making them explicit we are enabled to either reject, reconceptualize, and/or work toward more generalizable explanations. Once the case analysis is complete, then we do the cross-case comparison, searching for structure and process equivalences. But differences also matter. I had learned that selecting cases to vary the social setting (corporation, police department, family) produces different kinds of data – historical, political, economic, organizational, social psychological. Thus, the end result has a distinctive sociological scope: a general theory that situates individual interpretation, meaning, and action in relation to larger complex and layered forces that shape it (see Vaughan, 1998, 2002).

Coincidentally, when the *Challenger* accident occurred I was looking for a case of misconduct by a complex organization that was not a corporate profit-seeker to add to my project. The data analysis was guided by my 1983 theory, which can be summarized thus: the forces of competition and scarcity create pressures on organizations to violate laws and rules

(Vaughan, 1983, Chapter 4); organization structure and processes create opportunities to violate (Chapter 5); the regulatory structure systematically fails to deter (Chapter 6), thus the three *in combination* encourage individuals to engage in illegality and deviance in order to attain organization goals. To draw attention to analogies and differences, I used these three causal principles as sensitizing concepts to organize the data. But the data dragged me in new directions, changing the project in its theoretical explanation, size, and method.[6] Although the case seemed at the outset to be an exemplar of organizational misconduct, I was wrong. It was mistake, not misconduct. In the process of getting from one theoretical explanation to the other, the analysis outgrew my first idea for a chapter in a book of four case comparisons, outgrew my second idea for a slender volume that would be done in a year, and finally ended as a 500-page book that I had to rush to complete by the accident's ten-year anniversary.

Analytic induction, which forces researcher attention to evidence that does not fit the hypothesis, is nothing more nor less than learning by mistake. Repeatedly, I came across information that contradicted both my factual and theoretical assumptions, keeping me digging deeper and deeper, so the analysis was changing all the time. I was forced by confusion and contradiction from Volume 1 of the Commission's Report to Volumes 4 and 5, containing transcripts of the public hearings, and to NASA documents describing procedural requirements. A critical turning point came in the 13th month of the project. To determine whether this case was an example of misconduct or not, I had decided on the following strategy: Rule violations were essential to misconduct, as I was defining it. The rule violations identified in Volume 1 occurred not only on the eve of the launch, but on two occasions in 1985, and there were others before. I chose the three most controversial for in-depth analysis. I discovered that what I thought were rule violations were actions completely in accordance with NASA rules! This was not my last mistake, but it was perhaps the most significant because the Commission's identification of rule violations was the basis for my choice of the launch decision as an example of organizational misconduct. My hypothesis went into the trash can, and I started over.

My discovery of the Report's mistaken assertion of rule violations transformed my research. I now suspected that NASA actions that outsiders – the Commission, the press, the public, me – identified as rule violations and therefore deviant after the accident were defined as non-deviant and in fact fully conforming by NASA personnel taking those actions at the time. Immediately, the research became infinitely more complex and interesting. I had a possible alternative hypothesis – controversial decisions were not calculated deviance and wrongdoing, but normative to NASA insiders – and my first inkling about what eventually became one of the principle concepts in explaining the case: the normalization of deviance. The Commission identified 'rule violations' related to the Solid Rocket Boosters from the beginning of the Space Shuttle Program. Were these alleged rule violations true violations? Or would investigating them reveal the gap between outsider and insider definitions of these actions, too? I realized that understanding NASA culture and the meaning of events to insiders as they made decisions would be crucial. I shifted my focus from the 1986 launch and my singular examination of rule violations and began reconstructing a chronology of all decision making about the Solid Rocket Boosters (SRBs), 1977–85.

Thus, the research became an historical ethnography: an attempt to elicit structure and culture from the documents created prior to an event in order to understand how people in another time and place made sense of things. My work was in harmony with the work of many social historians and anthropologists who examine how cultures shape ways of thinking by analyzing documents. However, my research was distinctly ethnographic in the centrality of culture and the theoretically informed sociological/ethnographic writing and interpretation of it. My purpose was to connect the past to the present in a causal explanation. I wanted to explain individual meaning making, cultural understandings, and actions on the eve of the *Challenger* launch in relation to a) previous SRB decisions and b) historic institutional, ideological, economic, political, and organizational forces. In contrast to some archaeological revisits that focus on social change across generations,[7] my research setting was distinctly modern: a complex organization in which the technology for producing records and the process of record keeping were valued, thus creating the artifacts for its own analysis. But the research still would not have materialized were it not for the fact that the accident was a politically controversial, historical event. A Presidential Commission was convened with the power to mandate the retrieval of all documents related to the SRBs, require technicians, engineers, managers, administrators, astronauts and contractors to testify in public hearings, and later deposit evidence at the National Archives, Washington DC. The available data were certainly not all the evidence; however, far more were publicly available than for previous research on alleged or confirmed cases of organizational misconduct, where the usual problem is getting access to written records. More important was the unique content of the archival record, which allowed me to track the cultural construction of risk at NASA for nearly a decade, making historical ethnography possible.

My data sources were over 122,000 pages of NASA documents catalogued and available at the National Archives; Volumes 1, 2, 4, and 5 of the Report, with Volumes 4 and 5 alone containing 2500 pages of testimony transcripts from the Commission's public hearings (Presidential Commission, 1986) and the three-volume Report of the subsequent investigation by the

Committee on Science and Technology, US House of Representatives, which included two volumes of hearing transcripts (US Congress. House. Committee on Science and Astronautics, 1986a, 1986b). In addition, I relied upon transcripts of 160 interviews conducted by government investigators who supported Commission activities, totaling approximately 9000 pages stored at the National Archives. These were important because separate interviews were conducted for each person on the two topics that interested me: the *Challenger* teleconference and the history of SRB decision making. Nearly 60 percent of those interviewed by these investigators never testified before the Presidential Commission. Video recordings of all public hearings, available at National Archives' Motion Picture and Video Library, aided my interpretation of hearing transcripts. Using the Freedom of Information Act, I obtained copies of engineering risk assessments used in NASA's pre-launch decision making for all shuttle launches. Also, I conducted interviews in person and by telephone. Primary sources were key NASA and contractor personnel involved in SRB decisions, a Presidential Commission member, and three staff investigators. After initial interviews, all remained sources whom I consulted throughout the project as needed. I also interviewed NASA safety regulators, journalists, secretaries, space historians, and technical specialists, many of them more than once. The result was numerous conversations with the same people throughout the project that makes any tally of 'number of interviews' impossible.

Theorizing: turtles all the way down

Clifford Geertz tells this Indian story to draw an analogy with ethnography:

> An Englishman who, having been told that the world rested on a platform which rested on the back of an elephant which rested in turn on the back of a turtle, asked (perhaps he was an ethnographer; it is the way they behave), what did the turtle rest on? Another turtle. And that turtle? 'Ah, Sahib, after that it is turtles all the way down.' (Geertz, 1973: 28–9)

Geertz tells the story to point out that cultural analysis is necessarily incomplete, and the more deeply it goes, the less complete it is. When historical ethnography combines with a layered structural analysis that frames individual action and meaning making in a complex organization and its historic, political, economic, and institutional context, the result is sure to be, as the Indian said, 'turtles all the way down'. What matters is going beyond the obvious and dealing with the contradictions produced by going below the platform and the elephant. Here I show how going deeper into the archival record uncovered mistakes of fact and interpretation in Volume

1 of the Report, revealing NASA culture and the meaning of actions to insiders. I show the utility of mistakes in theorizing by tracing how my own mistakes revealed the Commission's mistakes and led me away from misconduct to mistake as an explanation. In keeping with the working principles of analogical theorizing, after explaining the case I discuss the theoretical results of comparing the NASA case with other cases in the conclusion.

I began the research analyzing newspaper accounts of the Presidential Commission's public hearings, but when the 250-page Volume 1 was published in June 1986, I treated it as primary data, a mistake on my part. It was far superior to press accounts, but when the other four volumes and data at the National Archives became available in September, I recognized it for what it was: a summary and the Commission's construction/interpretation of original documents, testimony, and interview data. The discursive framing and data in Volume 1 misled me on many issues. From the outset, culture was a central research question: was NASA's a risk-taking culture, where production pressures pushed schedule ahead of safety, as the Report implied? Culture was the question, but culture was also an obstacle to my analysis. Understanding events at NASA depended upon my ability to grasp NASA's technology, organization structure, bureaucratic and engineering discourse, and norms, rules, and procedures.

Immediately I had problems translating the technology and technical discourse (Figure 1). I knew nothing about engineering or shuttle technology. Volume 1 was full of illustrations and explanations for the lay reader of how the technology worked, so I began with the utmost confidence that I would be able to master the necessary technical information. I underestimated the challenge. Much of it was seat-of-the-pants learning: I studied memos and engineering documents, including the engineering charts showing SRB risk assessments for all launches. The interview transcripts at the Archives and public testimony were helpful because in them engineers and managers carefully and patiently tried to explain to confused government investigators and Commission members how the technology worked and why they decided as they did. Also, a NASA engineer, Leon Ray, and a contractor engineer, Roger Boisjoly (both with long experience working on the O-rings and key players in the post-accident controversies), helped me over the hard spots in telephone conversations over the years.

Uncovering cultural meanings also required translating NASA's bureaucratic discourse, a mind-numbing morass of acronyms and formalisms, designed for social control of both technological risk and people. By the documents reproduced in Volume 1 and the Commission's interpretation, the Report portrayed a culture of intentional risk-taking. But was it (Figure 2)? Commission member Richard J. Feynman expressed astonishment at finding the words 'acceptable risk' and 'acceptable erosion' in pre-launch engineering charts for the SRBs. Feynman stated that NASA officials were playing 'Russian roulette': going forward with each launch despite O-ring erosion because they got away with it the last time (Presidential Commission, 1986, Appendix F: 1–5). However, I noticed that the Commission had examined engineering charts for the SRBs only; I found the words 'acceptable risk' and 'acceptable anomalies' appearing in charts for other shuttle components throughout the program! At the National Archives, I stumbled across a document that explained this bizarre pattern. Written before the first shuttle launch, it was titled 'The Acceptable Risk Process' (Hammack and Raines, 1981). In it, NASA acknowledged that the shuttle technology, because of its experimental character, was inherently risky. Even after they had done everything possible to assure safety of all technical components before a launch, some residual risks would remain. Prior to a launch, the document continued, engineers had to determine whether or not those residual risks were acceptable – thus the language of 'acceptable risk' appeared in all engineering risk assessment documents. Part of the bureaucratic routine and discourse, 'The Acceptable Risk Process' and the words 'acceptable risk' on all documents indicated that engineering safety procedures had been followed, not violated, as Feynmann thought. For insiders, flying with known flaws was routine and taken-for-granted activity that conformed to NASA rules, not wrongdoing.

NASA's institutionalized rules and procedures were part of the culture and thus critical to my interpretation of it. At the National Archives, a video of the Commission's public hearings brought life and meaning to the hearing transcripts. One example will suffice. In 1985, after extensive O-ring damage during a mission, NASA managers imposed a 'Launch Constraint' on the SRBs. A Launch Constraint is an official status at NASA assigned in response to a flight safety issue that is serious enough to justify a decision not to launch. But NASA's Solid Rocket Booster Project Manager waived the launch constraint prior to each of the shuttle flights remaining in the year before Challenger, without fixing the O-ring problem. The video showed Commission members angered by what they concluded was a violation of the Launch Constraint rule. Repeatedly, Commission members used the word 'waive' as a verb – 'Why would you waive a Launch Constraint?' – their use of it indicating that they were equating 'waive' with 'ignore', or, more colloquially, 'blow it off'. However, digging deeper, I again found NASA rules and procedures that contradicted the Commission's interpretation. I learned that 'waiver' is a noun at NASA. A Launch Constraint is a procedure to assure that some items get an extra review prior to a launch, not to halt flight, as the Commission believed. A waiver is a formalized procedure that, upon completion of the extra review and based on extensive engineering risk assessment, allows an exception to some rule. Waivers are documents, signed and recorded, indicating rules have been

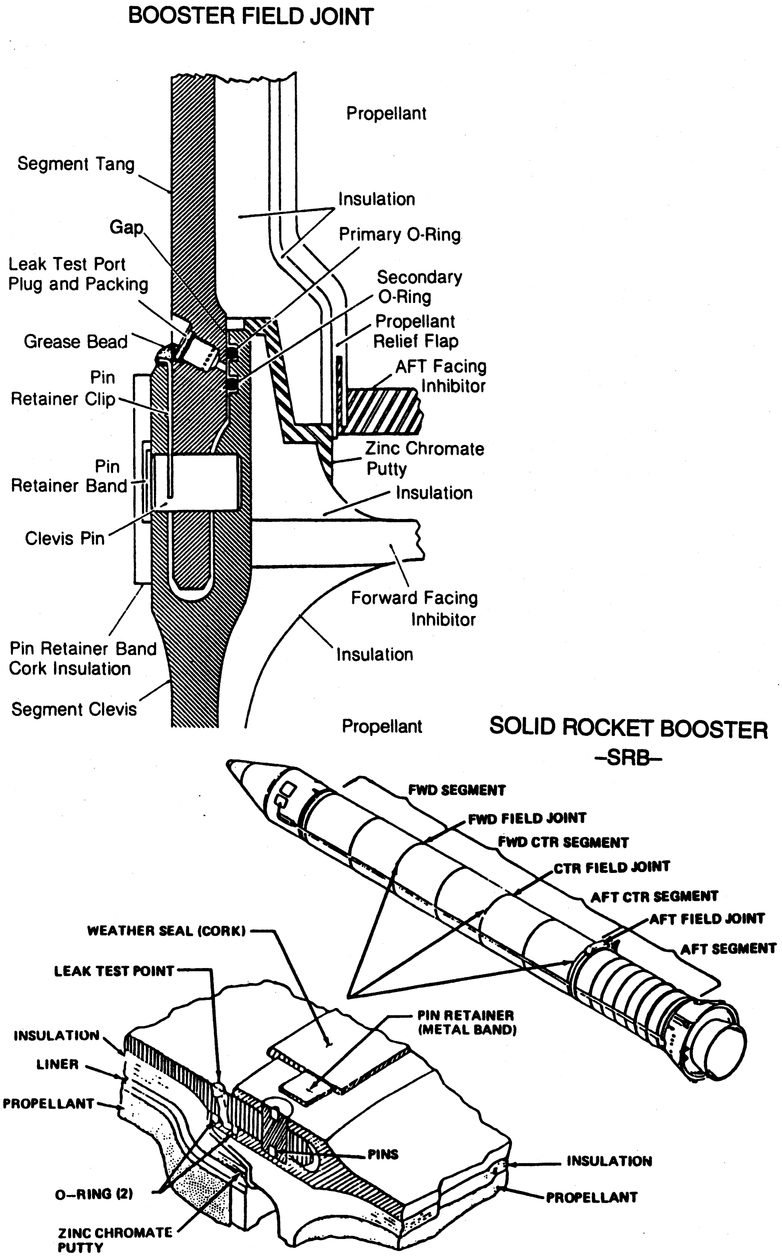

Figure 1 My introduction to the Solid Rocket Booster joint, Presidential Commission on the Space Shuttle *Challenger* Accident, Report, 1986, Volume 1: 57.

followed, not a surreptitious inattention to rules as the Commission concluded.

These discoveries strengthened my conviction that actions that appeared deviant to outsiders after the accident were normal and acceptable in NASA culture. One contradiction between Volume 1 and the archival record sent me in a direction that solidified the normalization of deviance as a concept. In the Report's discursive frame, managers were portrayed as the bad guys, production-oriented and ignoring dangers in order to meet the schedule. Engineers were the good guys, safety-oriented and insisting all along that the design was flawed and needed to be fixed. Reinforcing that dichotomy, Volume 1 reproduced memos and excerpts of memos from worried engineers warning managers about the booster problems long before the *Challenger* launch. As early as 1977, Volume 1 reported, NASA technician Leon Ray wrote a memo stating that the booster design was 'unacceptable'. And in a 1985 memo, contractor engineer Roger Boisjoly warned of impending 'catastrophe' if the design problems were not fixed. The Commission concluded that NASA managers were so dedicated to meeting the schedule

Figure 2 A risk-taking culture. This 1985 photo, which I found at the NASA Photo Archives, Washington DC, show two NASA technicians dressed in surgical scrubs using antiseptic tape to inspect and place an O-ring in its groove in a Solid Rocket Booster joint.

ORGANIZATION: EP25	MARSHALL SPACE FLIGHT CENTER SRM CLEVIS JOINT LEAKAGE STUDY	NAME: LEON RAY DATE: OCTOBER 21, 1977

DESIGN OPTIONS

OPTIONS	REMARKS
1. NO CHANGE	o UNACCEPTABLE -TANG CAN MOVE OUTBOARD AND CAUSE EXCESSIVE JOINT CLEARANCE RESULTING IN SEAL LEAKAGE. o ECCENTRIC TANG/CLEVIS INTERFACE CAN CAUSE O-RING EXTRUSION WHEN CASE IS PRESSURIZED.
2. SHIMS BETWEEN TANG AND CLEVIS (OUTSIDE)	o ACCEPTABLE SHORT-TERM FIX IF PROPER SHIM SIZE IS USED. o PROBABILITY OF ERROR IN CALCULATING PROPER SHIM SIZE. o REQUIRES INCREASED ASSEMBLY TIME FOR SHIM INSTALLATION AND JOINT CENTERING.
3. OVERSIZED O-RINGS	o UNACCEPTABLE SOLUTION - HIGH PROBABILITY OF O-RING DAMAGE OR CLEVIS DISTORTION DURING ASSEMBLY. o DEPARTS FROM RECOMMENDED DESIGN PRACTICES.
4. REDESIGN TANG AND REDUCE TOLERANCE ON CLEVIS	o BEST OPTION FOR LONG-TERM FIX - ELIMINATES USE OF SHIMS WHEN ALL REDESIGNED HARDWARE IS USED. o PREVENTS THE TYPE OF ERROR WHICH COULD RESULT IN CALCULATING JOINT CLEARANCE FOR SHIM INSTALLATION.
5. COMBINATION OF REDESIGN (AS IN OPTION 4) AND USE OF SHIMS	o ACCEPTABLE APPROACH. SHIMS WILL BE REQUIRED IN SOME CASES WHEN REDESIGNED HARDWARE AND PRESENT HARDWARE IS JOINTED. o SHIMS WILL BE DISCONTINUED WHEN PRESENT HARDWARE IS PHASED OUT.

Figure 3 NASA technician Leon Ray's 1977 memo. Report, 'SRM Clevis Joint Leakage Study', NASA, 21 October 1977, PC 102337, National Archives, Washington, DC.

that in the history of decision making, as on the eve of the launch, they had ignored the concerns of their engineers.

Another misrepresentation of the archival record on the Commission's part! When I found Ray's memo, it did not say that the booster design was unacceptable. Instead, Ray wrote that 'no change' in the design was 'unacceptable'. Then he listed a number of design options that would make it acceptable (Figure 3). Moreover, it turned out that Ray later became part of a team that implemented those same design options. Like Ray's memo, Boisjoly's warning of 'catastrophe' held a different meaning in NASA's culture. The word 'catastrophe' was a formalism, stripped of emotional meaning by its function in a bureaucratic tracking system for ranking failure effects by seriousness. 'Catastrophe' was an official category of risk and loss, one of several in a gradient of failure effects that were assigned for each of the shuttle's 60,000 component parts and recorded. Over 700 shuttle parts were assigned the same category as the SRBs. Boisjoly was simply stating the known failure consequences of an item in that category. To NASA managers and engineers, the memo was not the strong warning it appeared to be to the Commission. The words risk and catastrophe were neutralized by repeated bureaucratic use that had routine, taken-for-granted understandings. Testimony and interview transcripts showed that when managers and engineers wanted to convey concerns about risk to each other, they resorted to euphemism: if we do x, we will have 'a long day', or 'a bad day'.

Contradicting the Commission's portrayal of a continuing struggle between managers and engineers, prior to the teleconference Ray and Boisjoly both agreed that the SRBs were an acceptable risk. Further confirmation was forthcoming. Reconstructing the decision history, I discovered a five-step decision sequence in which technical deviations – anomalies found in the booster joint O-rings after a mission – first were identified as signals of potential danger, then, after engineering analysis, were redefined as an 'acceptable risk'. This decision sequence repeated, launch after launch. Here, full blown, was the evidence showing how O-ring erosion repeatedly was normalized! The first decision to accept risk established a quantitative engineering standard that, when followed by a successful mission, became a precedent for future decisions to fly with recurring anomalies. No one was playing 'Russian roulette'; engineering analysis of damage and success of subsequent missions convinced them that it was safe to fly. The repeating patterns were an indicator of culture – in this instance, the production of a cultural belief in risk acceptability. Thus, the 'production of culture' became my primary causal concept at the micro-level, explaining *how* they gradually accepted more and more erosion, making what I called 'an incremental descent into poor judgment'. The question now was *why*.

The surprise was that managers and engineers arrived at these decisions together and agreed. The engineering charts and risk assessments that were the basis for this pattern were created by the same engineers who opposed the *Challenger* launch. Because of the well-documented economic strain and schedule pressures at the agency, the Commission's finding of disagreement between managers and engineers in the years before *Challenger* made sense to me. After all, managers and engineers had different social locations in the organization and were thus subject to and responsible for different organization goals, managers for cost and schedule, engineers for safety. Were engineers bullied into agreement? Were they, too, susceptible to deadline and schedule pressures, in contradiction to the appearance of being defenders of the true and the good, as Volume 1 indicated? In an interview, a NASA manager told me, 'We are all engineers by training, so by training we think alike and our thought processes are alike.' I had been thinking much too locally about the effects of position in a structure. Although differently located in the NASA organization hierarchy, managers and engineers were similarly located in the engineering profession.

From the research on the engineering profession and how those characteristics were made visible in my data, an explanation of the similar viewpoints took shape. Engineers typically work in technical production systems that are organized by the principles of capitalism and bureaucratic hierarchy. Perucci (1970) explains that engineers are trained in the application of technology in production by technical schools and university programs underwritten by corporations and government projects that effectively monopolize technical intelligence. 'Servants of power', they develop a cultural belief system that caters to dominant industrial and government interests. The engineering worldview includes a preoccupation with 1) costs and efficiency, 2) conformity to rules and acceptance of hierarchical authority, and 3)production goals.

Specialization limits professional mobility, so identity and loyalty are tied to the employer. Engineers adopt the belief systems of the organizations that employ them, a transition for which their training prepares them.[8] Engineers expect a workplace dominated by production pressure, cost cutting, and limited resources. Conflict between cost and safety is an ongoing struggle (Zussman, 1985). Decision making is a story of compromise: 'satisficing', not maximizing, is the norm (Simon, 1957). NASA was not a corporate profit-seeker, but as part of a capitalistic system was subject to competitive pressures for space supremacy internationally and nationally that required NASA compete for a chunk of the federal budget. Further, at the inception of the Space Shuttle Program, historic political and budgetary decisions by powerful actors in the White House and Congress slashed NASA budgets and made efficiency the measure of success. To assure continued funding, NASA leaders accelerated the launch schedule and minded costs, thus altering the agency's pure science culture to operate more like a bureaucratic production system – the kind that engineers normally inhabit.

The fit between my data and the ideology of professional engineering showed the connection between the political/economic forces in NASA's institutional environment, the organization, and decisions about the boosters. Analogical theorizing is not restricted to tacking back and forth between cases of similar events in social settings that vary in size, complexity, and function. We import theories and concepts of other scholars as a project progresses either because they are analogical with our data or show a contradiction, in either instance illuminating our analysis. The new institutionalism describes how non-local environments, such as industries and professions, penetrate organizations, creating a frame of reference, or worldview, that individuals bring to decision making and action (Powell and DiMaggio, 1991). The theory has been criticized for its absence of agency, and so its authors proposed Bourdieu's *habitus* as a possible connective piece to explain action at the local level (Powell and Dillaggio, 1991: 15–27; Bourdieu, 1977; Jepperson, 1991). Once a student asked me, 'How do I know *habitus* when I see it?' We see it operating in what people say and do. First, the history of decision making itself was evidence: it was one of compromise between safety, cost, and schedule, in which launches continued while the scarce resources of a budget-constrained agency went to 'more serious' problems and the implementation of a permanent fix for the O-rings was repeatedly delayed. The consensus of managers and engineers about 'acceptable risk' showed the conjunction of the cultural beliefs of professional engineering, the organization culture, and practical action. Second, evidence supporting this theoretical connection was in the verbatim testimony and interviews, which showed NASA managers and engineers expressing the worldview of professional engineering, impressed upon them during their training and reinforced in the workplace. The following examples illustrate, respectively, conformity to bureaucratic ruling relations, satisficing, rules and protocols, cost and efficiency, and production goals:

> And if I look back on it now what I should have done is I should have done everything within my power to get it stopped . . . but, you know, really I'm not of that grade structure or anything. (Engineer, interview transcript, National Archives, 9 March 1986: 28–9)

> Engineering-wise, it was not the best design, we thought, but still no one was standing up saying, 'Hey, we got a totally unsafe vehicle.' With cost and schedule, you've got to have obviously a strong reason to go in and redesign something, because like everything else, it costs dollars and schedule. You have to be able to show you've got a technical issue that is unsafe to fly. And that really just was not on the table that I recall by any of the parties, either at Marshall or Thiokol [the contractor]. (Chief Engineer, Solid Rocket Booster Project, personal interview, Marshall Space Flight Center, Huntsville, Alabama, 8 June 1992)

> The problem was the increasing launch rate. We were just getting buried under all this stuff. We had trouble keeping the paperwork straight, and were accelerating things and working overtime to get things done that were required to be done in order to fly the next flight . . . The system was about to come down under its own weight just because of the necessity of having to do all these procedural things in an ever accelerating fashion. (Manager, Solid Rocket Booster Project, Marshall Space Flight Center, telephone interview, 5 August 1992)

> I was spending far more time each day dealing with parachute problems. This was a serious problem because it had economic consequences. If the parachutes didn't hold, the SRBs were not recoverable and this was expensive. They sank to the bottom of the sea. On the joints, we were just eroding O-rings. That didn't have serious economic consequences. (Manager, Solid Rocket Booster Project, Marshall Space Flight Center, personal interview, Huntsville, Alabama, 8 June 1992)

> No one has to tell you that schedule is important when you see people working evenings and weekends round the clock. (Engineer, interview transcript, National Archives, 14 March 1986: 37)

Similarly located in the engineering profession, managers and engineers shared categories of understanding that were reproduced in NASA's organization culture, affecting the definition of the situation for managers and engineers, driving launches forward. I called these macro-political/economic forces the 'culture of production'. Within the culture of production, cost/schedule/safety compromises were normal and non-deviant for managers and engineers alike. More and more, the explanation of NASA's history of booster decision making was shaping up to be one of conformity, not deviance or misconduct.

Now I had two concepts. The production of culture explained how managers and engineers gradually expanded the bounds of acceptable risk, continuing to fly with known flaws; the culture of production explained why. But a piece of the puzzle was still missing. The O-ring problems had gone on for years. Why had no one recognized what was happening and intervened, halting NASA's transition into disaster? Neither NASA's several safety organizations nor the four-tiered Flight Readiness Review, a formal, adversarial, open-to-all process designed to vet all engineering risk assessments prior to launch, called a halt to flying with O-ring damage. Although the Commission indicated that NASA middle managers had suppressed information, I concluded that structural secrecy, not individual secrecy, was the problem. Everyone knew about the recurring O-ring damage: the question was, how did they define risk? Aspects of structure affected not only the flow of information, a chronic problem in all organizations, but also how that information was interpreted. The result undermined social control attempts to ferret out flaws and risks, in effect keeping the seriousness of the O-ring problem secret.

Patterns of information obscured problem seriousness. In retrospect, outsiders saw O-ring damage as a strong signal of danger that was ignored, but for insiders each incident was part of an ongoing stream of decisions that affected its interpretation.[9] As the problem unfolded, engineers and managers saw signals that were mixed (a launch had damage, engineers implemented a fix, then several launches with no damage signaled that all was well); weak (e.g. damage resulted from a contingency unlikely to recur); and when damage became frequent, signals became taken-for-granted and routine, the repetition diminishing their importance. Organization structure created missing signals, preventing intervention. Safety oversight was undermined by information dependence. In Flight Readiness Review, thick packages of engineering charts assessing risk and day-long arguments at the lowest tier gradually were reduced to two pages and ten minutes by the time they arrived at the top review. By then, the risk assessment was condensed, contradictory data and ambiguity gone. Instead of reversing the pattern of flying with O-ring erosion, Flight Readiness Review ratified it. The structure of safety regulation also resulted in missing signals. External safety regulators had the advantage of independence, but were handicapped by inspection at infrequent intervals. Unless NASA engineers defined something as a serious problem, it was not brought to regulators' attention. As a result of structural secrecy, the cultural belief that it was safe to fly prevailed throughout the agency in the years prior to the *Challenger* launch.

The conventional wisdom and a revisionist account

I had the third concept explaining the normalization of deviance: the production of culture, the culture of production, and structural secrecy. No one factor alone was sufficient, but in combination the three comprised a theory explaining NASA's history of flying with known flaws. The behavior – the normalization of technical deviation on the SRBs – led to a new concept, the normalization of deviance, that explained what had happened as a socially organized phenomenon. This was progress. However, I worried about the surprising number of discrepancies between Volume 1 of the Commission's Report and the archival record. As I learned culture, I was revising history. My book was going to contradict everything in print – including the Report of a Presidential Commission. Careful documentation was essential. I also needed to explain the discrepancies between my account and these others to substantiate my developing argument to myself, first, but also eventually I had to convince readers: what accounted for the Commission's construction of documentary reality? Despite press concerns about cover-up when President Reagan named former Attorney General William Rogers as head, the other Commission members came from diverse backgrounds. Watching videos of the public hearings at the Archives convinced me that the Commission was trying hard to get to the bottom of things. The hearings began in a spirit of peaceful collaboration with NASA, but became harshly adversarial in tone and line of questioning after the Commission learned of the fateful teleconference, about which NASA had not informed them. Throughout the remainder of the hearings, several Commission members displayed emotion ranging from incredulity, disgust, and shock, to outrage, which could not have been feigned.

Turning to investigate the organization of the official investigation, I found that the Commission had made mistakes that, analogous to NASA, originated in structural secrecy and production pressure. Time constraints and the resulting division of labor created information dependence. The President mandated that the Commission complete its investigation in three months. They elected to conduct public hearings in which they interviewed witnesses, but to expedite the investigation they also recruited experienced government investigators to help them. These investigators conducted 160 interviews that averaged 40–60 pages each when transcribed. The archival database of supporting documents was huge, because the Commission asked NASA for every document related to the SRBs from the beginning of the Space Shuttle Program. From the interview transcripts and collection of documents, these investigators briefed the Commission on what topics were important to pursue and recommended witnesses to be called. In the briefing process, information was condensed, lost, and removed from its context.

A second source of mistakes was hindsight, which biased the sample of evidence the Commission considered and therefore their findings. Knowing of some harmful outcome, the tendency is to focus in retrospect on all the bad decisions that led to it (Starbuck and Milliken, 1988). The government investigators thus suggested calling witnesses who could explain the flawed decisions about the SRBs. Hindsight distorted their selection process: of the 15 working engineers who participated in the eve of launch teleconference, only the seven who opposed the launch were called to testify; those engineers in favor of launching were not. This obscured the complexity of making decisions about the shuttle's experimental technology at the same time it reinforced the evil managers/good engineers picture of the debate that night. Hindsight bias also explains two incidents mentioned earlier: pulling only flight readiness engineering charts for the boosters, not charts for other shuttle parts that would have showed that 'acceptable risk' was on all NASA engineering risk assessments; and taking Leon Ray's memo out of its context in the historical trajectory of decisions, obscuring Ray's later participation on a team that corrected the design problems his early memo identified. All data were available to the Commission by computer. However, time limits restricted their ability to do a thorough reading of the archival record. Instead, Commission members typed in key words or names, a strategy that also severed information and actions from its social, cultural, and historic setting.

The Commission's construction of documentary reality had directly affected mine. The organization of the investigation and hindsight had prevented the Commission from grasping NASA culture. I had duplicated the Commission's errors in my starting hypothesis. Working alone, I could never have amassed the amount of data the Commission did, but tenure gave me a resource they did not have: the gift of time to reconstruct the history of decision making chronologically, putting actions, meanings, and events back into social, historical, and cultural context, revising history, leading me to different conclusions. However, it was now 1992. I had not even begun to analyze the launch decision that initially drew me to this research. I had not predicted my difficulty in learning culture, the many contradictions challenging my main contentions, the constantly shifting terrain of my explanation, or the length of time the analysis was taking. I worked with an uncertainty unknown to me. I was an ethnographer, not an historian, yet I spent years with archival data, constructing a history, but not a normal history, a socio-cultural technical history. The research became a causal analysis, not of a single decision resulting in a cataclysmic event, as I had originally imagined, but of a gradual transition into disaster that extended nearly a decade (1977–86). I had analyzed the longitudinal process of a gradual transition out of intimate relationships by identifying turning points (Vaughan, 1986), but little else in my background prepared me for this. The combination of technology, complex organization, and historical ethnography had me inventing method as I went along.

In addition to the Report volumes of hearing testimony, I had a five-drawer file filled with photocopies of official interview transcripts, engineering charts of risk assessments for Flight Readiness Reviews, and other documents from the National Archives. How to deal with such an unwieldy documentary mass? Studying transitions out of relationships, I had coded interviews, marking key constructs and turning points in the margins, identifying patterns with a system using 4×6 index cards. I could remember who said what, remember the context, and the index cards enabled me to track the patterns. I began coding the *Challenger* interview transcripts, but after a month I realized that if I followed my old method the coding alone would take a year or more. Worse, so much information was there that I couldn't devise a short-cut tracking system that functioned as the index cards had (this was before computerized analytic tools for qualitative research). More important, my previous strategy was ill-suited for this project. Aggregating statements from all interviews by topic (a practice I had often used to identify patterns) would extract parts of each interview from its whole. But memory, which previously had preserved context if not in entirety at least sufficiently to guide me to the appropriate interview transcript, would not suffice with 9000 pages of transcripts. Each person was giving a chronological account of 1) the history of decision making, and 2) the eve of the launch. Keeping the decision stream of actions and interpretations whole was essential to see how people defined risk and why they decided as they did, incident to incident.

So I proceeded chronologically, analyzing launch decisions and other controversial decisions – the turning points – one by one. I examined documents to identify the people who participated in a decision or event and others who knew about it.[10] I compared their testimony and interview transcripts with documents showing what they did at the time, writing from all relevant transcripts and documents for each decision, integrating them to show all actions and perspectives. Because I wanted to know how interpretations varied depending on a person's position in the structure and their specialization, this strategy was complicated by NASA's matrix system, which increased the number of people and points of view.[11] Putting together all these pieces was interesting because the reconstruction of each turning point was shattering the construction of facts in Volume 1 at the same time it was revealing the production and reproduction of the cultural definition of 'acceptable risk' inside NASA. The process was like solving many small puzzles, each one a piece of a larger one. However, the larger one was distant. Analyzing the decision history was essential to making my case, but tedious and time consuming, requiring analysis of many pages of engineering charts of risk assessments for each launch – not exactly a 'good read'. Not only was the process uncertain, it seemed endless. I wondered when I would finish.

Analysis, writing, and theorizing are not separate processes, as we are taught. Some discovery – another technical mistake, a misunderstood procedure, an unforeseen contingency, action, or actor – would require correcting an interpretation in a previous chapter. Jettisoning outline after outline, I began writing the decision history but found myself constantly rewriting. What I intended as one chapter showing how managers and engineers normalized technical anomalies in the years prior to *Challenger* had, by 1992, grown into three chapters. Because observation of actions and culture prior to the accident were impossible, interviews were critically important. My interviewing was driven by the historical chronology, so ebbed and flowed throughout the project. The interviewees, subject matter, and timing were dictated by the gradually unfolding picture and the questions that arose.[12] I deferred interviews with the five key NASA managers until 1992. The Commission's interpretation of these managers' actions was the core of the conventional wisdom. When I began the research in 1986, however, I believed that interviews would not produce anything different than what was already on the public record. Only if I asked them different questions, based on a thorough understanding of the organization, its technology, and the archival evidence, would it benefit me to talk to them. By 1992, when the decision chronology was in decent shape, I felt I could ask informed questions that went beyond what the Commission had asked. The initial interviews, in person and four to eight hours in length, captured both their NASA and Commission experiences in-depth, clarified technical and organizational procedures, tested my interpretation of culture and theoretical explanation, and raised new issues. I did telephone interviews with these managers as needed for the rest of the project.

Even the book's architecture was an experiment. As my analysis began to look more like conformity than deviance, more like mistake than misconduct, I realized my construction of documentary reality would have to contend with the one created by the Commission's Volume 1. How to present my revisionist account? Through trial and error, I settled on a writing strategy that was analogical to my own theorizing process. The first chapter would be persuasive support for the Commission's amorally calculating manager, rational-choice explanation. The chapter would begin with a 5–10-page reconstruction of the eve of the launch teleconference that matched the Commission's historically accepted explanation, followed by the extensive post-accident evidence in the press and Volume 1 establishing NASA's political and economic constraints and the pressures to get the *Challenger* launch off on time. Chapter 2 would be a first-person account in which I dissuaded the reader of the straw argument in Chapter 1. I would walk the reader from my first hypothesis through all my mistakes and the evidence I found that contradicted the conventional wisdom, then lay out the argument of the book. The next chapters would map the interrelated parts of the causal theory. Chapters 3, 4, and 5 on the history of decision making – 'The Production of Culture' – would show how NASA defined and redefined risk, normalizing technical deviations. Chapter 6, 'The Culture of Production', would show the macro-level forces explaining why this normalization continued unabated despite the accumulation of incidents. Then Chapter 7, 'Structural Secrecy', would explain why no one had intervened to alter the definition of the situation.

The last chapter would be 'The Eve of the Launch Revisited'. The book's structure set the launch decision itself in historical context as one decision in a chain of decisions defining O-ring erosion as an acceptable risk. In bold font, I would reproduce verbatim the historically accepted conventional wisdom presented in Chapter 1, but divide it into short segments at critical turning points in decision making. Following each bold font segment, in regular font I would reconstruct that same chunk of time in thick description, using the testimony and interview transcripts of all participants, thereby restoring actions to their full context and complexity. The two constructions of documentary reality, the Commission's and mine, side by side, would be read by many readers who, I assumed, would have begun the book believing as the Commission's Volume 1 and press coverage led me to believe initially: production pressures and managerial wrongdoing. By this last chapter, however, readers would have been led to a different position than they held at the beginning of the book. Writing is teaching. As they read, they would have learned NASA technology, structure, and culture – rules, procedures, allegiance to hierarchy and authority relations, cost/efficiency/safety conflicts, and ideology of professional engineering. They would be acculturated. They would, as much as possible for an outsider, know the native view, or at least my interpretation of it. They would understand this reconstruction. When the moment of the *Challenger* launch arrived in my chronology, readers would know why the launch decision was made, requiring no further interpretation from me. The End.

But even when I thought I was at the end, I was not. I worked on the last chapter, reconstructing this event in a chronological play-by-play of the launch decision from interview transcripts of all 34 participants. I was excited and fascinated by the complexity of my reconstruction and the contrast with the bold font of the Commission's version. In contrast to the arduous writing of technical detail in the three decision-making chapters, I loved recreating this pivotal social scene: where to make the breaks in the stereotyped version; how to write a chronology that showed people on a teleconference in three separate geographic locations where actions were happening simultaneously; incorporating the people omitted from the Volume 1 account who by their presence or absence that night played an important role. I realized that this was the first time I had ever assembled all the data about the eve of the launch teleconference! The act of writing produced still more theorizing. In the second epiphany of my career, when the event was reconstructed I saw how the same factors that explained the normalization of deviance in the history of decision making explained the decision making on the eve of the launch! The production of culture, the culture of production, and structural secrecy worked together, as before, normalizing yet another anomaly – unprecedented cold temperature – and systematically producing a decision to proceed with the launch. I expected that the chapter would show the decision to be a mistake, but I had not imagined the form of the mistake nor that the social causes of previous decisions would be so perfectly reproduced in that fatal decision. It was conformity, not deviance, that caused the disaster. I added Chapter 9, 'Conformity and Tragedy', explaining the fateful teleconference described in Chapter 8 by showing how the patterns of the past were reproduced in that single event. Although the discussion that night was heated and adversarial, the outcome was a cooperative endeavor: all participants conformed to the dictates of the culture of production, thus expanding the bounds of acceptable risk one final time.

Theorizing and theory: history, analogy, and revisits

This revisit has been a doubling back in time to reconsider my process of theorizing disaster and the utility of analogical comparison, mistakes, and documentary evidence in that process. I turn now to what these analytic reflections mean for theorizing, theory, and ethnography. Ethnographers who engage with history have a unique translation problem, in that they theorize culture, structure, and history from documents created by others. When ethnography reaches into history, the completeness or incompleteness of the documentary record affects theorizing. Scarcity and abundance present different challenges. My research was surely unique, both in the volume of original documents available and the fact that they were conveniently located in one place. Although many organizations were involved in this event – three NASA space centers, two contractors, regulatory agencies, the Commission – for documents I only had to travel to the National Archives, where the Commission stashed them, or use the Freedom of Information Act. My problem was abundance, not scarcity. In both circumstances, however, ethnographers must consider what went unrecorded, what documents are missing, and what the effect of this historic sifting and sorting is upon the record available to us. The construction of the surviving documentary record also must always be questioned. Many of the mistakes I made in this research were a consequence of the Commission's framing discourse and data that comprised volume 1 of the Report. Time constraints, the division of labor, and hindsight biased the Commission's sample of evidence; ethnographers reconstructing history must be wary of how these same factors bias their own selection process.

The mistakes I made in this research were not only due to the construction of Volume 1, but also because of my difficulty as an outsider interpreting NASA culture from the documentary record. My mistakes could be explained because NASA was unique – a completely foreign culture to me, and unlike ethnographers who do their research in distant countries, I could not prepare by learning the language or something about the culture in advance because the accident was unexpected. On the other hand, in a practical sense the difficulties I had were hardly exceptional. They originated in factors common to all socially organized settings. Analyzing my mistakes, I realized that the aspects of NASA culture that caused me to stumble were the same factors that explained NASA decisions. The value of mistakes is in recognizing the social source of them. The experience of making mistakes is the experience of being behind; the result, however, is that they drive the explanation ahead.

Some mistakes in theorizing are recognizable prior to publication, when we make what Burawoy (2003) calls the 'valedictory revisit': with some trepidation, we give the completed draft to the cultural insiders as a means of correcting our interpretation. This strategy can be counted on to produce new data in the form of criticism, validation, and visceral emotional reaction. I mailed the manuscript to my NASA and contractor interviewees, following up on the phone. Uniformly, they were surprised, some even shocked, by Chapter 8, 'The Eve of the Launch Revisited'. In three geographic locations for the teleconference, participants' understandings of what happened that night were blocked by structural secrecy that was never remedied. Neither NASA nor the corporate contractor ever got all teleconference participants together after the accident to collectively discuss and analyze the sequence of events during the crisis. Until they read my reconstruction, they only knew what was happening at their location and what others said on the teleconference line. Reading my draft renewed their experience of grief, loss, responsibility, and the wish that they had acted differently. I was surprised that their criticisms were primarily minor and factual. No one contested my interpretation or conclusions, instead saying that I had helped them understand what happened, what they did, and why they did it. The single objection came from Roger Boisjoly, who said, 'You make us sound like puppets.' As the contractor engineer who most vigorously objected to the *Challenger* launch, he was angry. He felt stripped of his capacity to act by my culturally, politically, and historically deterministic explanation.

Some mistakes in theorizing may only be realized years later, on reflexive revisits such as this one. A reviewer for this journal asked if all mistakes were corrected, did no mistakes go unnoticed, were there no flaws in the book? At the time of publication, I felt the book's length, detailed technical information, and theoretical complexity, though necessary, were failings. Would anyone really read a 500-page academic book? Because the book was published on the 10th anniversary of the 1986 disaster, however, it received an extraordinary amount of press attention. The wide readership and positive reception were completely unexpected. NASA engineers, former and current, wrote validating my interpretation, but I heard nothing from NASA officials, a likely result, a space historian told me, of the agency's perennial barrage of criticism, resulting bunker mentality, and unwillingness to take advice from outsiders. Perhaps, but length and complexity also may have been an impediment. More than this, however, the reviewer's question caused me to revisit, not theorizing, but the theory itself. Could it have been different?

I was initially struck by the absence of women in the archival database. None occupied positions shown in the diagrams of NASA and contractor organizations. None testified before the Commission or participated in engineering decisions at any level. Only four women were connected to the accident: *Challenger* astronaut Judith Resnick and Teacher-in-Space Christa McAuliffe, former astronaut and Commission member Sally Ride, and Emily Trapnell of the Commission's investigative staff. Among the factors mentioned in post-accident press speculation about the causes was a 'can-do' attitude at NASA that drove the agency to take risks, but I did not incorporate gender into my explanation. If NASA's culture were a macho, risk-taking culture, then launch delays would have been infrequent. However, delays were so frequent that NASA often was chastised by the press. Indeed, *Challenger* was delayed three times, and *Columbia*, launched before it, was delayed seven times. The very SRB engineers who opposed *Challenger*'s launch had previously initiated a two-month launch delay. I concluded that gender was not a factor driving launch decisions, thinking also that if women had been participating in engineering decisions, they would have been subject to the same cultural beliefs of professional engineering as men. Because of the absence of women's viewpoints in the data, gender was not visible to me. In a perfect example of how the aspects of social settings that explain our research also can be obstacles to understanding it, the testimony and my interviews with men in a male-dominated culture did not enlighten me on this issue. Having 'resolved' the macho culture issue by the frequency of launch delays and the engineering evidence behind those delays, I went no further. Had I sought NASA women employees outside the archival database for interviews (i.e. non-technical staff), I would have been able to further clarify the question.

The final important reason to revisit the theory of the book is to examine the results of analogical theorizing as a method. After explaining the case, the next step is the cross-case comparison. How is this case analogous to and different from the guiding theory, which was an outgrowth of other cases? Have any generic structures and processes been identified? What are the theoretical implications? (For full assessment, see Vaughan, 1996: 395–415.) Recall that the three interrelated parts of the theory of organizational misconduct guiding this analysis worked as follows: historical political/economic forces create structural pressures on organizations to violate; organization structure and processes create opportunities to violate; the regulatory environment systematically fails, thus the three in combination encourage individuals to engage in illegality and deviance in order to attain organization goals. This case was not an example of misconduct as I originally thought: rules were not violated. Still, harm was done. Moreover, NASA's actions were deviant in the eyes of outsiders, and, after the accident, also in the eyes of those who made decisions. Affirming the deviance behind NASA's mistake is the remarkable extent to which the case conformed to the theory. Consider how the explanatory concepts support the generalizability of the theory across cases. The culture of production is analogous to the forces of the political/economic environment: the ideologies of professional engineering and historic shifts in policy decisions of Congress and the White House at the start of the Shuttle Program combined to reproduce in NASA the capitalistic conditions of competition and scarcity associated with corporate crime. The production of a cultural belief in acceptable risk was a key organizational process that allowed NASA to continue launching with flaws. Reinforced by the culture of production, this cultural belief drove launch decisions despite increasing concern about safety as O-ring damage increased. Structural secrecy described how both organization structure and the structure of safety regulation were systematic sources of regulatory failure. They precluded agents charged with monitoring risk assessments from deterring NASA from disaster by suppressing the seriousness of the O-ring problems. Exposing macro-, meso-, and micro-connections, these three factors in combination perpetuated the decisions that resulted in the accident.

How was this case different from other cases? The logic of comparing cases of similar events in a variety of social settings is that each case produces different data, thus bringing into focus social dimensions not previously noted. The NASA case produced differences that elaborated the original theory at all levels of analysis. First, history emerged as a causal factor. Zald has pointed out that organizations exist in history, embedded in institutional environments, and they exist as history, products of accumulated experience over time (1990). History was cause at both the institutional and organizational level, and also a third: the history of precedent-setting decisions about O-ring erosion. This finding shows the importance of longitudinal studies of organization processes, suggesting that historical/documentary evidence might productively be incorporated into traditional ethnographic work in organizations or communities, possibly producing revisionist accounts that transcend other conventional wisdoms.[13] Second, culture comes alive as a mechanism joining political/economic forces, organizations, and individuals, motivating action. My analysis shows how taken-for-granted assumptions, dispositions, and classification schemes figure into goal-oriented behavior in a prerational, preconscious manner that precedes and prefigures individual choice. It affirms a theory of practical action that links institutional forces, social location, and *habitus* to individual thought and action (Vaughan, 1996: 222–37, 402–5, 2002). Third, the case produced extensive micro-level data that showed how unexpected technical deviation was first accepted then normalized at NASA.

This latter discovery shows that analogical theorizing can uncover generic social processes, previously unidentified, that generalize across cases. Although no rules were violated, the normalization of deviance in organizations helps to explain misconduct in and by organizations when it does occur. The persistent question about organizational misconduct is how educated, employed, apparently upstanding citizens can become amorally calculating managers, engaging in illegality to achieve organization goals. The socially organized processes by which deviance was normalized at NASA show how people can be blinded to and insulated from the harmful consequences of their actions because those actions are culturally consistent and conforming within that setting. We see additional evidence of the role of conformity in deviant outcomes in Arendt's *Eichmann in Jerusalem* (1964) and Kelman and Hamilton's *Crimes of Obedience* (1989). These two works identify the historic and organizational forces at work in the normalization of deviance, but do not trace the incremental process behind it. Recall that NASA's long prelude to disaster was typified by anomalies occurring at intervals across time, no single incident appearing significant, the time between them reducing the salience of each. My research on uncoupling showed an analogous pattern, revealing that when relationships end, warning signs are mixed, weak, and routine, obscuring problem seriousness so that the partner being left behind fails to notice and act until too late (Vaughan, 2002). The concept also suggests how social work institutions come to normalize evidence of foster families abusing children; for nation states, it may explain cultural shifts in political ideology or, at the societal level, the transition from Victorian repression of sexuality to media expression that is uncensored and routine. These examples suggest the normalization of deviance as a generalizable concept showing that the gradual routinization and acceptance of anomalies, driven by invisible socially organized forces, is part of all change.

On the other hand, the theory that explained the normalization of deviance at NASA was a theory of systematic reproduction and sameness, not change. What was striking was the repetition of decisions despite changing personnel and increasing O-ring damage. The *Challenger* disaster was an accident, the result of a mistake that was socially organized and systematically produced. Contradicting the rational choice theory of the amorally calculating manager argument, the accident had systemic causes that transcended individuals and time. In the last chapter of the book, I argued that strategies for change must address the social causes of a problem. Because the causes of *Challenger* were in NASA's organizational system – the layered structures and processes of the agency's historic political and budgetary environment, the organization itself, and individual sense making – simply firing personnel or moving them to other positions at the agency would not prevent future accidents because new people in the same positions would be subject to identical forces. The flawed system would produce another accident. I concluded the book with these words:

> After the *Challenger* disaster, both official investigations decried the competitive pressures and economic scarcity that had politicized the space agency, asserting that goals and resources must be brought into alignment. Steps were taken to assure that this happened. But at this writing, that supportive political environment has changed. NASA is again experiencing the economic strain that prevailed at the time of the disaster. Few of the people in top NASA administrative positions exposed to the lessons of the *Challenger* tragedy are still there. The new leaders stress safety, but they are fighting for dollars and making budget cuts. History repeats, as economy and production are again priorities. (Vaughan, 1996: 422)

I predicted another accident, but I did not predict the consequences of such an event for me. On 1 February 2003, NASA's Space Shuttle *Columbia* disintegrated upon reentry to earth's atmosphere. As a consequence, my *Challenger* research revisited me, making me an expert to consult about this second NASA accident. Theory, analogy, and history again played themselves out, as the causes of *Challenger* repeated to produce *Columbia*. Reconsidering the causal theory that explained the loss of *Challenger* and the ethnographic practices that led to a theory that generalized from the first accident to the second prepares the way for an ethnographic account in this journal of this revisit, begun immediately at *Columbia*'s loss, showing the connection between ethnography, theory, public discourse, and policy.

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Notes

1 In this article, I reproduce selected aspects of my 1996 findings in condensed form to track down how I came to them. In order to focus on the theorizing process, I use citations only when the point is specific enough to warrant doing so, rather than citing the original evidence or the relevant literature from the 1996 book to support every point.
2 There are, of course, exceptions. See, for example, Whyte (1955) and Burawoy (1979), who, long before it was acceptable to write in first person, integrated into the text explanations of how their concrete experiences in the setting led to specific theoretical insights.
3 Becker (1998), Mitaugh (2000), and Katz (2001) are three recent works that explore the cognitive process of theorizing. However, my point is that graduate training in theory is institutionalized; training in theorizing is not.
4 Specifically I mean mistakes and confusions in theorizing. Ethnographers, probably more than researchers using other methods, do discuss mistakes and dilemmas while in the field and after. Perhaps the most well-known example is Whyte's description of his illegal voting (1955).
5 See also Snow et al., 2003.
6 Analytic induction (AI) typically is used as a tool by social psychologists analyzing social processes who treat individuals as cases (Robinson, 1951). If the case does not fit the hypothesis, either a) the hypothesis is reformulated or b) the phenomenon to be explained is re-defined, excluding the deviant case, sometimes seeking replacement cases that fit the hypothesis. Excluding deviant cases is not an option, in my view, because retention drives theory elaboration in new directions, preventing automatic verification (see also Burawoy, 1998).
7 See, for example, Haney (2002), Hondagneu-Sotelo (1994) and Kligman (1998).
8 Bensman and Lilienfeld (1991), in *Craft and Consciousness: Occupational Technique and the Development of World Images*, examine professional

training, noting the systematic production of particular worldviews associated with various crafts.

9 Emerson (1983) describes the importance of 'holistic effects' in decision making, noting how a single decision is shaped by its position in a decision stream.

10 All Flight Readiness Review documents were signed by participants at each level of the four-tiered process. Letters, memos, technical reports also identified people and their participation. The amount of paper and bureaucracy involved in all this internal tracking also conveyed an important message about the culture.

11 A matrix organization is one designed on principles of flexibility across formal organizational boundaries. Specialists from other parts of NASA were 'matrixed in' to join those permanently assigned to work on a shuttle part when problems or controversies arose that required additional expertise. This strategy is often used by organizations to manage large complex technical projects (see Davis et al., 1978).

12 For example, in 1988, I did telephone interviews with 18 people responsible for safety regulation who had official oversight responsibilities at NASA Headquarters, at several space centers, on external safety panels, and Congressional committees because I needed to know the scope of safety regulation at the time. Whenever I had questions about the Presidential Commission's investigation, I contacted a Presidential Commission member, who had agreed to be an anonymous informant, or one of the Commission's investigative staff; when I was reconstructing decisions that required evaluating testimony about wind and temperature conditions at the Florida launch site, I contacted the National Climatic Data Center in Maryland and the National Weather Service in Titusville, Florida to secure temperature records for Cape Canaveral. As mentioned earlier, I consulted Roger Boisjoly and Leon Ray regularly on technical issues, but I also consulted them about procedural, cultural, organizational, and social, economic, and political influences on decision making.

13 I thank Rachel Sherman for this observation.

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