"What Do <u>You</u> Care What Other People Think?"

Further Adventures of a Curious Character

Richard P. Feynman

as told to Ralph Leighton

Preface

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Preface

BECAUSE of the appearance of *"Surely You're Joking, Mr. Feynman!"* a few things need to be explained here.

First, although the central character in this book is the same as before, the "adventures of a curious character" here are different: some are light and some tragic, but most of the time Mr. Feynman is surely *not* joking—although it's often hard to tell.

Second, the stories in this book fit together more loosely than those in *"Surely You're Joking . . . ,"* where they were arranged chronologically to give a semblance of order. (That resulted in some readers getting the mistaken idea that *SYJ* is an autobiography.) My motivation is simple: ever since hearing my first Feynman stories, I have had the powerful desire to share them with others.

Finally, most of these stories were not told at drumming sessions, as before. I will elaborate on this in the brief outline that follows.

Part 1, "A Curious Character," begins by describing the influence of those who most shaped Feynman's personality—his father, Mel, and his first love, Arlene. The first story was adapted from "The Pleasure of Finding Things Out," a BBC program produced by Christopher Sykes. The story of Arlene, from which the title of this book was taken, was painful for Feynman to recount. It was assembled over the past ten years out of pieces from six different stories. When it was finally complete, Feynman was especially fond of this story, and happy to share it with others.

The other Feynman stories in Part 1, although generally lighter in tone, are included here because there won't be a second volume of *SYJ*. Feynman was particularly proud of "It's as Simple as One, Two, Three," which he occasionally thought of writing up as a psychology paper. The letters in the last chapter of Part 1 have been provided courtesy of Gweneth Feynman, Freeman Dyson, and Henry Bethe.

Part 2, "Mr. Feynman Goes to Washington," is, unfortunately, Feynman's last big adventure. The story is particularly long because its content is still timely. (Shorter versions have appeared in *Engineering and Science* and *Physics Today*.) It was not published sooner because Feynman underwent his third and fourth major surgeries—plus radiation, hyperthermia, and other treatments—since serving on the Rogers Commission.

Feynman's decade-long battle against cancer ended on February 15, 1988, two weeks after he taught his last class at Caltech. I decided to include one of his most eloquent and inspirational speeches, "The Value of Science," as an epilogue.

Ralph Leighton
March 1988

'WhatDoYiu Care What Other People Think?"

Part 2

MR. FEYNMAN GOES TO WASHINGTON: INVESTIGATING THE SPACE SHUTTLE CHALLENGER DISASTER

Preliminaries

IN THIS STORY I'm going to talk a lot about NASA,* but when I say "NASA did this" and "NASA did that," I don't mean all of NASA; I just mean that part of NASA associated with the shuttle.

To remind you about the shuttle, the large central part is the tank, which holds the fuel: liquid oxygen is at the top, and liquid hydrogen is in the main part. The engines which burn that fuel are at the back end of the orbiter, which goes into space. The crew sits in the front of the orbiter; behind them is the cargo bay.

During the launch, two solid-fuel rockets boost the shuttle for a few minutes before they separate and fall back into the sea. The tank separates from the orbiter a few minutes later—much higher in the atmosphere—and breaks up as it falls back to earth.

The solid rocket boosters are made in sections. There are two types of joints to hold the sections together: the permanent "factory joints" are sealed at the Morton Thiokol factory in Utah; the temporary "field joints" are sealed before each flight—"in the field"—at the Kennedy Space Center in Florida.

*The National Aeronautics and Space Administration.

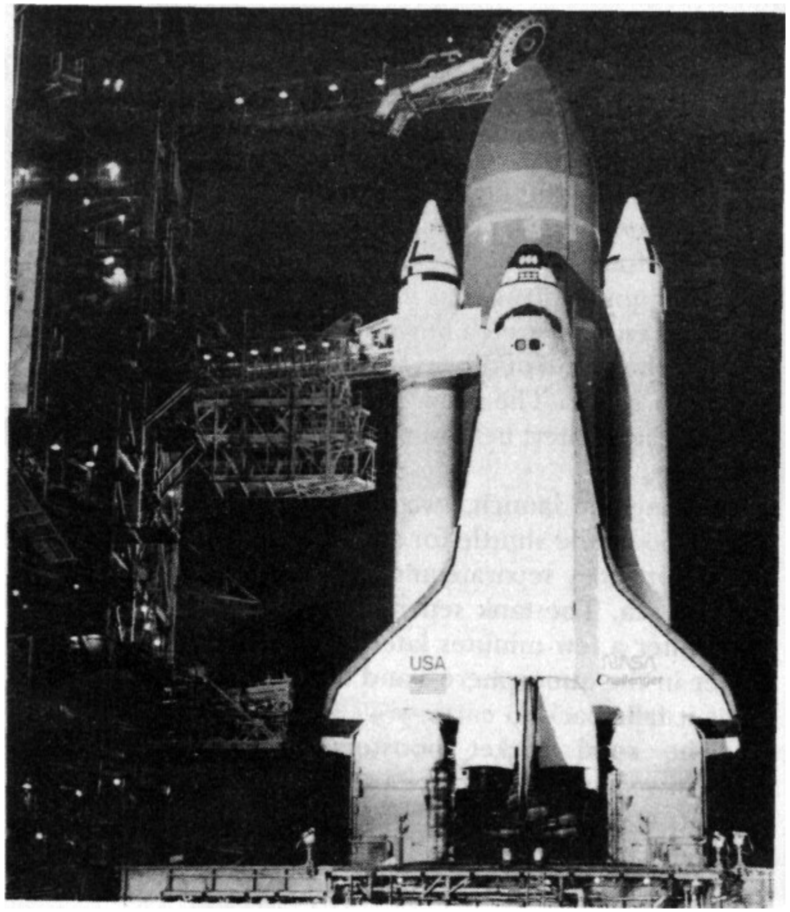


FIGURE 1. The space shuttle **Challenger.** *The fuel tank, flanked by two solid-fuel rocket boosters, is attached to the orbiter, whose main engines burn liquid hydrogen and liquid oxygen. (© NASA.)*

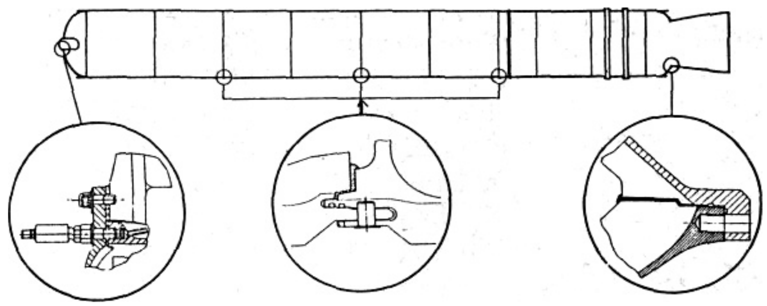


FIGURE 2. Locations and close-up views of booster-rocket field joints.

Committing Suicide

AS YOU probably know, the space shuttle *Challenger* had an accident on Tuesday, January 28, 1986. I saw the explosion on TV, but apart from the tragedy of losing seven people, I didn't think much about it.

In the newspaper I used to read about shuttles going up and down all the time but it bothered me a little bit that I never saw in any scientific journal any results of anything that had ever come out of the experiments on the shuttle that were supposed to be so important. So I wasn't paying very much attention to it.

Well, a few days after the accident, I get a telephone call from the head of NASA, William Graham, asking me to be on the committee investigating what went wrong with the shuttle! Dr. Graham said he had been a student of mine at Caltech, and later had worked at the Hughes Aircraft Company, where I gave lectures every Wednesday afternoon.

I still wasn't exactly sure who he was.

When I heard the investigation would be in Washington, my immediate reaction was not to do it: I have a principle of not going anywhere near Washington or having anything to do with government, so my immediate reaction was--how am I gonna get out of this?

I called various friends like Al Hibbs and Dick Davies, but they explained to me that investigating the *Challenger* accident was very important for the nation, and that I should do it.

My last chance was to convince my wife. "Look," I said. "Anybody could do it. They can get somebody else."

"No," said Gweneth. "If you don't do it, there will be twelve people, all in a group, going around from place to place together. But if you join the commission, there will be eleven people—all in a group, going around from place to place together—while the twelfth one runs around all over the place, checking all kinds of unusual things. There probably won't be anything, but if there is, you'll find it." She said, "There isn't anyone else who can do that like you can."

Being very immodest, I believed her.

Well, it's one thing to figure out what went wrong with the shuttle. But the next thing would be to find out what was the matter with the organization of NASA. Then there are questions like, "Should we continue with the shuttle system, or is it better to use expendable rockets?" And then come even bigger questions: "Where do we go from here?" "What should be our future goals in space?" I could see that a commission which started out trying to find out what happened to the shuttle could end up as a commission trying to decide on national policy, and go on forever!

That made me quite nervous. I decided to get out at the end of six months, no matter what.

But I also resolved that while I was investigating the accident, I shouldn't do anything else. There were some physics problems I was playing with. There was a computer class at Caltech I was teaching with another professor. (He offered to take over the course.) There was the Thinking Machines Company in Boston I was going to consult for.

(They said they would wait.) My physics would have to wait, too.

By this time it was Sunday. I said to Gweneth, "I'm gonna commit suicide for six months," and picked up the telephone.

The Cold Facts

WHEN I called Graham and accepted, he didn't know exactly what the commission was going to do, who it was going to be under, or even if I would be accepted onto it. (There was still hope!)

But the next day, Monday, I got a telephone call at 4 P.M.: "Mr. Feynman, you have been accepted onto the commission"—which by that time was a "presidential commission" headed by William P. Rogers.

I remembered Mr. Rogers. I felt sorry for him when he was secretary of state, because it seemed to me that President Nixon was using the national security adviser (Kissinger) more and more, to the point where the secretary of state was not really functioning.

At any rate, the first meeting would be on Wednesday. I figured there's nothing to do on Tuesday—I could fly to Washington Tuesday night—so I called up Al Hibbs and asked him to get some people at JPL* who know something about the shuttle project to brief me.

On Tuesday morning I rush over to JPL, full of steam, ready to roll. Al sits me down, and different engineers come in, one after the other, and explain the various parts of the shuttle. I don't know *how* they knew, but they knew all about the shuttle. I got a very

*NASA's Jet Propulsion Laboratory, located in Pasadena; it is administered by Caltech.

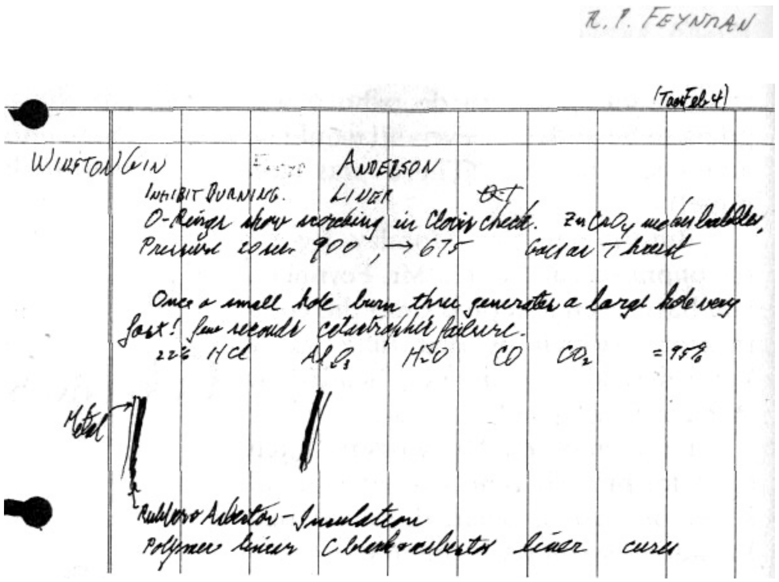


FIGURE 3. The beginning of Feynman's notes from his informal JPL briefing.

thorough, high-speed, intense briefing. The guys at JPL had the same enthusiasm that I did. It was really quite exciting.

When I look at my notes now, I see how quickly they gave me hints about where to look for the shuttle's problems. The first line of my notes says "Inhibit burning. Liner." (To inhibit propellant from burning through the metal wall of each booster rocket, there's a liner, which was not working right.) The second line of my notes says "O-rings show scorching in clevis check." It was noticed that hot gas occasionally burned past the O-rings in booster-rocket field joints.

On the same line it says "Zn CrO_4 makes bubbles." (The zinc chromate putty, packed as an insulator behind the O-rings, makes bubbles which can become enlarged

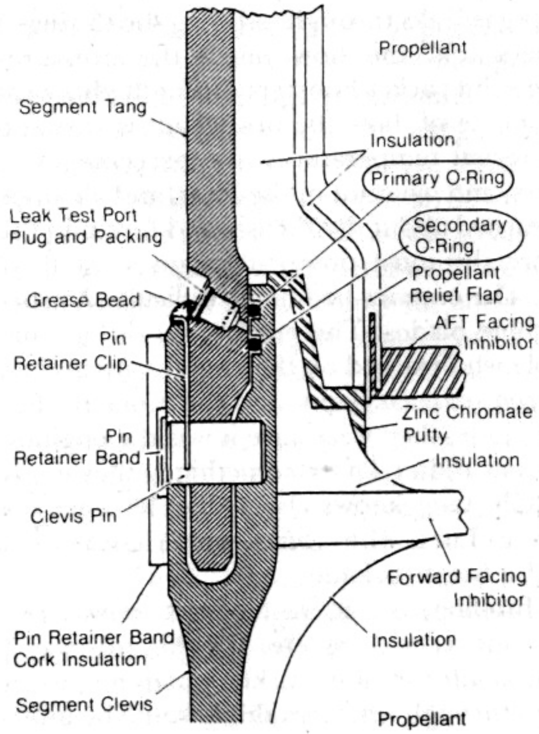


FIGURE 4. Detailed diagram of a field joint.

FIGURE 5. Photograph of bubbles in zinc chromate putty, which can lead to erosion of the O-rings.

very fast when hot gas leaks through, eroding the O-rings.)

The engineers told me how much the pressures change inside the solid rocket boosters during flight, what the propellant is made of, how the propellant is cast and then baked at different temperatures, the percentages of asbestos, polymers, and whatnot in the liner, and all kinds of other stuff. I learned about the thrusts and forces in the engines, which are the most powerful engines for their weight ever built. The engines had many difficulties, especially cracked turbine blades. The engineers told me that some of the people who worked on the engines always had their fingers crossed on each flight, and the moment they saw the shuttle explode, they were sure it was the engines.

If the engineers didn't know something, they'd say something like, "Oh, Lifer knows about that; let's get *him* in." Al would call up Lifer, who would come right away. I couldn't have had a better briefing.

It's called a briefing, but it wasn't brief: it was *very* intense, very fast, and very complete. It's the only way I know to get technical information quickly: you don't just sit there while they go through what *they* think would be interesting; instead, you ask a lot of questions, you get quick answers, and soon you begin to understand the circumstances and learn just what to ask to get the next piece of information you need. I got one hell of a good education that day, and I sucked up the information like a sponge.

That night I took the red-eye* to Washington, and got there early Wednesday morning. (I never took the red-eye again—I learned!)

I checked into the Holiday Inn in downtown Washington, and got a cab to take me to the first meeting of the commission.

"Where to?" the driver says.

*Note for foreign readers: a flight that leaves the West Coast around 11 P.M. and arrives on the East Coast around 7 A.M., five hours and three time zones later.

All I have is a little piece of paper. "1415 8th Street."

We start off. I'm new in Washington. The Capitol is over here, the Washington Monument is over there; everything seems very close. But the taxi goes on and on, farther and farther into worse and worse territory. Buildings get smaller, and they begin to look run down a little bit. Finally, we get onto 8th Street, and as we go along, the buildings begin to disappear altogether. Finally we find the address—by interpolation: it's an empty lot between two buildings!

By this time I realize something is completely cockeyed. I don't know what to do, because I've only got this slip of paper, and I don't know where to go.

I say to the taxi driver, "The meeting I'm going to has something to do with NASA. Can you take me to NASA?"

"Sure," he says. "You know where it is, don't you? It's right where I picked you up!"

It was true. NASA I could have walked to from the Holiday Inn: it was right across the street! I go in, past the guard at the gate, and start wandering around.

I find my way to Graham's office, and ask if there's a meeting about the shuttle.

"Yes, I know where it is," somebody says. "I'll take you down there."

They take me to a room and, sure enough, there's a big meeting going on: there are bright lights and television cameras down in front; the room is completely full, bursting with people, and all I can do is barely squash my way into the back. I'm thinking, "There's only one door to this place. How the hell am I gonna get down to the front from here?"

Then I overhear something a little bit—it's so far down there that I can't make out exactly what it is—but it's evidently a different subject!

So I go back to Graham's office and find his secretary. She calls around and finds out where the commission is meeting. "I don't know, either," she says to the person on the other end. "He simply wandered in here!"

The meeting was in Mr. Rogers' law offices, at 1415 *H* Street. My slip of paper said 1415 *8th* Street. (The address had been given over the telephone.)

I finally got to Mr. Rogers' office—I was the only one late—and Mr. Rogers introduced me to the other commissioners. The only one I had ever heard of besides Mr. Rogers was Neil Armstrong, the moon man, who was serving as vice-chairman. (Sally Ride was on the commission, but I didn't realize who she was until later.*) There was a very handsome-looking guy in a uniform, a General Kutyna (pronounced Koo-TEE-na). He looked formidable in his outfit, while the other people had on ordinary suits.

This first meeting was really just an informal get-together. That bothered me, because I was still wound up like a spring from my JPL briefing the day before.

Mr. Rogers did announce a few things. He read from the executive order that defined our work:

The Commission shall:
1. Review the circumstances surrounding the accident and establish the probable cause or causes of the accident; and
2. Develop recommendations for corrective or other action based upon the Commission's findings and determinations.

Mr. Rogers also said we would complete our investigation within 120 days.

That was a relief: the scope of our commission would be limited to investigating the accident, and our work might be finished before I was done committing suicide!

Mr. Rogers asked each of us how much of our time we could spend on the commission. Some of the commissioners were retired, and almost everybody said they had rearranged their schedules. I said, "I'm ready to work

*Note for foreign readers: Sally Ride was the first American woman in space.

100 percent, starting right now!"

Mr. Rogers asked, "Who will be in charge of writing the report?"

A Mr. Hotz, who had been the editor of *Aviation Week* magazine, volunteered to do that.

Then Mr. Rogers brought up another matter. "I've been in Washington a long time," he said, "and there's one thing you all must know: no matter what we do, there will always be leaks to the press. The best we can do is just try to minimize them. The proper way to deal with leaks is to have public meetings. We will have closed meetings, of course, but if we find anything important, we will have an open meeting right away, so the public will always know what is going on."

Mr. Rogers continued, "To start things off right with the press, our first official meeting will be a public meeting. We'll meet tomorrow at 10 A.M."

As we were leaving the get-together, I heard General Kutyna say, "Where's the nearest Metro station?"

I thought, "This guy, I'm gonna get along with him fine: he's dressed so fancy, but inside, he's straight. He's not the kind of general who's looking for his driver and his special car; he goes back to the Pentagon by the Metro." Right away I liked him, and over the course of the commission I found my judgment in this case was excellent.

The next morning, a limousine called for me—someone had arranged for us to arrive at our first official meeting in limousines. I sat in the front seat, next to the driver.

On the way to the meeting, the driver says to me, "I understand a lot of important people are on this commission . . ."

"Yeah, I s'pose . . ."

"Well, I collect autographs," he says. "Could you do me a favor?"

"Sure," I say.

I'm reaching for my pen when he says, "When we get there, could you point out to me which one Neil Armstrong is, so I can get his autograph?"

Before the meeting started, we were sworn in. People were milling around; a secretary handed us each a badge with our picture on it so we could go anywhere in NASA. There were also some forms to sign, saying you agree to this and that so you can get your expenses paid, and so on.

After we were sworn in, I met Bill Graham. I did recognize him, and remembered him as a nice guy.

This first public meeting was going to be a general briefing and presentation by the big cheeses of NASA—Mr. Moore, Mr. Aldrich, Mr. Lovingood, and others. We were seated in big leather chairs on a dais, and there were bright lights and TV cameras pointing at us every time we scratched our noses.

I happened to sit next to General Kutyna. Just before the meeting s.tarted, he leans over and says, "Co-pilot to pilot: comb your hair."

I say, "Pilot to co-pilot: can I borrow your comb?"

The first thing we had to learn was the crazy acronyms that NASA uses all over the place: "SRMs" are the solid rocket motors, which make up most of the "SRBs," the solid rocket boosters. The "SSMEs" are the space shuttle main engines; they burn "LH" (liquid hydrogen) and "LOX" (liquid oxygen), which are stored in the "ET," the external tank. Everything's got letters.

And not just the big things: practically every valve has an acronym, so they said, "We'll give you a dictionary for the acronyms—it's really very simple." Simple, sure, but the dictionary is a great, big, fat book that you've gotta keep looking through for things like "HPFTP" (high-pressure fuel turbopump) and "HPOTP" (high-pressure oxygen turbopump).

Then we learned about "bullets"—little black circles in front of phrases that were supposed to summarize

STS 51-L CARGO ELEMENTS

- TRACKING AND DATA RELAY SATELLITE-B/INERTIAL UPPER STAGE
- SPARTAN-HALLEY/MISSION PECULIAR SUPPORT STRUCTURE
- CREW COMPARTMENT
  - TISP - TEACHER IN SPACE PROGRAM
  - CHAMP - COMET HALLEY ACTIVE MONITORING PROGRAM
  - FDE - FLUID DYNAMICS EXPERIMENT
  - STUDENT EXPERIMENTS
  - RME - RADIATION MONITORING EXPERIMENT
  - PPE - PHASE PARTITIONING EXPERIMENT

FIGURE 6. An example of "bullets."

things. There was one after another of these little goddamn bullets in our briefing books and on the slides.

It turned out that apart from Mr. Rogers and Mr. Acheson, who were lawyers, and Mr. Hotz, who was an editor, we all had degrees in science: General Kutyna had a degree from MIT; Mr. Armstrong, Mr. Covert, Mr. Rummel, and Mr. Sutter were all aeronautical engineers, while Ms. Ride, Mr. Walker, Mr. Wheelon, and I were all physicists. Most of us seemed to have done some preliminary work on our own. We kept asking questions that were much more technical than some of the big cheeses were prepared for.

When one of them couldn't answer a question, Mr. Rogers would reassure him that we understood he wasn't expecting such detailed questions, and that we were satisfied, for the time being at least, by the perpetual answer, "We'll get that information to you later."

The main thing I learned at that meeting was how inefficient a public inquiry is: most of the time, other people are asking questions you already know the answer to—or are not interested in—and you get so fogged out that you're hardly listening when important points are being passed over.

What a contrast to JPL, where I had been filled with all sorts of information very fast. On Wednesday we have a "get-together" in Mr. Rogers' office—that takes two hours—and then we've got the rest of the day to do what? Nothing. And that night? Nothing. The next day, we have the public meeting—"We'll get back to you on that"—which equals nothing! Although it *looked* like we were doing something every day in Washington, we were, in reality, sitting around doing nothing most of the time.

That night I gave myself something to do: I wrote out the kinds of questions I thought we should ask during our investigation, and what topics we should study. My plan was to find out what the rest of the commission wanted to do, so we could divide up the work and get going.

The next day, Friday, we had our first real meeting. By this time we had an office—we met in the Old Executive Office Building—and there was even a guy there to transcribe every word we said.

Mr. Rogers was delayed for some reason, so while we waited for him, General Kutyna offered to tell us what an accident investigation is like. We thought that was a good idea, so he got up and explained to us how the air force had proceeded with its investigation of an unmanned Titan rocket which had failed.

I was pleased to see that the system he described—what the questions were, and the way they went about finding the answers—was very much like what I had laid out the night before, except that it was much more methodical than I had envisioned. General Kutyna warned us that sometimes it looks like the cause is obvious, but when you investigate more carefully you have to change your mind. They had very few clues, and changed their minds three times in the case of the Titan.

I'm all excited. I want to do this kind of investigation, and figure we can get started right away—all we have to do is decide who will do what.

But Mr. Rogers, who came in partway through General Kutyna's presentation, says, "Yes, your investigation was a great success, General, but we won't be able to use your methods here because we can't get as much information as you had."

Perhaps Mr. Rogers, who is not a technical man, did not realize how patently false that was. The Titan, being an unmanned rocket, didn't have anywhere near the number of check gadgets the shuttle did. We had television pictures showing a flame coming out the side of a booster rocket a few seconds before the explosion; all we could see in General Kutyna's pictures of the Titan was a lousy dot in the sky—-just a little, tiny flash—and he was able to figure stuff out from that.

Mr. Rogers says, "I have arranged for us to go to Florida next Thursday. We'll get a briefing there from NASA officials, and they'll take us on a tour of the Kennedy Space Center."

I get this picture of the czarina coming to a Potemkin village: everything is all arranged; they show us how the rocket looks and how they put it together. It's not the way to find out how things *really* are.

Then Mr. Armstrong says, "We can't expect to do a technical investigation like General Kutyna did." This bothered me a lot, because the only things I pictured myself doing were technical! I didn't know exactly what he meant: perhaps he was saying that all the technical lab work would be done by NASA.

I began suggesting things I could do.

While I'm in the middle of my list, a secretary comes in with a letter for Mr. Rogers to sign. In the interim, when I've just been shut up and I'm waiting to come back, various other commission members offer to work with me. Then Mr. Rogers looks up again to continue the meeting, but he calls on somebody else—as if he's absentminded and forgot I'd been interrupted. So I have to get the floor again, but when I start my stuff again, another "accident" happens.

In fact, Mr. Rogers brought the meeting to a close while I was in midstream! He repeated his worry that we'll never really figure out what happened to the shuttle.

This was extremely discouraging. It's hard to understand now, because NASA has been taking at least two years to put the shuttle back on track. But at the time, I thought it would be a matter of days.

I went over to Mr. Rogers and said, "We're going to Florida next Thursday. That means we've got nothing to do *for Jive days:* what'll I do for five days?"

"Well, what would you have done if you hadn't been on the commission?"

"I was going to go to Boston to consult, but I canceled it in order to work 100 percent."

"Well, why don't you go to Boston for the five days?"

I couldn't take that. I thought, "I'm dead already! The goddamn thing isn't working right." I went back to my hotel, devastated.

Then I thought of Bill Graham, and called him up. "Listen, Bill," I said. "You got me into this; now you've gotta save me: I'm completely depressed; I can't stand it."

He says, "What's the matter?"

"I want to *do* something! I want to go around and talk to some engineers!"

He says, "Sure! Why not? I'll arrange a trip for you. You can go wherever you want: you could go to Johnson, you could go to Marshall, or you could go to Kennedy ..."

I thought I wouldn't go to Kennedy, because it would look like I'm rushing to find out everything ahead of the

others. Sally Ride worked at Johnson, and had offered to work with me, so I said, "I'll go to Johnson."

"Fine," he says. "I'll tell David Acheson. He's a personal friend of Rogers, and he's a friend of mine. I'm sure everything will be okay."

Half an hour later, Acheson calls me: "I think it's a great idea," he says, "and I told Mr. Rogers so, but he says no. I just don't know why I can't convince him."

Meanwhile, Graham thought of a compromise: I would stay in Washington, and he would get people to come to his office at NASA, right across the street from my hotel. I would get the kind of briefing I wanted, but I wouldn't be running around.

Then Mr. Rogers calls me: he's against Graham's compromise. "We're all going to Florida next Thursday," he says.

I say, "If the idea is that we sit and listen to briefings, it won't work with me. I can work much more efficiently if I talk to engineers directly."

"We have to proceed in an orderly manner."

"We've had several meetings by now, but we still haven't been assigned anything to do!"

Rogers says, "Well, do you want me to bother all the other commissioners and call a special meeting for Monday, so we can make such assignments?"

"Well, yes!" I figured our job was to work, and we *should* be bothered—you know what I mean?

So he changes the subject, naturally. He says, "I understand you don't like the hotel you're in. Let me put you in a good hotel."

"No, thank you; everything is fine with my hotel."

Pretty soon he tries again, so I say, "Mr. Rogers, my personal comfort is not what I'm concerned with. I'm trying to get to work. I want to *do* something!"

Finally, Rogers says it's okay to go across the street to talk to people at NASA.

I was obviously quite a pain in the ass for Mr. Rogers.

Later, Graham tried to explain it to me. "Suppose you, as a technical person, were given the job as chairman of a committee to look into some legal question. Your commission is mostly lawyers, and one of them keeps saying, 'I can work more effectively if I talk directly to other lawyers.' I assume you'd want to get your bearings first, before letting anybody rush off investigating on his own."

Much later, I appreciated that there were lots of problems which Mr. Rogers had to address. For example, any piece of information any of us received had to be entered into the record and made available to the other commissioners, so a central library had to be set up. Things like that took time.

On Saturday morning I went to NASA. Graham brought in guys to tell me all about the shuttle. Although they were pretty high up in NASA, the guys were technical.

The first guy told me all about the solid rocket boosters—the propellant, the motor, the whole thing except the seals. He said, "The seals expert will be here this afternoon."

The next guy told me all about the engine. The basic operation was more or less straightforward, but then there were all kinds of controls, with backing and hauling from pipes, heating from this and that, with high-pressure hydrogen pushing a little propeller which turns something else, which pumps oxygen through a vent valve—that kind of stuff.

It was interesting, and I did my best to understand it, but after a while I told the fella, "That's as much as I'm going to take, now, on the engine."

"But there are many problems with the engines that you should hear about," he says.

I was hot on the trail of the booster rocket, so I said, "I'll have to put off the main engines till later, when I have more time."

Then a guy came in to tell me about the orbiter. I felt terrible, because he had come in on a Saturday to see me, and it didn't look like the orbiter had anything to do with the accident. I was having enough trouble understanding the rest of the shuttle—there's only a certain amount of information per cubic inch a brain can hold—so I let him tell me some of the stuff, but soon I had to tell him that it was getting too detailed, so we just had a pleasant conversation.

In the afternoon, the seals expert came in—his name was Mr. Weeks—and gave me what amounted to a continuation of my JPL briefing, with still more details.

There's putty and other things, but the ultimate seal is supposed to be two rubber rings, called O-rings, which are approximately a quarter of an inch thick and lie on a circle 12 feet in diameter—that's something like 37 feet long.

When the seals were originally designed by the Morton Thiokol Company, it was expected that pressure from the burning propellant would squash the O-rings. But because the joint is stronger than the wall (it's three times thicker), the wall bows outward, causing the joint to bend a little—enough to lift the rubber O-rings off the seal area. Mr. Weeks told me this phenomenon is called "joint rotation," and it was discovered very early, before they ever flew the shuttle.

The pieces of rubber in the joints are called O-rings, but they're not used like normal O-rings are. In ordinary circumstances, such as sealing oil in the motor of an automobile, there are sliding parts and rotating shafts, but the gaps are always the same. An O-ring just sits there, in a fixed position.

But in the case of the shuttle, the gap *expands* as the pressure builds up in the rocket. And to maintain the seal, the rubber has to *expand fast* enough to close the gap—and during a launch, the gap opens in a fraction of a second.

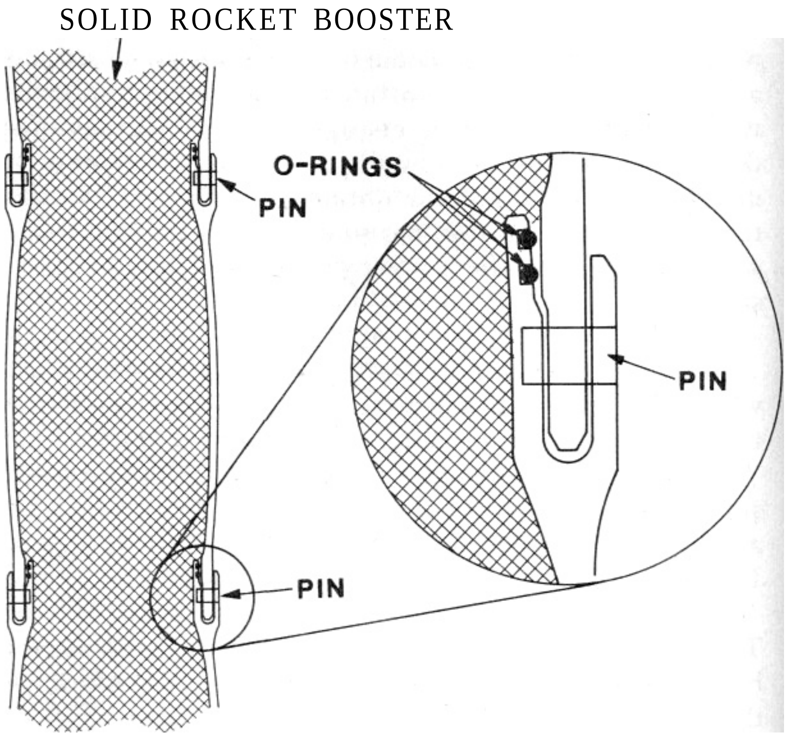


FIGURE 7. Joint rotation is caused by pressure from inside the rocket pushing the walls out farther than the joints. A gap opens, and hot gas flows past one or both of the 0-rings.

Thus the resilience of the rubber became a very essential part of the design.

When the Thiokol engineers were discovering these problems, they went to the Parker Seal Company, which manufactures the rubber, to ask for advice. The Parker Seal Company told Thiokol that O-rings are not meant to be used that way, so they could give no advice.

Although it was known from nearly the beginning that the joint was not working as it was designed to, Thiokol kept struggling with the device. They made a number of

makeshift improvements. One was to put shims in to keep the joint tight, but the joint still leaked. Mr. Weeks showed me pictures of leaks on previous flights—what the engineers called "blowby," a blackening behind an O-ring where hot gas leaked through, and what they called "erosion," where an O-ring had burned a little bit. There was a chart showing all the flights, and how serious the blowby and erosion were on each one. We went through the whole history up to *the* flight, 51-L.

I said, "Where does it say they were ever discussing the problem—how it's going along, or whether there's some progress?"

The only place was in the "flight readiness reviews"—between flights there was no discussion of the seals problem!

We looked at the summary of the report. Everything was behind little bullets, as usual. The top line says:

- The lack of a good secondary seal in the field joint is most critical and ways to reduce joint rotation should be incorporated as soon as possible to reduce criticality.

And then, near the bottom, it says:

- Analysis of existing data indicates that it is safe to continue flying existing design as long as all joints are leak checked* with a 200 psig stabilization . . .

I was struck by the contradiction: "If it's 'most critical,' how could it be 'safe to continue flying'? What's the *logic* of this?"

Mr. Weeks says, "Yes, I see what you mean! Well, let's see: it says here, 'Analysis of existing data . . .'"

We went back through the report and found the analysis. It was some kind of computer model with various as-

*Later in our investigation we discovered that it was this leak check which was a likely cause of the dangerous bubbles in the zinc chromate putty that I had heard about at JPL.

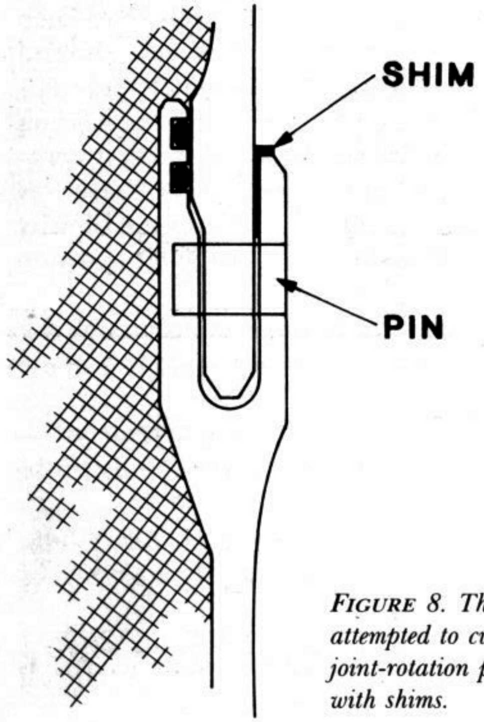


FIGURE 8. Thiokol attempted to cure the joint-rotation problem with shims.

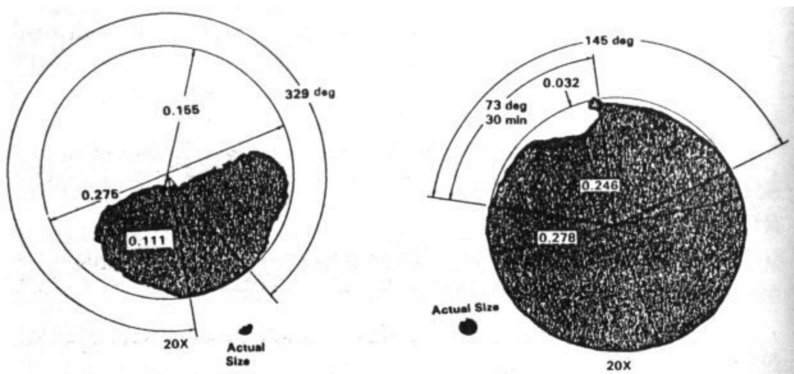


FIGURE 9. Two examples of O-ring erosion. Such erosion would occur unpredictably along 2 or 3 inches of the 37-foot O-ring.

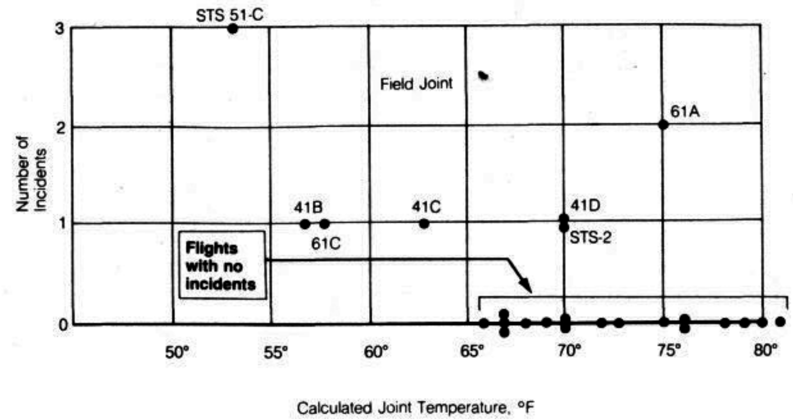


FIGURE 10. The correlation between temperature and O-ring incidents.

Recommendations

- The lack of a good secondary seal in the field joint is most critical and ways to reduce joint rotation should be incorporated as soon as possible to reduce criticality
- The flow conditions in the joint areas during ignition and motor operation need to be established through cold flow modeling to eliminate O-ring erosion
- QM-5 static test should be used to qualify a second source of the only flight certified joint filler material (asbestos-filled vacuum putty) to protect the flight program schedule
- VLS-1 should use the only flight certified joint filler material (Randolph asbestos-filled vacuum putty) in all joints
- Additional hot and cold subscale tests need to be conducted to improve analytical modeling of O-ring erosion problem and for establishing margins of safety for eroded O-rings
- Analysis of existing data indicates that it is safe to continue flying existing design as long as all joints are leak checked with a 200 psig stabilization pressure, are free of contamination in the seal areas and meet O-ring squeeze requirements
- Efforts needs to continue at an accelerated pace to eliminate SRM seal erosion

FIGURE 11. The self-contradictory recommendations of the seals report are underlined.

sumptions that were not necessarily right. You know the danger of computers, it's called GIGO: garbage in, garbage out! The analysis concluded that a little unpredictable leakage here and there could be tolerated, even though it wasn't part of the original design.

If *all* the seals had leaked, it would have been obvious even to NASA that the problem was serious. But only a few of the seals leaked on only some of the flights. So NASA had developed a peculiar kind of attitude: if one of the seals leaks a little and the flight is successful, the problem isn't so serious. Try playing Russian roulette that way: you pull the trigger and the gun doesn't go off, so it must be safe to pull the trigger again . . .

Mr. Weeks said there was a rumor that the history of the seals problem was being leaked to the newspapers. That bothered him a little bit, because it made NASA look like it was trying to keep things secret.

I told him I was entirely satisfied with the people Graham had brought in to talk to me, and that since I had already heard about the seals problem at JPL, it wasn't any big deal.

The next day, Sunday, Bill Graham took me with his family to the National Air and Space Museum. We had an early breakfast together, and then we went across the street to the museum.

I was expecting to see big crowds there, but I had forgotten that Graham was such a big shot. We had the whole place to ourselves for a while.

We did see Sally Ride there. She was in a display case, in an astronaut's suit, holding a helmet and everything. The wax model looked exactly like her.

At the museum there was a special theater with a movie about NASA and its achievements. The movie was wonderful. I had not fully appreciated the enormous number of

people who were working on the shuttle, and all the effort that had gone into making it. And you know how a movie is: they can make it dramatic. It was so dramatic that I almost began to cry. I could see that the accident was a terrible blow. To think that so many people were working so hard to make it go—and then it busts—made me even more determined to help straighten out the problems of the shuttle as quickly as possible, to get all those people back on track. After seeing this movie I was very changed, from my semi anti-NASA attitude to a very strong pro-NASA attitude.

That afternoon, I got a telephone call from General Kutyna.

"Professor Feynman?" he says. "I have some urgent news for you. Uh, just a minute."

I hear some military-type band music in the background.

The music stops, and General Kutyna says, "Excuse me, Professor; I'm at an Air Force Band concert, and they just played the national anthem."

I could picture him in his uniform, standing at attention while the band is playing the "Star Spangled Banner," saluting with one hand and holding the telephone with the other. "What's the news, General?"

"Well, the first thing is, Rogers told me to tell you not to go over to NASA."

I didn't pay any attention to that, because I had already gone over to NASA the day before.

He continued, "The other thing is, we're going to have a special meeting tomorrow afternoon to hear from a guy whose story came out in the *New York Times* today."

I laughed inside: so we're going to have a special meeting on Monday, anyway!

Then he says, "I was working on my carburetor this morning, and I was thinking: the shuttle took off when the temperature was 28 or 29 degrees. The coldest temperature previous to that was 53 degrees. You're a professor; *what, sir, is the effect of cold on the O-rings?"*

"Oh!" I said. "It makes them stiff. Yes, of course!"

That's all he had to tell me. It was a clue for which I got a lot of credit later, but it was his observation. A professor of theoretical physics always has to be told what to look for. He just uses his knowledge to explain the observations of the experimenters!

On Monday morning General Kutyna and I went over to Graham's office and asked him if he had any information on the effects of temperature on the O-rings. He didn't have it on hand, but said he would get it to us as soon as possible.

Graham did, however, have some interesting photographs to show us. They showed a flame growing from the right-hand solid rocket booster a few seconds before the explosion. It was hard to tell exactly where the flame was coming out, but there was a model of the shuttle right there in the office. I put the model on the floor and walked around it until it looked exactly like the picture—in size, and in orientation.

I noticed that on each booster rocket there's a little hole—called the leak test port—where you can put pressure in to test the seals. It's *between* the two O-rings, so if it's not closed right and if the first O-ring fails, the gas would go out through the hole, and it would be a catastrophe. It was just about where the flame was. Of course, it was still a question whether the flame was coming out of the leak test port or a larger flame was coming out farther around, and we were seeing only the tip of it.

That afternoon we had our emergency closed meeting to hear from the guy whose story was in the *New York Times.* His name was Mr. Cook. He was in the budget department

of NASA when he was asked to look into a possible seals problem and to estimate the costs needed to rectify it.

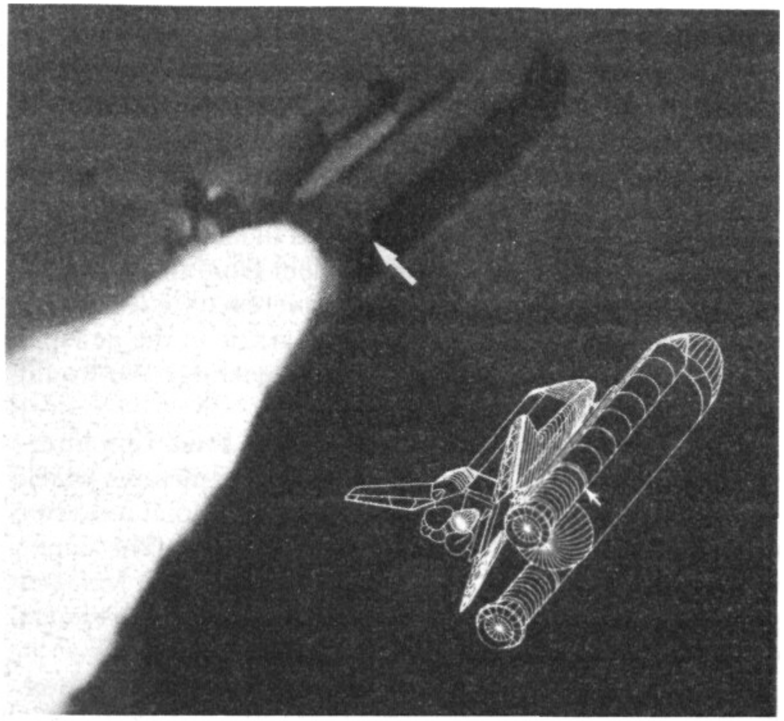
By talking to the engineers, he found out that the seals had been a big problem for a long time. So he reported that it would cost so-and-so much to fix it—a lot of money. From the point of view of the press and some of the commissioners, Mr. Cook's story sounded like a big expose, as if NASA was hiding the seals problem from us.

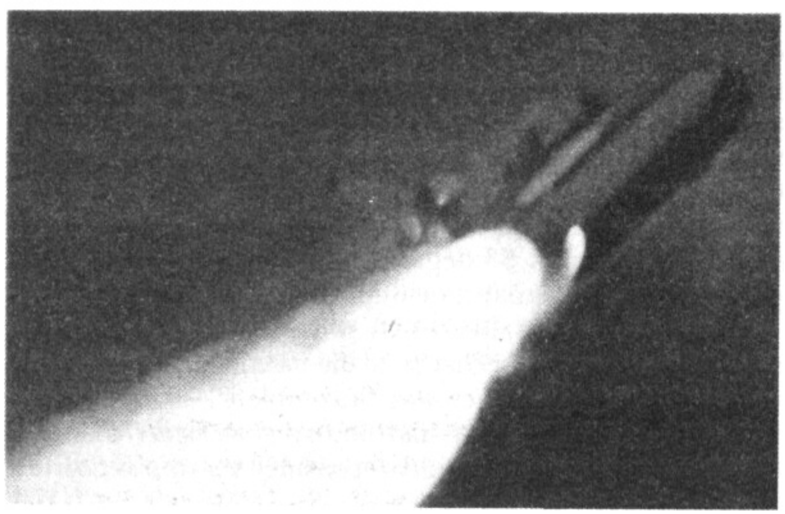
I had to sit through this big, unnecessary excitement, wondering if every time there was an article in the newspaper, would we have to have a special meeting? We would never get anywhere that way!

But later, during that same meeting, some very interesting things happened. First, we saw some pictures which showed puffs of smoke coming out of a field joint just after ignition, before the shuttle even got off the pad. The smoke was coming out of the same place—possibly the leak test port—where the flame appeared later. There wasn't much question, now. It was all fitting together.

Then something happened that was completely unexpected. An engineer from the Thiokol Company, a Mr. McDonald, wanted to tell us something. He had come to our meeting on his own, uninvited. Mr. McDonald reported that the Thiokol engineers had come to the conclusion that low temperatures had something to do with the seals problem, and they were very, very worried about it. On the night before the launch, during the flight readiness review, they told NASA the shuttle shouldn't fly if the temperature was below 53 degrees—the previous lowest temperature—and on that morning it was 29.

Mr. McDonald said NASA was "appalled" by that statement. The man in charge of the meeting, a Mr. Mulloy, argued that the evidence was "incomplete"—some flights with erosion and blowby had occurred at *higher* than 53 degrees—so Thiokol should reconsider its opposition to flying.





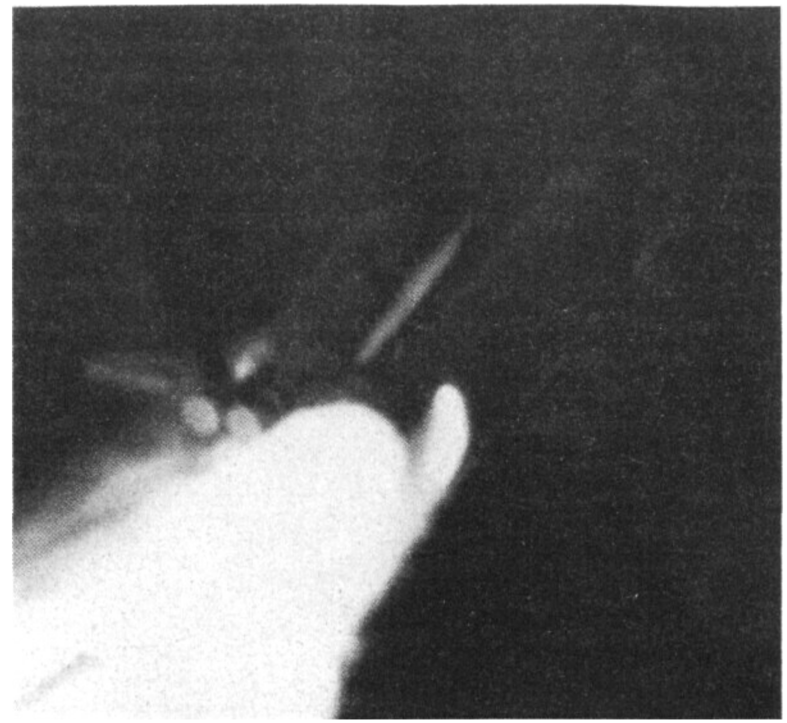


FIGURE 12. Progression of a flame, possibly from the leak test port area. (© NASA.)

Thiokol reversed itself, but McDonald refused to go along, saying, "If something goes wrong with this flight, I wouldn't want to stand up in front of a board of inquiry and say that I went ahead and told them to go ahead and fly this thing outside what it was qualified to."

That was so astonishing that Mr. Rogers had to ask, "Did I understand you correctly, that you said . . . ," and he repeated the story. And McDonald says, "Yes, sir."

The whole commission was shocked, because this was the first time any of us had heard *this* story: not only was there a failure in the seals, but there may have been

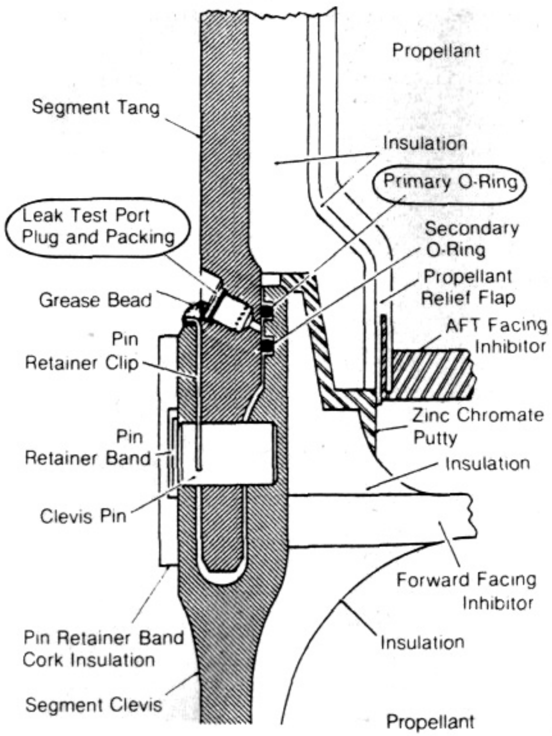


FIGURE 13. An incorrectly sealed leak test port could provide an escape route for a flame which burns past the primary O-ring.

a failure in management, too.

Mr. Rogers decided that we should look carefully into Mr. McDonald's story, and get more details before we made it public. But to keep the public informed, we would have an open meeting the following day, Tuesday, in which Mr. Cook would testify.

I thought, "This is going to be like an act: we're going to say the same things tomorrow as we did today, and we won't learn anything new."

As we were leaving, Bill Graham came over with a stack of papers for me.

"Geez! That's fast!" I said. "I only asked you for the information this morning!" Graham was always very cooperative.

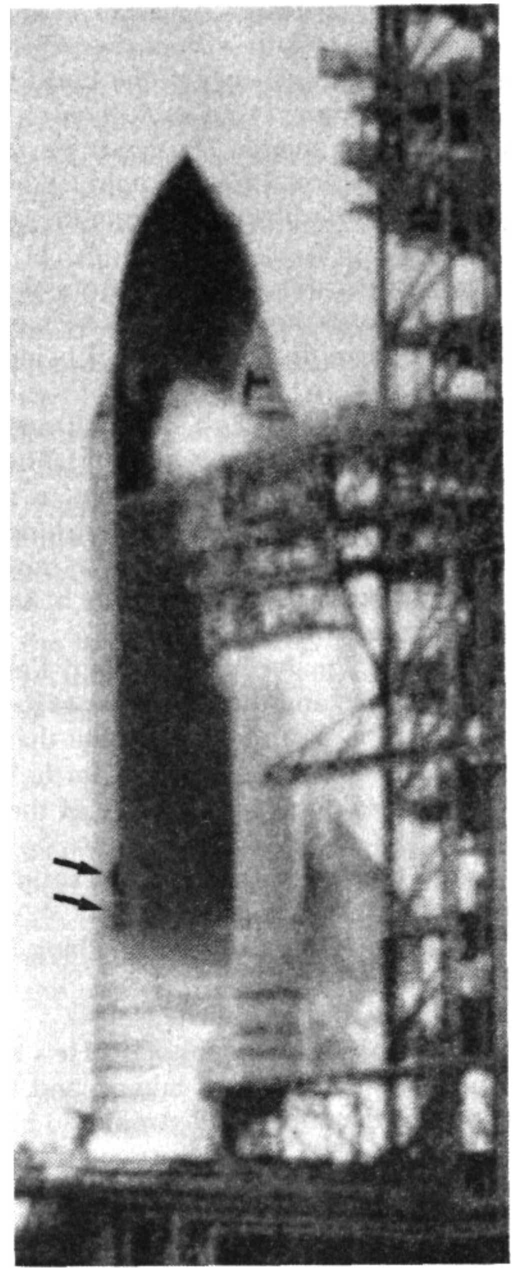


FIGURE 14. Puffs of black "smoke" (fine, unburned particles) were seen escaping from the same place where the flame was observed. (© NASA.)

The paper on top says, "Professor Feynman of the Presidential Commission wants to know about the effects over time of temperature on the resiliency of the O-rings . . ."—it's a memorandum addressed to a subordinate.

Under that memo is another memo: "Professor Feynman of the Presidential Commission wants to know . . ."—from that subordinate to *his* subordinate, and so on down the line.

There's a paper with some numbers on it from the poor bastard at the bottom, and then there's a series of submission papers which explain that the answer is being sent up to the next level.

So here's this stack of papers, just like a sandwich, and in the middle is the answer—to the wrong question! The answer was: "You squeeze the rubber for two hours at a certain temperature and pressure, and then see how long it takes to creep back"—over *hours.* I wanted to know how fast the rubber responds in *milliseconds* during a launch. So the information was of no use.

I went back to my hotel. I'm feeling lousy and I'm eating dinner; I look at the table, and there's a glass of ice water. I say to myself, "Damn it, / can find out about that rubber *without* having NASA send notes back and forth: I just have to *try* it! All I have to do is get a sample of the rubber."

I think, "I could do this tomorrow while we're all sittin' around, listening to this Cook crap we heard today. We always get ice water in those meetings; that's something I can do to save time."

Then I think, "No, that would be gauche."

But then I think of Luis Alvarez, the physicist. He's a guy I admire for his gutsiness and sense of humor, and I think, "If Alvarez was on this commission, he would do it, and that's good enough for me."

There are stories of physicists—great heroes—who have gotten information one, two, three—just like that—

where everybody else is trying to do it in a complicated way. For example, after ultraviolet rays and X-rays had been discovered, there was a new type, called N-rays, discovered by Andre Blondel, in France. It was hard to detect the N-rays: other scientists had difficulty repeating Blondel's experiments, so someone asked the great American physicist R. W. Wood to go to Blondel's laboratory.

Blondel gave a public lecture and demonstration. N-rays were bent by aluminum, so he had all kinds of lenses lined up, followed by a big disk with an aluminum prism in the middle. As the aluminum prism slowly turned, the N-rays came up this way and bent that way, and Blondel's assistant reported their intensity—different numbers for different angles.

N-rays were affected by light, so Blondel turned out the lights to make his readings more sensitive. His assistant continued to report their intensity.

When the lights came back on, there's R. W. Wood in the front row, holding the prism high in the air, balanced on the tips of his fingers, for all to see! So that was the end of the N-ray.

I think, "Exactly! I've got to get a sample of the rubber." I call Bill Graham.

It's impossible to get: it's kept somewhere down at Kennedy. But then Graham remembers that the model of the field joint we're going to use in our meeting tomorrow has two samples of the rubber in it. He says, "We could meet in my office before the meeting and see if we can get the rubber out."

The next morning I get up early and go out in front of my hotel. It's eight in the morning and it's snowing. I find a taxi and say to the driver, "I'd like to go to a hardware store."

"A hardware store, sir?"

"Yeah. I gotta get some tools."

"Sir, there's no hardware stores around here; the Capitol is over there, the White House is over there—wait a minute: I think I remember passing one the other day."

He found the hardware store, and it turned out it didn't open till 8:30—it was about 8:15—so I waited outside, in my suitcoat and tie, a costume I had assumed since I came to Washington in order to move among the natives without being too conspicuous.

The suitcoats that the natives wear inside their buildings (which are well heated) are sufficient for walking from one building to another—or from a building to a taxi if the buildings are too far apart. (All the taxis are heated.) But the natives seem to have a strange fear of the cold: they put overcoats on top of their suitcoats if they wish to step outside. I hadn't bought an overcoat yet, so I was still rather conspicuous standing outside the hardware store in the snow.

At 8:30 I went in and bought a couple of screwdrivers, some pliers, and the smallest C-clamp I could find. Then I went to NASA.

On the way to Graham's office, I thought maybe the clamp was too big. I didn't have much time, so I ran down to the medical department of NASA. (I knew where it was, because I had been going there for blood tests ordered by my cardiologist, who was trying to treat me by telephone.) I asked for a medical clamp like they put on tubes.

They didn't have any. But the guy says, "Well, let's see if your C-clamp fits inside a glass!" It fitted very easily.

I went up to Graham's office.

The rubber came out of the model easily with just a pair of pliers. So there I was with the rubber sample in my hand. Although I knew it would be more dramatic and honest to do the experiment for the first time in the public meeting, I did something that I'm a little bit ashamed of. I cheated. I couldn't resist. I tried it. So, following the example of having a closed meeting before an open meet-

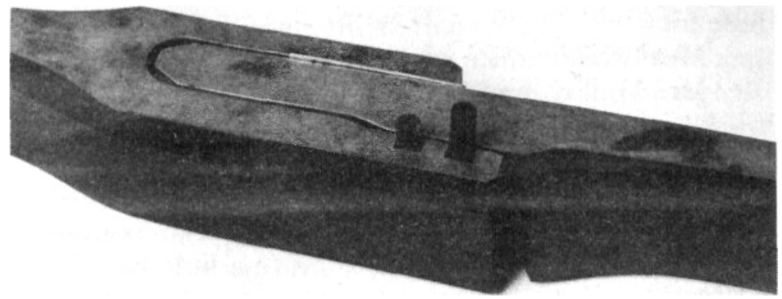


FIGURE 15. The field-joint model from which Feynman got the O-ring sample.

ing, I discovered it worked before I did it in the open meeting. Then I put the rubber back into the model so Graham could take it to the meeting.

I go to the meeting, all ready, with pliers in one pocket and a C-clamp in the other. I sit down next to General Kutyna.

At the previous meeting, there was ice water for everybody. This time, there's no ice water. I get up and go over to somebody who looks like he's in charge, and I say, "I'd like a glass of ice water, please."

He says, "Certainly! Certainly!"

Five minutes later, the guards close the doors, the meeting starts, and I haven't got my ice water.

I gesture over to the guy I just talked to. He comes over and says, "Don't worry, it's coming!"

The meeting is going along, and now Mr. Mulloy begins to tell us about the seals. (Apparently, NASA wants to tell us about the seals before Mr. Cook does.) The model starts to go around, and each commissioner looks at it a little bit.

Meanwhile, no ice water!

Mr. Mulloy explains how the seals are supposed to work—in the usual NASA way: he uses funny words and acronyms, and it's hard for anybody else to understand.

In order to set things up while I'm waiting for the ice water, I start out: "During a launch, there are vibrations which cause the rocket joints to move a little bit—is that correct?"

"That is correct, sir."

"And inside the joints, these so-called O-rings are supposed to expand to make a seal—is that right?"

"Yes, sir. In static conditions they should be in direct contact with the tang and clevis* and squeezed twenty-thousandths of an inch."

"Why don't we take the O-rings out?"

"Because then you would have hot gas expanding through the joint . . ."

"Now, in order for the seal to work correctly, the O-rings must be made of rubber—not something like lead, which, when you squash it, it stays."

"Yes, sir."

"Now, if the O-ring wasn't resilient for a second or two, would that be enough to be a very dangerous situation?"

"Yes, sir."

That led us right up to the question of cold temperature and the resilience of the rubber. I wanted to prove that Mr. Mulloy must have known that temperature had an effect, although—according to Mr. McDonald—he claimed that the evidence was "incomplete." But still, no ice water! So I had to stop, and somebody else started asking questions.

*The tang is the male part of the joint; the clevis is the female part (see Figure 13).

The model comes around to General Kutyna, and then to me. The clamp and pliers come out of my pocket, I take the model apart, I've got the O-ring pieces in my hand, but I still haven't got any ice water! I turn around again and signal the guy I've been bothering about it, and he signals back, "Don't worry, you'll get it!"

Pretty soon I see a young woman, way down in front, bringing in a tray with glasses on it. She gives a glass of ice water to Mr. Rogers, she gives a glass of ice water to Mr. Armstrong, she works her way back and forth along the rows of the dais, giving ice water to everybody! The poor woman had gotten everything together—jug, glasses, ice, tray, the whole thing—so that everybody could have ice water.

So finally, when I get my ice water, I don't drink it! I squeeze the rubber in the C-clamp, and put them in the glass of ice water.

After a few minutes, I'm ready to show the results of my little experiment. I reach for the little button that activates my microphone.

General Kutyna, who's caught on to what I'm doing, quickly leans over to me and says, "Co-pilot to pilot: not now."

Pretty soon, I'm reaching for my microphone again.

"Not now!" He points in our briefing book—with all the charts and slides Mr. Mulloy is going through—and says, "When he comes to this slide, here, that's the right time to do it."

Finally Mr. Mulloy comes to the place, I press the button for my microphone, and I say, "I took this rubber from the model and put it in a clamp in ice water for a while."

I take the clamp out, hold it up in the air, and loosen it as I talk: "I discovered that when you undo the clamp, the rubber doesn't spring back. In other words, for more than a few seconds, there is no resilience in this particular material when it is at a temperature of 32 degrees. I believe that

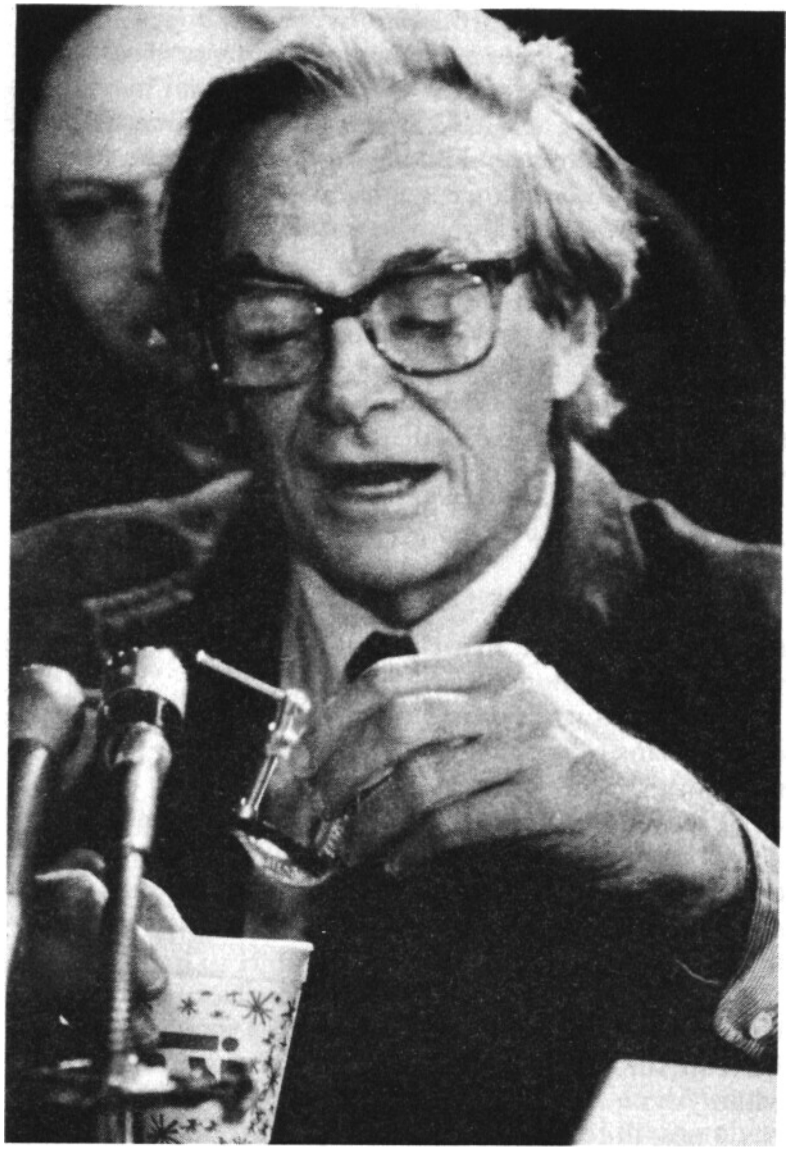


FIGURE 15A. The O-ring ice-water demonstration. (© MARILYNN K. YEE, NYT PICTURES.)

has some significance for our problem."

Before Mr. Mulloy could say anything, Mr. Rogers says, "That is a matter we will consider, of course, at length in the session that we will hold on the weather, and I think it is an important point which I'm sure Mr. Mulloy acknowledges and will comment on in a further session."

During the lunch break, reporters came up to me and asked questions like, "Were you talking about the O-ring, or the putty?" and "Would you explain to us what an O-ring is, exactly?" So I was rather depressed that I wasn't able to make my point. But that night, all the news shows caught on to the significance of the experiment, and the next day, the newspaper articles explained everything perfectly.

Check Six!

MY cousin Frances educated me about the press. She had been the AP White House correspondent during the Nixon and Ford administrations, and was now working for CNN. Frances would tell me stories of guys running out back doors because they're afraid of the press. From her I got the idea that the press isn't doing anything evil; the reporters are simply trying to help people know what's going on, and it doesn't do any harm to be courteous to them.

I found out that they're really quite friendly, if you give them a chance. So I wasn't afraid of the press, and I would always answer their questions.

Reporters would explain to me that I could say, "Not for attribution." But I didn't want any hocus-pocus. I didn't want it to sound like I'm leaking something. So whenever I talked to the press, I was straight. As a result of this, my name was in the newspaper every day, all over the place!

It seemed like I was always the one answering the reporters' questions. Often the rest of the commissioners would be anxious to go off to lunch, and I'd still be there, answering questions. But I figured, "What's the point of having a public meeting if you run away when they ask you what a word meant?"

When we'd finally get to our lunch, Mr. Rogers would remind us to be care-

ful not to talk to the press. I would say something like, "Well, I was just telling them about the O-rings."

He would say, "That's okay. You've been doing all right, Dr. Feynman; I have no problem with that." So I never did figure out, exactly, what he meant by "not talking to the press."

Being on the commission was rather tense work, so I enjoyed having dinner once in a while with Frances and Chuck, my sister's son, who was working for the *Washington Post.* Because Mr. Rogers kept talking about leaks, we made sure we never said a *word* about anything I was doing. If CNN needed to find out something from me, they'd have to send a different reporter. The same went for the *Post.*

I told Mr. Rogers about my relatives working for the press: "We've agreed not to talk about my work. Do you think there's any problem?"

He smiled and said, "It's perfectly all right. I have a cousin in the press, too. There's no problem at all."

On Wednesday the commission had nothing to do, so General Kutyna invited me over to the Pentagon to educate me on the relationship between the air force and NASA.

It was the first time I had ever been in the Pentagon. There were all these guys in uniform who would take orders—not like in civilian life. He says to one of them, "I'd like to use the briefing room . . ."

"Yes, sir!"

"... and we'll need to see slides number such-and-such and so-and-so."

"Yes, sir! Yes, sir!"

We've got all these guys working for us while General Kutyna gives me a big presentation in this special briefing room. The slides are shown from the back on a transparent wall. It was really fancy.

General Kutyna would say things like, "Senator So-and-so is in NASA's pocket," and I would say, half-joking, "Don't give me these side remarks, General; you're filling my head! But don't worry, I'll forget it all." I wanted to be naive: I'd find out what happened to the shuttle first; I'd worry about the big political pressures later.

Somewhere in his presentation, General Kutyna observed that everybody on the commission has some weakness because of their connections: he, having worked very closely with NASA personnel in his former position as Air Force Space Shuttle Program manager, finds it difficult, if not impossible, to drive home some of the tougher questions on NASA management. Sally Ride still has a job with NASA, so she can't just say everything she wants. Mr. Covert had worked on the engines, and had been a consultant to NASA, and so on.

I said, "I'm associated with Caltech, but I don't consider that a weakness!"

"Well," he says, "that's right. You're invincible—as far as we can see. But in the air force we have a rule: check six."

He explained, "A guy is flying along, looking in all directions, and feeling very safe. Another guy flies up behind him (at 'six o'clock'—'twelve o'clock' is directly in front), and shoots. Most airplanes are shot down that way. Thinking that you're safe is very dangerous! Somewhere, there's a weakness you've got to find. You must always check six o'clock."

An underling comes in. There's some mumbling about somebody else needing the briefing room now. General Kutyna says, "Tell them I'll be finished in ten minutes."

"Yes, sir!"

Finally, we go out. There, in the hall, are TEN GENERALS waiting to use the room—and I had been sitting in there, getting this personal briefing. I felt great.

For the rest of the day, I wrote a letter home. I began to worry about "check six" when I described Mr. Rogers' reaction to my visiting Frances and Chuck. I wrote,

... I was pleased by Rogers' reaction, but now as I write this I have second thoughts. It was too easy—after he explicitly talked about the importance of no leaks etc. at earlier meetings. Am I being set up? (SEE, DARLING, WASHINGTON PARANOIA IS SETTING IN.) ... I think it is possible that there are things in this somebody might be trying to keep me from finding out and might try to discredit me if I get too close. . . . So, reluctantly, I will have to not visit Frances and Chuck any more. Well, I'll ask Fran first if that is *too* paranoid. Rogers seemed so agreeable and reassuring. It was so easy, yet I am probably a thorn in his side. . . .

Tomorrow at 6:15 am we go by special airplane (two planes) to Kennedy Space Center to be "briefed." No doubt we shall wander about, being shown everything—gee whiz—but no time to get into technical details with anybody. Well, it won't work. If I am not satisfied by Friday, I will stay over Sat & Sun, or if they don't work then, Monday & Tuesday. I am determined to do the job of finding out what happened—let the chips fall!

My guess is that I will be allowed to do this, overwhelmed with data and details . . . , so they have time to soften up dangerous witnesses etc. But it won't work because (1) I do technical information exchange and understanding much faster than they imagine, and (2) I already smell certain rats that I will not forget, because I just love the smell of rats, for it is the spoor of exciting adventure.

I feel like a bull in a china shop. The best thing is to put the bull out to work on the plow. A better metaphor will be an ox in a china shop, because the china is the bull, of course.*

So, much as I would rather be home and doing something else, I am having a wonderful time.

Love,
Richard

The press was reporting rumors that NASA was under great political pressure to launch the shuttle, and there were various theories as to where the pressure was coming

*The thing Feynman was going to break up was the baloney (the "bull——") about how good everything was at NASA.

from. It was a great big world of mystery to me, with tremendous forces. I would investigate it, all right, and if I protected myself, nothing would happen. But I hadda watch out.

Gumshoes

FINALLY, early on Thursday morning, we get to Florida. The original idea was that we would go around the Kennedy Space Center at Cape Canaveral and see everything on a guided tour. But because information was coming out in the newspapers so fast, we had a public meeting first.

First, we saw some detailed pictures of the smoke coming out of the shuttle while it was still on the launch pad. There are cameras all over the place watching the launch—something like a hundred of them. Where the smoke came out, there were two cameras looking straight at it—but both failed, curiously. Nevertheless, from other cameras we could see four or five puffs of black smoke coming out from a field joint. This smoke was not burning material; it was simply carbon and mucky stuff that was pushed out because of pressure inside the rocket.

The puffs stopped after a few seconds: the seal got plugged up somehow, temporarily, only to break open again a minute later.

There was some discussion about how much matter came out in the smoke. The puffs of smoke were about six feet long, and a few feet thick. The amount of matter depends on how fine the particles are, and there could always be a big piece of glop inside the smoke cloud, so it's hard to judge. And because the pictures were taken from

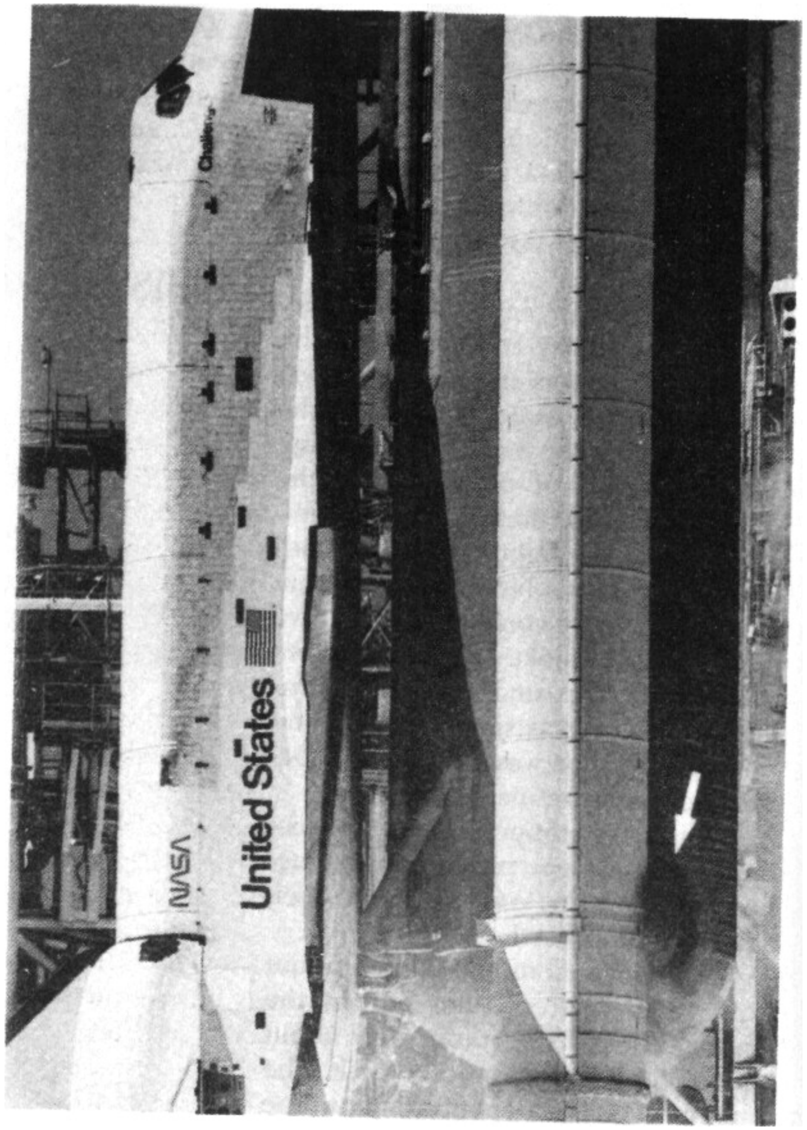


FIGURE 16. Detailed picture, taken from the launch pad, of the "smoke." (© NASA.)

the side, it was possible there was more smoke farther around the rocket.

To establish a minimum, I assumed a particle size that would produce as much smoke as possible out of a given amount of material. It came out surprisingly small—approximately one cubic inch: if you have a cubic inch of stuff, you can get that much smoke.

We asked for pictures from other launches. We found out later that there had never been any puffs of smoke on any previous flights.

We also heard about the low temperatures before the launch from a man named Charlie Stevenson, who was in charge of the ice crew. He said the temperature had gone down to 22 degrees during the night, but his crew got readings as low as 8 degrees at some places on the launch pad, and they couldn't understand why.

During the lunch break, a reporter from a local TV station asked me what I thought about the low temperature readings. I said it seemed to me that the liquid hydrogen and oxygen had chilled the 22-degree air even further as it flowed down the big fuel tank onto the rocket booster. For some reason, the reporter thought I had just told him some important, secret information, so he didn't use my name in his report that evening. Instead, he said, "This explanation comes from a Nobel Prize winner, so it must be right."

In the afternoon, the telemetering people gave us all kinds of information on the last moments of the shuttle. Hundreds of things had been measured, all of which indicated that everything was working as well as it could under the circumstances: the pressure in the hydrogen tank suddenly fell a few seconds after the flame had been observed; the gyros which steer the shuttle were working perfectly until one had to work harder than the other because there were side forces from the flame shooting out of the side of the booster rocket; the main engines even shut themselves down when the hydrogen tank exploded, because there was a pressure drop in the fuel lines.

That meeting lasted until 7:30 in the evening, so we postponed the tour until Friday and went straight to a dinner set up by Mr. Rogers.

At the dinner I happened to be seated next to Al Keel, who had joined the commission on Monday as its executive officer to help Mr. Rogers organize and run our work. He came to us from the White House—from something called the OMB*—and had a good reputation for doing a fine job at this and that. Mr. Rogers kept saying how lucky we were to get somebody with such high qualifications.

One thing that impressed me, though, was that Dr. Keel had a Ph.D. in aerospace, and had done some post-doc work at Berkeley. When he introduced himself on Monday, he joked that the last "honest work" he had done for a living was some aerodynamics work for the shuttle program ten or twelve years ago. So I felt very comfortable with him.

Well, I haven't been talking to Dr. Keel for more than five minutes, when he tells me he's never been so insulted in his life, that he didn't take this job to be so insulted, and that he doesn't want to talk to me anymore!

Now, I have a way of not remembering things when I do something dumb or annoying to people, so I forget what I said that put him out. Whatever it was, I thought I was joking, so I was very surprised by his reaction. I had undoubtedly said some boorish, brash, damn-fool thing, which I therefore can't remember!

Then there was a rather tense period of five or ten minutes, with me apologizing and trying to get a conversation going again. We finally got to talking again, somewhat. We were not big friends, but at least there was peace.

On Friday morning, we had another public meeting, this time to hear people from Thiokol and NASA talk about

*The Office of Management and Budget.

the night before the launch. Everything came out so slowly: the witness doesn't really want to tell you everything, so you have to get the answers out by asking exactly the right questions.

Other guys on the commission were completely awake—Mr. Sutter, for instance. "Exactly what were your quality criteria for acceptance under such-and-such and so-and-so?"—he'd ask specific questions like that, and it would turn out they didn't have any such criteria. Mr. Covert and Mr. Walker were the same way. Everybody was asking good questions, but I was fogged out most of the time, feeling a little bit behind.

Then this business of Thiokol changing its position came up. Mr. Rogers and Dr. Ride were asking two Thiokol managers, Mr. Mason and Mr. Lund, how many people were against the launch, even at the last moment.

"We didn't poll everyone," says Mr. Mason.

"Was there a substantial number against the launch, or just one or two?"

"There were, I would say, probably five or six in engineering who at that point would have said it is not as conservative to go with that temperature, and we don't know. The issue was we didn't know for sure that it would work."

"So it was evenly divided?"

"That's a very estimated number."

It struck me that the Thiokol managers were waffling. But I only knew how to ask simpleminded questions. So I said, "Could you tell me, sirs, the names of your four best seals experts, in order of ability?"

"Roger Boisjoly and Arnie Thompson are one and two. Then there's Jack Kapp and, uh . . . Jerry Burns."

I turned to Mr. Boisjoly, who was right there, at the meeting. "Mr. Boisjoly, were you in agreement that it was okay to fly?"

He says, "No, I was not."

I ask Mr. Thompson, who was also there.

"No, I was not."

I say, "Mr. Kapp?"

Mr. Lund says, "He is not here. I talked to him after the meeting, and he said, 'I would have made that decision, given the information we had.' '

"And the fourth man?"

"Jerry Burns. I don't know what his position was."

"So," I said, "of the four, we have one 'don't know,' one 'very likely yes,' and the two who were mentioned right away as being the *best* seal experts, *both said no."* So this "evenly split" stuff was a lot of crap. The guys who knew the *most* about the seals—what were *they* saying?

Late in the afternoon, we were shown around the Kennedy Space Center. It was interesting; it wasn't as bad as I had predicted. The other commissioners asked a lot of important questions. We didn't have time to see the booster-rocket assembly, but near the end we were going to see the wreckage that had been recovered so far. I was pretty tired of this group stuff, so I excused myself from the rest of the tour.

I ran down to Charlie Stevenson's place to see more pictures of the launch. I also found out more about the unusually low temperature readings. The guys were very cooperative, and wanted me to work with them. I had been waiting for *ten days* to run around in one of these places, and here I was, *at last!*

At dinner that night, I said to Mr. Rogers, "I was thinking of staying here over the weekend."

"Well, Dr. Feynman," he said, "I'd prefer you come back to Washington with us tonight. But of course, you're free to do whatever you want."

"Well, then," I said, "I'll stay."

On Saturday I talked to the guy who had actually taken the temperature readings the morning of the launch—a nice fella named B. K. Davis. Next to each temperature he had written the exact time he had measured it, and then took a picture of it. You could see large gaps between the times as he climbed up and down the big launch tower. He measured the temperature of the air, the rocket, the ground, the ice, and even a puddle of slush with antifreeze in it. He did a very complete job.

NASA had a theoretical calculation of how the temperatures should vary around the launch pad: they should have been more uniform, and higher. Somebody thought that heat radiating to the clear sky had something to do with it. But then someone else noticed that BK's reading for the slush was much lower than the photograph indicated: at 8 degrees, the slush—even with antifreeze in it—should have been frozen solid.

Then we looked at the device the ice crew used for measuring the temperatures. I got the instruction manual out, and found that you're supposed to put the instrument out in the environment for at least 20 minutes before using it. Mr. Davis said he had taken it out of the box—at 70 degrees—and began making measurements right away. Therefore we had to find out whether the errors were reproducible. In other words, could the circumstances be duplicated?

On Monday I called up the company that made the device, and talked to one of their technical guys: "Hi, my name is Dick Feynman," I said. "I'm on the commission investigating the *Challenger* accident, and I have some questions about your infrared scanning gun …"

"May I call you right back?" he says.

"Sure."

After a little while he calls me back: "I'm sorry, but it's proprietary information. I can't discuss it with you."

By this time I realized what the real difficulty was: the company was *scared green* that we were going to blame the accident on their instrument. I said, "Sir, your scanning gun has nothing to do with the accident. It was used by the people here in a way that's contrary to the procedures in your instruction manual, and I'm trying to figure out if we can reproduce the errors and determine what the temperatures really were that morning. To do this, I need to know more about your instrument."

The guy finally came around, and became quite cooperative. With his help, I advised the ice-crew guys on an experiment. They cooled a room down to about 40 degrees, and put a big block of ice in it—with ice, you can be sure the surface temperature is 32 degrees. Then they brought in the scanning gun from a room which was 70 degrees inside, and made measurements of the ice block every 30 seconds. They were able to measure how far off the instrument was as a function of time.

Mr. Davis had written his measurements so carefully that it was very easy to fix all the numbers. And then, remarkably, the recalculated temperatures were close to what was expected according to the theoretical model. It looked very sensible.

The next time I talked to a reporter, I straightened everything out about the temperatures, and informed him that the earlier theory expounded by the Nobel Prize winner was wrong.

I wrote a report for the other commissioners on the temperature problem, and sent it to Dr. Keel.

Then I investigated something we were looking into as a possible contributing cause of the accident: when the booster rockets hit the ocean, they became out of round a little bit from the impact. At Kennedy they're taken apart, and the sections—four for each rocket—are sent by rail to Thiokol in Utah, where they are packed with new propellant. Then they're put back on a train to Florida. During transport, the sections (which are hauled on their side) get squashed a little bit—the softish propellant is very heavy. The total amount of squashing is only a fraction of an inch,

but when you put the rocket sections back together, a small gap is enough to let hot gases through: the O-rings are only a quarter of an inch thick, and compressed only two-hundredths of an inch!

I thought I'd do some calculations. NASA gave me all the numbers on how far out of round the sections can get, so I tried to figure out how much the resulting squeeze was, and where it was located—maybe the minimum squeeze was where the leak occurred. The numbers were measurements taken along three diameters, every 60 degrees. But three matching diameters won't guarantee that things will fit; six diameters, or any other number of diameters, won't do, either.

For example, you can make a figure something like a triangle with rounded corners, in which three diameters, 60 degrees apart, have the same length.

I remembered seeing such a trick at a museum when I was a kid. There was a gear rack that moved back and forth perfectly smoothly, while underneath it were some noncircular, funny-looking, crazy-shaped gears turning on shafts that wobbled. It looked impossible, but the reason it worked was that the gears were shapes whose diameters were always the same.

So the numbers NASA gave me were useless.

During that weekend, just as I had predicted in my letter home, I kept getting notes from the commission headquarters in Washington: "Check the temperature readings, check the pictures, check this, check that . . ."—there was quite a list. But as the instructions came in, I had done most of them already.

One note had to do with a mysterious piece of paper. Someone at Kennedy had reportedly written "Let's go for it" while assembling one of the solid booster rockets. Such language appeared to show a certain recklessness. My mission: find that piece of paper.

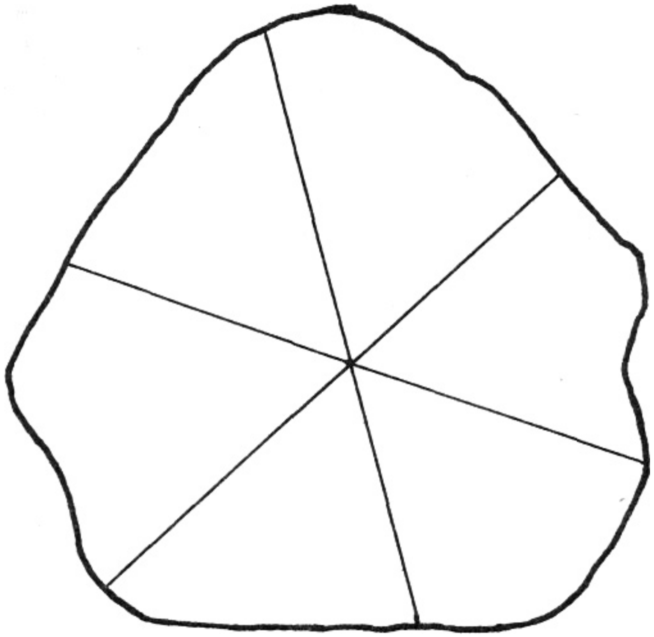


FIGURE 17. This figure has all its diameters the same length—yet it is obviously not round!

Well, by this time I understood how much paper there was in NASA. I was sure it was a trick to make me get lost, so I did nothing about it.

Instead, I pursued something surreptitiously.

It was rumored that the reason NASA tried to make the shuttle fly on January 28th, in spite of the cold, was that the president was going to give his State of the Union address that night. According to the theory, the White House had it all cooked up so that during the State of the Union address, the teacher, Mrs. McAuliffe, would talk to the president and Congress from space. It was gonna be great: the president would say, "Hello! How are you doing?" And she would say, "Fine"—something very dramatic.

Since it sounded logical, I began by supposing it was very likely possible. But was there any evidence? This kind of thing I didn't know how to investigate. I could only think of this: it's very hard to get through to the president; I also can't just call up an astronaut and talk to her—if she's in space. Therefore, switching the signals down from the shuttle over to the president while he's talking to Congress must be a complicated business.

To find out whether anybody had set up to do that, I went down to the lowest levels and asked guys at the bottom some technical questions.

They showed me the antennas, they told me about the frequencies, they showed me the big radio system and the computer system; they showed me all the ways they did things.

I said, "If you had to send a transmission somewhere else—to Marshall, say—how would you do it?"

They said, "Oh, we're just a relay station. Everything is automatically sent over to Houston, and they switch everything out from there. We don't do any switching here."

So I didn't find any evidence—at least at Kennedy. But the guys there were so nice to me, and everything was so pleasant, that I feel bad. I don't like to cheat people. It was a little sneaky, what I was doing. Nevertheless, I thought I'd better do the same thing when I got to Houston.

On Monday, Mr. Hotz came down to Florida to work with me. (He told me later that he had been sent down with specific instructions to see what I was doing, and to keep me from "going wild.") Mr. Hotz brought a list of things to look into: "There are a lot of things on this list," he said, "so I'd be happy to split the work with you." Some things he said he could do more easily, and the rest of the things I had already done—except for that piece of paper which said "Let's go for it." Mr. Hotz hinted around that it might have come from the diary of someone in the booster-rocket assembly. That wasn't enough of a clue for me; I just wasn't gonna do it. Instead, I went to see a Mr. Lamberth, who had said he wanted to talk to me.

Mr. Lamberth was way up in the works, a big cheese in charge of assembling the solid-rocket boosters. He wanted to tell me about some problems he had. "The workers used to have much better discipline," he explained, "but nowadays they're not like they used to be." He gave me a couple of examples.

The first incident had to do with taking the booster rockets apart after they had been recovered from the sea. The rocket sections are held together by 180 pins—each about an inch and a half in diameter and two inches long—all the way around.

There was some kind of procedure for taking sections apart, in which the workers were supposed to pull the rocket up a certain distance. They had gotten to paying attention only to the amount *force* they were applying—about 11,000 pounds. That was a better method, from a physical standpoint, because the idea is to take the load off the pins.

One time the force gauge wasn't working right. The workers kept putting more force on, wondering why they weren't reaching 11,000 pounds, when all of a sudden one of the pins broke.

Mr. Lamberth reprimanded the workers for not following procedures. It reminded me of when I tried to make things work better at my aunt's hotel: your method is better than the regular way, but then you have a little accident . . .*

The second story Mr. Lamberth told me had to do with putting the rocket sections together. The regular procedure was to stack one section on top of the other and match the upper section to the lower one.

*The reference is to Feynman's method of slicing string beans, recounted in *Surely You're Joking, Mr. Feynman!*

If a section needed to be reshaped a little bit, the procedure was to first pick up the section with a crane and let it hang sideways a few days. It's rather simpleminded.

If they couldn't make a section round enough by the hanging method, there was another procedure: use the "rounding machine"—a rod with a hydraulic press on one end and a nut on the other—and increase the pressure.

Mr. Lamberth told me the pressure shouldn't exceed 1200 pounds per square inch (psi). One time, a section wasn't round enough at 1200 psi, so the workers took a wrench and began turning the nut on the other end. When they finally got the section round enough, the pressure was up to 1350. "This is another example of the lack of discipline among the workers," Mr. Lamberth said.

I had wanted to talk with the assembly workers anyway (I love that kind of thing), so I arranged to see them the next day at 2:30 in the afternoon.

At 2:30 I walk into this room, and there's a long table with thirty or forty people—they're all sitting there with morose faces, very serious, ready to talk to The Commissioner.

I was terrified. I hadn't realized my terrible power. I could see they were worried. They must have been told I was investigating the errors they had made!

So right away I said, "I had nothin' to do, so I thought I'd come over and talk to the guys who put the rockets together. I didn't want everybody to stop working just 'cause I wanna find out something for my own curiosity; I only wanted to talk with the workers …"

Most of the people got up and left. Six or seven guys stayed—the crew who actually put the rocket sections together, their foreman, and some boss who was higher up in the system.

Well, these guys were still a little bit scared. They didn't really want to open up. The first thing I think to say is, "I have a question: when you measure the three diameters and all the diameters match, do the sections really fit together? It seems to me that you could have some bumps on one side and some flat areas directly across, so the three diameters would match, but the sections wouldn't fit."

"Yes, yes!" they say. "We get bumps like that. We call them nipples."

The only woman there said, "It's got nothing to do with me!"—and everybody laughed.

"We get nipples all the time," they continued. "We've been tryin' to tell the supervisor about it, but we never get anywhere!"

We were talking details, and that works wonders. I would ask questions based on what could happen theoretically, but to them it looked like I was a regular guy who knew about their technical problems. They loosened up very rapidly, and told me all kinds of ideas they had to improve things.

For example, when they use the rounding machine, they have to put a rod through holes exactly opposite each other. There are 180 holes, so they have to make sure the other end of the rod goes through the hole 90 holes away. Now, it turns out you have to climb up into an awkward place to count the holes. It's very slow and very difficult.

They thought it would be very helpful if there were four paint marks, 90 degrees apart, put on at the factory. That way, they would never have to count more than 22 holes to the nearest mark. For example, if they put the rod through a hole which is 9 holes clockwise from a paint mark, then the other end of the rod would go through the hole which is 9 holes clockwise from the opposite mark.

The foreman, Mr. Fichtel, said he wrote a memo with this suggestion to his superiors two years ago, but nothing had happened yet. When he asked why, he was told the suggestion was too expensive.

"Too expensive to paint *four little lines ?"* I said in disbelief.

They all laughed. "It's not the paint; it's the paperwork," Mr. Fichtel said. "They would have to revise all the manuals."

The assembly workers had other observations and suggestions. They were concerned that if two rocket sections scrape as they're being put together, metal filings could get into the rubber seals and damage them. They even had some suggestions for redesigning the seal. Those suggestions weren't very good, but the point is, the workers were *thinking!* I got the impression that they were *not* undisciplined; they were very interested in what they were doing, but they weren't being given much encouragement. Nobody was paying much attention to them. It was remarkable that their morale was as high as it was under the circumstances.

Then the workers began to talk to the boss who had stayed. "We're disappointed by something," one of them said. "When the commission was going to see the booster-rocket assembly, the demonstration was going to be done by the managers. Why wouldn't you let *us* do it?"

"We were afraid you'd be frightened by the commissioners and you wouldn't want to do it."

"No, no," said the workmen. "We think we do a good job, and we wanted to show what we do."

After that meeting, the boss took me to the cafeteria. As we were eating—the workmen weren't with us anymore—he said, "I was surprised they were so concerned about that."

Later, I talked to Mr. Fichtel about this incident of increasing the pressure past 1200. He showed me the notes he made as he went along: they weren't the formal papers that are stamped; they were part of an informal but carefully written diary.

I said, "I hear the pressure got up to 1350."

"Yes," he said, "we had tightened the nut at the other end."

"Was that the regular procedure?"

"Oh, yes," he said, "it's in the book."

He opens up the manual and shows me the procedure. It says, "Build up the pressure on the hydraulic jack. If this is insufficient to obtain desired roundness, then very carefully tighten nut on other end to get to the desired roundness"—it said so in black and white! It didn't say that tightening the nut would increase the pressure past 1200 psi; the people who wrote the manual probably weren't quite aware of that.

Mr. Fichtel had written in his diary, "We very carefully tightened the nut"—exactly the same language as the instructions.

I said, "Mr. Lamberth told me he admonished you about going above 1200."

"He never admonished me about that—why should he?"

We figured out what probably happened. Mr. Lamberth's admonishment went down through the levels until somebody in middle management realized that Mr. Fichtel had gone by the book, and that the error was in the manual. But instead of telling Mr. Lamberth about the error, they simply threw away the admonishment, and just kept quiet.

Over lunch, Mr. Fichtel told me about the inspection procedures. "There's a sheet for each procedure, like this one for the rounding procedure," he said. "On it there are boxes for stamps—one from the supervisor, one from quality control, one from the manufacturer, and for the bigger jobs, one from NASA."

He continued, "We make the measurements, go through one course of rounding, and then make the measurements again. If they don't match well enough, we repeat the steps. Finally, when the diameter differences are small enough, we go for it."

I woke up. "What do you mean, 'go for it'?" I said. "It sounds sort of cavalier …"

"No, no," he says. "That's just the lingo we use when we mean that all the conditions are satisfied, and we're ready to move to the next phase of the operation."

"Do you ever write that down—that 'go for it'?"

"Yes, sometimes."

"Let's see if we can find a place where you wrote it."

Mr. Fichtel looked through his diary, and found an example. The expression was completely natural to him—it wasn't reckless or cavalier; it was just his way of speaking.

On Monday and Tuesday, while I was running around down at Kennedy, Mr. Rogers was in Washington appearing before a Senate committee. Congress was considering whether it should have its own investigation.

Senator Hollings, from South Carolina, was giving Mr. Rogers a hard time: "Secretary Rogers," he says, "I'm anxious that you have an adequate staff thayah. How many *investigators* does yo' commission have?"

Mr. Rogers says, "We don't have investigators in the police sense. We're reading documents, understanding what they mean, organizing hearings, talking to witnesses—that sort of thing. We'll have an adequate staff, I assure you."

"Well, that's the point," Senator Hollings says. "From my experience in investigating cases, I'd want four or five investigators steeped in science and space technology going around down there at Canaveral talking to everybody, eating lunch with them. You'd be amazed, if you eat in the restaurants around there for two or three weeks, what you'll find out. You can't just sit and read what's given to you."

"We're not just going to sit and read," Mr. Rogers says defensively. "We've gotten a lot of people in a room and asked them questions all at the same time, rather than have a gumshoe walking around, talking to people one at a time."

"I understand," says Senator Rollings. "Yet I'm concerned about yo' product if you don't have some gumshoes. That's the trouble with presidential commissions; I've been on 'em: they go on what's *fed* to 'em, and they don't look behind it. Then we end up with investigative reporters, people writing books, and everything else. People are *still* investigating the Warren Commission Report around this town."*

Mr. Rogers calmly says, "I appreciate your comments, Senator. You'll be interested to know that one of our commission members—he's a Nobel laureate—is down there in Florida today, investigating in the way you'd like him to investigate."

(Mr. Rogers didn't know it, but I was actually eating lunch with some engineers when he said that!)

Senator Rollings says, "I'm not questioning the competence of the Nobel laureate; I've been reading with great interest what he said. There's no question about the competence of the commission itself. It's just that when you investigate a case, you need investigators. You have already brought to the public's attention a lot of very interesting facts, so I think you haven't been negligent in any fashion."

So I saved Mr. Rogers a little bit. He saw that he had an answer for Mr. Rollings by the *good luck* that I stayed in Florida anyway, against his wishes!

*Note for foreign readers: the Warren Report was issued in 1964 by the Warren Commission, headed by retired Supreme Court Chief Justice Earl Warren, which investigated the assassination of President John F. Kennedy.

Fantastic Figures

ON Tuesday afternoon I flew back to Washington, and went to the next meeting of the commission, on Wednesday. It was another public meeting. A manager of the Thiokol Company named Mr. Lund was testifying. On the night before the launch, Mr. Mulloy had told him to put on his "management hat" instead of his "engineering hat," so he changed his opposition to launch and overruled his own engineers. I was asking him some harsh questions when suddenly I had this feeling of the Inquisition.

Mr. Rogers had pointed out to us that we ought to be careful with these people, whose careers depend on us. He said, "We have all the advantages: we're sitting up here; they're sitting down there. They have to answer our questions; we don't have to answer their questions." Suddenly, all this came back to me and I felt terrible, and I couldn't do it the next day. I went back to California for a few days, to recover.

While I was in Pasadena, I went over to JPL and met with Jerry Solomon and Meemong Lee. They were studying the flame which appeared a few seconds before the main fuel tank exploded, and were able to bring out all kinds of details. (JPL has good enhancers of TV pictures from all their experience with planetary missions.) Later, I took the enhancements over to

Charlie Stevenson and his crew at Kennedy to expedite things.

Somewhere along the line, somebody from the staff brought me something to sign: it said that my expenses were so-and-so much, but they weren't—they were more. I said, "This is not the amount I actually spent."

The guy said, "I know that, sir; you're only allowed a maximum of $75 a day for the hotel and food."

"Then why did you guys set me up in a hotel which costs $80 or $90 a night, and then you give me only $75 a day?"

"Yes, I agree; it's too bad, but that's the way it goes!"

I thought of Mr. Rogers' offer to put me in a "good hotel." What did he mean by that—that it would cost me *more?*

If you're asked to contribute months of time and effort to the government (and you lose money you would have made consulting for a company), the government ought to appreciate it a little more than to be cheap about paying you back. I'm not trying to make money off the government, but I'm not wanting to *lose* money, either! I said, "I'm not going to sign this."

Mr. Rogers came over and promised he would straighten it out, so I signed the paper.

I really think Mr. Rogers tried to fix it, but he was unable to. I thought of fighting this one to the end, but then I realized it's impossible: if I had been paid for my actual expenses, then of course all the other commissioners would have to be paid, too. That would be all right, but it would also mean that this commission was the only commission to be paid its actual expenses—and pretty soon, word would get out.

They have a saying in New York: "You can't fight City Hall," meaning "It's impossible." But this time, it was a hell of a lot bigger than City Hall: the $75 a day rule is a law of the United States! It might have been fun to fight it to the end, but I guess I was tired—I'm not as young as I used to be—so I just gave up.

Somebody told me they heard commissioners make $1000 a day, but the truth is, our government doesn't even pay their costs.

At the beginning of March, about a month after the commission started, we finally split up into working groups: the Pre-Launch Activities group was headed by Mr. Acheson; Mr. Sutler was in charge of the Design, Development, and Production panel; General Kutyna was leader of the Accident Analysis group; and Dr. Ride was in charge of the Mission Planning and Operations group.

I spent most of my time in Kutyna's group. I was in Ride's group, too, but I ended up not doing very much for her.

General Kutyna's group went to Marshall Space Flight Center in Huntsville, Alabama, to do its work. The first thing that happened there was, a man named Ullian came in to tell us something. As range safety officer at Kennedy, Mr. Ullian had to decide whether to put destruct charges on the shuttle. (If a rocket goes out of control, the destruct charges enable it to be blown up into small bits. That's much less perilous than a rocket flying around loose, ready to explode when it hits the ground.)

Every unmanned rocket has these charges. Mr. Ullian told us that 5 out of 127 rockets that he looked at had failed—a rate of about 4 percent. He took that 4 percent and divided it by 4, because he assumed a manned flight would be safer than an unmanned one. He came out with about a 1 percent chance of failure, and that was enough to warrant the destruct charges.

But NASA told Mr. Ullian that the probability of failure was more like 1 in 10^5.

I tried to make sense out of that number. "Did you say 1 in 10^5?"

"That's right; 1 in 100,000."

"That means you could fly the shuttle *every day* for an average of *300 years* between accidents—every day, one flight, for 300 years—which is obviously crazy!"

"Yes, I know," said Mr. Ullian. "I moved my number up to 1 in 1000 to answer all of NASA's claims—that they were much more careful with manned flights, that the typical rocket isn't a valid comparison, et cetera—and put the destruct charges on anyway."

But then a new problem came up: the Jupiter probe, *Galileo,* was going to use a power supply that runs on heat generated by radioactivity. If the shuttle carrying *Galileo* failed, radioactivity could be spread over a large area. So the argument continued: NASA kept saying 1 in 100,000 and Mr. Ullian kept saying 1 in 1000, at best.

Mr. Ullian also told us about the problems he had in trying to talk to the man in charge, Mr. Kingsbury: he could get appointments with underlings, but he never could get through to Kingsbury and find out how NASA got its figure of 1 in 100,000. The details of the story I can't remember exactly, but I thought Mr. Ullian was doing everything sensibly.

Our panel supervised the tests that NASA was doing to discover the properties of the seals—how much pressure the putty could take, and so on—in order to find out exactly what had happened. General Kutyna didn't want to jump to conclusions, so we went over and over things, checking all the evidence and seeing how well everything fitted together.

There was an awful lot of detailed discussion about exactly what happened in the last few seconds of the flight, but I didn't pay much attention to any of it. It was as though a train had crashed because the track had a gap in it, and we were analyzing which cars broke apart first, which cars broke apart second, and why some car turned over on its side. I figured once the train goes off the track, it doesn't

make any difference—it's done. I became bored.

So I made up a game for myself: "Imagine that something else had failed—the main engines, for instance—and we were making the same kind of intensive investigation as we are now: would we discover the same slipping safety criteria and lack of communication?"

I thought I would do my standard thing—find out from the engineers how the engine works, what all the dangers are, what problems they've had, and everything else—and then, when I'm all loaded up so I know what I'm talking about, I'd confront whoever was claiming the probability of failure was 1 in 100,000.

I asked to talk to a couple of engineers about the engines. The guy says, "Okay, I'll fix it up. Is nine tomorrow morning okay?"

This time there were three engineers, their boss, Mr. Lovingood, and a few assistants—about eight or nine people.

Everybody had big, thick notebooks, full of papers, all nicely organized. On the front they said:

REPORT ON MATERIAL GIVEN TO COMMISSIONER RICHARD P. FEYNMAN ON MARCH WA-WA,* 1986.

I said, "Geez! You guys must have worked hard all night!"

"No, it's not so much work; we just put in the regular papers that we use all the time."

I said, "I just wanted to talk to a few engineers. There are so many problems to work on, I can't expect you all to stay here and talk to me."

But this time, everybody stayed.

Mr. Lovingood got up and began to explain everything to me in the usual NASA way, with charts and graphs which matched the information in my big book—all with bullets, of course.

*Feynman's way of saying, "whatever it was," I won't bother you with all the details, but I wanted to understand everything about the engine. So I kept asking my usual dumb-sounding questions.

After a while, Mr. Lovingood says, "Dr. Feynman, we've been going for two hours, now. There are 123 pages, and we've only covered 20 so far."

My first reaction was to say, "Well, it isn't really going to take such a long time. I'm always a little slow at the beginning; it takes me a while to catch on. We'll be able to go much faster near the end."

But then I had a second thought. I said, "In order to speed things up, I'll tell you what I'm doing, so you'll know where I'm aiming. I want to know whether there's the same lack of communication between the engineers and the management who are working on the engine as we found in the case of the booster rockets."

Mr. Lovingood says, "I don't think so. As a matter of fact, although I'm now a manager, I was trained as an engineer."

"All right," I said. "Here's a piece of paper each. Please write on your paper the answer to this question: what do you think is the probability that a flight would be uncompleted due to a failure in this engine?"

They write down their answers and hand in their papers. One guy wrote "$99\text{-}^{44}/100$% pure" (copying the Ivory soap slogan), meaning about 1 in 200. Another guy wrote something very technical and highly quantitative in the standard statistical way, carefully defining everything, that I had to translate—which also meant about 1 in 200. The third guy wrote, simply, "1 in 300."

Mr. Lovingood's paper, however, said,

> Cannot quantify. Reliability is judged from:
> - past experience
> - quality control in manufacturing
> - engineering judgment

"Well," I said, "I've got four answers, and one of them weaseled." I turned to Mr. Lovingood: "I think you weaseled."

"I don't think I weaseled."

"You didn't tell me *what* your confidence was, sir; you told me *how* you determined it. What I want to know is: after you determined it, what *was* it?"

He says, "100 percent"—the engineers'jaws drop, my jaw drops; I look at him, everybody looks at him—"uh, uh, minus epsilon!"

So I say, "Well, yes; that's fine. Now, the only problem is, WHAT IS EPSILON?"

He says, "10^{-5}." It was the same number that Mr. Ullian had told us about: 1 in 100,000.

I showed Mr. Lovingood the other answers and said, "You'll be interested to know that there *is* a difference between engineers and management here—a factor of more than 300."

He says, "Sir, I'll be glad to send you the document that contains this estimate, so you can understand it."*

I said, "Thank you very much. Now, let's get back to the engine." So we continued and, just like I guessed, we went faster near the end. I had to understand how the engine worked—the precise shape of the turbine blades,

*Later, Mr. Lovingood sent me that report. It said things like "The probability of mission success is necessarily very close to 1.0"—does that mean it is close to 1.0, or it *ought to be* close to 1.0?—and "Historically, this high degree of mission success has given rise to a difference in philosophy between unmanned and manned space flight programs; i.e., numerical probability versus engineering judgment." As far as I can tell, "engineering judgment" means they're just going to make up numbers! The probability of an engine-blade failure was given as a universal constant, as if all the blades were exactly the same, under the same conditions. The whole paper was quantifying everything. Just about every nut and bolt was in there: "The chance that a HPHTP pipe will burst is 10^{-7}." You can't estimate things like that; a probability of 1 in 10,000,000 is almost impossible to estimate. It was clear that the numbers for each part of the engine were chosen so that when you add everything together you get 1 in 100,000.

exactly how they turned, and so on—so I could understand its problems.

After lunch, the engineers told me all the problems of the engines: blades cracking in the oxygen pump, blades cracking in the hydrogen pump, casings getting blisters and cracks, and so on. They looked for these things with periscopes and special instruments when the shuttle came down after each flight.

There was a problem called "subsynchronous whirl," in which the shaft gets bent into a slightly parabolic shape at high speed. The wear on the bearings was so terrible—all the noise and the vibration—that it seemed hopeless. But they had found a way to get rid of it. There were about a dozen very serious problems; about half of them were fixed.

Most airplanes are designed "from the bottom up," with parts that have already been extensively tested. The shuttle, however, was designed "from the top down"—to save time. But whenever a problem was discovered, a lot of redesigning was required in order to fix it.

Mr. Lovingood isn't saying much now, but different engineers, depending on which problem it is, are telling me all this stuff, just like I could have found out if I went down to the engineers at Thiokol. I gained a great deal of respect for them. They were all very straight, and everything was great. We went all the way down to the end of the book. We made it.

Then I said, "What about this high-frequency vibration where some engines get it and others don't?"*

There's a quick motion, and a little stack of papers

*I had heard about this from Bill Graham. He said that when he was first on the job as head of NASA, he was looking through some reports and noticed a little bullet: "o 4,000 cycle vibration is within our data base." He thought that was a funny-looking phrase, so he began asking questions. When he got all the way through, he discovered it was a rather serious matter: some of the engines would vibrate so much that they couldn't be used. He used it as an example of how difficult it is to get information unless you go down and check on it yourself.

appears. It's all put together nicely; it fits nicely into my book. It's all about the 4000-cycle vibration!

Maybe I'm a little dull, but I tried my best not to accuse anybody of anything. I just let them show me what they showed me, and acted like I didn't see their trick. I'm not the kind of investigator you see on TV, who jumps up and accuses the corrupt organization of withholding information. But I was fully aware that they hadn't told me about the problem until I asked about it. I usually acted quite naive—which I was, for the most part.

At any rate, the engineers all leaped forward. They got all excited and began to describe the problem to me. I'm sure they were delighted, because technical people love to discuss technical problems with technical people who might have an opinion or a suggestion that could be useful. And of course, they were very anxious to cure it.

They kept referring to the problem by some complicated name—a "pressure-induced vorticity oscillatory wa-wa," or something.

I said, "Oh, you mean a whistle!"

"Yes," they said; "it exhibits the characteristics of a whistle."

They thought the whistle could be coming from a place where the gas rushed through a pipe at high speed and split into three smaller pipes—where there were two partitions. They explained how far they had gotten in figuring out the problem.

When I left the meeting, I had the definite impression that I had found the same game as with the seals: management reducing criteria and accepting more and more errors that weren't designed into the device, while the engineers are screaming from below, "HELP!" and "This is a RED ALERT!"

The next evening, on my way home in the airplane, I was having dinner. After I finished buttering my roll, I took the little piece of thin cardboard that the butter pat comes on, and bent it around in a U shape so there were two edges facing me. I held it up and started blowing on it, and pretty soon I got it to make a noise like a whistle.

Back in California, I got some more information on the shuttle engine and its probability of failure. I went to Rocketdyne and talked to engineers who were building the engines. I also talked to consultants for the engine. In fact one of them, Mr. Covert, was on the commission. I also found out that a Caltech professor had been a consultant for Rocketdyne. He was very friendly and informative, and told me about all the problems the engine had, and what he thought the probability of failure was.

I went to JPL and met a fellow who had just written a report for NASA on the methods used by the FAA* and the military to certify their gas turbine and rocket engines. We spent the whole day going back and forth over how to determine the probability of failure in a machine. I learned a lot of new names—like "Weibull," a particular mathematical distribution that makes a certain shape on a graph. He said that the original safety rules for the shuttle were very similar to those of the FAA, but that NASA had modified them as they began to get problems.

It turned out that NASA's Marshall Space Center in Huntsville designed the engine, Rocketdyne built them, Lockheed wrote the instructions, and NASA's Kennedy Space Center installed them! It may be a genius system of organization, but it was a complete fuzdazzle, as far as I was concerned. It got me terribly confused. I didn't know whether I was talking to the Marshall man, the Rocketdyne man, the Lockheed man, or the Kennedy man! So in the middle of all this, I got lost. In fact, all during this time—in March and April—I was running back and forth so much between California, Alabama, Houston, Florida, and Washington, B.C., that I often didn't know what day it was, or where I was.

*Note for foreign readers: Federal Aviation Administration.

After all this investigating on my own, I thought I'd write up a little report on the engine for the other commissioners. But when I looked at my notes on the testing schedules, there was some confusion: there would be talk about "engine #12" and how long "the engine" flew. But no engine ever was like that: it would be repaired all the time. After each flight, technicians would inspect the engines and see how many cracked blades there were on the rotor, how many splits there were in the casing, and so on. Then they'd repair "the engine" by putting on a new casing, a new rotor, or new bearings—they would replace lots of parts. So I would read that a particular engine had rotor #2009, which had run for 27 minutes in flight such-and-such, and casing #4091, which had run for 53 minutes in flights such-and-such and so-and-so. It was all mixed up.

When I finished my report, I wanted to check it. So the next time I was at Marshall, I said I wanted to talk to the engineers about a few very technical problems, just to check the details—I didn't need any management there.

This time, to my surprise, nobody came but the three engineers I had talked to before, and we straightened everything out.

When I was about to leave, one of them said, "You know that question you asked us last time—with the papers? We felt that was a loaded question. It wasn't fair."

I said, "Yes, you're quite right. It *was* a loaded question. I had an idea of what would happen."

The guy says, "I would like to revise my answer. I want to say that I cannot quantify it." (This guy was the one who had the most detailed answer before.)

I said, "That's fine. But do you agree that the chance of failure is 1 in 100,000?"

"Well, uh, no, I don't. I just don't want to answer."

Then one of the other guys says, "I said it was 1 in 300, and I still say it's 1 in 300, but I don't want to tell you how I got my number."

I said, "It's okay. You don't have to."

An Inflamed Appendix

ALL during this time, I had the impression that somewhere along the line the whole commission would come together again so we could talk to each other about what we had found out.

In order to aid such a discussion, I thought I'd write little reports along the way: I wrote about my work with the ice crew (analyzing the pictures and the faulty temperature readings); I wrote about my conversations with Mr. Lamberth and the assembly workers; and I even wrote about the piece of paper that said "Let's go for it." All these little reports I sent to Al Keel, the executive officer, to give to the other commissioners.

Now, this particular adventure—investigating the lack of communication between the managers and the engineers who were working on the engine—I also wrote about, on my little IBM PC at home. I was kind of tired, so I didn't have the control I wanted—it wasn't written with the same care as my other reports. But since I was writing it only as a report to the other commissioners, I didn't change the language before I sent it on to Dr. Keel. I simply attached a note that said "I think the other commissioners would be interested in this, but you can do with it what you want—it's a little strong at the end."

He thanked me, and said he sent my report to everybody.

Then I went to the Johnson Space Center, in Houston, to look into the avionics. Sally Ride's group was there, investigating safety matters in connection with the astronauts' experiences. Sally introduced me to the software engineers, and they gave me a tour of the training facilities for the astronauts.

It's really quite wonderful. There are different kinds of simulators with varying degrees of sophistication that the astronauts practice on. One of them is just like the real thing: you climb up, you get in; at the windows, computers are producing pictures. When the pilot moves the controls, the view out of the windows changes.

This particular simulator had the double purpose of teaching the astronauts and checking the computers. In the back of the crew area, there were trays full of cables running down through the cargo bay to somewhere in the back, where instruments simulated signals from the engines—pressures, fuel flow rates, and so on. (The cables were accessible because the technicians were checking for "cross talk"—interferences in the signals going back and forth.)

The shuttle itself is operated essentially by computer. Once it's lit up and starts to go, nobody inside does anything, because there's tremendous acceleration. When the shuttle reaches a certain altitude, the computers adjust the engine thrust down for a little while, and as the air thins out, the computers adjust the thrust up again. About a minute later, the two solid rocket boosters fall away, and a few minutes after that, the main fuel tank falls away; each operation is controlled by the computers. The shuttle gets into orbit automatically—the astronauts just sit in their seats.

The shuttle's computers don't have enough memory to hold all the programs for the whole flight. After the shuttle gets into orbit, the astronauts take out some tapes and load in the program for the next phase of the flight—

there are as many as six in all. Near the end of the flight, the astronauts load in the program for coming down.

The shuttle has four computers on board, all running the same programs. All four are normally in agreement. If one computer is out of agreement, the flight can still continue. If only two computers agree, the flight has to be curtailed and the shuttle brought back immediately.

For even more safety, there's a fifth computer—located away from the other four computers, with its wires going on different paths—which has only the program for going up and the program for coming down. (Both programs can barely fit into its memory.) If something happens to the other computers, this fifth computer can bring the shuttle back down. It's never had to be used.

The most dramatic thing is the landing. Once the astronauts know where they're supposed to land, they push one of three buttons—marked Edwards, White Sands, and Kennedy—which tells the computer where the shuttle's going to land. Then some small rockets slow the shuttle down a little, and get it into the atmosphere at just the right angle. That's the dangerous part, where all the tiles heat up.

During this time, the astronauts can't see anything, and everything's changing so fast that the descent has to be done automatically. At around 35,000 feet the shuttle slows down to less than the speed of sound, and the steering can be done manually, if necessary. But at 4000 feet something happens that is not done by the computer: the pilot pushes a button to lower the landing wheels.

I found that very odd—a kind of silliness having to do with the psychology of the pilots: they're heroes in the eyes of the public; everybody has the idea that they're steering the shuttle around, whereas the truth is they don't have to do anything until they push that button to lower the landing gear. They can't stand the idea that they really have nothing to do.

I thought it would be safer if the computer would lower the landing wheels, in case the astronauts were unconscious for some reason. The software engineers agreed, and added that putting down the landing wheels at the wrong time is very dangerous.

The engineers told me that ground control can send up the signal to lower the landing wheels, but this backup gave them some pause: what happens if the pilot is half-conscious, and thinks the wheels should go down at a certain time, and the controller on the ground knows it's the wrong time? It's much better to have the whole thing done by computer.

The pilots also used to control the brakes. But there was lots of trouble: if you braked too much at the beginning, you'd have no more brake-pad material left when you reached the end of the runway—and you're still moving! So the software engineers were asked to design a computer program to control the braking. At first the astronauts objected to the change, but now they're very delighted because the automatic braking works so well.

Although there's a lot of good software being written at Johnson, the computers on the shuttle are so obsolete that the manufacturers don't make them anymore. The memories in them are the old kind, made with little ferrite cores that have wires going through them. In the meantime we've developed much better hardware: the memory chips of today are much, much smaller; they have much greater capacity; and they're much more reliable. They have internal error-correcting codes that automatically keep the memory good. With today's computers we can design separate program modules so that changing the payload doesn't require so much program rewriting.

Because of the huge investment in the flight simulators and all the other hardware, to start all over again and re-

place the millions of lines of code that they've already built up would be very costly.

I learned how the software engineers developed the avionics for the shuttle. One group would design the software programs, in pieces. After that, the parts would be put together into huge programs, and tested by an independent group.

After both groups thought all the bugs had been worked out, they would have a simulation of an entire flight, in which every part of the shuttle system is tested. In such cases, they had a principle: this simulation is not just an exercise to check if the programs are all right; it is a *real flight*—if anything fails now, it's extremely serious, as if the astronauts were really on board and in trouble. Your reputation is on the line.

In the many years they had been doing this, they had had only six failures at the level of flight simulation, and not one in an actual flight.

So the computer people looked like they knew what they were doing: they knew the computer business was vital to the shuttle but potentially dangerous, and they were being extremely careful. They were writing programs that operate a very complex machine in an environment where conditions are changing drastically—programs which measure those changes, are flexible in their responses, and maintain high safety and accuracy. I would say that in some ways they were once in the forefront of how to ensure quality in robotic or interactive computer systems, but because of the obsolete hardware, it's no longer true today.

I didn't investigate the avionics as extensively as I did the engines, so I might have been getting a little bit of a sales talk, but I don't think so. The engineers and the managers communicated well with each other, and they were all very careful not to change their criteria for safety.

I told the software engineers I thought their system and their attitude were very good.

One guy muttered something about higher-ups in NASA wanting to cut back on testing to save money: "They keep saying we always pass the tests, so what's the use of having so many?"

Before I left Houston, I continued my surreptitious investigation of the rumor that the White House had put pressure on NASA to launch the shuttle. Houston is the center of communication, so I went over to the telemetry people and asked about their switching system. I went through the same stuff as I did in Florida—and they were just as nice to me—but this time I found out that if they wanted to tie in the shuttle to the Congress, the White House, or to anywhere, they need a three-minute warning—not three months, not three days, not three hours—three minutes. Therefore they can do it whenever they want, and nothing has to be written down in advance. So that was a blind alley.

I talked to a *New York Times* reporter about this rumor one time. I asked him, "How do you find out if things like this are true?"

He says, "One of the things I thought to do was to go down and talk to the people who run the switching system. I tried that, but I wasn't able to come up with anything."

During the first half of April, General Kutyna's group received the final results of the tests NASA was making at Marshall. NASA included its own interpretations of the results, but we thought we should write everything over again in our own way. (The only exceptions were when a test didn't show anything.)

General Kutyna set up a whole system at Marshall for writing our group's report. It lasted about two days. Before we could get anywhere, we got a message from Mr. Rogers:

"Come back to Washington. You shouldn't do the writing down there."

So we went back to Washington, and General Kutyna gave me an office in the Pentagon. It was fine, but there was no secretary, so I couldn't work fast.

Bill Graham had always been very cooperative, so I called him up. He arranged for me to use a guy's office—the guy was out of town—and his secretary. She was very, very helpful: she could write up something as fast as I could say it, and then she'd revamp it, correcting my mistakes. We worked very hard for about two or three days, and got large pieces of the report written that way. It worked very well.

Neil Armstrong, who was in our group, is extremely good at writing. He would look at my work and immediately find every weak spot, just like that—he was right every time—and I was very impressed.

Each group was writing a chapter or two of the main report. Our group wrote some of the stuff in "Chapter 3: The Accident," but our main work was "Chapter 4: The Cause of the Accident." One result of this system, however, was that we never had a meeting to discuss what each of our groups found out—to comment on each other's findings from our different perspectives. Instead, we did what they call "wordsmithing"—or what Mr. Hotz later called "tombstone engraving"—correcting punctuation, refining phrases, and so on. We never had a *real* discussion of ideas, except incidentally in the course of this wordsmithing.

For example, a question would come up: "Should this sentence about the engines be worded this way or that way?"

I would try to get a little discussion started. "From my own experiences, I got the impression that the engines aren't as good as you're saying here . . ."

So they'd say, "Then we'll use the more conservative wording here," and they'd go on to the next sentence.

Perhaps that's a very efficient way to get a report out quickly, but we spent meeting after meeting doing this wordsmithing.

Every once in a while we'd interrupt that to discuss the typography and the color of the cover. And after each discussion, we were asked to vote. I thought it would be most efficient to vote for the same color we had decided on in the meeting before, but it turned out I was always in the minority! We finally chose red. (It came out blue.)

One time I was talking to Sally Ride about something I mentioned in my report on the engines, and she didn't seem to know about it. I said, "Didn't you see my report?"

She says, "No, I didn't get a copy."

So I go over to Keel's office and say, "Sally tells me she didn't get a copy of my report."

He looks surprised, and turns to his secretary. "Please make a copy of Dr. Feynman's report for Dr. Ride."

Then I discover Mr. Acheson hasn't seen it.

"Make a copy and give it to Mr. Acheson."

I finally caught on, so I said, "Dr. Keel, I don't think anybody has seen my report."

So he says to his secretary, "Please make a copy for all the commissioners and give it to them."

Then I said to him, "I appreciate how much work you're doing, and that it's difficult to keep everything in mind. But I thought you told me that you showed my report to everybody."

He says, "Yes, well, I meant all of the staff."

I later discovered, by talking to people on the staff, that they hadn't seen it either.

When the other commissioners finally got to see my report, most of them thought it was very good, and it ought to be in the commission report somewhere.

Encouraged by that, I kept bringing up my report. "I'd like to have a meeting to discuss what to do with it," I kept saying.

"We'll have a meeting about it next week" was the standard answer. (We were too busy wordsmithing and voting on the color of the cover.)

Gradually I realized that the way my report was written, it would require a lot of wordsmithing—and we were running out of time. Then somebody suggested that my report could go in as an appendix. That way, it wouldn't have to be wordsmithed to fit in with anything else.

But some of the commissioners felt strongly that my report should go in the main report somehow: "The appendices won't come out until months later, so nobody will read your report if it's an appendix," they said.

I thought I'd compromise, however, and let it go in as an appendix.

But now there was a new problem: my report, which I had written on my word processor at home, would have to be converted from the IBM format to the big document system the commission was using. They had a way of doing that with an optical scanning device.

I had to go to a little bit of trouble to find the right guy to do it. Then, it didn't get done right away. When I asked what happened, the guy said he couldn't find the copy I had given him. So I had to give him another copy.

A few days later, I finished writing my report about the avionics, and I wanted to combine it with my report on the engines. So I took the avionics report to the guy and I said, "I'd like to put this in with my other report."

Then I needed to see a copy of my new report for some reason, but the guy gave me an old copy, before the avionics was added. "Where's the new one with the avionics?" I said.

"I can't find it"—and so on. I don't remember all the details, but it seemed my report was always missing or half-cooked. It could easily have been mistakes, but there were too many of them. It was quite a struggle, nursing my report along.

Then, in the last couple of days, when the main report is ready to be sent to the printer, Dr. Keel wants my report to be wordsmithed too, even though it's going in as an appendix. So I took it to the regular editor there, a capable man named Hansen, and he fixed it up without changing the sense of it. Then it was put back into the machine as "Version #23"—there were revisions and revisions.

(By the way: *everything* had 23 versions. It has been noted that computers, which are supposed to increase the speed at which we do things, have not increased the speed at which we write reports: we used to make only three versions—because they're so hard to type—and now we make 23 versions!)

The next day I noticed Keel working on my report: he had put all kinds of big circles around whole sections, with X's through them; there were all kinds of thoughts left out. He explained, "This part doesn't have to go in because it says more or less what we said in the main report."

I tried to explain that it's much easier to get the logic if all the ideas are together, instead of everything being distributed in little pieces all over the main report. "After all," I said, "it's only gonna be an appendix. It won't make any difference if there's a little repetition."

Dr. Keel put back something here and there when I asked him to, but there was still so much missing that my report wasn't anything like it was before.

The Tenth Recommendation

SOMETIME in May, at one of our last meetings, we got around to making a list of possible recommendations. Somebody would say, "Maybe one of the things we should discuss is the establishment of a safety board."

"Okay, we'll put that down."

I'm thinking, "At last! We're going to have a discussion!"

But it turns out that this tentative list of topics *becomes* the recommendations—that there be a safety board, that there be a this, that there be a that. The only discussion was about which recommendation we should write first, which one should come second, and so forth.

There were many things I wanted to discuss further. For example, in regard to a safety board, one could ask: "Wouldn't such a committee just add another layer to an already overgrown bureaucracy?"

There had been safety boards before. In 1967, after the Apollo accident, the investigating committee at the time invented a special panel for safety. It worked for a while, but it didn't last.

We didn't discuss why the earlier safety boards were no longer effective; instead, we just made up more safety boards: we called them the "Independent Solid Rocket Motor Design Oversight Committee," the "Shuttle Transportation System Safety Advisory

Panel," and the "Office of Safety, Reliability, and Quality Assurance." We decided who would oversee each safety board, but we didn't discuss whether the safety boards created by our commission had any better chance of working, whether we could fix the existing boards so they *would* work, or whether we should have them at all.

I'm not as sure about a lot of things as everybody else. Things need to be thought out a little bit, and we weren't doing enough *thinking* together. Quick decisions on important matters are not very good—and at the speed we were going, we were bound to make some impractical recommendations.

We ended up rearranging the list of possible recommendations and wordsmithing them a little, and then we voted yes or no. It was an odd way of doing things, and I wasn't used to it. In fact, I got the feeling we were being railroaded: things were being decided, somehow, a little out of our control.

At any rate, in our last meeting, we agreed to nine recommendations. Many of the commissioners went home after that meeting, but I was going to New York a few days later, so I stayed in Washington.

The next day, I happened to be standing around in Mr. Rogers's office with Neil Armstrong and another commissioner when Rogers says, "I thought we should have a tenth recommendation. Everything in our report is so negative; I think we need something positive at the end to balance it."

He shows me a piece of paper. It says,

> The Commission strongly recommends that NASA continue to receive the support of the Administration and the nation. The agency constitutes a national resource and plays a critical role in space exploration and development. It also provides a symbol of national pride and technological leadership. The Commission applauds NASA's spectacular achievements of the past and anticipates impressive achievements to come. The findings and recommendations presented in this report are intended to contribute to the future NASA successes that the nation both expects and requires as the 21st century approaches.

In our four months of work as a commission, we had never discussed a policy question like that, so I felt there was no reason to put it in. And although I'm not saying I disagreed with it, it wasn't obvious that it was true, either. I said, "I think this tenth recommendation is inappropriate."

I think I heard Armstrong say, "Well, if somebody's not in favor of it, I think we shouldn't put it in."

But Rogers kept working on me. We argued back and forth a little bit, but then I had to catch my flight to New York.

While I was in the airplane, I thought about this tenth recommendation some more. I wanted to lay out my arguments carefully on paper, so when I got to my hotel in New York, I wrote Rogers a letter. At the end I wrote, "This recommendation reminds me of the NASA flight reviews: 'There are critical problems, but never mind—keep on flying!' '

It was Saturday, and I wanted Mr. Rogers to read my letter before Monday. So I called up his secretary—everybody was working seven days a week to get the report out in time—and I said, "I'd like to dictate a letter to you; is that all right?"

She says, "Sure! To save you some money, let me call you right back." She calls me back, I dictate the letter, and she hands it directly to Rogers.

When I came back on Monday, Mr. Rogers said, "Dr. Feynman, I've read your letter, and I agree with everything it says. But you've been out-voted."

"Out-voted? How was I out-voted, when there was no meeting?"

Keel was there, too. He says, "We called everybody, and they all agree with the recommendation. They all voted for it."

"I don't think that's fair!" I protested. "If I could have presented my arguments to the other commissioners, I don't think I'd have been out-voted." I didn't know what to do, so I said, "I'd like to make a copy of it."

When I came back, Keel says, "We just remembered that we didn't talk to Hotz about it, because he was in a meeting. We forgot to get his vote."

I didn't know what to make of that, but I found out later that Mr. Hotz was in the building, not far from the copy machine.

Later, I talked to David Acheson about the tenth recommendation. He explained, "It doesn't really mean anything; it's only motherhood and apple pie."

I said, "Well, if it doesn't mean anything, it's not necessary, then."

"If this were a commission for the National Academy of Sciences, your objections would be proper. But don't forget," he says, "this is a presidential commission. We should say something for the President."

"I don't understand the difference," I said. *"Why can't I be careful and scientific when I'm writing a report to the President?"*

Being naive doesn't always work: my argument had no effect. Acheson kept telling me I was making a big thing out of nothing, and I kept saying it weakened our report and it shouldn't go in.

So that's where it ended up: "The Commission strongly recommends that NASA continue to receive the support of the Administration and the nation . . ."—all this "motherhood and apple pie" stuff to "balance" the report.

While I was flying home, I thought to myself, "It's funny that the only part of the report that was *genuinely* balanced was my own report: I said negative things about the engine, and positive things about the avionics. And I had to struggle with them to get it in, even as a lousy appendix!"

I thought about the tenth recommendation. All the other recommendations were based on evidence we had found, but this one had no evidence whatsoever. I could see the whitewash dripping down. It was *obviously* a mistake! It would make our report look bad. I was very disturbed.

When I got home, I talked to Joan, my sister. I told her about the tenth recommendation, and how I had been "out-voted."

"Did you call any of the other commissioners and talk to them yourself?" she said.

"Well, I talked to Acheson, but he was for it."

"Any others?"

"Uh, no." So I called up three other commissioners—I'll call them A, B, and C.

I call A, who says, *"What* tenth recommendation?"

I call B, who says, "Tenth recommendation? What are you talking about?"

I call C, who says, "Don't you remember, you dope? I was in the office when Rogers first told us, and I don't see anything wrong with it."

It appeared that the only people who knew about the tenth recommendation were the people who were in the office when Rogers told us. I didn't bother to make any more telephone calls. After all, it's enough—I didn't feel that I had to open all the safes to check that the combination is the same!*

Then I told Joan about my report—how it was so emasculated, even though it was going in as an appendix.

*This refers to "Safecracker Meets Safecracker," another story told in *Surely You're Joking, Mr. Feynman!*

She says, "Well, if they do that to your report, what have you accomplished, being on the commission? What's the result of all your work?"

"Aha!"

I sent a telegram to Mr. Rogers:

PLEASE TAKE MY SIGNATURE OFF THE REPORT UNLESS TWO THINGS OCCUR: 1) THERE IS NO TENTH RECOMMENDATION, AND 2) MY REPORT APPEARS WITHOUT MODIFICATION FROM VERSION #23.

(I knew by this time I had to define everything carefully.)

In order to get the number of the version I wanted, I called Mr. Hotz, who was in charge of the documentation system and publishing the report. He sent me Version #23, so I had something definite to publish on my own, if worse came to worst.

The result of this telegram was that Rogers and Keel tried to negotiate with me. They asked General Kutyna to be the intermediary, because they knew he was a friend of mine. What *a good* friend of mine he was, they didn't know.

Kutyna says, "Hello, Professor, I just wanted to tell you that I think you're doing very well. But I've been given the job of trying to talk you out of it, so I'm going to give you the arguments."

"Fear not!" I said. "I'm not gonna change my mind. Just give me the arguments, and fear not."

The first argument was that if I don't accept the tenth recommendation, they won't accept my report, even as an appendix.

I didn't worry about that one, because I could always put out my report myself.

All the arguments were like that: none of them was very good, and none of them had any effect. I had thought through carefully what I was doing, so I just stuck to my guns.

Then Kutyna suggested a compromise: they were willing to go along with my report as I wrote it, except for one sentence near the end.

I looked at the sentence and I realized that I had already made my point in the previous paragraph. Repeating the point amounted to polemics; removing the phrase made my report much better. I accepted the compromise.

Then I offered a compromise on the tenth recommendation: "If they want to say something nice about NASA at the end, just don't call it a recommendation, so people will know that it's not in the same class as the other recommendations: call it a 'concluding thought' if you want. And to avoid confusion, don't use the words 'strongly recommends.' Just say 'urges'—'The Commission urges that NASA continue to receive the support of the Administration and the nation.' All the other stuff can stay the same."

A little bit later, Keel calls me up: "Can we say *'strongly* urges'?"

"No. Just 'urges'."

"Okay," he said. And that was the final decision.

Meet the Press

I PUT my name on the main report, my own report got in as an appendix, and everything was all right. In early June we went back to Washington and gave our report to the President in a ceremony held in the Rose Garden. That was on a Thursday. The report was not to be released to the public until the following Monday, so the President could study it.

Meanwhile, the newspaper reporters were working like demons: they knew our report was finished and they were trying to scoop each other to find out what was in it. I knew they would be calling me up day and night, and I was afraid I would say something about a technical matter that would give them a hint.

Reporters are very clever and persistent. They'll say, "We heard such-and-such — is it true?" And pretty soon, what you're thinking you didn't tell them shows up in the newspaper!

I was determined not to say a word about the report until it was made public, on Monday. A friend of mine convinced me to go on the "MacNeil/Lehrer Newshour," so I said yes for Monday evening's show.

I also had my secretary set up a press conference for Tuesday at Caltech. I said, "Tell the reporters who want to talk to me that I haven't any comment on anything: any questions they have, I'll be glad to answer on

FIGURE 18. The Commission Report was presented to the president in the Rose Garden at the White House. Visible, from left to right, are General Kutyna, William Rogers, Eugene Covert, President Reagan, Neil Armstrong, and Richard Feynman. (© PETE SOUZA, THE WHITE HOUSE.)

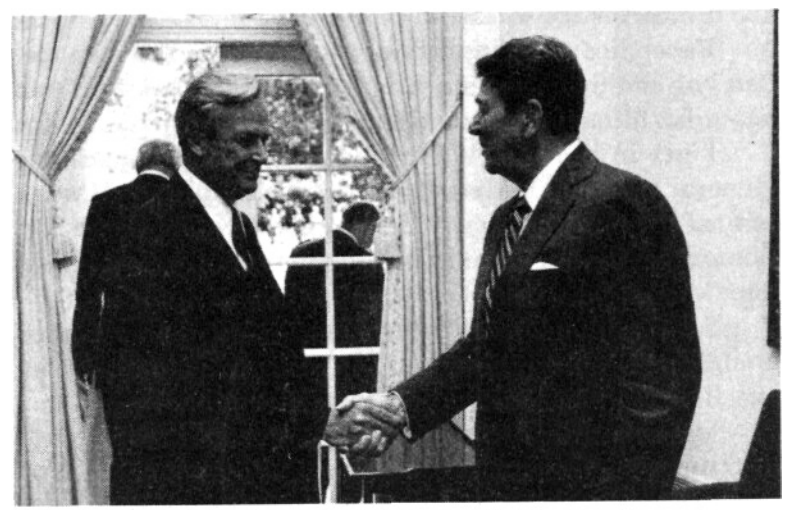


FIGURE 19. At the reception. (© PETE SOUZA, THE WHITE HOUSE.)

Tuesday at my press conference."

Over the weekend, while I was still in Washington, it leaked somehow that I had threatened to take my name off the report. Some paper in Miami started it, and soon the story was running all over about this argument between me and Rogers. When the reporters who were used to covering Washington heard "Mr. Feynman has nothing to say; he'll answer all your questions at his press conference on Tuesday," it sounded suspicious—as though the argument was still on, and I was going to have this press conference on Tuesday to explain why I took my name off the report.

But I didn't know anything about it. I isolated myself from the press so much that I wasn't even reading the newspapers.

On Sunday night, the commission had a goodbye dinner arranged by Mr. Rogers at some club. After we finished eating, I said to General Kutyna, "I can't stay around anymore. I have to leave a little early."

He says, "What can be so important?"

I didn't want to say.

He comes outside with me, to see what this "important" something is. It's a bright red sports car with two beautiful blonds inside, waiting to whisk me away.

I get in the car. We're about to speed off, leaving General Kutyna standing there scratching his head, when one of the blonds says, "Oh! General Kutyna! I'm Ms. So-and-so. I interviewed you on the phone a few weeks ago."

So he caught on. They were reporters from the "MacNeil/Lehrer Newshour."

They were very nice, and we talked about this and that for the show Monday night. Somewhere along the line I told them I was going to have my own press conference on Tuesday, and I was going to give out my report—even though it was going to appear as an appendix three months

later. They said my report sounded interesting, and they'd like to see it. By this time we're all very friendly, so I gave them a copy.

They dropped me off at my cousin's house, where I was staying. I told Frances about the show, and how I gave the reporters a copy of my report. Frances puts her hands to her head, horrified.

I said, "Yes, that was a dumb mistake, wasn't it! I'd better call 'em up and tell 'em not to use it."

I could tell by the way Frances shook her head that it wasn't gonna be so easy!

I call one of them up: "I'm sorry, but I made a mistake: I shouldn't have given my report to you, so I'd prefer you didn't use it."

"We're in the news business, Dr. Feynman. The goal of the news business is to get news, and your report is newsworthy. It would be completely against our instincts and practice not to use it."

"I know, but I'm naive about these things. I simply made a mistake. It's not fair to the other reporters who will be at the press conference on Tuesday. After all, would you like it if you came to a press conference and the guy had mistakenly given his report to somebody else? I think you can understand that."

"I'll talk to my colleague and call you back."

Two hours later, they call back—they're both on the line—and they try to explain to me why they should use it: "In the news business, it's customary that whenever we get a document from somebody the way we did from you, it means we can use it."

"I appreciate that there are conventions in the news business, but I don't know anything about these things, so as a courtesy to me, please don't use it."

It went back and forth a little more like that. Then another "We'll call you back," and another long delay. I could tell from the long delays that they were having a lot of trouble with this problem.

I was in a very good fettle, for some reason. I had already lost, and I knew what I needed, so I could focus easily. I had no difficulty admitting complete idiocy—which is usually the case when I deal with the world—and I didn't think there was any law of nature which said I had to give in. I just kept going, and didn't waver at all.

It went late into the night: one o'clock, two o'clock, we're still working on it. "Dr. Feynman, it's very unprofessional to give someone a story and then retract it. This is not the way people behave in Washington."

"It's obvious I don't know anything about Washington. But this is the way *I* behave—like a fool. I'm sorry, but it was simply an error, so as a courtesy, please don't use it."

Then, somewhere along the line, one of them says, "If we go ahead and use your report, does that mean you won't go on the show?"

"You said it; *I* didn't."

"We'll call you back."

Another delay.

Actually, I hadn't decided whether I'd refuse to go on the show, because I kept thinking it was possible I could undo my mistake. When I thought about it, I didn't think I could legitimately play that card. But when one of them made the mistake of proposing the possibility, I said, *"You* said it; *I* didn't"—very cold—as if to say, "I'm not threatening you, but you can figure it out for yourself, honey!"

They called me back, and said they wouldn't use my report.

When I went on the show, I never got the impression that any of the questions were based on my report. Mr. Lehrer did ask me whether there had been any problems between me and Mr. Rogers, but I weaseled: I said there had been no problems.

After the show was over, the two reporters told me

they thought the show went fine without my report. We left good friends.

I flew back to California that night, and had my press conference on Tuesday at Caltech. A large number of reporters came. A few asked questions about my report, but most of them were interested in the rumor that I had threatened to take my name off the commission report. I found myself telling them over and over that I had no problem with Mr. Rogers.

Afterthoughts

NOW that I've had more time to think about it, I still like Mr. Rogers, and I still feel that everything's okay. It's my judgment that he's a fine man. Over the course of the commission I got to appreciate his talents and his abilities, and I have great respect for him. Mr. Rogers has a very good, smooth way about him, so I reserve in my head the possibility—not as a suspicion, but as an unknown—that I like him because he knew how to make me like him. I prefer to assume he's a genuinely fine fellow, and that he is the way he appears. But I was in Washington long enough to know that I can't tell.

I'm not exactly sure what Mr. Rogers thinks of me. He gives me the impression that, in spite of my being such a pain in the ass to him in the beginning, he likes me very much. I may be wrong, but if he feels the way I feel toward him, it's good.

Mr. Rogers, being a lawyer, had a difficult job to run a commission investigating what was essentially a technical question. With Dr. Keel's help, I think the technical part of it was handled well. But it struck me that there were several fishinesses associated with the big cheeses at NASA.

Every time we talked to higher level managers, they kept saying they didn't know anything about the problems below them. We're getting this kind of thing again in the Iran-Contra

hearings, but at that time, this kind of situation was new to me: either the guys at the top didn't know, in which case they should have known, or they did know, in which case they're lying to us.

When we learned that Mr. Mulloy had put pressure on Thiokol to launch, we heard time after time that the next level up at NASA knew nothing about it. You'd think Mr. Mulloy would have notified a higher-up during this big discussion, saying something like, "There's a question as to whether we should fly tomorrow morning, and there's been some objection by the Thiokol engineers, but we've decided to fly anyway—what do you think?" But instead, Mulloy said something like, "All the questions have been resolved." There seemed to be some reason why guys at the lower level didn't bring problems up to the next level.

I invented a theory which I have discussed with a considerable number of people, and many people have explained to me why it's wrong. But I don't remember their explanations, so I cannot resist telling you what I think led to this lack of communication in NASA.

When NASA was trying to go to the moon, there was a great deal of enthusiasm: it was a goal everyone was anxious to achieve. They didn't know if they could do it, but they were all working together.

I have this idea because I worked at Los Alamos, and I experienced the tension and the pressure of everybody working together to make the atomic bomb. When somebody's having a problem—say, with the detonator—everybody knows that it's a big problem, they're thinking of ways to beat it, they're making suggestions, and when they hear about the solution they're excited, because that means their work is now useful: if the detonator didn't work, the bomb wouldn't work.

I figured the same thing had gone on at NASA in the early days: if the space suit didn't work, they couldn't go to the moon. So everybody's interested in everybody else's problems.

But then, when the moon project was over, NASA had all these people together: there's a big organization in Houston and a big organization in Huntsville, not to mention at Kennedy, in Florida. You don't want to fire people and send them out in the street when you're done with a big project, so the problem is, what to do?

You have to convince Congress that there exists a project that only NASA can do. In order to do so, it is necessary—at least it was *apparently* necessary in this case—to exaggerate: to exaggerate how economical the shuttle would be, to exaggerate how often it could fly, to exaggerate how safe it would be, to exaggerate the big scientific facts that would be discovered. "The shuttle can make so-and-so many flights and it'll cost such-and-such; we went to the moon, so we can *do* it!"

Meanwhile, I would guess, the engineers at the bottom are saying, "No, no! We can't make that many flights. If we had to make that many flights, it would mean such-and-such!" And, "No, we can't do it for that amount of money, because that would mean we'd have to do thus-and-so!"

Well, the guys who are trying to get Congress to okay their projects don't want to hear such talk. It's better if they don't hear, so they can be more "honest"—they don't want to be in the position of lying to Congress! So pretty soon the attitudes begin to change: information from the bottom which is disagreeable—"We're having a problem with the seals; we should fix it before we fly again"—is suppressed by big cheeses and middle managers who say, "If you tell me about the seals problems, we'll have to ground the shuttle and fix it." Or, "No, no, keep on flying, because otherwise, it'll look bad," or "Don't tell me; I don't want to hear about it."

Maybe they don't say explicitly "Don't tell me," but they discourage communication, which amounts to the same thing. It's not a question of what has been written down, or who should tell what to whom; it's a question of

whether, when you *do* tell somebody about some problem, they're *delighted* to hear about it and they say "Tell me more" and "Have you tried such-and-such?" or they say "Well, see what you can do about it"—which is a completely different atmosphere. If you try once or twice to communicate and get pushed back, pretty soon you decide, "To hell with it."

So that's my theory: because of the exaggeration at the top being inconsistent with the reality at the bottom, communication got slowed up and ultimately jammed. That's how it's possible that the higher-ups didn't know.

The other possibility is that the higher-ups did know, and they just *said* they didn't know.

I looked up a former director of NASA—I don't remember his name now—who is the head of some company in California. I thought I'd go and talk to him when I was on one of my breaks at home, and say, "They all *say* they haven't heard. Does that make any sense? How does someone go about investigating them?"

He never returned my calls. Perhaps he didn't want to talk to the commissioner investigating higher-ups; maybe he had had enough of NASA, and didn't want to get involved. And because I was busy with so many other things, I didn't push it.

There were all kinds of questions we didn't investigate. One was this mystery of Mr. Beggs, the former director of NASA who was removed from his job pending an investigation that had nothing to do with the shuttle; he was replaced by Graham shortly before the accident. Nevertheless, it turned out that, every day, Beggs came to his old office. People came in to see him, although he never talked to Graham. What was he doing? Was there some activity still being directed by Beggs?

From time to time I would try to get Mr. Rogers interested in investigating such fishinesses. I said, "We have lawyers on the commission, we have company managers, we have very fine people with a large range of experiences. We have people who know how to get an answer out of a guy when he doesn't want to say something. I don't know how to do that. If a guy tells me the probability of failure is 1 in 10^5, I know he's full of crap—but I don't know what's natural in a bureaucratic system. We oughta get some of the big shots together and ask them questions: just like we asked the second-level managers like Mr. Mulloy, we should ask the first level."

He would say, "Yes, well, I think so."

Mr. Rogers told me later that he wrote a letter to each of the big shots, but they replied that they didn't have anything they wanted to say to us.

There was also the question of pressure from the White House.

It was the President's idea to put a teacher in space, as a symbol of the nation's commitment to education. He had proposed the idea a year before, in his State of the Union address. Now, one year later, the State of the Union speech was coming up again. It would be perfect to have the teacher in space, talking to the President and the Congress. All the circumstantial evidence was very strong.

I talked to a number of people about it, and heard various opinions, but I finally concluded that there was no pressure from the White House.

First of all, the man who pressured Thiokol to change its position, Mr. Mulloy, was a second-level manager. Ahead of time, nobody could predict what might get in the way of a launch. If you imagine Mulloy was told "Make sure the shuttle flies tomorrow, because the President wants it," you'd have to imagine that *everybody else* at his level had to be told—and there are a lot of people at his level. To tell that many people would make it sure to leak out. So that way of putting on pressure was very unlikely.

By the time the commission was over, I understood much better the character of operations in Washington and in NASA. I learned, by seeing how they worked, that the people in a big system like NASA *know* what has to be done—*without* being told.

There was *already* a big pressure to keep the shuttle flying. NASA had a flight schedule they were trying to meet, just to show the capabilities of NASA—never mind whether the president was going to give a speech that night or not. So I don't believe there was any direct activity or any special effort from the White House. There was no need to do it, so I don't believe it was done.

I could give you an analog of that. You know those signs that appear in the back windows of automobiles—those little yellow diamonds that say BABY ON BOARD, and things like that? You don't have to *tell* me there's a baby on board; I'm gonna drive carefully *anyway!* What am I supposed to do when I see there's a baby on board: act differently? As if I'm suddenly gonna drive more carefully and not hit the car because there's a baby on board, when all I'm trying to do is not hit it anyway!

So NASA was trying to get the shuttle up anyway: you don't have to say there's a baby on board, or there's a teacher on board, or it's important to get this one up for the President.

Now that I've talked to some people about my experiences on the commission, I think I understand a few things that I didn't understand so well earlier. One of them has to do with what I said to Dr. Keel that upset him so much. Recently I was talking to a man who spent a lot of time in Washington, and I asked him a particular question which, if he didn't take it right, could be considered a grave insult. I would like to explain the question, because it seems to me to be a real possibility of what I said to Dr. Keel.

The only way to have real success in science, the field

I'm familiar with, is to describe the evidence very carefully without regard to the way you feel it should be. If you have a theory, you must try to explain what's good and what's bad about it equally. In science, you learn a kind of standard integrity and honesty.

In other fields, such as business, it's different. For example, almost every advertisement you see is obviously designed, in some way or another, to fool the customer: the print that they don't want you to read is small; the statements are written in an obscure way. It is obvious to anybody that the product is not being presented in a scientific and balanced way. Therefore, in the selling business, there's a lack of integrity.

My father had the spirit and integrity of a scientist, but he was a salesman. I remember asking him the question "How can a man of integrity be a salesman?"

He said to me, "Frankly, many salesmen in the business are not straightforward—they think it's a better way to sell. But I've tried being straightforward, and I find it has its advantages. In fact, I wouldn't do it any other way. If the customer thinks at all, he'll realize he has had some bad experience with another salesman, but hasn't had that kind of experience with you. So in the end, several customers will stay with you for a long time and appreciate it."

My father was not a big, successful, famous salesman; he was the sales manager for a medium-sized uniform company. He was successful, but not enormously so.

When I see a congressman giving his opinion on something, I always wonder if it represents his *real* opinion or if it represents an opinion that he's designed in order to be elected. It seems to be a central problem for politicians. So I often wonder: what is the relation of integrity to working in the government?

Now, Dr. Keel started out by telling me that he had a degree in physics. I always assume that everybody in physics has integrity—perhaps I'm naive about that—so I must

have asked him a question I often think about: "How can a man of integrity get along in Washington?"

It's very easy to read that question another way: "Since you're getting along in Washington, you can't be a man of integrity!"

Another thing I understand better now has to do with where the idea came from that cold affects the O-rings. It was General Kutyna who called me up and said, "I was working on my carburetor, and I was thinking: what is the effect of cold on the O-rings?"

Well, it turns out that one of NASA's own astronauts told him there was information, somewhere in the works of NASA, that the O-rings had no resilience whatever at low temperatures—and NASA wasn't saying anything about it.

But General Kutyna had the career of that astronaut to worry about, so the *real* question the General was thinking about while he was working on his carburetor was, "How can I get this information out without jeopardizing my astronaut friend?" His solution was to get the professor excited about it, and his plan worked perfectly.

Appendix F: Personal Observations on the Reliability of the Shuttle

It appears that there are enormous differences of opinion as to the probability of a failure with loss of vehicle and of human life.* The estimates range from roughly 1 in 100 to 1 in 100,000. The higher figures come from working engineers, and the very low figures come from management. What are the causes and consequences of this lack of agreement? Since 1 part in 100,000 would imply that one could launch a shuttle each day for 300 years expecting to lose only one, we could properly ask, "What is the cause of management's fantastic faith in the machinery?"

We have also found that certification criteria used in flight readiness reviews often develop a gradually decreasing strictness. The argument that the same risk was flown before without failure is often accepted as an argument for the safety of accepting it again. Because of this, obvious weaknesses are accepted again and again—sometimes without a sufficiently serious attempt to remedy them, sometimes without a flight delay because of their continued presence.

*Leighton's note: The version printed as Appendix F in the commission report does not appear to have been edited, so I took it upon myself to smooth it out a little bit.

There are several sources of information: there are published criteria for certification, including a history of modifications in the form of waivers and deviations; in addition, the records of the flight readiness reviews for each flight document the arguments used to accept the risks of the flight. Information was obtained from direct testimony and reports of the range safety officer, Louis J. Ullian, with respect to the history of success of solid fuel rockets. There was a further study by him (as chairman of the Launch Abort Safety Panel, LASP) in an attempt to determine the risks involved in possible accidents leading to radioactive contamination from attempting to fly a plutonium power supply (called a radioactive thermal generator, or RTG) on future planetary missions. The NASA study of the same question is also available. For the history of the space shuttle main engines, interviews with management and engineers at Marshall, and informal interviews with engineers at Rocketdyne, were made. An independent (Caltech) mechanical engineer who consulted for NASA about engines was also interviewed informally. A visit to Johnson was made to gather information on the reliability of the avionics (computers, sensors, and effectors). Finally, there is the report "A Review of Certification Practices Potentially Applicable to Man-rated Reusable Rocket Engines," prepared at the Jet Propulsion Laboratory by N. Moore et al. in February 1986 for NASA Headquarters, Office of Space Flight. It deals with the methods used by the FAA and the military to certify their gas turbine and rocket engines. These authors were also interviewed informally.

Solid Rocket Boosters (SRB)

An estimate of the reliability of solid-fuel rocket boosters (SRBs) was made by the range safety officer by studying the experience of all previous rocket flights. Out of a total of nearly 2900 flights, 121 failed (1 in 25). This includes, however, what may be called "early errors"—rockets flown for the first few times in which design errors are discovered and fixed. A more reasonable figure for the mature rockets might be 1 in 50. With special care in selecting parts and in inspection, a figure below 1 in 100 might be achieved, but 1 in 1000 is probably not attainable with today's technology. (Since there are two rockets on the shuttle, these rocket failure rates must be doubled to get shuttle failure rates due to SRB failure.)

NASA officials argue that the figure is much lower. They point out that "since the shuttle is a manned vehicle, the probability of mission success is necessarily very close to 1.0." It is not very clear what this phrase means. Does it mean it *is* close to 1 or that it *ought to be* close to 1 ? They go on to explain, "Historically, this extremely high degree of mission success has given rise to a difference in philosophy between manned space flight programs and unmanned programs; i.e., numerical probability usage versus engineering judgment." (These quotations are from "Space Shuttle Data for Planetary Mission RTG Safety Analysis," pages 3-1 and 3-2, February 15, 1985, NASA, JSC.) It is true that if the probability of failure was as low as 1 in 100,000 it would take an inordinate number of tests to determine it: you would get nothing but a string of perfect flights with no precise figure—other than that the probability is likely less than the number of such flights in the string so far. But if the real probability is not so small, flights would show troubles, near failures, and possibly actual failures with a reasonable number of trials, and standard statistical methods could give a reasonable estimate. In fact, previous NASA experience had shown, on occasion, just such difficulties, near accidents, and even accidents, all giving warning that the probability of flight failure was not so very small.

Another inconsistency in the argument not to determine reliability through historical experience (as the range safety officer did) is NASA's appeal to history: "Historically, this high degree of mission success . . ." Finally, if we are to replace standard numerical probability usage with engineering judgment, why do we find such an enormous disparity between the management estimate and the judgment of the engineers? It would appear that, for whatever purpose—be it for internal or external consumption—the management of NASA exaggerates the reliability of its product to the point of fantasy.

The history of the certification and flight readiness reviews will not be repeated here (see other parts of the commission report), but the phenomenon of accepting seals that had shown erosion and blowby in previous flights is very clear. The *Challenger* flight is an excellent example: there are several references to previous flights; the acceptance and success of these flights are taken as evidence of safety. But erosion and blowby are not what the design expected. They are warnings that something is wrong. The equipment is not operating as expected, and therefore there is a danger that it can operate with even wider deviations in this unexpected and not thoroughly understood way. The fact that this danger did not lead to a catastrophe before is no guarantee that it will not the next time, unless it is completely understood. When playing Russian roulette, the fact that the first shot got off safely is of little comfort for the next. The origin and consequences of the erosion and blowby were not understood. Erosion and blowby did not occur equally on all flights or in all joints: sometimes there was more, sometimes less. Why not sometime, when whatever conditions determined it were right, wouldn't there be still more, leading to catastrophe?

In spite of these variations from case to case, officials behaved as if they understood them, giving apparently logical arguments to each other—often citing the "success" of previous flights. For example, in determining if flight 51-L was safe to fly in the face of ring erosion in flight 51-C, it was noted that the erosion depth was only one-third of the radius. It had been noted in an experiment cutting the ring that cutting it as deep as one radius was necessary before the ring failed. Instead of being very concerned that variations of poorly understood conditions might reasonably create a deeper erosion this time, it was asserted there was "a safety factor of three."

This is a strange use of the engineer's term "safety factor." If a bridge is built to withstand a certain load without the beams permanently deforming, cracking, or breaking, it may be designed for the materials used to actually stand up under three times the load. This "safety factor" is to allow for uncertain excesses of load, or unknown extra loads, or weaknesses in the material that might have unexpected flaws, et cetera. But if the expected load comes on to the new bridge and a crack appears in a beam, this is a failure of the design. There was no safety factor at all, even though the bridge did not actually collapse because the crack only went one-third of the way through the beam. The O-rings of the solid rocket boosters were not designed to erode. Erosion was a clue that something was wrong. Erosion was not something from which safety could be inferred.

There was no way, without full understanding, that one could have confidence that conditions the next time might not produce erosion three times more severe than the time before. Nevertheless, officials fooled themselves into thinking they had such understanding and confidence, in spite of the peculiar variations from case to case. A mathematical model was made to calculate erosion. This was a model based not on physical understanding but on empirical curve fitting. Specifically, it was supposed that a stream of hot gas impinged on the O-ring material, and the heat was determined at the point of stagnation (so far, with reasonable physical, thermodynamical laws). But to determine how much rubber eroded, it was assumed that the erosion varied as the .58 power of heat, the .58 being determined by a nearest fit. At any rate, adjusting some other numbers, it was determined that the model agreed with the erosion (to a depth of one-third the radius of the ring). There is nothing so wrong with this analysis as believing the answer! Uncertainties appear everywhere in the model. How strong the gas stream might be was unpredictable; it depended on holes formed in the putty. Blowby showed that the ring might fail, even though it was only partially eroded. The empirical formula was known to be uncertain, for the curve did not go directly through the very data points by which it was determined. There was a cloud of points, some twice above and some twice below the fitted curve, so erosions twice those predicted were reasonable from that cause alone. Similar uncertainties surrounded the other constants in the formula, et cetera, et cetera. When using a mathematical model, careful attention must be given to the uncertainties in the model.

Space Shuttle Main Engines (SSME)

During the flight of the 51-L the three space shuttle main engines all worked perfectly, even beginning to shut down in the last moments as the fuel supply began to fail. The question arises, however, as to whether—had the engines failed, and we were to investigate them in as much detail as we did the solid rocket booster—we would find a similar lack of attention to faults and deteriorating safety criteria. In other words, were the organization weaknesses that contributed to the accident confined to the solid rocket booster sector, or were they a more general characteristic of NASA? To that end the space shuttle main engines and the avionics were both investigated. No similar study of the orbiter or the external tank was made.

The engine is a much more complicated structure than the solid rocket booster, and a great deal more detailed engineering goes into it. Generally, the engineering seems to be of high quality, and apparently considerable attention is paid to deficiencies and faults found in engine operation.

The usual way that such engines are designed (for military or civilian aircraft) may be called the component system, or bottom-up design. First it is necessary to thoroughly understand the properties and limitations of the materials to be used (turbine blades, for example), and tests are begun in experimental rigs to determine those. With this knowledge, larger component parts (such as bearings) are designed and tested individually. As deficiencies and design errors are noted they are corrected and verified with further testing. Since one tests only parts at a time, these tests and modifications are not overly expensive. Finally one works up to the final design of the entire engine, to the necessary specifications. There is a good chance, by this time, that the engine will generally succeed, or that any failures are easily isolated and analyzed because the failure modes, limitations of materials, et cetera, are so well understood. There is a very good chance that the modifications to get around final difficulties in the engine are not very hard to make, for most of the serious problems have already been discovered and dealt with in the earlier, less expensive stages of the process.

The space shuttle main engine was handled in a different manner—top down, we might say. The engine was designed and put together all at once with relatively little detailed preliminary study of the materials and components. But now, when troubles are found in bearings, turbine blades, coolant pipes, et cetera, it is more expensive and difficult to discover the causes and make changes. For example, cracks have been found in the turbine blades of the high-pressure oxygen turbopump. Are they caused by flaws in the material, the effect of the oxygen atmosphere

on the properties of the material, the thermal stresses of startup or shutdown, the vibration and stresses of steady running, or mainly at some resonance at certain speeds, or something else? How long can we run from crack initiation to crack failure, and how does this depend on power level? Using the completed engine as a test bed to resolve such questions is extremely expensive. One does not wish to lose entire engines in order to find out where and how failure occurs. Yet, an accurate knowledge of this information is essential to acquiring a confidence in the engine reliability in use. Without detailed understanding, confidence cannot be attained.

A further disadvantage of the top-down method is that if an understanding of a fault is obtained, a simple fix—such as a new shape for the turbine housing—may be impossible to implement without a redesign of the entire engine.

The space shuttle main engine is a very remarkable machine. It has a greater ratio of thrust to weight than any previous engine. It is built at the edge of—sometimes outside of—previous engineering experience. Therefore, as expected, many different kinds of flaws and difficulties have turned up. Because, unfortunately, it was built in a top-down manner, the flaws are difficult to find and to fix. The design aim of an engine lifetime of 55 mission equivalents (27,000 seconds of operation, either in missions of 500 seconds each or on a test stand) has not been obtained. The engine now requires very frequent maintenance and replacement of important parts such as turbopumps, bearings, sheet metal housings, et cetera. The high-pressure fuel turbopump had to be replaced every three or four mission equivalents (although this may have been fixed, now) and the high-pressure oxygen turbopump every five or six. This was, at most, 10 percent of the original design specifications. But our main concern here is the determination of reliability.

In a total of 250,000 seconds of operation, the main engines have failed seriously perhaps 16 times. Engineers pay close attention to these failings and try to remedy them as quickly as possible by test studies on special rigs experimentally designed for the flaw in question, by careful inspection of the engine for suggestive clues (like cracks), and by considerable study and analysis. In this way, in spite of the difficulties of top-down design, through hard work many of the problems have apparently been solved.

A list of some of the problems (and their status) follows:

Turbine blade cracks in high-pressure fuel turbopumps (HPFTP). (May have been solved.)

Turbine blade cracks in high-pressure oxygen fuel turbopumps (HPOTP). (Not solved.)

Augmented spark igniter (ASI) line rupture. (Probably solved.)

Purge check valve failure. (Probably solved.)

ASI chamber erosion. (Probably solved.)

HPFTP turbine sheet metal cracking. (Probably solved.)

HPFTP coolant liner failure. (Probably solved.)

Main combustion chamber outlet elbow failure. (Probably solved.)

Main combustion chamber inlet elbow weld offset. (Probably solved.)

HPOTP subsynchronous whirl. (Probably solved.)

Flight acceleration safety cutoff system (partial failure in a redundant system). (Probably solved.)

Bearing spalling. (Partially solved.)

A vibration at 4000 hertz making some engines inoperable. (Not solved.)

Many of these apparently solved problems were the early difficulties of a new design: 13 of them occurred in the first 125,000 seconds and only 3 in the second 125,000

seconds. Naturally, one can never be sure that all the bugs are out; for some, the fix may not have addressed the true cause. Thus it is not unreasonable to guess there may be at least one surprise in the next 250,000 seconds, a probability of 1/500 per engine per mission. On a mission there are three engines, but it is possible that some accidents would be self-contained and affect only one engine. (The shuttle can abort its mission with only two engines.) Therefore, let us say that the unknown surprises do not, in and of themselves, permit us to guess that the probability of mission failure due to the space shuttle main engines is less than 1/500. To this we must add the chance of failure from known, but as yet unsolved, problems. These we discuss below.

(Engineers at Rocketdyne, the manufacturer, estimate the total probability as 1/10,000. Engineers at Marshall estimate it as l/300, while NASA management, to whom these engineers report, claims it is 1/100,000. An independent engineer consulting for NASA thought 1 or 2 per 100 a reasonable estimate.)

The history of the certification principles for these engines is confusing and difficult to explain. Initially the rule seems to have been that two sample engines must each have had twice the time operating without failure, as the operating time of the engine to be certified (rule of *2x).* At least that is the FAA practice, and NASA seems to have adopted it originally, expecting the certified time to be 10 missions (hence 20 missions for each sample). Obviously, the best engines to use for comparison would be those of greatest total operating time (flight plus test), the so-called fleet leaders. But what if a third sample engine and several others fail in a short time? Surely we will not be safe because two were unusual in lasting longer. The short time might be more representative of the real possibilities, and in the spirit of the safety factor of 2, we should only operate at half the time of the short-lived samples.

The slow shift toward a decreasing safety factor can be seen in many examples. We take that of the HPFTP turbine blades. First of all the idea of testing an entire engine was abandoned. Each engine has had many important parts (such as the turbopumps themselves) replaced at frequent intervals, so the rule of *2x* must be shifted from engines to components. Thus we accept an HPFTP for a given certification time if two samples have each run successfully for twice that time (and, of course, as a practical matter, no longer insisting that this time be as long as 10 missions). But what is "successfully"? The FAA calls a turbine blade crack a failure, in order to really provide a safety factor greater than 2 in practice. There is some time that an engine can run between the time a crack originally starts and the time it has grown large enough to fracture. (The FAA is contemplating new rules that take this extra safety time into account, but will accept them only if it is very carefully analyzed through known models within a known range of experience and with materials thoroughly tested. None of these conditions applies to the space shuttle main engines.)

Cracks were found in many second-stage HPFTP turbine blades. In one case three were found after 1900 seconds, while in another they were not found after 4200 seconds, although usually these longer runs showed cracks. To follow this story further we must realize that the stress depends a great deal on the power level. The *Challenger* flight, as well as previous flights, was at a level called 104 percent of rated power during most of the time the engines were operating. Judging from some material data, it is supposed that at 104 percent of rated power, the time to crack is about twice that at 109 percent, or full power level (FPL). Future flights were to be at 109 percent because of heavier payloads, and many tests were made at this level. Therefore, dividing time at 104 percent by 2, we obtain units called equivalent full power level (EFPL). (Obviously, some uncertainty is introduced by that, but it has not been studied.) The earliest cracks mentioned above occurred at 1375 seconds EFPL.

Now the certification rule becomes "limit all second-stage blades to a maximum of 1375 seconds EFPL." If one objects that the safety factor of 2 is lost, it is pointed out that the one turbine ran for 3800 seconds EFPL without cracks, and half of this is 1900 so we are being more conservative. We have fooled ourselves in three ways. First, we have only one sample, and it is not the fleet leader: the other two samples of 3800 or more seconds EFPL had 17 cracked blades between them. (There are 59 blades in the engine.) Next, we have abandoned the *2x* rule and substituted equal time (1375). And finally, the 1375 is where a crack was discovered. We can say that no crack had been found below 1375, but the last time we looked and saw no cracks was 1100 seconds EFPL. We do not know when the crack formed between these times. For example, cracks may have been formed at 1150 seconds EFPL. (Approximately two-thirds of the blade sets tested in excess of 1375 seconds EFPL had cracks. Some recent experiments have, indeed, shown cracks as early as 1150 seconds.) It was important to keep the number high, for the shuttle had to fly its engines very close to their limit by the time the flight was over.

Finally, it is claimed that the criteria have not been abandoned, and that the system is safe, by giving up the FAA convention that there should be no cracks, and by considering only a completely fractured blade a failure. With this definition no engine has yet failed. The idea is that since there is sufficient time for a crack to grow to fracture, we can ensure that all is safe by inspecting all blades for cracks. If cracks are found, replace the blades, and if none are found, we have enough time for a safe mission. Thus, it is claimed, the crack problem is no longer a flight safety problem, but merely a maintenance problem.

This may in fact be true. But how well do we know that cracks always grow slowly enough so that no fracture can occur in a mission? Three engines have run for long time periods with a few cracked blades (about 3000 seconds EFPL), with no blade actually breaking off.

A fix for this cracking may have been found. By changing the blade shape, shot-peening the surface, and covering it with insulation to exclude thermal shock, the new blades have not cracked so far.

A similar story appears in the history of certification of the HPOTP, but we shall not give the details here.

In summary, it is evident that the flight readiness reviews and certification rules show a deterioration in regard to some of the problems of the space shuttle main engines that is closely analogous to the deterioration seen in the rules for the solid rocket boosters.

Avionics

By "avionics" is meant the computer system on the orbiter as well as its input sensors and output actuators. At first we will restrict ourselves to the computers proper, and not be concerned with the reliability of the input information from the sensors of temperature, pressure, et cetera; nor with whether the computer output is faithfully followed by the actuators of rocket firings, mechanical controls, displays to astronauts, et cetera.

The computing system is very elaborate, having over 250,000 lines of code. Among many other things it is responsible for the automatic control of the shuttle's entire ascent into orbit, and for the descent until the shuttle is well into the atmosphere (below Mach 1), once one button is pushed deciding the landing site desired. It would be possible to make the entire landing automatic. (The landing gear lowering signal is expressly left out of computer control, and must be provided by the pilot, ostensibly for safety

reasons.) During orbital flight the computing system is used in the control of payloads, in the display of information to the astronauts, and in the exchange of information with the ground. It is evident that the safety of flight requires guaranteed accuracy of this elaborate system of computer hardware and software.

In brief, hardware reliability is ensured by having four essentially independent identical computer systems. Where possible, each sensor also has multiple copies—usually four—and each copy feeds all four of the computer lines. If the inputs from the sensors disagree, either a certain average or a majority selection is used as the effective input, depending on the circumstances. Since each computer sees all copies of the sensors, the inputs are the same, and because the algorithms used by each of the four computers are the same, the results in each computer should be identical at each step. From time to time they are compared, but because they might operate at slightly different speeds, a system of stopping and waiting at specified times is instituted before each comparison is made. If one of the computers disagrees or is too late in having its answer ready, the three which do agree are assumed to be correct and the errant computer is taken completely out of the system. If, now, another computer fails, as judged by the agreement of the other two, it is taken out of the system, and the rest of the flight is canceled: descent to the landing site is instituted, controlled by the two remaining computers. It is seen that this is a redundant system since the failure of only one computer does not affect the mission. Finally, as an extra feature of safety, there is a fifth independent computer, whose memory is loaded with only the programs for ascent and descent, and which is capable of controlling the descent if there is a failure of more than two of the computers of the main line of four.

There is not enough room in the memory of the main-line computers for all the programs of ascent, descent, and payload programs in flight, so the memory is loaded by the astronauts about four times from tapes.

Because of the enormous effort required to replace the software for such an elaborate system and to check out a new system, no change in the hardware has been made since the shuttle transportation system began about fifteen years ago. The actual hardware is obsolete—for example, the memories are of the old ferrite-core type. It is becoming more difficult to find manufacturers to supply such old-fashioned computers that are reliable and of high enough quality. Modern computers are much more reliable, and they run much faster. This simplifies circuits and allows more to be done. Today's computers would not require so much loading from tapes, for their memories are much larger.

The software is checked very carefully in a bottom-up fashion. First, each new line of code is checked; then sections of code (modules) with special functions are verified. The scope is increased step by step until the new changes are incorporated into a complete system and checked. This complete output is considered the final product, newly released. But working completely independently is a verification group that takes an adversary attitude to the software development group and tests the software as if it were a customer of the delivered product. There is additional verification in using the new programs in simulators, et cetera. An error during this stage of verification testing is considered very serious, and its origin is studied very carefully to avoid such mistakes in the future. Such inexperienced errors have been found only about six times in all the programming and program changing (for new or altered payloads) that has been done. The principle followed is: all this verification is not an aspect of program safety; it is a test of that safety in a noncatastrophic verification. Flight safety is to be judged solely on how well the programs do in the verified tests. A failure here generates considerable concern.

To summarize, then, the computer software checking system is of highest quality. There appears to be no process of gradually fooling oneself while degrading standards, the process so characteristic of the solid rocket booster and space shuttle main engine safety systems. To be sure, there have been recent suggestions by management to curtail such elaborate and expensive tests as being unnecessary at this late date in shuttle history. Such suggestions must be resisted, for they do not appreciate the mutual subtle influences and sources of error generated by even small program changes in one part of a program on another. There are perpetual requests for program changes as new payloads and new demands and modifications are suggested by the users. Changes are expensive because they require extensive testing. The proper way to save money is to curtail the number of requested changes, not the quality of testing for each.

One might add that the elaborate system could be very much improved by modern hardware and programming techniques. Any outside competition would have all the advantages of starting over. Whether modern hardware is a good idea for NASA should be carefully considered now.

Finally, returning to the sensors and actuators of the avionics system, we find that the attitude toward system failure and reliability is not nearly as good as for the computer system. For example, a difficulty was found with certain temperature sensors sometimes failing. Yet eighteen months later the same sensors were still being used, still sometimes failing, until a launch had to be scrubbed because two of them failed at the same time. Even on a succeeding flight this unreliable sensor was used again. And reaction control systems, the rocket jets used for reorienting and control in flight, still are somewhat unreliable. There is considerable redundancy, but also a long history of failures, none of which has yet been extensive enough to seriously affect a flight. The action of the jets is checked by sensors: if a jet fails to fire, the computers choose another jet to fire. But they are not designed to fail, and the problem should be solved.

Conclusions

If a reasonable launch schedule is to be maintained, engineering often cannot be done fast enough to keep up with the expectations of the originally conservative certification criteria designed to guarantee a very safe vehicle. In such situations, safety criteria are altered subtly—and with often apparently logical arguments—so that flights can still be certified in time. The shuttle therefore flies in a relatively unsafe condition, with a chance of failure on the order of a percent. (It is difficult to be more accurate.)

Official management, on the other hand, claims to believe the probability of failure is a thousand times less. One reason for this may be an attempt to assure the government of NASA's perfection and success in order to ensure the supply of funds. The other may be that they sincerely believe it to be true, demonstrating an almost incredible lack of communication between the managers and their working engineers.

In any event, this has had very unfortunate consequences, the most serious of which is to encourage ordinary citizens to fly in such a dangerous machine—as if it had attained the safety of an ordinary airliner. The astronauts, like test pilots, should know their risks, and we honor them for their courage. Who can doubt that McAuliffe* was equally a person of great courage, who was closer to an awareness of the true risks than NASA management would have us believe?

Let us make recommendations to ensure that NASA

'Note for foreign readers: Christa McAuliffe, a schoolteacher, was to have been the first ordinary citizen in space—a symbol of the nation's commitment to education, and of the shuttle's safety.

officials deal in a world of reality, understanding technological weaknesses and imperfections well enough to be actively trying to eliminate them. They must live in a world of reality in comparing the costs and utility of the shuttle to other methods of entering space. And they must be realistic in making contracts and in estimating the costs and difficulties of each project. Only realistic flight schedules should be proposed—schedules that have a reasonable chance of being met. If in this way the government would not support NASA, then so be it. NASA owes it to the citizens from whom it asks support to be frank, honest, and informative, so that these citizens can make the wisest decisions for the use of their limited resources.

For a successful technology, reality must take precedence over public relations, for Nature cannot be fooled.