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