Peter Galison

Author of Error

THE NUCLEUS OF ERROR

WHEN THE 1938 NOBEL PRIZE IN PHYSICS WAS GIVEN TO ENRICO FERMI, the short citation said this: "[F]or his demonstrations of the existence of new radioactive elements produced by neutron irradiation, and for his related discovery of nuclear reactions brought about by slow neutrons." Professor H. Pleijel, chairman of the Nobel Prize committee for physics, reiterated the citation in his presentation of the award, congratulating Enrico Fermi for producing elements beyond the end of the then-known periodic table (that is, those that would fall on the chart of elements beyond uranium (element 92)). Hailed for work that had begun in earnest in 1934, accolades fell on Fermi for finding a remarkable way to produce a myriad of radioisotopes and for also producing, for the first time in history, "transuranic" (beyond uranium) elements number 93 and number 94--"these new elements," Pleijel noted, "he called Ausenium and Heperium" (Pleijel, 1938).

Understandably, Fermi was proud of his accomplishment in producing these extraordinary new forms of matter. In his prize acceptance speech on December 12, 1938, Fermi recalled that

> we concluded that the carriers [of these radioactive properties] were one or more elements of atomic number larger than 92; we, in Rome, used to call the elements 93 and 94 Ausenium and Hesperium respectively. It is known that O. Hahn and L. Meitner have investigated very carefully and extensively the decay products of irradiated uranium, and

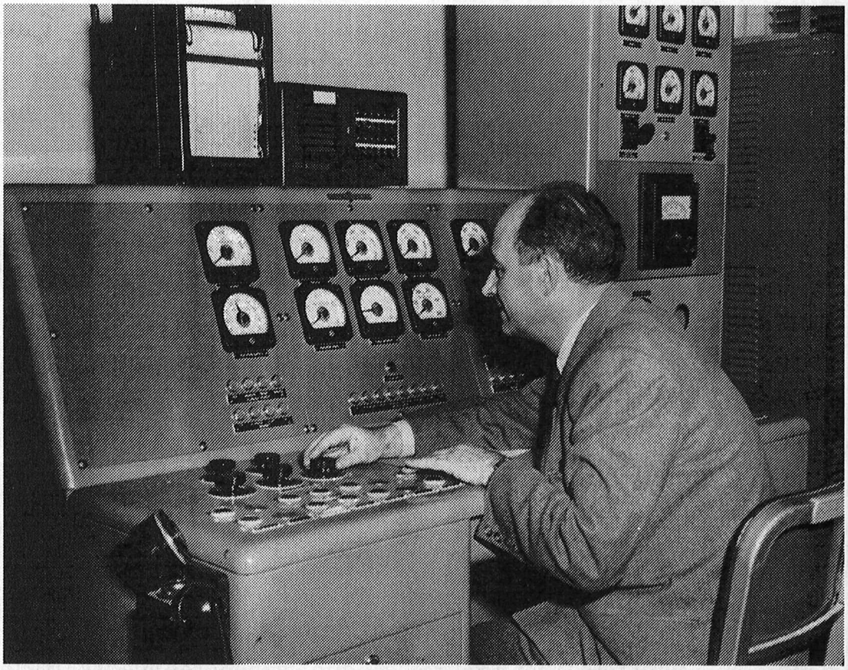
> were able to trace among them elements up to the atomic number 96 (Fermi, 1938: 416-17).[1]

Celebrated across Italy, blessed by the highest authority of scientific accolade, Fermi's production of transuranics stood as one of the greatest of scientific discoveries.

Reading a bit further in Fermi's speech, one comes across one of the most extraordinary footnotes in the history of science—oddly stuck in the text sometime between his December 1938 appearance in Stockholm and the Nobel book's appearance in print—to the effect that Otto Hahn and Fritz Strassmann had just found *barium* among the disintegration products of bombarded uranium. Though it entered only as a late footnote to Fermi's presentation, this news hit the physics world of 1939 with the force of a bomb: it meant that Fermi had almost certainly not seen what he (and the Nobel Prize committee) thought he had seen: the decay of one or more neutrons within the uranium nucleus into a proton, and by doing so transforming uranium nuclei into nuclei of new (transuranic) elements.

Instead, if Hahn and Strassmann were right, uranium had split into two approximately equal parts. This, Fermi allowed, "makes it necessary to reexamine all the problems of the transuranic elements, as many of them might be found to be products of a splitting of uranium" (Fermi, 1938: 417). What Fermi, his collaborators, the physics community, and indeed the Nobel Prize committee had taken to be transuranic elements—elements past uranium on the periodic chart of the elements—were not that at all. They were nuclear fragments from far lower in the periodic table of the elements, not the exotic new elements 93 and 94 but the decidedly unexotic elements like element 56, barium (useful for clearing moisture from vacuum tubes, and not headline news).

One of the great triumphs of Fermi's physics, a triumph hailed by the scientific world's greatest honor, was a world-historical mistake. In experiment after experiment from 1934—and in the following years—



Chicago Tribune/KRT

Enrico Fermi in his lab.

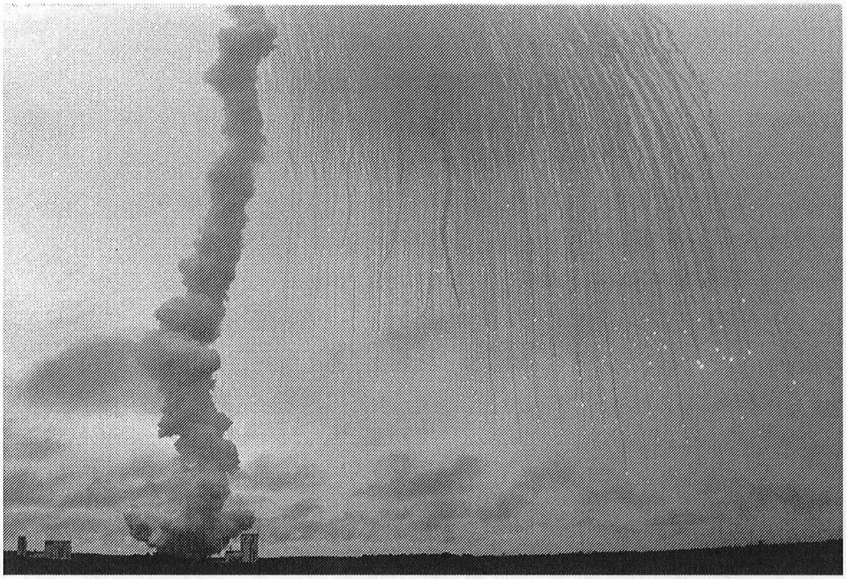
the pope of physics had missed nuclear fission. What would the world have been like had Fermi *not* made this mistake, if he had used his slow neutrons on uranium and realized that there were reaction products, like barium, that lived far down the period chart? What would it have meant had fission been found five years before 1938 and 1939?

Writing counterfactual history is a difficult way to make a living. But let's put it this way. From the time that Hahn and Strassmann submitted their paper in February 1939, it was less than seven years before the night sky over Alamagordo lit up with the nuclear light of the Trinity test. If Fermi had announced the discovery of fission in 1934, could the Americans have had a weapon early in the war? Could the Axis have had one? The Soviets? Maybe the Nazis could have been stopped in their tracks. Then again maybe the Nazis would have flattened London and Moscow and incinerated the Allies in their cities. Or perhaps the Russians would have decimated Berlin and written the script for a very different Cold War. I do not know how to write this otherworldly history. No one does. So was Fermi's misreading catastrophic? Or was it a global blessing? Hard to say. But one thing is clear: under any definition of "big," Fermi's interpretation of what he was seeing on that bench in his Roman laboratory was a big mistake.

Mistakes in the refined quarters of the great physics centers are generally not much probed. They are a kind of embarrassment—eccentric, noisy great uncles climbing the walls of the attic while the decorous dinner party progresses downstairs. These errors should not be there, but there they are. How many grand announcements have we seen of the passage of a magnetic monopole (a magnet with just a north or just a south pole)? How many here today, gone tomorrow accounts of the detection of the W particle (mediating the weak interaction) were there before the entity was nailed down at CERN in 1983? Other discoveries too have gone south: evidence of cold fusion froze and cracked when the process was investigated. Hints have flown by (and away) of supersymmetry, a symmetry that predicted a doppelgänger for every particle (for example, there would be a version of light in which photons came both as they do now and in a distinct brand that obeyed the exclusion principle). In the 1950s, Werner Heisenberg and Wolfgang Pauli once announced with great *éclat* to have found the theory to end all theories in elementary particle physics. Then their theory caved in on itself, leaving nothing but some forgotten headlines on yellowing newsprint. Even Einstein startled the world several times with various variations on his swan song of unified field theory. But none of these departures from the canonical history of physics figures much in our textbooks or in our histories. No, neither scientists nor the institutions that support them are much interested in looking too carefully in the attic of discarded knowledge. We leave the mad uncles alone.

ERRORS OF ROCKET SCIENCE

The world of technological-scientific systems is *very* different. When engineers make a major error it is common for a failure inquiry to be



The *Ariane* 5 rocket just after detonation.

established, staffed by heavyweights, properly funded, and well publicized. This is true, of course, for airplane accidents, but much more widely as well. For example, take flight of the *Ariane* 5 rocket, which was launched from France's Centre Spatial Guyanais on June 4, 1996. The weather was good, with decent if not ideal visibility, and no lightning. Liftoff was initiated at a local time of H_0 = 9:33:59 AM. Everything looked good until 36 seconds into the flight, when the vehicle veered off its intended path, broke up, and exploded. For reasons that were not at first clear, the back-up inertial reference system failed at H_0 +36 seconds, followed immediately by a failure of the active inertial reference system and a sudden re-direction of the solid rocket boosters and the main Vulcain engine. The vehicle swerved dramatically off path and split the boosters from the core, triggering a massive self-destruction. Debris fell over a mangrove swamp and savanna not far from the launch pad. It was almost immediately clear that the "origin" of the failure lay in the inertial reference system at H_0 +36.7 seconds. The director of the European Space Agency and the chairman of the French Space Agency (Centre National d'Etudes Spatiales) convened an inquiry board.[2]

The inquiry board drew on university, corporate, and national bodies from France, Sweden, Germany, Italy, and the United Kingdom. Their mandate was to find the "causes of the failure" and the "systems . . . responsible," and to identify other failures in "similar systems" that could be tied to the accident.

Already, contrasts with the scientific case are apparent. The very idea of conducting an inquiry into a scientific misfiring is almost unimaginable—barring the suspicion of gross malfeasance as there was in the case of Hendrik Schoen, who claimed to have made the world's smallest transistor, or the case of some of the cold fusion boosters, who were better at boosting than fusion.

Just as they would in an airplane crash, for example (or a nuclear power plant failure), the board investigating *Ariane* 5 pushed the explanation back, step by step. First the board established that the launcher began its disintegration at H_0 +39 seconds because the rocket's behavior caused aerodynamic loads sufficient to separate the boosters and therefore to trigger system's self-destruct mechanism. Second, the board found that the high angle of attack resulted from rocket nozzles deflecting to their extreme positions. Third—back another step—the rocket booster nozzles deflected because they were ordered to do so by the on-board computer that in turn received *its* marching orders from the inertial reference system that was supposed to be providing information about the rocket's actual flight path. It wasn't. When the inertial system's computer should have been sending flight path information, it was, instead, merely delivering a diagnostic error code. That code, misinterpreted as navigation instructions, sent the rocket helplessly and erroneously flying wildly off-course. Fourth, digging to the next causal layer: the board found that the inertial system had sent the diagnostic code because the inertial reference system had failed, and that failure issued from an underlying software error. Finally, piling mistake on mistake, the backup inertial system could not take over because it too had failed, for the same software reason, 72 thousandths of a second before the active system stopped.

The cause of the inertial reference system could be traced even further back to the software responsible for aligning the platform before launch—40 seconds after launch this setting is supposed to shut down. You might wonder why a preflight alignment should continue after launch at all. Apparently (according to board) this feature of the software had been introduced for an earlier rocket, the *Ariane* 4, where leaving the alignment software running for those 40 seconds after the scheduled launch allowed for a quick restart if the countdown was scrubbed just before the rocket actually lifted off. (It could take up to 45 minutes to recalculate the horizontal bias, making a new launch sequence impossible, if the alignment system were shut down.)

It is true that if the rocket successfully headed into the skies, the *after* liftoff values of horizontal bias would have been, by definition, useless, since this "horizontal bias" was nothing but the apparent motion of the rocket as it stood fixed and strapped down on the pad. In the earlier *Ariane* 4 rocket, the computer's continued calculation of horizontal bias, though meaningless, was perfectly harmless. Numbers might emerge from the horizontal bias processor but they were well within bounds of the computer and in any case were not being used to alter the rocket's trajectory.

So what happened with the inertial system in *Ariane* 5's maiden flight? The software sleuths dug down into computer code, line by line. Noting that the computer was manipulating seven floating-point variables (numbers expressed, roughly speaking, in scientific notation, for example, 3.457 x 25), the engineers "protected" four of them—that is, they ensured that when these numbers were converted to positive or negative integers, the conversion would be executed without creating any problems. They left three of the variables unprotected, however, to avoid overloading the calculating capacity of the processor.

In the new *Ariane* 5, however, in the minute or so after launch, the rocket achieved a much higher horizontal velocity than its predecessor. When inertial system's computer began converting the unprotected (and in *Ariane* 5 much larger-valued) variables into integers, it caused the program to issue the code for an "exception" (a violation of the usual order of programming flow) to the launcher's computer. This "exception" code fed into both the primary and backup inertial system, shutting both down; the inertial system then sent its own diagnostic code to the main launcher computer, which interpreted this shutdown announcement as bona fide flight data. At that moment the rocket was doomed. Piloted by no more than an error code, the rocket had at its helm the ghost of a departed navigational system. The board confirmed this scenario in two ways. First it simulated the failure using inputs to the inertial system that mocked up the rocket's horizontal velocity. Simulated catastrophic failure immediately followed. Second, the board reconfirmed its picture of the disaster when it recovered the actual inertial reference systems from the swamps and pulled apart the memory readouts that, somehow, survived both the explosion and their half-mile tumble to earth.

Thus, if we ask "Why did *Ariane* 501 fail?" we are driven back to a few, poorly documented lines of "Ada" code, a few symbols that had failed to protect the conversion of three floating-point numbers inside a processor. From deep inside this massive spaceship, these miscreant numbers had silently drifted into forbidden territory. Two seconds after the horizontal bias variable had wandered across the "no trespassing" boundary, the rocket and its valuable satellites were reduced to a rain of space junk tumbling into a mangrove swamp.

In one sense the discovery of the unprotected floating-point variables ends the inquiry. The board had its answer, having followed the chain of causality like an expert tracker pursuing its quarry into its lair. But at the same time the report was not complete. Other questions began pushing the inquiry in the opposite direction. It was not, however, toward a smaller and smaller crack in the edifice that could tear the whole structure to shreds. Instead, other questions pushed the investigators to widen their inquiry about how the original defect came to be there at all.

The report asks *why* the unprotected variables were left vulnerable. Answer: they were buried. Why? They were not entered intentionally, to be sure, but instead, and just as effectively, they were hidden by complexity in the deep recesses of the vast reams of undocumented code. Another query: Why was the old code from the 1980s-era *Ariane* 4 carried over to the new, 1996 *Ariane* 5? Answer: because, quite understandably, no one wanted to disturb dense, half-forgotten computer code that had worked, and well. But even these broader queries did not go far enough.

A more probing kind of question could be asked, one that works not toward ever-more proximate, immediate causes, but outward. And here, though it was not given enormous play in the report, lies a different kind of reflection: The board: "The reason behind this drastic action [shutting down of the inertial reference system processor] lies in the culture within the *Ariane* programme of only addressing random hardware failures. From this point of view exception- or error-handling mechanisms are designed for a random hardware failure that can quite rationally be handled by a backup system." In other words, the whole of the *Ariane* program had carried with it a broader "culture" of isolated hardware subject to random, not systemic, failure.

Even with hardware, of course, systems can be tightly coupled. Failure of one may not at all be independent of the other. In one famous airplane crash, three hydraulic lines were to back one another up, each provided with three independent pumps. How could they fail? If one pump had a one in a hundred chance of dying on the job, then the likelihood of all nine pumps giving up was one in a billion billions. It could never happen. Except it turned out that if a turbine blade broke it could (and did) slice through all three lines. You can pump a long time on a line with no fluid in it and not have much effect. Forget the one in a billion billions—accidents like these happened several times in the space of a few years (see Galison, 2000: 3-43).

But the board was making a second point: software is not, in certain respects *like* hardware. "[T]he Board wishes to point out that software is an expression of a highly detailed design and does not fail in the same sense as a mechanical system." The board also noted that "an underlying theme in the development of *Ariane* 5 is the bias toward the mitigation of random failure. The supplier of the [inertial reference system] was only following the specification given to it, which stipulated that in the event of any detected exception the processor was to be stopped. The exception which occurred was not due to random failure but a design error. . . ." The big mistake in this accident was not "random." Not only would this disaster, absent a change in code, happen every time *Ariane* 5 was launched, it would do so in a particularly effective way by killing both inertial devices at essentially the same moment.

BIG MISTAKES

What to make of these two stories? I find two lessons. First, the scientific community rarely subjects its errors to the kind of analysis that engineers do regularly. We could well ask why. Both fields use mathematics, make models, offer predictions, carry out simulations. Nor is the contrast between nuclear physics and rocket science a distinction between head work and hand work. After all, we are talking here not about just any physicist but about Enrico Fermi and his group—Fermi, who happily and easily could take his car apart and put it back together, Fermi who designed new instruments, and (a few years later) built the world's first nuclear reactor. This was no head-in-the-clouds theorist.

Nor is the asymmetry between the treatment of mistakes in engineering and in physics a matter of engineering teams versus scientific individuals. Indeed, in these experiments Fermi was heading one of the first real teams in modern physics. Small by the standards of the later twentieth or early twenty-first centuries, Fermi's papers of the 1930s were innovative in no small measure because they regularly had five names attached to them. By the turn of the twentieth into the twenty-first century, physics teams with more than 2,000 Ph.Ds and a comparable number of technical support personnel could be found. No, the difference is not team versus individual.

One might suspect that the difference between scientific and engineering mistakes reduces to the consequences of the failure. People die, fortunes vanish in engineering mistakes. False claims in physics might prove embarrassing, one might say, but no more than that. Is that all there is to the difference: that engineering has real-world consequences and "pure" science does not? I do not believe this for a second. In fact, I chose these examples very deliberately because they invert the stereotypes: nuclear fission surely did affect lives as few other technical events ever have. And *Ariane* 501's flaming demise did not harm anyone. We will have to look elsewhere. But before we do, we might ask a different question.

What if we asked a question that would surely horrify pure physicists in every country: What might a board of inquiry have done had it, counterfactually, inquired into the causes of Fermi's big mistake? Such a board might have tracked the error back to the Fermi group's assumption that nuclear transformation could only occur by small increments. Two possibilities: a neutron would decay to a proton, electron, and neutrino (raising the nucleus up one step on the periodic table), or perhaps an alpha ray (two protons and two neutrons) would fly off the uranium nucleus, dropping the uranium nucleus two steps down Dmitrii Mendeleev's great chart. Either way, the implication was the same: nuclear transformations were small, piecemeal affairs. If Fermi and his band checked for the presence of reaction products several steps down the periodic chart, they quite reasonably could conclude that they had conducted the scientific equivalence of due diligence. In other words, having verified that there were no reaction products anywhere between element 86 and 92, Fermi and his collaborators concluded that the reaction products must have been *above* element 92.

Our imaginary Nuclear Board of Inquiry might have gone further. It could have invoked Fermi's own earlier, and extraordinarily successful work, on the theory of beta decay. After such a remarkable achievement, he might well have been looking more at nuclear processes that embodied these interactions rather than anything that would cause the destruction of the immense uranium nucleus as a whole. Or the board might have pursued, as so many other accident inquiry boards have done, the dark side of group culture. Was it a command problem in which Fermi's background in physics trumped others' expertise in chemistry? (Fermi's collaborators called him "The Pope" to signal his infallibility.) Was it a scientific culture rushed, in those heady, frightening years of Italian fascism, to be the first to pitch an Italian flag of conquest on transuranic land? (The state did, in fact, celebrate Fermi's triumph as an *Italian* victory.) Was it the triumphant distraction of finding a way to produce artificial radioactivity in so many other elements? Was it a question of instrument design in that the fission fragments could not penetrate the material surrounding the uranium sample? These are questions that cannot, of course, be answered with certainty at this remove, but they do suggest, I think, some of what is *not* systematically pursued in the natural sciences that *is* ferociously excavated in the investigation of a space shuttle accident, a reactor disaster, a bridge collapse, or indeed the demise of any major commercial airliner.

My own suspicion is that the difference between the way we handle scientific and engineering mistakes is not to be explained entirely in the domain of practical consequences. Instead, I suspect, there is something deep-seated in the larger ambitions and ideology of the two branches of technical work, at least as they have been construed up to the early twenty-first century. Physicists have tended to see themselves as bearing the legacy of natural philosophy, pre-Socratics through Newton and Einstein. The axis of their concern has been structured around a kind of empirical metaphysics: "Is this true? Is it real?" Engineers, by contrast, have seen themselves as makers, less troubled by nonexistence than malfunction. Not "Is this true? Is this real?" but instead "Does this work? Is this robust?"

The engineer's orientation is not an unencumbered one: engineers are regulated, licensed, employed in different ways than physicists are. Engineers live in a different kind of world of intellectual property and in different relations to corporations, military contracts, and liability law. One nexus of this very different set of affiliations is the accident report. The 1938 United States Civil Aviation Act required that the Civil Aeronautics Authority create accident reports culminating in a "probable cause." "Probable cause," in turn, derives from the Fourth Amendment of the US Constitution, probable cause being needed for the issuance of a warrant. The inculpatory aspect of the inquiry has been with us from the get-go. For a scientist to create this or that mistake is simply to fail to achieve a goal—more like a novelist's unsuccessful second book than a licensed engineer's failure to conform to bridge-building standards set by a federal mandate.[3]

So while Fermi may have understood as much about how things work as any physicist of the twentieth century, he was, nonetheless living in a very different technical ethos. That said, we are left with an intriguing question that I can only sketch.

During the early years of the twenty-first century, the boundary between science and engineering has been eroding in fields from genetic engineering to the nanoscientific intersection of surface chemistry, atomic physics, biology, and electrical engineering. A generation of students began to "grow up" knowing as much about patents, copyright and venture capital as they do about *Physical Review Letters*, *Cell*, or *Nature*. What will big mistakes look like in this engineering-science trading zone 50 years from now? Will we be looking for "probable cause" in the failures of scientific research-hunting for catastrophic microfaults and large-scale technical cultures gone astray? Will we be probing universities the way we regularly do the Federal Aviation Authority, NASA, or the European Space Agency today? Imagine a report that began this way: "Nanoscientific Failure: Report by the Inquiry Board"? Words like these no longer seem impossible—they no longer feel as unpronounceable as a "failure report" on Einstein's unsuccessful "Entwurf" theory of gravity. That new, almost-imaginable title may be a harbinger of the new configuration of science and engineering, a sign of things to come.

NOTES

1. On the crucial role of Lise Meitner and Frisch in the discovery of fission, see Sime (1996) and references there; on Fermi, see Holton,

"Enrico Fermi and the Miracle of Two Tables," and his references to other biographical and historical studies (forthcoming).

2. Here and later, all references are to the accident report of *Ariane* 5 Flight 501 are drawn from "*Ariane* 5: Flight 501 Failure. Report by the Inquiry Board." Paris, July 19, 1996 <http://sunnyday.mit.edu/accidents/Ariane5accidentreport.html>.
3. For more on "probable cause," both legally and epistemically, see Galison, 2000: 34-43.

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