Limits of safety

Harry Collins

The Challenger Launch Decision: Risky Technology, Culture, and Deviance at NASA.

By Diana Vaughan. *University of Chicago Press*: 1996. Pp. 575. \$24.95.

No Downlink: A Dramatic Narrative About the Challenger Accident and Our Time. By Claus Jensen. *Farrar, Straus, Giroux*: 1996. Pp. 397. \$25.

THE space shuttle Challenger blew up on 28 January 1986, when exhaust gases escaped through a joint in one of its solid-propellant rocket boosters, burnt a large hole in the side and ruptured the fuel tank holding liquid hydrogen and oxygen. It is now thought that cold weather caused the rubber O-rings, which should have sealed the joint, to lose their resilience. Three images seem to tell the story: the diverging pattern of smoke in the sky; Richard Feynman, America's favourite physicist, dipping O-ring material in a glass of ice-water and showing that it stiffens with cold; and anxious managers, struggling to maintain the public credibility of the ailing NASA (National Aeronautics and Space Administration), overruling the engineers who wanted to veto the launch because of the cold. With this accounting, the tragedy became a triumph for technology and homespun genius; all the failure can be found in the politics. Like a folk tale, the story supports a mythical version of scientific and technological practice — a myth that has outlived its usefulness.

Claus Jensen is a Danish professor of literature and his writing holds one's attention throughout. His opening chapter covers the launch and termination of the fatal Challenger mission. The phrase 'no downlink' refers to the severing of radio contact after the explosion as well as the severing of management from its engineer subordinates. He sets Challenger into the context of Charles Perrow's idea of 'normal accidents' — accidents that are inevitable in any complex technological organization. Some revelations about Challenger take one's breath away. For instance, there was the blatant interference of a senator from Utah in giving the contract to home-based Morton Thiokol in the face of bids from contractors with more suitable experience, an example of what Americans call 'pork-barreling'.

But other 'obvious' mistakes were all too normal in a system as complex as this. Jensen's book is also full of the sort of astonishing facts that bring out the school-child in all but determined cynics: the shuttle's turbo-pumps can empty an Olympic swimming pool in 25 seconds; its insulating tiles can be balanced comfortably in the hand even when one side is

red-hot; and its solid rocket boosters burn around ten tons of fuel a second. One of the reasons the book is so gripping is that the professor of literature cannot resist a beginning, a middle and an end. Feynman is the hero who reveals the management's

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Challenger's lift-off on its ill-fated mission. A puff of black smoke can be seen at the base of the solid rocket booster where an O-ring seal failed on ignition.

mistakes, which were made in the climate of America's undiminished technological ambition and pursued within a diminished financial regime. The pressure on NASA was translated into pressure on subcontractors, so that when the engineers warned that the solid booster seals could not be guaranteed at the unprecedentedly low temperatures of the launch, they were overridden by Thiokol decision-makers scared of NASA. No-one could face another costly, exhausting and embarrassing postponement. It all seems quite tidy.

Diane Vaughan, a professor of sociology, is more ambitious. Where Jensen worked from written sources available in Denmark, she has delved deep into the archives and conducted interviews with

many of the protagonists. Typically American, her book is long and stuffed full of technical detail, engineering charts and diagrams. Her unapologetic presentation of the technicalities is the book's main strength. It makes for a less exciting read than Jensen's but, to use the anthropological jargon, it is the 'thick description' that demythologizes the retrospective account of the Challenger tragedy. Jensen tells a fascinating story, but Vaughan advances an important thesis.

Vaughan juxtaposes the backward-looking account with a version of events embedded in the rich and complex context of day-to-day engineering life. She takes 55 pages to cover the eve-of-launch discussion. We learn that it is not simply that financial and administrative pressure forced engineers and managers to ignore their better judgement. We learn that it is not just that anyone would have noticed the O-rings would be stiff when cold if only they had done something as simple as dip them into ice-water — as in Feynman's limelight-grabbing performance on television. Everyone knew the O-rings stiffened as they cooled, but they did not know how serious the problem was. The charts of stiffness discussed before the launch showed that the Viton rubber did not stiffen catastrophically until below 25 degrees Fahrenheit. Admittedly, a group of engineers, the most well known being Roger Boisjoly, recommended that there should be no launch below 53 degrees, but they did so on the basis of having experienced only two other cases of O-ring erosion, one of which took place at 75 degrees.

So, before the launch decision, erosion had been experienced during both the coldest and the hottest launch, and that is why the managers, whose job it was to cope with the conservatism of engineers, might reasonably have concluded that unprecedented cold was not necessarily going to be fatal. In any case, during the build-up to the preceding launch, temperatures well below 53 degrees had been experienced during countdowns and not a word of warning had come from the Thiokol engineers. Further, in the immediately preceding Challenger countdown, called off because of high cross-winds, the temperature had been 38 degrees, yet the cold had not been discussed. Again, not all the Thiokol engineers agreed, but it is only the testimony of those who got it 'right' that has been given salience. So the sudden attempt to impose a cold-weather veto was bound to seem infuriating and arbitrary to the managers, especially as the engineers did not press home their case against the counter-view.

In retrospect, of course, the decision was wrong. In retrospect we can see that

the O-ring joint should have been redesigned years before. The O-rings were known not to perform according to the original design because the thin casing flexed unexpectedly and the joints tended to widen when the rockets exploded into life. But the O-rings had always worked, and the pragmatic approach became 'normalized'. In retrospect it was not so difficult for Feynman to show that the shuttle was a far more dangerous machine than the American public had been led to believe. But in retrospect it was a far more dangerous machine than anybody believed, or they would not have launched it. It was nobody's intention to kill the astronauts or lose their own jobs.

Feynman concluded that, given the state of the shuttle at the time of Challenger, the chances of disaster from one cause or another were between one and two in a hundred — that is, the shuttle programme could expect between 1 and 2 per cent fatalities. Subsequently, NASA has spent a great deal of money to reduce the risk, but I wonder if it is worth it.

I have been lucky enough to watch a shuttle launch (the night-launched Hubble Space Telescope repair mission). I found myself deeply moved by fearful empathy for the fragile humans sitting on that column of fire gradually dwindling to

a distant star, but I would volunteer for the ride in a trice even after reading these two books. Thirty workers were killed building the Golden Gate Bridge, and they were not volunteers. If only we could find a way of spending the shuttle improvement money on those who take risks because that is the only way they can earn a living, accept that it is in the nature of pioneering technology to be risky and get on with it. Of course, when it is not volunteers' lives that are at risk but communities of innocent people, as in nuclear power engineering, the moral is completely turned around; in such circumstances those who argue that complex technology can be made safe are deeply culpable.

The danger of the simple folk tale of Challenger, which safeguards the science at the expense of the managers, is that it strengthens the idea that complex technology is perfectible. Vaughan's book shows why we should not believe it; we make our technology and we can control and understand it, but only to the extent that we can control and understand ourselves. □

Harry Collins is at the Centre for the Study of Knowledge, Expertise and Science, University of Southampton, Southampton SO17 1BJ, UK.

Self-fulfilling prophecies

David Edgerton

Knowing Machines: Essays on Technical Change. By Donald MacKenzie. MIT Press: 1996. Pp. 338. \$35, £22.50.

ENGINEERS have long argued that one of the principal problems of modern society has been its slow adaptation to the needs of the new technologies they created. In the nineteenth and twentieth centuries, some engineers such as Herbert Spencer went on to be pioneers of the new science of sociology. After 1945, engineers urged the development of industrial sociology and the taking into account of the 'human factor' in industry. They generally took the view that modern technologies appeared almost out of nowhere, and at an advancing rate, but societies and cultures had no tendency to change, except belatedly under the influence of new technologies. The resulting 'cultural lag' meant not only that technologies were too slowly applied but, more importantly, that they were inappropriately applied: to war, to the creation of unemployment and so on. The scientific study of society promised to produce a kind of social engineering that would reduce the lag and ensure that technology was properly used.

The same sort of arguments are still to be found in popular discussions of tech-

nology and society; we are constantly told we are not ready for this or that technology and that public understanding is deficient. But academic sociology's approach to technology has moved on. In the late 1960s and 1970s, notably in Edinburgh, a group of sociologists, historians and philosophers argued that sociology, born out of science, should be applied to the study of science itself. This was a daring and controversial move, not least because it suggested that social factors went into the making of scientific knowledge. The 'Edinburgh School' developed — from the standard canons of scientific method — a series of prescriptions as to how one should study science. Perhaps the most famous was the symmetry principle: that the same analytical tools should be applied to studying all science, whether 'good' or 'bad'.

In the case of technology, this prescription amounts to saying that we should not explain the success of a technology by assuming its intrinsic superiority, while accounting for the failure of good technology by invoking social factors. Both technical and social factors should be involved in discussions of success and failure. Donald MacKenzie has been one of the pioneers of applying these powerful tools to technological knowledge (both formal

and tacit) and to technological innovation in particular. The approach yields a rich account of invention and innovation far from the banal stories of heroic invention by unique people 'ahead of their time'.

MacKenzie's introductory essay provides the clearest guide I know to the methods and concerns of the new sociology of technological knowledge and innovation. It is especially attractive because it deals seriously with a least some of the criticisms of his approach, and distances itself from some of the excesses. MacKenzie focuses on such issues as who new technology is best for, the path-dependence of innovation, the need for symmetrical explanation of success and failure, the importance of self-fulfilling and self-negating prophecies in technological innovation and the importance of the study of formal and tacit knowledge in technology.

Reprinted from a wide variety of sources, including *Nature*, *Technology and Culture* and the *American Journal of Sociology*, the essays in the book cover an amazing mix of topics in great depth and with great subtlety. MacKenzie is a fluent writer who avoids jargon and presents theoretical arguments lucidly. His historical narratives are tightly constructed and rich in detail.

Many of the studies are concerned with the history of computation, especially in relation to the military. The author outlines the influence of two laboratories, Los Alamos and Livermore, on the design and specification of supercomputers. They defined, he argues, what a supercomputer should be. He also provides an acute biographical sketch of Seymour Cray (written with Boelie Elzen), the most famous supercomputing entrepreneur.

Sociologists of knowledge have been traditionally very alert to controversies in science and technology, because they provide a glimpse into the nature of science and technology that is closed off when sciences and technologies settle down to a consensus. Of particular interest are MacKenzie's accounts of debates about computer-generated proofs of the integrity of computer programs.

MacKenzie is also concerned with competition between technologies, and provides a fine account of the development of the laser gyroscope for inertial navigators in civil aircraft. His point is that the early laser gyros were not manifestly superior or even superior at all to mechanical gyros. But it was believed that they would be and as a result they partially became so, because of the shift of development resources from mechanical to optical systems. So we should not assume from the ultimate success of the laser gyroscope that its victory was a foregone conclusion. Things could have turned out differently.

One of the great propaganda clichés of the nuclear age is that 'we can't uninvent nuclear weapons'. MacKenzie (writing in