Also by Alvin Toffler

The Culture Consumers

The Schoolhouse in the City (Editor)

Future Shock

The Futurists

Edited with an introduction by

Alvin Toffler

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Hazards of Prophecy

Arthur C. Clarke

Prophecy is always a risky business—a fact dramatically demonstrated in this selection. Arthur C. Clarke, the prolific author of novels, essays, and stories (one of which served as the basis for the movie *2001*), himself has made more than one mind-jogging forecast. In 1945, he predicted the use of satellites for radio and television communication. *Profiles of the Future,* from which this delightful piece is drawn, presents a series of startling assertions about our technological tomorrow.

The Failure of Nerve. Before one attempts to set up in business as a prophet, it is instructive to see what success others have made of this dangerous occupation—and it is even more instructive to see where they have failed.

With monotonous regularity, apparently competent men have laid down the law about what is technically possible or impossible—and have been proved utterly wrong, sometimes while the ink was scarcely dry from their pens. On careful analysis, it appears that these debacles fall into two classes, which I will call "failures of nerve" and "failures of imagination."

The failure of nerve seems to be the more common; it occurs when *even given all the relevant facts* the would-be prophet cannot see that they point to an inescapable conclusion. Some of these failures are so ludicrous as to be almost unbelievable, and would form an interesting subject for psychological analysis. "They said it couldn't be done" is a phrase that occurs throughout the history of invention; I do not know if anyone has ever looked into the reasons *why* "they" said so, often with quite unnecessary vehemence.

It is now impossible for us to recall the mental climate which existed when the first locomotives were being built, and critics gravely asserted that suffocation lay in wait for anyone who reached the awful speed of thirty miles an hour. It is equally difficult to believe that, only eighty years ago, the idea of the domestic electric light was pooh-poohed by all the "experts"—with the exception of a thirty-one-year-old American inventor named Thomas Alva Edison. When gas securities nose-dived in 1878 because Edison (already a formidable figure, with the phonograph and the carbon microphone to his credit) announced that he was working on the incandescent lamp, the British Parliament set up a committee to look into the matter. (Westminster can beat Washington hands down at this game.)

The distinguished witnesses reported, to the relief of the gas companies, that Edison's ideas were "good enough for our transatlantic friends . . . but unworthy of the attention of practical or scientific men." And Sir William Preece, engineer-in-chief of the British Post Office, roundly declared that "Subdivision of the electric light is an absolute *ignis fatuus.*" One feels that the fatuousness was not in the *ignis.*

The scientific absurdity being pilloried, be it noted, is not some wild-and-woolly dream like perpetual motion, but the humble little electric light bulb, which three generations of men have taken for

granted, except when it burns out and leaves them in the dark. Yet although in this matter Edison saw far beyond his contemporaries, he too in later life was guilty of the same shortsightedness that afflicted Preece, for he opposed the introduction of alternating current.

The most famous, and perhaps the most instructive, failures of nerve have occurred in the fields of aero- and astronautics. At the beginning of the twentieth century, scientists were almost unanimous in declaring that heavier-than-air flight was impossible, and that anyone who attempted to build airplanes was a fool. The great American astronomer, Simon Newcomb, wrote a celebrated essay which concluded:

> The demonstration that no possible combination of known substances, known forms of machinery and known forms of force, can be united in a practical machine by which man shall fly long distances through the air, seems to the writer as complete as it is possible for the demonstration of any physical fact to be.

Oddly enough, Newcomb was sufficiently broad-minded to admit that some wholly new discovery—he mentioned the neutralization of gravity—might make flight practical. One cannot, therefore, accuse him of lacking imagination; his error was in attempting to marshal the facts of aerodynamics when he did not understand that science. His failure of nerve lay in not realizing that the means of flight were already at hand.

For Newcomb's article received wide publicity at just about the time that the Wright brothers, not having a suitable antigravity device in their bicycle shop, were mounting a gasoline engine on wings. When news of their success reached the astronomer, he was only momentarily taken back. Flying machines *might* be a marginal possibility, he conceded—but they were certainly of no practical importance, for it was quite out of the question that they could carry the extra weight of a passenger as well as that of a pilot.

Such refusal to face facts which now seem obvious has continued throughout the history of aviation. Let me quote another astronomer, William H. Pickering, straightening out the uninformed public a few years *after* the first airplanes had started to fly.

The popular mind often pictures gigantic flying machines speeding across the Atlantic and carrying innumerable passengers in a way analogous to our modern steamships. . . . It seems safe to say that such ideas must be wholly visionary, and even if a machine could get across with one or two passengers the expense would be prohibitive to any but the capitalist who could own his own yacht.

Another popular fallacy is to expect enormous speed to be obtained. It must be remembered that the resistance of the air increases as the square of the speed and the work as the cube. . . . If with 30 h.p. we can now attain a speed of 40 m.p.h., then in order to reach a speed of 100 m.p.h. we must use a motor capable of 470 h.p. . . . it is clear that with our present devices there is no hope of competing for racing speed with either our locomotives or our automobiles.

It so happens that most of his fellow astronomers considered Pickering far *too imaginative;* he was prone to see vegetation—and even evidence for insect life—on the Moon. I am glad to say that by the time he died in 1938 at the ripe age of eighty, Professor Pickering had seen airplanes traveling at 400 m.p.h., and carrying considerably more than "one or two" passengers.

Closer to the present, the opening of the space age has produced a mass vindication (and refutation) of prophecies on a scale and at a speed never before witnessed. Having taken some part in this myself, and being no more immune than the next man to the pleasures of saying, "I told you so," I would like to recall a few of the statements about space flight that have been made by prominent scientists in the past. It is necessary for *someone* to do this, and to jog the remarkably selective memories of the pessimists. The speed with which those who once declaimed, "It's impossible" can switch to, "I said it could be done all the time" is really astounding.

As far as the general public is concerned, the idea of space flight as a serious possibility first appeared on the horizon in the 1920's, largely as a result of newspaper reports of the work of the American Robert Goddard and the Rumanian Hermann Oberth. (The much earlier studies of Tsiolkovsky in Russia then being almost unknown outside his own country.) When the ideas of Goddard and Oberth, usually distorted by the press, filtered through to the scientific world, they were received with hoots of derision. For a sample of the kind of criticism the pioneers of astronautics had to face, I present this masterpiece from a paper published by one

Professor A. W. Bickerton, in 1926. It should be read carefully, for as an example of the cocksure thinking of the time it would be very hard to beat.

This foolish idea of shooting at the moon is an example of the absurd length to which vicious specialisation will carry scientists working in thought-tight compartments. Let us critically examine the proposal. For a projectile entirely to escape the gravitation of the earth, it needs a velocity of 7 miles a second. The thermal energy of a gramme at this speed is 15,180 calories. . . . The energy of our most violent explosive—nitro-glycerine—is less than 1,500 calories per gramme. Consequently, even had the explosive nothing to carry, it has only one-tenth of the energy necessary to escape the earth. . . . Hence the proposition appears to be basically impossible. . . .

Indignant readers in the Colombo public library pointed angrily to the **Silence** notices when I discovered this little gem. It is worth examining it in some detail to see just where "vicious specialisation," if one may coin a phrase, led the professor so badly astray.

His first error lies in the sentence: "The energy of our most violent explosive—nitro-glycerine . . ." One would have thought it obvious that *energy,* not violence, is what we want from a rocket fuel; and as a matter of fact nitroglycerin and similar explosives contain much less energy, weight for weight, than such mixtures as kerosene and liquid oxygen. This had been carefully pointed out by Tsiolkovsky and Goddard years before.

Bickerton's second error is much more culpable. What of it, if nitroglycerin has only a tenth of the energy necessary to escape from the Earth? That merely means that you have to use at least ten pounds of nitroglycerin to launch a single pound of payload.[1]

For the fuel itself has not got to escape from Earth; it can all be burned quite close to our planet, and as long as it imparts its energy to the payload, this is all that matters. When Lunik II lifted thirty-three years after Professor Bickerton said it was impossible, most of its several hundred tons of kerosene and liquid oxygen never got very far from Russia—but the half-ton payload reached the Mare Imbrium.

As a comment on the above, I might add that Professor Bickerton, who was an active popularizer of science, numbered among his published books one with the title *Perils of a Pioneer.* Of the perils that all pioneers must face, few are more disheartening than the Bickertons.

Right through the 1930's and 1940's, eminent scientists continued to deride the rocket pioneers—when they bothered to notice them at all. Anyone who has access to a good college library can find, preserved for posterity in the dignified pages of the January 1941 *Philosophical Magazine*, an example that makes a worthy mate to the one I have just quoted.

It is a paper by the distinguished Canadian astronomer Professor J. W. Campbell, of the University of Alberta, entitled "Rocket Flight to the Moon." Opening with a quotation from a 1938 Edmonton paper to the effect that "rocket flight to the Moon now seems less remote than television appeared a hundred years ago," the professor then looks into the subject mathematically. After several pages of analysis, he arrives at the conclusion that it would require *a million tons* of take-off weight to carry *one pound* of payload on the round trip.

The correct figure, for today's primitive fuels and technologies, is very roughly one ton per pound—a depressing ratio, but hardly as bad as that calculated by the professor. Yet his mathematics was impeccable; so what went wrong?

Merely his initial assumptions, which were hopelessly unrealistic. He chose a path for the rocket which was fantastically extravagant in energy, and he assumed the use of an acceleration so low that most of the fuel would be wasted at low altitudes, fighting the Earth's gravitational field. It was as if he had calculated the performance of an automobile—when the brakes were on. No wonder that he concluded: "While it is always dangerous to make a negative prediction, it would appear that the statement that rocket flight to the moon does not seem so remote as television did less than one hundred years ago is over-optimistic." I am sure that when the *Philosophical Magazine* subscribers read those words, back in 1941, many of them thought, "Well, *that* should put those crazy rocket men in their place!"

Yet the correct results had been published by Tsiolkovsky, Oberth and Goddard years before; though the work of the first two would have been very hard to consult at the time, Goddard's paper "A Method of Reaching Extreme Altitudes" was already a classic and had been issued by that scarcely obscure body, the

Smithsonian Institution. If Professor Campbell had only consulted it (or indeed *any* competent writer on the subject—there were some, even in 1941) he would not have misled his readers and himself.

The lesson to be learned from these examples is one that can never be repeated too often, and is one that is seldom understood by laymen—who have an almost superstitious awe of mathematics. But mathematics is only a tool, though an immensely powerful one. No equations, however impressive and complex, can arrive at the truth if the initial assumptions are incorrect. It is really quite amazing by what margins competent but conservative scientists and engineers can miss the mark, when they start with the preconceived idea that what they are investigating is impossible. When this happens, the most well-informed men become blinded by their prejudices and are unable to see what lies directly ahead of them. What is even more incredible, they refuse to learn from experience and will continue to make the same mistake over and over again.

Some of my best friends are astronomers, and I am sorry to keep throwing stones at them—but they do seem to have an appalling record as prophets. If you still doubt this, let me tell a story so ironic that you might well accuse me of making it up. But I am not that much of a cynic; the facts are on file for anyone to check.

Back in the dark ages of 1935, the founder of the British Interplanetary Society, P. E. Cleator, was rash enough to write the first book on astronautics published in England. His *Rockets through Space* gave an (incidentally highly entertaining) account of the experiments that had been carried out by the German and American rocket pioneers, and their plans for such commonplaces of today as giant multi-stage boosters and satellites. Rather surprisingly, the staid scientific journal *Nature* reviewed the book in its issue for March 14, 1936, and summed up as follows:

> It must be said at once that the whole procedure sketched in the present volume presents difficulties of so fundamental a nature that we are forced to dismiss the notion as essentially impracticable, in spite of the author's insistent appeal to put aside prejudice and to recollect the supposed impossibility of heavier-than-air flight before it was actually accomplished. An analogy such as this may be misleading, and we believe it to be so in this case. . . .

Well, the whole world now knows just how misleading this analogy was, though the reviewer, identified only by the unusual initials R.v.d.R.W. was of course fully entitled to his opinion.

Just twenty years later—*after* President Eisenhower had announced the United States satellite program—a new Astronomer Royal arrived in England to take up his appointment. The press asked him to give his views on space flight, and after two decades Dr. Richard van der Riet Woolley had seen no reason to change his mind. "Space travel," he snorted, "is utter bilge."

The newspapers did not allow him to forget this, when Sputnik I went up the very next year. And now—irony piled upon irony—Dr. Woolley is, by virtue of his position as Astronomer Royal, a leading member of the committee advising the British government on space research. The feelings of those who have been trying, for a generation, to get the United Kingdom interested in space can well be imagined.[2]

Even those who suggested that rockets might be used for more modest, though much more reprehensible, purposes were overruled by the scientific authorities—except in Germany and Russia.

When the existence of the 200-mile-range V-2 was disclosed to an astonished world, there was considerable speculation about intercontinental missiles. This was firmly squashed by Dr. Vannevar Bush, the civilian general of the United States scientific war effort, in evidence before a Senate committee on December 3, 1945. Listen:

> There has been a great deal said about a 3,000 miles high-angle rocket. In my opinion such a thing is impossible for many years. The people who have been writing these things that annoy me, have been talking about a 3,000 mile high-angle rocket shot from one continent to another, carrying an atomic bomb and so directed as to be a precise weapon which would land exactly on a certain target, such as a city.
>
> I say, technically, I don't think anyone in the world knows how to do such a thing, and I feel confident that it will not be done for a very long period of time to come. . . . I think we can leave that out of our thinking. I wish the American public would leave that out of their thinking.

A few months earlier (in May 1945) Prime Minister Churchill's scientific advisor Lord Cherwell had expressed similar views in a House of Lords debate. This was only to be expected, for Cherwell

was an extremely conservative and opinionated scientist who had advised the government that the V-2 itself was only a propaganda rumor.[3]

In the May 1945 debate on defense, Lord Cherwell impressed his peers by a dazzling display of mental arithmetic from which he correctly concluded that a very long-range rocket must consist of more than 90 per cent fuel, and thus would have a negligible payload. The conclusion he let his listeners draw from this was that such a device would be wholly impracticable.

That was true enough in the spring of 1945, but it was no longer true in the summer. One astonishing feature of the House of Lords debate is the casual way in which much-too-well-informed peers used the words "atomic bomb," at a time when this was the best-kept secret of the war. (The Alamogordo test was still two months in the future!) Security must have been horrified, and Lord Cherwell—who of course knew all about the Manhattan Project—was quite justified in telling his inquisitive colleagues not to believe everything they heard, even though in this case it happened to be perfectly true.

When Dr. Bush spoke to the Senate committee in December of the same year, the only important secret about the atomic bomb was that it weighed five tons. Anyone could then work out in his head, as Lord Cherwell had done, that a rocket to deliver it across intercontinental ranges would have to weigh about 200 tons—as against the mere 14 tons of the then awe-inspiring V-2.

The outcome was the greatest failure of nerve in all history, which changed the future of the world—indeed, of many worlds. Faced with the same facts and the same calculations, American and Russian technology took two separate roads. The Pentagon—accountable to the taxpayer—virtually abandoned long-range rockets for almost half a decade, until the development of thermonuclear bombs made it possible to build warheads five times lighter yet several hundred times more powerful than the low-powered and now obsolete device that was dropped on Hiroshima.

The Russians had no such inhibitions. Faced with the need for a 200-ton rocket, they went right ahead and built it. By the time it was perfected, it was no longer required for military purposes, for Soviet physicists had bypassed the United States' billion-dollar tritium bomb cul-de-sac and gone straight to the far cheaper lithium bomb. Having backed the wrong horse in rocketry, the Russians then entered it for a much more important event—and won the race into space.

Of the many lessons to be drawn from this slice of recent history, the one that I wish to emphasize is this. Anything that is theoretically possible will be achieved in practice, no matter what the technical difficulties, if it is desired greatly enough. It is no argument against any project to say: "The idea's fantastic!" Most of the things that have happened in the last fifty years have been fantastic, and it is only by assuming that they will continue to be so that we have any hope of anticipating the future.

To do this—to avoid that failure of nerve for which history exacts so merciless a penalty—we must have the courage to follow all technical extrapolations to their logical conclusion. Yet even this is not enough, as I shall now demonstrate. To predict the future we need logic; but we also need faith and imagination which can sometimes defy logic itself.

The Failure of Imagination. [Until now I have] suggested that many of the negative statements about scientific possibilities, and the gross failures of past prophets to predict what lay immediately ahead of them, could be described as failures of nerve. All the basic facts of aeronautics were available—in the writings of Cayley, Stringfellow, Chanute, and others—when Simon Newcomb "proved" that flight was impossible. He simply lacked the courage to face those facts. All the fundamental equations and principles of space travel had been worked out by Tsiolkovsky, Goddard, and Oberth for years—often decades—when distinguished scientists were making fun of would-be astronauts. Here again, the failure to appreciate the facts was not so much intellectual as moral. The critics did not have the courage that their scientific convictions should have given them; they could not believe the truth even when it had been spelled out before their eyes, in their own language of mathematics. We all know this type of cowardice, because at some time or other we all exhibit it.

The second kind of prophetic failure is less blameworthy, and more interesting. It arises when all the available facts are appreciated *and* marshaled correctly—but when the really vital facts

are still undiscovered, and the possibility of their existence is not admitted.

A famous example of this is provided by the philosopher Auguste Comte, who in his *Cours de Philosophie Positive* (1835) attempted to define the limits within which scientific knowledge must lie. In his chapter on astronomy (Book 2, Chapter 1) he wrote these words concerning the heavenly bodies:

> We see how we may determine their forms, their distances, their bulk, their motions, but we can never know anything of their chemical or mineralogical structure; and much less, that of organised beings living on their surface. . . . We must keep carefully apart the idea of the solar system and that of the universe, and be always assured that our only true interest is in the former. Within this boundary alone is astronomy the supreme and positive science that we have determined it to be . . . the stars serve us scientifically only as providing positions with which we may compare the interior movements of our system.

In other words, Comte decided that the stars could never be more than celestial reference points, of no intrinsic concern to the astronomer. Only in the case of the planets could we hope for any definite knowledge, and even that knowledge would be limited to geometry and dynamics. Comte would probably have decided that such a science as "astrophysics" was *a priori* impossible.

Yet within half a century of his death, almost the whole of astronomy *was* astrophysics, and very few professional astronomers had much interest in the planets. Comte's assertion had been utterly refuted by the invention of the spectroscope, which not only revealed the "chemical structure" of the heavenly bodies but has now told us far more about the distant stars than we know of our planetary neighbors.

Comte cannot be blamed for not imagining the spectroscope; *no one* could have imagined it, or the still more sophisticated instruments that have now joined it in the astronomer's armory. But he provides a warning that should always be borne in mind; even things that are undoubtedly impossible with existing or foreseeable techniques may prove to be easy as a result of new scientific breakthroughs. From their very nature, these breakthroughs can never be anticipated; but they have enabled us to bypass so many insuperable obstacles in the past that no picture of the future can hope to be valid if it ignores them.

Another celebrated failure of imagination was that persisted in by Lord Rutherford, who more than any other man laid bare the internal structure of the atom. Rutherford frequently made fun of those sensation mongers who predicted that we would one day be able to harness the energy locked up in matter. Yet only five years after his death in 1937, the first chain reaction was started in Chicago. What Rutherford, for all his wonderful insight, had failed to take into account was that a nuclear reaction might be discovered that would release more energy than that required to start it. To liberate the energy of matter, what was wanted was a nuclear "fire" analogous to chemical combustion, and the fission of uranium provided this. Once that was discovered, the harnessing of atomic energy was inevitable, though without the pressures of war it might well have taken the better part of a century.

The example of Lord Rutherford demonstrates that it is not the man who knows most about a subject, and is the acknowledged master of his field, who can give the most reliable pointers to its future. Too great a burden of knowledge can clog the wheels of imagination; I have tried to embody this fact of observation in Clarke's Law, which may be formulated as follows:

When a distinguished but elderly scientist states that something is possible, he is almost certainly right. When he states that something is impossible, he is very probably wrong.

Perhaps the adjective "elderly" requires definition. In physics, mathematics, and astronautics it means over thirty; in the other disciplines, senile decay is sometimes postponed to the forties. There are, of course, glorious exceptions; but as every researcher just out of college knows, scientists of over fifty are good for nothing but board meetings, and should at all costs be kept out of the laboratory!

Too much imagination is much rarer than too little; when it occurs, it usually involves its unfortunate possessor in frustration and failure—unless he is sensible enough merely to write about his ideas, and not to attempt their realization. In the first category we find all the science-fiction authors, historians of the future, creators of utopias—and the two Bacons, Roger and Francis.

Friar Roger (c. 1214–1292) imagined optical instruments and mechanically propelled boats and flying machines—devices far beyond the existing or even foreseeable technology of his time. It is hard to believe that these words were written in the thirteenth century:

Instruments may be made by which the largest ships, with only one man guiding them, will be carried with greater velocity than if they were full of sailors. Chariots may be constructed that will move with incredible rapidity without the help of animals. Instruments of flying may be formed in which a man, sitting at his ease and meditating in any subject, may beat the air with his artificial wings after the manner of birds . . . as also machines which will enable men to walk at the bottom of the seas. . . .

This passage is a triumph of imagination over hard fact. Everything in it has come true, yet at the time it was written it was more an act of faith than of logic. It is probable that all long-range prediction, if it is to be accurate, must be of this nature. The real future is not *logically* foreseeable.

A splendid example of a man whose imagination ran ahead of his age was the English mathematician Charles Babbage (1792–1871). As long ago as 1819, Babbage had worked out the principles underlying automatic computing machines. He realized that all mathematical calculations could be broken down into a series of step-by-step operations that could, in theory, be carried out by a machine. With the aid of a government grant which eventually totaled £17,000—a very substantial sum of money in the 1820's—he started to build his "analytical engine."

Though he devoted the rest of his life, and much of his private fortune, to the project, Babbage was unable to complete the machine. What defeated him was the fact that precision engineering of the standard he needed to build his cogs and gears simply did not exist at the time. By his efforts he helped to create the machine-tool industry—so that in the long run the government got back very much more than its £17,000—and today it would be a perfectly straightforward matter to complete Babbage's computer, which now stands as one of the most fascinating exhibits in the London Science Museum. In his own lifetime, however, Babbage was only able to demonstrate the operation of a relatively small portion of the complete machine. A dozen years after his death, his biographer wrote: "This extraordinary monument of theoretical genius accordingly remains, and doubtless will forever remain, a theoretical possibility."

There is not much left of that "doubtless" today. At this moment there are thousands of computers working on the principles that Babbage clearly outlined more than a century ago—but with a range and a speed of which he could never have dreamed. For what makes the case of Charles Babbage so interesting, and so pathetic, is that he was not one but *two* technological revolutions ahead of his time. Had the precision-tool industry existed in 1820, he could have built his "analytical engine" and it would have worked, much faster than a human computer, but very slowly by the standards of today. For it would have been geared—literally—to the speed with which cogs and shafts and cams and ratchets can operate.

Automatic calculating machines could not come into their own until electronics made possible speeds of operation thousands and millions of times swifter than could be achieved with purely mechanical devices. This level of technology was reached in the 1940's, and Babbage was then promptly vindicated. His failure was not one of imagination: it lay in being born a hundred years too soon.

One can only prepare for the unpredictable by trying to keep an open and unprejudiced mind—a feat which is extremely difficult to achieve, even with the best will in the world. Indeed, a completely open mind would be an empty one, and freedom from all prejudices and preconceptions is an unattainable ideal. Yet there is one form of mental exercise that can provide good basic training for would-be prophets: Anyone who wishes to cope with the future should travel back in imagination a single lifetime—say to 1900—and ask himself just how much of today's technology would be, not merely incredible, but *incomprehensible* to the keenest scientific brains of that time.

1900 is a good round date to choose because it was just about then that all hell started to break loose in science. As James B. Conant has put it:

> Somewhere about 1900 science took a *totally* unexpected turn. There had previously been several revolutionary theories and more than one epoch-

> making discovery in the history of science, but what occurred between 1900 and, say, 1930 was something different; it was a failure of a general prediction about what might be confidently expected from experimentation.

P. W. Bridgman has put it even more strongly:

> The physicist has passed through an intellectual crisis forced by the discovery of experimental facts of a sort which he had not previously envisaged, and which he would not even have thought possible.

The collapse of "classical" science actually began with Roentgen's discovery of X-rays in 1895; here was the first clear indication, in a form that everyone could appreciate, that the common-sense picture of the universe was not sensible after all. X-rays —the very name reflects the bafflement of scientists and laymen alike—could travel through solid matter, like light through a sheet of glass. No one had ever imagined or predicted such a thing; that one would be able to peer into the interior of the human body—and thereby revolutionize medicine and surgery—was something that the most daring prophet had never suggested.

The discovery of X-rays was the first great breakthrough into the realms where no human mind had ever ventured before. Yet it gave scarcely a hint of still more astonishing developments to come —radioactivity, the internal structure of the atom, relativity, the quantum theory, the uncertainty principle. . . .

As a result of this, the inventions and technical devices of our modern world can be divided into two sharply defined classes. On the one hand there are those machines whose working would have been fully understood by any of the great thinkers of the past; on the other, there are those that would be utterly baffling to the finest minds of antiquity. And not merely of antiquity; there are devices now coming into use that might well have driven Edison or Marconi insane had they tried to fathom their operation.

Let me give some examples to emphasize this point. If you showed a modern diesel engine, an automobile, a steam turbine, or a helicopter to Benjamin Franklin, Galileo, Leonardo da Vinci, and Archimedes—a list spanning two thousand years of time—not one of them would have any difficulty in understanding how these machines worked. Leonardo, in fact, would recognize several from his notebooks. All four men would be astonished at the materials and the workmanship, which would have seemed magical in its precision, but once they had got over that surprise they would feel quite at home—as long as they did not delve too deeply into the auxiliary control and electrical systems.

But now suppose that they were confronted by a television set, an electronic computer, a nuclear reactor, a radar installation. Quite apart from the complexity of these devices, the individual elements of which they are composed would be incomprehensible to any man born before this century. Whatever his degree of education or intelligence, he would not possess the mental framework that could accommodate electron beams, transistors, atomic fission, wave guides and cathode-ray tubes.

The difficulty, let me repeat, is not one of complexity; some of the simplest modern devices would be the most difficult to explain. A particularly good example is given by the atomic bomb (at least, the early models). What could be simpler than banging two lumps of metal together? Yet how could one explain to Archimedes that the result could be more devastation than that produced by all the wars between the Trojans and the Greeks?

Suppose you went to any scientist up to the late nineteenth century and told him: "Here are two pieces of a substance called uranium 235. If you hold them apart, nothing will happen. But if you bring them together suddenly, you will liberate as much energy as you could obtain from burning ten thousand tons of coal." No matter how farsighted and imaginative he might be, your pre-twentieth century scientist would have said: "What utter nonsense! That's magic, not science. Such things can't happen in the real world." Around 1890, when the foundations of physics and thermodynamics had (it seemed) been securely laid, he could have told you exactly why it was nonsense.

"Energy cannot be created out of nowhere," he might have said. "It has to come from chemical reactions, electrical batteries, coiled springs, compressed gas, spinning flywheels, or some other clearly defined source. All such sources are ruled out in this case —and even if they were not, the energy output you mention is absurd. Why, it is more than a *million* times that available from the most powerful chemical reaction!"

The fascinating thing about this particular example is that, even when the existence of atomic energy was fully appreciated—say right up to 1940—almost all scientists would still have laughed at

the idea of liberating it by bringing pieces of metal together. Those who believed that the energy of the nucleus ever could be released almost certainly pictured complicated electrical devices—"atom smashers" and so forth—doing the job. (In the long run, this will probably be the case; it seems that we will need such machines to fuse hydrogen nuclei on the industrial scale. But once again, who knows?)

The wholly unexpected discovery of uranium fission in 1939 made possible such absurdly simple (in principle, if not in practice) devices as the atomic bomb and the nuclear chain reactor. No scientist could ever have predicted them; if he had, all his colleagues would have laughed at him.

It is highly instructive, and stimulating to the imagination, to make a list of the inventions and discoveries that have been anticipated—and those that have not. Here is my attempt to do so.

All the items on the left have already been achieved or discovered, and all have an element of the unexpected or the downright astonishing about them. To the best of my knowledge, not one was foreseen very much in advance of the moment of revelation.

On the right, however, are concepts that have been around for hundreds or thousands of years. Some have been achieved; others will be achieved; others may be impossible. But which?

The Unexpected	The Expected
X-rays	automobiles
nuclear energy	flying machines
radio, TV	steam engines
electronics	submarines
photography	spaceships
sound recording	telephones
quantum mechanics	robots
relativity	death rays
transistors	transmutation
masers; lasers	artificial life
superconductors; superfluids	immortality
atomic clocks; Mössbauer effect	invisibility
determining composition of celestial bodies	levitation
	teleportation
dating the past (Carbon 14, etc.)	communication with dead
detecting invisible planets	observing the past, the future
the ionosphere; Van Allen Belts	telepathy

The right-hand list is deliberately provocative; it includes sheer fantasy as well as serious scientific speculation. But the only way of discovering the limits of the possible is to venture a little way past them into the impossible. . . . this is exactly what I hope to do; yet I am very much afraid that from time to time I too will exhibit failure of imagination if not failure of nerve. For as I glance down the left-hand column I am aware of a few items which, only ten years ago, I would have thought were impossible. . . .

Prospects of Technological Progress

Olaf Helmer

A mathematician-philosopher, Olaf Helmer is best known as the codeveloper (with Norman Dalkey) of the so-called "Delphi" method of forecasting. He also helped found, and is research director of, the Institute for the Future, one of the world's leading futurist "think tanks." Here, in nontechnical language, he makes a series of highly optimistic suggestions about the way things may turn out. Are they too optimistic?

Much has already been said about the prospects of technological progress during the remainder of this century, and I have little to add to these prognostications. I would like to use this opportunity, not primarily to make technological forecasts, but to discuss the role that the forecasting of technological developments plays