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COLD WAR SCIENCES (2): SCIENCES FROM INFORMATION SYSTEMS

Radar, recall, was built into an extensive 'information system'. Transmitters sent out pulsed radio beams and receivers picked up echoes, but that was merely the beginning of the process. The data collected were repeatedly sifted, as useful 'information' was separated and abstracted as it passed from receiver stations, through filter rooms, to operations rooms, where military decisions could be made after surveying the massively reduced representation of the world. Numerous skills were required. At Britain's Telecommunications Research Establishment, physicists and engineers might devise and design radio transmitting and receiving equipment and figure out how to reduce and represent data. Others, working closely with military officers, would work out how to fit equipment with existing military hardware: aircraft, ships, coastal reporting stations. As we have seen, this working world had already produced its own science, operational research. In this chapter I review other radar and information and global sciences.

Radio astronomy

Another new science was substantially started by extending the radar surveillance of the sky further into astronomical spaces. Before the Second World War, isolated small-scale radio astronomy projects had begun in the United States. Karl Jansky, an electrical engineer investigating sources of interference for Bell Laboratories, found cosmic sources of radio noise in the early 1930s, including one identified with the centre of our galaxy in Sagittarius.[1] An amateur, Grote Reber, with his hand-built dish telescope, had confirmed Jansky's source and identified others, among them ones in Cassiopeia and Cygnus as well as the sun. However, Jansky and Reber's pioneering work was matched and rapidly overtaken by the radio astronomy invented by scientists returning to academic study after wartime radar work. Post-war, radio astronomy became an autonomous specialty, differentiated from experimental physics, electrical engineering and optical astronomy, and pursued in highly organized Big Science projects in Britain, Australia, the Netherlands and the United States.

In the Netherlands, Jan Hendrik Oort began his astronomical investigations in 1945 with the excellent German war-surplus radar dishes. In Australia, a government establishment, the Radiophysics Laboratory of the Commonwealth Scientific and Industrial Research Organization (CSIRO), under ex-TRE and ex-MIT Rad Lab physicist Edward G. Bowen and Joseph L. Pawsey, developed radar astronomy into radio astronomy. In Britain, physicists leaving the Telecommunications Research Establishment brought with them equipment, contacts, and experience of scanning the skies with radar. At Cambridge University, an extramural site of the Cavendish Laboratory was set up by ex-TRE physicist Martin Ryle in the countryside. At Manchester University, ex-TRE physicist Bernard Lovell set up an outpost of Patrick Blackett's physics department in a former university botanical station south of the city, at Jodrell Bank. Both tapped contacts for the loan (and gifts) of war-surplus radio equipment. They set about investigating the phenomena noted during wartime when echoes from aircraft had had to be distinguished from sources of radio 'noise'. At Cambridge the early focus was on solar noise. At Manchester it was on radar studies of meteors – or, rather, of the trail of ionized gases left in the wake of a meteor that reflected radio waves for several minutes. (During the Second World War the Army Operational Research Group, based at TRE, had tried to distinguish reflections from meteor trails from incoming V-2 rockets.) The research soon uncovered previously unknown daytime meteor showers; radio astronomy was already contributing to the stock of knowledge held by optical astronomy.

Competition between the Sydney, Cambridge and Manchester groups encouraged each to pursue different technical strategies to explore the unknown radio universe, a process that David Edge and Michael Mulkay argue reveals the social character of scientific specialty formation and growth.[2] At Cambridge, Martin Ryle's team focused on building interferometers, instruments that matched the resolution of large radio telescopes by combining the measurements taken by several small antennae. Ryle's group built a series of interferometer telescopes in the 1950s and 1960s. Interferometers were especially effective at mapping the radio sources and thereby revealing information about their distribution. At first these sources were described as 'radio stars', not unreasonably, since it was well known that the sun was a radio source. However, only some of the radio stars congregated along the galactic plane, suggesting that many were extra-galactic objects. The second and third Cambridge catalogues of radio sources, '2C' and '3C', were published in 1955 and 1959 respectively. Ryle used data from the surveys, in particular the relationship between number and brightness of sources, to argue that they did not fit the steady-state model of the universe. A fierce controversy followed, with the steady-staters both criticizing the data and arguing that their model could be tweaked to fit.

At the Jodrell Bank Research Station, Lovell's team concentrated on research that could be done effectively with large single 'multipurpose' dishes.[3] The first one they built in the late 1940s was a bowl of wire, 218 feet in diameter, which sat on the ground reflecting radio waves to a receiver mounted on a pole. It had been designed to search for cosmic ray echoes – a research interest of Blackett. By 'happy accident', recalled Robert Hanbury-Brown, 'they had built a powerful instrument with which . . . to study the radio emissions from space'. This transit instrument was a proof of the concept of large dish radio telescopes.

Lovell's team then imagined a fully steerable version, possessing a dish of great collecting power and capable of targeting any point in the sky. With a 250-foot steel mesh bowl and a mount powered by electric motors, this was a huge – and expensive – project. The government was asked for £154,800 for the 'construction of a large radio telescope for investigating galactic and solar radio emission, meteoric phenomena, aurorae, lunar and planetary echoes'.[4] Like other Big Science projects, the Jodrell Bank Radio Telescope won sufficient backing because it provided solutions for other people's problems as well as a fine scientific instrument. British optical astronomers, specifically the elite professional Royal Astronomical Society, were sold on the idea of the instrument sparking a resurgence of British astronomy unrestricted by inclement weather. The government decision-makers applauded a 'Great Public Spectacle', a visible flagship, a carrier of 'national prestige', a prestigious scientific project suitable for a nation otherwise struggling with post-war winds of change.

In 1954, with the radio telescope project underway, the radio astronomers requested design changes. There were several reasons. Astronomers at Harvard University had discovered that cosmic emissions on the 21-centimetre band from neutral hydrogen could be detected. Dutch astronomers, including Oort, swiftly demonstrated that, by mapping hydrogen, the most common element in the universe, extraordinary details of galactic structure could be revealed. Oort's maps of the mid-1950s showed the separate arms of the Milky Way for the first time. (The ability to investigate a wavelength as small as 21 centimetres was a direct result of wartime innovation: the techniques of generating and measuring near-microwave wavelengths such as 21 centimetres were precisely those developed for radar systems such as Lovell's Home-Sweet-Home.) For the Radio Telescope to operate at smaller wavelengths, a denser, therefore heavier, mesh was required. The design changes therefore meant more money was needed.

As costs spiralled, other, Cold War, justifications for the Radio Telescope were proposed.[5] Lovell lobbied the armed service departments with arguments that the telescope could be used as an early warning system, or that radio stars could be used by missiles in a form of astro-navigation. In October 1957, only two months after the telescope was completed, in debt, the instrument was deployed in a highly publicized tracking of the Soviet satellite Sputnik. Not only were there very few instruments capable of tracking the satellite and its ICBM-like rocket, also in orbit, but the use of a British instrument to locate a Soviet satellite, while the United States panicked, also sent a politically useful message. When the debt was finally cleared it was by philanthropist and rabid anti-communist Lord Nuffield, who had justified his earlier financial support for the telecope with the hope that it 'might one day be directed if need be on the steppes of Russia'.[6]

Radio astronomers, therefore, were able to exploit Cold War concerns to secure scientific facilities on a scale that would not have been possible otherwise. A project for the United States Navy that would have dwarfed even the Jodrell Bank telescope provides another example. The telescope designed for Sugar Grove, West Virginia, had a planned span of 600 feet and would have weighed 36,000 tons. Congress, spooked by Sputnik, awarded $79 million, and construction started. It was cancelled in 1962. However, the spiralling costs were only one cause. Another was that Sugar Grove, while publicly a radio telescope, was secretly also designed to be an eavesdropping instrument, recording radio transmissions from Russia as they echoed back from the moon. The success of spy satellites removed this secret justification for its existence.

Combinations of 'black' (secret) and 'white' (public) science were typical of Cold War projects. Historian David van Keuren, who uses this colour terminology, has described another fascinating example of this combination.[7] The GRAB – Galactic RAdiation Background – satellite was launched on 22 June 1960. GRAB was part the first astronomical satellite and part the first intelligence satellite, a forerunner of the famous Corona series. Van Keuren emphasizes that the need for cover gave scientists far more leverage than might have been expected (say, from a reading of Forman's 'mutual orientation' of interests). He quotes the GRAB astronomer Herbert Friedman: 'We weren't destitute for opportunities. But the fact that the intelligence people were happy to have a cover for what they were doing made it opportune for us to move in there.'

The astronomers were pleased with the GRAB results: a demonstration that solar flares produced X-rays, which ionized the upper atmosphere, blocking out radar transmissions, was both useful and basic research. The 'intelligence people' regarded GRAB as a 'revolution', providing information on the number and character of Soviet radars (including the revelation that the Soviets had an anti-ballistic missile radar) – information that the Strategic Air Command put to immediate use, plotting attack flight paths into the Soviet Union. Both sides were happy. The scientists in particular gained resources and data they would not otherwise have had. The whole story was declassified only in 1998, after the Cold War. In retrospect the name 'GRAB' should perhaps have raised suspicions!

Like light, radio waves pass through the earth's atmosphere. Optical and radio astronomy could therefore be land-based projects. The atmosphere is not transparent to other parts of the electromagnetic spectrum. The development of ultraviolet, X-ray and gamma ray astronomy therefore depended on space technologies. Again there was often a link to national security. For example, the first X-ray astronomical satellite, Uhuru, launched in 1970, was designed by Riccardo Giacconi's team at American Science and Engineering, Boston. Giacconi and his team had previously worked on using X-ray instrumentation in satellites to detect X-ray flashes from atmospheric nuclear tests in the early 1960s.[8] Indeed, the first cosmic X-ray sources were found in this search for clandestine nuclear tests.

Radio astronomy therefore grew out of the working world of radar and was sustained as a form of Big Science by the resources made available during the Cold War. The scientific return has been extraordinary. Bell Telephone Laboratories, still concerned with radio interference with their systems and also developing an interest in satellite communication, built a large unusual horn-shaped radio antenna for use in the Echo and Telstar satellite programmes. In 1964, two Bell employees, Arno Penzias and Robert W. Wilson, using this acutely sensitive radio antenna, measured a faint but pervasive residual radio hiss that they could not locate or eradicate. Word reached astrophysicists at Princeton, among them Robert Dicke, who in the 1940s had predicted that remnants of the big bang would be detectable as a pervasive microwave hiss. Observation and interpretation were connected in what is now recognized as the detection of the 3 K cosmic microwave background radiation, core evidence for the big bang and further empirical difficulty for the steady state theory.

Radio astronomy also produced novel astronomical objects. In the 1950s, it was known that many of the radio sources listed in the Cambridge surveys were small in diameter. (The Jodrell Bank Radio Telescope contributed to this finding.) Some radio sources were matched to optical objects. In 1963, using measurements made with the large Parkes telescope in Australia during an occultation by the moon of a radio source, Dutch-American astronomer Maarten Schmidt at Mount Palomar identified source 3C273 with an odd bluish star. Schmidt's investigations of 3C273's spectrum revealed an extraordinarily high red shift, suggesting that the source lay at a great distance. This 3C273 was a member of a new class of astronomical object: very distant and, to be detectable at all, intrinsically very bright. Indeed, these 'quasars', quasi-stellar objects, astronomers concluded, must be among the most violently energetic objects in the universe.

In July 1967, the young Cambridge radio astronomer Jocelyn Bell had noticed some 'scruff' in a pen-recording of a radio source in the small constellation of Vulpecula. Cleverly thinking to check the fine structure of this 'scruff', she found a very fast regular repeating pulse of radio waves. Bell's team leader, Anthony Hewish, led the investigation, determining that the source was of cosmic origin (but ruling out 'little green men'). After many such checks had been conducted in private, the discovery of 'pulsars' was announced in late 1967. The speed of the pulses gave clues about the size of the transmitting object – the faster, the smaller – and these pulsars were very fast. Thomas Gold, in 1968, argued that pulsars were spinning neutron stars, super-compressed hitherto hypothetical bodies in which the atomic structure had been crushed to a dense sea of neutrons. By the late 1960s, therefore, radio astronomy, barely two decades after it grew from wartime radar, had provided scientists with a universe populated by novel entities as well as strong evidence of a Big Bang.

Cybernetics

Another new science, cybernetics, was a result of direct reflection on the working world of anti-aircraft radar research. Bombers approaching at high speed gave human anti-aircraft gunners very little time to register the position of a target and direct fire. This acute problem was certainly an obvious focus for technical innovation, some of which was led by Warren Weaver for the NDRC;[9] it did not seem at first glance the likely foundation of a new science. Norbert Wiener, ex-child prodigy, PhD in mathematics from Harvard at the age of eighteen, was an accomplished middle-aged MIT mathematician when he was given the problem of radically improving anti-aircraft technology in 1940.[10] Assisted by electrical engineer Julian Bigelow, Wiener considered the problem of feeding back radar echo data on the position of an approaching aircraft as a way of automating anti-aircraft fire. He called his device an 'anti-aircraft predictor'. The task was not as simple as merely extrapolating the path of the aircraft, as one would of the smooth path of a cannon ball (Wiener knew his ballistics anyway). Rather, the pilot's actions, conscious and unconscious, continually shook the flight path. Wiener and Bigelow reported their breakthrough insight:

> We realized that the 'randomness' or irregularity of an airplane's path is introduced by the pilot; that in attempting to force his dynamic craft to execute a useful manoeuver, such as straight-line flight or 180 degree turn, the pilot behaves like a servo-mechanism.[11]

The pilot's consciousness could be represented in the action of the anti-aircraft machinery, and the whole could be generalized by focusing on negative feedback, corrective, loops. The anti-aircraft predictor's capture of the pilot's psyche would interest, reported Wiener already with eye to the future, the 'physiologist, the neuropathologist, and the expert in aptitude cases'. 'More to the point', Peter Galison reminds us of the direct task at hand, 'it suggested that a more refined AA predictor would use a pilot's own characteristic flight patterns to calculate his particular future moves and to kill him.'[12]

'It does not seem even remotely possible to eliminate the human element as far as it shows itself in enemy behavior', recalled Wiener. 'Therefore, in order to obtain as complete a mathematical treatment as possible of the over-all control problem, it is necessary to assimilate the different parts of the system to a single basis, either human or mechanical.'[13] Wiener's reflections on his anti-aircraft device thus led him to a foundational project: to dissolve disciplinary boundaries in the natural, human and physical sciences and recast a new science, 'cybernetics', in terms of feedback. With the Mexican neurophysiologist Arturo Rosenblueth, Wiener and Bigelow let the civilian world gain some sense of this scheme in a 1943 paper, 'Behavior, purpose, and teleology'. Wiener also began working with two computing pioneers, Howard Aiken and John von Neumann. The group established a Teleological Society. 'We were all convinced', recalled one member, 'that the subject embracing both the engineering and neurology aspects is essentially one.'[14] The Josiah Macy Jr. Foundation stumped up funds for a series of cybernetics conferences. In 1948 Wiener published *Cybernetics* (the neologism of the title was based on the Greek for 'steersman'). Its subtitle named the project: 'control and communication in the animal and the machine'.

Core exemplars, central texts and foundational meetings form the heart of a new discipline. Cybernetics was a project for a new 'universal discipline', one which subsumed and reinterpreted others – psychology and engineering, among many others.[15] Cybernetics also blurred the boundaries between living and non-living systems. 'We believe that men and other animals are like machines from the scientific standpoint because we believe that the only fruitful methods for the study of human and animal behavior are the methods applicable to the behavior of mechanical objects as well', Rosenblueth and Wiener had argued in 1950. 'Thus our main reason for selecting the term in question was to emphasise that as objects of scientific enquiry, humans do not differ from machines.'[16] Such remarkable ambition was significantly encouraged and sustained in a Cold War working world where the integration of humans and machine in military systems was a central problem.

Cybernetics was taken up in different ways in different societies. In Britain, W. Grey Walter, a neurophysiologist who had worked on radar during the war, built robot tortoises that could seek out 'food', interact with each other, and even provide a model of 'affection'.[17] Walter characterized the brain as an electrical scanning device, not unlike radar. Soviet scientists attracted by cybernetics faced the problem that their chosen subject was denounced by ideologues. 'Cybernetics: a reactionary pseudo-science arising in the USA after the Second World War and receiving wide dissemination in other capitalist countries', ran the definition in one mid-1950s Soviet dictionary: 'Cybernetics clearly reflects one of the basic features of the bourgeois world-view – its inhumanity, striving to transform workers into an extension of the machine, into a tool of production, and instrument of war.'[18] After the death of Stalin in 1953, Soviet cyberneticians were able to withstand ideological attack, partly by learning to present cybernetics in appropriate rhetorical form, but mostly because they could claim that cybernetics in general, and the computer specifically, were essential to Soviet Cold War defence projects.[19]

Computers and the Cold War

Historian Paul Edwards argues that the computer was the central tool and metaphor of the Cold War. His argument is best seen in the way the computer was placed at the centre of the post-war American early warning system. The story of how it happens also illustrates some of the contingencies of Cold War projects. In 1944, Jay Forrester, an electrical engineer at the Servomechanisms Laboratory of MIT, was developing a digital version of an analogue flight simulator. This became Project Whirlwind, a vision of digital computing as a component of centralized control systems. When rising costs caused the first sponsor, the Office of Naval Research, to get cold feet, Forrester hawked the project around other sponsors. By turns it became a solution for problems of logistical planning, air traffic control, life insurance calculations and missile testing, before Forrester reached the air force. The air force, pondering a proposal from another MIT professor, George Valley, saw Project Whirlwind as the centre of an air defence system.

Forrester's Whirlwind became the electronic computer at the centre of SAGE, the Semi-Automatic Ground Environment. SAGE was a computer-controlled air defence system of national coverage, in which incoming radar information would be passed to a combat centre, which in turn would control direction centres. 'The work of producing SAGE', notes Edwards, 'was simultaneously technical, strategic, and political.'[20] Technically, SAGE demanded innovations in online representation of electronic data and real-time computing. (Existing stored-program computers did not operate in real time; rather, computing tasks were completed in batches.) SAGE required new techniques of data transmission and reception (such as modems) as well as video and graphical displays. Magnetic core memories, a considerable advance over older Williams tubes and delay lines, were developed for the Whirlwind. The main beneficiary in business terms was IBM, which received the substantial contract for providing the machines and turned its access to government-funded cutting-edge military technology into post-war domination of the computer market.

Strategically, acceptance of SAGE demanded a transformation in the organization and values of the armed services. SAGE pointed towards automated, centralized, computerized control of warfare. SAGE went against traditional military values of human command and the delegation of responsibilities for interpreting how an order be fulfilled.[21] The SAGE project was also an important step in confirming the mutual orientation, in Forman's terminology, of the military patron and the academic laboratory: the air force changed in following Forrester's vision of computerized control, while, in this case, MIT changed in becoming, functionally, a research and development arm of the United States Air Force. Politically, SAGE illustrates Edwards's broader argument: the computer not only served as a tool at the centre of Cold War systems but also provided a metaphor of control in a contained, 'closed world'.[22]

Military agencies were the primary, generous patrons of computers. It was the Ministry of Supply, responsible for British atomic projects, that stumped up most of the cash for the first electronic-stored program computer built in 1948 at Manchester University by ex-radar scientists. The first Soviet stored-program computers, starting with the MESM built by Sergei Lebedev's team in 1951, were needed to meet military demands from ballistic missile, atomic weapon and missile defence projects.[23] However, it was the combination of military demand and industrial innovation in the United States that produced the world-changing developments in computing in the second half of the twentieth century. In general, sustained by the continuing Cold War and boosted further by burgeoning commercial applications, computers became smaller and cheaper to make and operate.

In the process, science has changed. In parallel with Edwards's argument, the changes can be said to have a dual nature: computers have become revolutionary tools for scientific research but also a guiding metaphor for new disciplinary programmes. Examples of computer-as-tool can be found in radio astronomy, X-ray crystallography, meteorology,[24] high-energy physics and, indeed, well before the end of the century, in every science. Examples of sciences that took the computer as a metaphor guiding research programmes were artificial intelligence, cognitive psychology, immunology and, in a subterranean but profound way, the reinterpretation of genetics as a science of codes.

Computers as tools of revolution in science

Particle accelerator laboratories and astronomical observatories were both places where complex expensive equipment was set up to generate masses of data that could be simplified and rearranged to produce representations of the natural world. These representations, stand-ins for nature, were the subjects of scientific work. A large optical telescope, such as the 200-inch instrument at Palomar, produced photographs which could be measured, compared and combined. By the 1980s, chemical photography was being replaced with the digital read-outs of charge-coupled devices (CCDs). By then the computer was already the tool guiding the control of large telescopes' movements as well as the means by which data were handled.

Optical astronomy was following the lead taken by radio astronomy. Computers, such as the Ferranti Argus used with the Jodrell Bank Mark II telescope, were deployed to control the movement of telescopes from the early 1960s. But the greatest leap in performance came with the use of computers to process the immense amount of information generated. The techniques of aperture synthesis, developed primarily by Martin Ryle's team at Cambridge, provide the most startling example. Data from small, widely spaced telescopes could be combined in a way that mimicked the capabilities of very large telescopes. Indeed, by waiting for the earth's rotation to sweep the telescopes through space while observing the same patch of sky, the capability of a telescope many thousands of miles in equivalent aperture could be matched, at least in terms of resolution. But the reduction of these data by hand, or even by mechanical calculator, would have been far too costly in terms of hours of human labour. The future of radio astronomy depended on computers.

Computers had been deployed even earlier in nuclear weapons science, X-ray crystallography and high-energy physics. Developing the atomic bomb had raised critical questions, such as how much energy would be released effectively. Direct experimental investigation was ruled out: no measuring instrument could withstand the atomic blast long enough to report. Nor did theory provide a way forward: the equations generated were intractable. For the thermonuclear bomb, John von Neumann and Stanislaw Ulam offered instead an artificial version: simulation by numerical methods, named 'Monte Carlo' by their collaborator, Nicholas Metropolis.[25] They tried the simulation out on the ENIAC, a fast and flexible wartime calculator, in the late 1940s, but the technique was only fully exploited on the early stored-program computers, such as the MANIAC built at Los Alamos.

In X-ray crystallography, the process of reducing data involved, first, making careful measurements of the position and dimensions of the black spots found on photographs of X-ray diffraction patterns and, second, a fourier transformation, a mathematical procedure which entailed calculating and summing large numbers of trigonometrical series.[26] This calculation, described by A. Lindo Patterson in the 1930s, was laborious even for simple crystalline molecules. As larger molecules drew interest, so the computing task became even more time-consuming. Two physicists working in Lawrence Bragg's X-ray crystallography laboratory in interwar Manchester developed a bespoke tool to assist the work: Beevers–Lipson strips, cards holding the relevant values of sines and cosines. Placing the appropriate cards together, the sum could be more easily calculated. X-ray crystallographers also knew that a heavy element within a molecule could be used as a peg around which structure could be more easily determined. X-ray crystallographers worked iteratively: taking a diffraction picture, deriving partial indications of the structure, drawing conclusions, trying a new angle, and repeating the process.

As larger and larger molecules were tackled, the Beevers–Lipson method was translated first to punched cards and then to electronic stored-program computers. Linus Pauling at Caltech and Bragg's teams had used punched cards. Oxford X-ray crystallographer Dorothy Hodgkin took up punched-card methods after her derivation of penicillin structure.[27] But for her new project, the three-dimensional structure of vitamin B_{12}, she turned in the early 1950s to electronic computers, both analogue instruments, such as Ray Pepinsky's X-RAC, which had been built with Office of Naval Research and private philanthropic funding, and digital stored-program computers, such as the Ferranti mark 1 at Manchester and the SWAC, built at the University of California, Los Angeles, for the National Bureau of Standards. Likewise, Max Perutz and John Kendrew at Cambridge would use the Cambridge EDSAC computer (as well as time begged on industrial and military computers) to derive structures of haemoglobin and myoglobin, respectively.[28] These were massive molecules of great interest to life scientists. Reduction of the data to resolve structure at the order of a tenth of a nanometer required many hundreds of hours of computer time – far longer than would have been feasible by hand. Perutz, Kendrew and Hodgkin would all receive Nobel prizes to mark the impact of the computer as a tool for molecular biology.

In high-energy physics, there were differing views about the proper use of computers. Galison argues that two distinct traditions can be found in microphysics.[29] In the 'image' tradition, what was sought were picturing technologies that generated mimetic representations, ideally 'golden events': the capture, for example, of an interesting particle decay on a single cloud chamber photograph. In the second, 'logic' tradition, the preference was for counting techniques: the clicks of a Geiger–Müller counter accumulating as data that could be statistically analysed. As the scale of physics increased after the 1940s, both traditions embraced the computer as a means of reorganizing work, but in different ways. Within the image tradition, a physicist such as Luis Alvarez at Berkeley might insist that human judgement (at least of the professional physicist) must remain, while at CERN, scientists in the logic tradition, such as Leo Kowarski, might aim for thoroughgoing automation. At stake was the role of the individual physicist in Big Science projects: a mere machine-minder or a creative partner. Nevertheless, from the mid-1960s, argues Galison, the traditions became merged; in instruments such as the SLAC-SBL Solenoidal Magnetic Detector, computerization was central, mediating between other parts of the complex system.[30] At CERN, Georges Charpak's multiwire proportional chamber, developed in 1968, made the detection of particles an electronic event, replacing photography, inviting further computerized control and analysis, and transforming the experimenters' art.

The computer, by creating a simplified accessible manipulable representation of nuclear reactions, had been an essential tool of the nuclear weapons scientist. The computer, by speeding up calculation, made aperture synthesis and three-dimensional modelling of large proteins feasible. The computer, as control of a particle detector, could count many millions of events. The computer was, in this sense, a revolutionary tool for astronomy, molecular biology and particle physics; a similar case can be made for its causing a qualitative shift, a 'phase change', in other data-rich subjects.[31] Indeed, in Douglas Robertson's view, the computer became, in the second half of the twentieth century, the most important tool in science: 'more important to astronomy than the telescope', 'more important to biology than the microscope', 'more important to high-energy physics than the particle accelerator', 'more important to mathematics than Newton's invention of the calculus', and 'more important to geophysics than the seismograph'.[32]

There is something right and something wrong with this view. Robertson is wrong in the sense that astronomy, say, has always been, largely, the science of inscriptions – whether photographs, pen-recordings or outputs from CCDs – made by telescopes. He is also wrong to argue that there was an unambiguous qualitative shift: computerization nearly always automated previous methods of mechanical and human labour processes of calculation; the transition was often relatively smooth.[33] Robertson, however, is right in his argument that what instruments are capable of doing, and what reduction and analysis is capable of achieving, is now defined by computing power. This has made the sciences of the late twentieth and early twenty-first centuries strikingly more similar to each other – sciences of a digital working world – than perhaps they were in earlier incarnations.

The computer, argues Robertson, vastly extended the scientists' ability to see. Certainly new ways of visualization in the sciences have been dramatically transformed. At MIT from the mid-1960s, for example, computer scientists developed ways to display molecules in three dimensions, allowing chemists to interact with the representations.[34] These methods at first complemented and later supplanted other ways of molecular model building, such as using sticks and balls, plasticine, cardboard or paper cut-outs. Medical imaging technologies provide even more startling examples. From the mid-1960s, Godfrey Hounsfield, an ex-radar engineer working for Electrical and Musical Industries (EMI), developed a method of focusing X-ray beams and recording the data digitally.[35] By combining the data from many thin slices and using a computer to piece the data together again, Hounsfield and his colleague A. J. Ambrose experimented with producing images of the inside of brains. An early experiment, with a cow's head from a local butcher, failed to produce a good image; but they were delighted when a second cow's head, this time from a kosher butcher, who had bled the animal to death rather than bludgeoning it, was revealed in intricate glory. The EMI company, flush with money from the success of the Beatles, invested in this technology of computerized tomography (CT) scanning, which found direct medical uses, locating a brain tumour on its first scan of a patient in October 1971. Hounsfield and an independent scientist, Albert Cormack, received a Nobel Prize in 1979.

Along a similar timescale, CT has been joined by other medical imaging techniques. Nuclear magnetic resonance imaging (MRI), which had the advantage of not requiring X-rays, instead had its roots in the precision measurements of magnetic moments of protons and neutrons in the early Cold War by physicists such as Isidor Rabi and Felix Bloch. Nuclear magnetic resonance then became a tool for inorganic chemists in the 1950s and organic chemists in the 1960s; only in the 1970s did it become a method of medical imaging, developed in competition between British and American groups.[36] Like CT, MRI is expensive, reliant on computer power (even the first CT scan required 28,000 readings), and indicative of a scaling up of hospital medicine in the West.

The 'computer began as a "tool", an object for the manipulation of machines, objects and equations', summarizes Peter Galison. 'But bit by bit (byte by byte), computer designers deconstructed the notion of a tool itself as the computer came to stand not for a tool, but for nature.'[37] The Monte Carlo simulations had been one, precociously early, indication of how computer models could 'stand in' for nature. In other sciences, the computer became a dominant organizing metaphor. Brain and mind sciences provide examples: while CT and MRI might deploy the computer as a tool for processing images, in artificial intelligence and cognitive psychology the computer stood for nature.

The computer as mind

There have been two, often competing, approaches to modelling human brains and minds since the mid-twentieth century. In the first, bottom-up approach, the structure of the brain was directly modelled, in much reduced form, in hardware. Warren McCulloch and the young unstable prodigy Walter Pitts had written a paper, published in 1943, which treated the nerve cells as switches, and argued that phenomena of the mind (for example, memory) could be found in feedback loops within networks of neurons and that networks could operate like logical circuits. McCulloch and Pitts established that their networks of neuron-switches were fully equivalent to Turing's universal machines. Von Neumann and Wiener had read this paper with great interest.[38] After the war, following in their footsteps, a young Harvard student, Marvin Minsky, assembled with electronics student Dean Edwards and psychologist George Miller a neural network machine (the 'Snarc') from war-surplus supplies and a small grant from the Office of Naval Research. The Snarc simulated a rat learning the route through a maze, and amounted to both a materialization of behaviourist theories of stimulus and reward and an application of cybernetic theories of the brain.

Frank Rosenblatt, working at Cornell University, developed the neural net-as-learning device even further, into the theory and practice of what he called 'perceptrons'. By 1958, Rosenblatt was making great claims about their achievements, telling the *New Yorker* that:

> [The perceptron] can tell the difference between a dog and a cat, though so far, according to our calculations, it wouldn't be able to tell whether

> the dog was to the left or the right of the cat. We still have to teach it depth perception and refinements of judgment.[39]

Yet this bottom-up approach already had a critical and equally hubristic rival. The idea that a computer could not, at least under favourable conditions, be distinguished from a human mind had been discussed with subtlety and wit by Alan Turing in a paper for the philosophical journal *Mind* in 1950. In it he had raised and dismissed many objections to the claim that machines could think. In the hand of American scientists, many of whom were riding a tide of Cold War and philanthropic patronage in the early 1950s, the step was made from Turing's question 'can machines think?' to the questionable claim 'machines can think'. Rather than bottom-up constructions of simplified brains, the second approach labelled symbol-manipulating software as minds in action. The main protagonists were John McCarthy, Herbert Simon, Allen Newell and Minsky, who jumped ship from neural net research in 1955.

John McCarthy came from a family of Marxists, an encouraging upbringing, perhaps, for regarding ideas as material products. He trained as a mathematician and worked at Bell Labs, with Minsky under Claude Shannon, in 1953. He gained direct experience of stored-program computers while working for IBM in the summer of 1955, after which he taught at Dartmouth College, New Hampshire, a site of pioneering computing experimentation. Minsky and McCarthy persuaded the Rockefeller Foundation to cover the cost of a summer workshop at Dartmouth in 1956. 'Every aspect of learning or any feature of intelligence', ran the proposal for the workshop discussion, 'can in principle be so precisely described that a machine can be made to simulate it.' The workshop was a gathering of ambitious men: Minsky, McCarthy, Shannon, Nathaniel Rochester of IBM, the former assistant to Norbert Wiener, Oliver Selfridge, Trenchard More, Arthur Samuel, Herbert Simon and Allen Newell. They declared the existence of a new discipline, coining a new name: 'artificial intelligence'.

Although many attendees thought the 1956 conference a relative failure, the aspirations expressed were sky high. The only concrete result presented was Logic Theorist, a program written by Simon, Newell and J. C. Shaw. Herbert Simon was a Cold War political scientist who had practical experience of Marshall Plan administration and Ford Foundation projects.[40] In books such as *Administrative Behavior* (1948) he had presented his theory of 'bounded rationality', proposing that people in organizations did not consider all the possible options, making a choice that maximized their interests (as neoclassical economic theory assumed) rather than decisions being taken by starting with acceptance criteria, reviewing few possibilities, and often choosing the first that passed the criteria. Simon had speculated further that the human mind worked in a similar way: heuristic rule-bound problem-solving.

Logic Theorist ran on the JOHNNIAC computer at RAND, where Newell worked. It modelled mathematical deduction, starting with the axioms and deriving the theorems of Bertrand Russell and Alfred North Whitehead's *Principia Mathematica*. A 'clear descendant of Simon's principle of bounded rationality', it did so by following heuristics:[41] 'Our theory is a theory of the information processes involved in problem-solving', the programmers argued, as they explicitly rejected the bottom-up approach. It was 'not a theory of neural or electronic mechanisms for information processing'. Symbol manipulation was how the mind operated, they reasoned, so a machine manipulating symbols was mind-like. Simon later recalled his view of Logic Theorist's significance, a stroke that cut the Gordian knot of post-Descartes philosophy: 'We invented a computer program capable of thinking non-numerically, and thereby solved the venerable mind/body problem, explaining how a system composed of matter can have the properties of mind.'[42]

Turing, in his *Mind* paper of 1950, had speculated that a computer might pass his imitation test in fifty years. Newell and Simon, in 1958, went further:

1 . . . within ten years a digital computer will be the world's chess champion, unless the rules bar it from competition.
2 . . . within ten years a digital computer will discover and prove an important new mathematical theorem.
3 . . . within ten years a digital computer will write music that will be accepted by critics as possessing considerable aesthetic value.
4 . . . within ten years most theories in psychology will take the form of computer programs, or of qualitative statements about the characteristics of computer programs.[43]

Artificial intelligence's promises attracted scepticism from some quarters of academia. Hubert Dreyfus attacked the 'artificial intelligentsia' in a RAND pamphlet, *Alchemy and AI*, in 1964, later presenting the criticisms at length in his *What Computers Can't Do* (1972). Joseph Weizenbaum, author of a lovely program, ELIZA, which seemed to converse intelligently but was just a box of simple tricks, wrote about the limits in *Computer Power and Human Reason* (1976). Yet, through the Cold War, the optimism for achieving artificial intelligence in the short-term returned as regularly as waves to the shore: feats such as natural language use or intelligent pattern recognition were forever merely a decade away.

The sustenance of optimism for near-future success in artificial intelligence was a Cold War effect. 'Virtually all' the funds for artificial intelligence research in the United States came from the Information Processing Techniques Office (IPTO) of the Advanced Projects Research Agency of the Department of Defense.[44] The ideology of automated command and control favoured AI projects. IPTO's leader, the visionary computer scientist J. C. R. Licklider, had written in an influential essay, 'Man–computer symbiosis', in 1960:

> The military commander . . . faces a greater probability of having to make critical decisions in short intervals of time. It is easy to overdramatize the notion of the ten-minute war, but it would be dangerous to count on having more than 10 minutes in which to make a critical decision. As military system ground environments and control centers grow in capability and complexity, therefore, a real requirement for automatic speech production and recognition in computers seems likely to develop.[45]

Artificial intelligence promised a high-risk, high-gain solution to the problems of automating military decision support. The automation of natural language – production, translation, understanding – was needed by military commanders but also by the secret eavesdroppers on Soviet communications. Many Cold War patrons would be lured by AI. RAND considered Newell and Simon's work 'important for developing and testing theories about human intelligence and decision-making and building [its own] computer programming capabilities', and soon set up a Systems Research Laboratory to 'examine how human-machine systems perform under stress', simulating parts of SAGE.[46] In turn, the research agendas of some of the best technical universities in the United States – MIT, Stanford, Carnegie Mellon – were further aligned along Cold War interests. In contrast, bottom-up neural net approaches, fiercely attacked by the symbol-manipulators, struggled to find funds.

Nevertheless, artificial intelligence would mutate in the face of criticism. One approach was to simplify the representations of the working world even further until it became tractable with the techniques to hand. Terry Winograd's SHRDLU, developed at MIT in 1968–70, was the best example: a 'microworld' of simple shapes that the machine could reason with and move about. A second approach was to model the narrow but deep knowledge of scientific specialties. Dendral, built at Stanford University from 1965, was an early example. It was a collaboration between the computer scientist Edward Feigenbaum, the molecular biologist Joshua Lederberg and the chemist Carl Djerassi, among others, with the aim of automating an organic chemist's ability to draw conclusions about molecules from the output of a mass spectrometer. For Lederberg this was one response among many to an 'instrumentation crisis in biology': 'an immense amount of information is locked up in spectra . . . which require the intensive development of a "man–computer symbiosis" for adequate resolution.'[47] Since diagnostic expertise was expensive to make and replicate, there followed considerable commercial interest in this approach. An expert system for mimicking doctors' judgements about infection treatments, Mycin, for example, followed Dendral in the early 1970s.

Mind as a computer

Artificial intelligence saw the computer as mind. Cognitive psychology saw the mind as a computer, and in doing so broke the rules of earlier psychology. 'You couldn't use a word like "mind" in a psychology journal', Herbert Simon wrote of the dominance of behaviourism, 'you'd get your mouth washed out with soap.'[48] But in books such as George Miller's *Language and Communication* (1951) and especially Miller, Eugene Galanter and Karl Pribam's *Plans and the Structure of Behavior* (1960), a manifesto for cognitivism was developed.[49] The latter was 'the first text to examine virtually every aspect of human psychology, including instincts, motor skills, memory, speech, values, personality, and problem solving', writes Paul Edwards, 'through the lens of the computer analogy'.[50] Cognitive psychology would thrive on Western campuses from MIT to Sussex. Closely connected with this research programme was the revolution in linguistics instigated by Noam Chomsky at MIT from the mid-1950s. Natural language competence was the achievement of the application of deep-lying heuristics, a universal grammar, common to all humans. Information processing had its modern birth barely a decade before in the sciences of the working world of radar; by 1955 the model had been naturalized. Universalism – whether it was Rostow's economics, Turing's machines or Chomsky's grammar – was a distinctive shared feature of projects of the Cold War.

The work of computing

However, artificial intelligence, with its grand statements about solving age-old philosophical problems and promising machines that matched the greatest human achievements, was a tiny component of the growth from the 1950s of computer science, a true science of the post-war working world. Aspray sees computer science as an 'amalgam' of four intellectual traditions: the mathematical or logical (which thrived largely on inherited scholarly status), hardware engineering (universities were a major source of computer prototypes until the 1960s), software engineering (which saw continuities with civil engineering) and the experimental 'science of the artificial'.[51] Computer science, as previous sciences had done, set up and experimented with artificial representations of working world problems. 'Computer science', wrote Simon, Alan Perlis and Newell in *Science* in 1967, was the 'study of computers'.[52] Computers, in turn, were materializations of the organization of human work.[53] In *The Sciences of the Artificial* (1968), Simon defended this account of the relationship between science and world, one which I have tried to generalize.

'Programmer' became a new occupation in the 1950s, and faculties in computer science sprang up from the late 1950s to teach generations of university students.[54] The first machines had had to be programmed by technicians at the most basic level, little above feeding in strings of zeroes and ones. To make programming easier, high-level computer languages were developed, important ones including FORTRAN (designed for scientists, the name a contraction of 'formula translation'; the language came with IBM computers from 1957) and COBOL ('Common Business Oriented Language', which owed its success to the insistence of the Department of Defense that the United States government would only buy COBOL compatible computers, 1959).[55] The informal anecdotal techniques of programming were made explicit in textbooks such as Donald Knuth's *The Art of Computer Programming* (1968), as was a defining focus on algorithms. Many programmers, in a rapidly expanding industry, were nevertheless trained on the job, a route that encouraged women to be programmers.

The stored-program computers of the 1950s were typically large mainframes. They would be programmed by punching out a batch of cards which would be taken to the mainframe building. Users would return later to see if their program had run well or not. By the late 1950s the cost of a computer had dropped somewhat, enough for many medium-sized businesses, large laboratories or smaller government departments to own their own machines, but still computers followed this same mainframe pattern. It would change in the 1960s, when the scale of computing tasks reached a crisis point

Code Science: DNA and the rewriting of molecular biology

The proliferation of information technologies, and the high status attached to them during the Cold War, encouraged talk of 'information' and 'codes'. The most remarkable example of this rhetorical turn was the transformation in the concepts and techniques of the sciences of molecular biology and inheritance. By 1940 there were three unresolved problems in genetics.[56] We have already seen one of them: the relationship between genetics and evolution was articulated from the 1930s to 1950s in the Evolutionary Synthesis. The second, broadly a relationship between genetics and development, would remain problematic for much of the century. Certainly in the mid-century most embryologists were sceptical of the capacity of the chromosomal theory of the gene to account for a whole life history of an organism.

The third outstanding problem was the chemical composition of the gene. In the words of Hermann Muller, the genes must be 'autocatalytic', capable of duplicating themselves, and 'heterocatalytic', able to control the formation of other substances found in living creatures. Chromosomes were made up mostly of protein with a little nucleic acid. Deoxyribonucleic acid (DNA) was a highly unlikely candidate, since it was considered to have a rather dull structure of uniformly repeating nucleotides. DNA, the reasoning went, probably served as scaffolding, supporting the true genetic substance. Proteins, however, were incredibly diverse in structure. We have seen how pre-war philanthropic programmes encouraged the 'protein paradigm', the working assumption that genes were, in some as yet unknown way, composed of proteins.[57]

Progress came when scientists focused on manipulating and experimenting with micro-organisms in sophisticated ways. Micro-organisms can multiply at astonishing rates, and some process simple foods, making them useful laboratory subjects. We have already seen how George Beadle and others worked with *Neurospora* to show that genes affecting nutrition controlled the formation of enzymes. Also in the 1940s, Oswald Avery at the Rockefeller Institute in New York was working with *Streptococcus pneumoniae*, or pneumococcus, a bacterium that came in two forms, rough and smooth.[58] The smooth form was virulent, the rough form not. They could be cultured in medium and could be injected into mice. From work that had been done in a London pathological laboratory by Frederick Griffith in the 1920s it was known that mice injected with live rough bacteria and dead smooth bacteria still perished. What was more, live smooth bacteria could now be found in the mouse blood. Somehow the virulence had been transferred and inherited. Avery asked: what substance was responsible for this transformation? Avery's team worked hard to extract samples of the 'transformative principle', growing pneumococci in vats of broth, spinning them in centrifuges, and treating them with brine and enzymes to remove sugars and most proteins to produce a fibrous substance. They ran test after test to give clues about the substance's chemical nature: everything pointed towards DNA.

In the 1944 paper that summarized the work Avery was cautious. In private he wrote to his brother:

> If we are right, & of course that's not yet proven, then it means that nucleic acids are not merely structurally important but functionally active substances in determining the biochemical activities and specific characteristics of cells – & that by means of a known chemical substance it is possible to induce predictable and hereditary changes in the cells. This is something that has long been the dream of geneticists . . . Sounds like a virus – may be a gene.[59]

Avery was not a geneticist, he was 'by training a physician', 'not exactly a biochemist, but an immunologist and microbiologist'.[60] The phage group of Salvador Luria and Max Delbrück was also interdisciplinary. From 1939, electron microscopes, built in the United States by RCA to imported designs, began to reveal the shape of phages ('Mein Gott!', exclaimed one professor, quoted by Judson. 'They've got tails').[61] The phage group agreed in 1944 to focus attention on seven particular bacteriophages of the human gut bacterium *E. coli*, which they labelled T1 to T7. Electron microscopy showed T2, T4 and T6 to look like little watch-towers, with box heads and spindly legs. In an elegant experiment of 1943, Luria and Delbrück showed that some *E. coli* cultures were resistant to phage while others were not, and concluded that bacterial mutation must have taken place. By the late 1940s and early 1950s the phage group had techniques to work with mutation (phages as tools, *E. coli* as subject) and speculations about the action of phages: 'I've been thinking', one of them wrote, 'that the virus may act like a little hypodermic needle full of transforming principles.'[62]

Yet the phage group remained convinced that protein was the genetic substance. But in 1952, group members Alfred Hershey and Martha Chase used radioisotope tracers – phage protein marked with radioactive sulphur, phage DNA marked with radioactive phosphorus – to show that the T2 phage's 'transforming principle' was the DNA. A young phage group member was reading a letter from Hershey describing this experiment in Oxford – his mentor, Luria, a pre-war Marxist, had had his visa for travel from the United States refused.[63] The young man was James D. Watson.

The great global upheavals of the 1930s and 1940s brought scientists from many disciplines together and into play in the DNA story, albeit in some unexpected ways. One effect, already discussed, had been to disperse the great quantum physicists away from Central Europe. Delbrück had arrived in the United States, where he had hitched up with Luria, another émigré. The journey of Erwin Schrödinger was even more extraordinary. He had left Germany because he despised Nazism and drifted to Ireland, where he lived with his wife and mistress in a *ménage à trois*. In Dublin he wrote a book, with the bold title *What is Life?* (1944), in which he gave a quantum physicist's analysis of how mere molecules might produce the inherited order of life. By analogy with the Morse code, he spoke of a 'code-script' whereby the rearrangement of a molecule might provide 'the miniature code [that] should correspond with a highly complicated and specified plan of development and should somehow contain the means to put it in operation'. Perhaps, he wrote, it might be an 'aperiodic crystal'.[64] The precise influence of Schrödinger's book on molecular biology is a subject of controversy among historians;[65] undoubtedly it was read by physicists and turned some towards biology.

Donald Fleming compares the caution of biologists such as Avery with the ambition and gusto of the physicists turned biologists and concludes that the latter's willingness to strike directly at the molecular centre of what life is came from the intellectual culture of émigré physics.[66] It was transferred, like a T2 injecting its host, from the phage group to Watson. Other key players in the DNA story, notably Francis Crick (who had worked on mines for the Admiralty) and Maurice Wilkins (who took part in the Manhattan Project), were also physicists who had turned to biology. Fleming suggests that two forces were at work: a desire to turn from the sciences of death to the sciences of life and, less grandiose, a movement of 'loners or small-team men who lamented the passing of do-it-yourself physics' in the age of cyclotrons.[67] The phage group also inherited some features of Bohr's Copenhagen, some social (an openness), some intellectual (perhaps specifically the notion of complementarity) and some cultural (not least the lived experience of scientific revolution).

With experiments by the early 1950s pointing towards DNA as the genetic substance, the precise structure of DNA became much more interesting. (However, what we now see as its obvious relevance is an artefact of retrospection; formal genetics was seen at the time as sufficient, not requiring knowledge of the material structure of the gene.) Three groups followed this interest. At King's College London, the skills and endurance of Rosalind Franklin – preparing crystalline samples of DNA and producing sharp photographs of X-ray diffraction patterns – led the way.[68] Franklin's boss was Maurice Wilkins, although she did not see the relationship as such. In California, Linus Pauling, fresh from his triumphant demonstration of the alpha helix form of some proteins, but hounded by McCarthyites, turned his attention to the structure of DNA in the summer of 1951.[69] Finally, the American James Watson, picking up European post-doctoral experience after being brought up in the phage group, shared his enthusiasm for DNA with Francis Crick at the Cavendish Laboratory, Cambridge.

Watson and Crick were convinced that DNA must have a helical structure. Franklin, in her laboratory notebooks, dismissed the idea. Nevertheless, Franklin's X-ray diffraction photographs were the best source of data, and the Cambridge duo, in an underhand manner, sneaked a look at the latest photographs via contact with Wilkins. If Watson's memoir (1968) is to be trusted, then he viewed science as a race in which the prizes went to winners, and sharp practice was fair game.[70] Maurice Wilkins, as he testified in his own memoirs (2003), had a completely opposite view of the nature of science. It had been 'great community spirit and co-operation' in the King's College Laboratory that produced the 'valuable result' of sharp diffraction photographs.[71] Cooperation was the mark, for Wilkins, of productive scientific communities. He recalled:

> Francis and Jim asked me whether I would mind if they started building models again. I found this question horrible. I did not like treating science as a race, and I especially did not like the idea of them racing against me. I was strongly attached to the idea of the scientific community.[72]

For Wilkins, cooperative science meant pooling knowledge, sharing photographs. Franklin's view was that these were her results, for her use. The controversy over credit in the DNA story was therefore at root a divergence over the proper social character of science.

Crick and Watson did indeed start building models again, using the King's College data to constrain their dimensions. They knew DNA contained nucleotide bases: guanine (G), cytosine (C), adenine (A) and thymine (T). By cutting out precise card shapes of the bases and arranging them, they matched G to C and A to T. (A clue to this pairing would soon be recognized in the biochemical finding of Erwin Chargaff of the one-to-one ratio of G to C and A to T in DNA. Crick seems to have known Chargaff's result without immediately dwelling on its significance. Biochemistry and molecular biology were antagonistic and often talked past rather than to each other.) Crick and Watson settled on a double helix structure, with paired bases on the inside. The result was published in *Nature* on 25 April 1953. Articles describing the X-ray diffraction photographs were published alongside. Journalist and seasoned Cavendish publicist Ritchie Calder wrote up the story for the newspapers.

'We wish to suggest a structure for the salt of deoxyribose nucleic acid (D.N.A.)', is how Watson and Crick had opened their short paper. 'This structure has novel features which are of considerable biological interest.'[73] 'It has not escaped our notice', they remarked near the end, 'that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material.' It was a structural explanation of autocatalysis: the double helix could be unzipped and then two copies rebuilt from each half.

The elucidation of the structure of DNA is – now – regarded as one of the stellar achievements in science in the twentieth century. However, despite initial newspaper coverage, its fame has grown in intensity rather than shining brightly since 1953. There are three concentrically packed factors fuelling this. First, the relationship between DNA, as the chemical of genes, and the cells and bodies it affected needed to be worked out, which took time. How, for example, did DNA, the stuff of genetics, relate to proteins, the stuff of bodies? Second, the wider world needed to be interested in that relationship, and indeed in the science of molecular biology. The promise and then delivery of the promise of biotechnology from the 1970s into the twenty-first century would retrospectively brighten the light of Watson and Crick's discovery. Finally, in the process, the relationship between germ plasm and soma, to use tactically useful archaic terms, was redescribed within a powerful new discourse, a way of speaking about a subject that encourages certain ways of thinking while closing others. You may be wondering why the famous story of DNA is in a chapter about Cold War science. The direct politics of the Cold War did shape the story slightly: Linus Pauling, for example, might have seen the King's College photographs had he not been denied permission to travel by anti-communists. But this is relatively uninteresting historical contingency. Rather it is in the redescription of life in terms of 'information discourse',[74] a Cold War language, that interests us here.

Among the readers of Watson and Crick's paper was George Gamow. 'I am a physicist, not a biologist', he wrote to the pair. 'But I am very much excited by your article . . . and think that [it] brings Biology into the group of the "exact" sciences.'[75] In particular, Gamow saw the task of determining how four nucleotides related to the twenty amino acids that in different combinations composed proteins as a cryptanalytical problem; and Gamow knew the right people to have first crack:

> at this time I was consultant in the Navy and I knew some people in this top secret business in the Navy basement who were deciphering and broke the Japanese code and so on. So I talked to the admiral, the head of the Bureau of Ordnance . . . So I told them the problem, gave them the protein things [the twenty amino acids], and they put it in a machine and after two weeks they informed me that there is no solution. Ha![76]

If there was to be no two-week solution, Gamow would have to work harder. He proposed that triplets of nucleotides – such as AGC or TGA – were numerous enough ($4 \times 4 \times 4 = 64$) to cover the twenty amino acids. He wrote papers, controversial among biologists, which presented the problem of DNA-protein specificity in terms of 'information transfer, cryptanalysis, and linguistics'.[77] He gathered like-minded scientists together to tackle what was now seen as the 'coding' problem. His co-worker, Martynas Yčas, a Russian émigré working for the United States Army, defined the coding problem as understanding the 'storage, transfer, and the replication of information'.[78] (Gamow's network, which included Francis Crick, even had a special tie, and they called themselves the 'RNA Tie Club'). With the help of Los Alamos colleague Nicholas Metropolis, Gamow ran Monte Carlo simulations of different coding approaches on the MANIAC computer. This work saw the coding problem as a cryptanalytical one to be tackled with the resources of the Cold War security state; the process was black-boxed, with the input (four letters for nucleotides) and output (twenty amino acids) the only considerations. This theoretical attack on the coding problem was largely unsuccessful, but it did have the effect of rephrasing genetics in terms of information. Most famously, this rephrasing was expressed in 1958 in Francis Crick's 'sequence hypothesis' and 'Central Dogma'.

The former stated that 'The specificity of a piece of nucleic acid is expressed solely by the sequences of its bases, and that this sequence is a (simple) code for the amino acid sequence of a particular protein.' While the Central Dogma stated:

> Once 'information' has passed into protein it cannot get out again. In more detail, the transfer of information from nucleic to nucleic acid, or from nucleic acid to protein may be possible, but transfer from protein to protein, or from protein to nucleic acid is impossible. Information means here the precise determination of sequence, either of bases in the nucleic acid or of the amino acid residues in the protein.[79]

Others – even biochemists – would learn to speak in the same informational language.

In 1960, biochemists at Wendell Stanley's Virus Laboratory at Berkeley, including Heinz Fraenkel-Conrat, succeeded first in reconstituting Tobacco Mosaic Virus (TMV) from its constituent RNA core and protein coat – greeted in the press as the creation of artificial life – but also in establishing the sequence of 158 amino acid residues that made up the protein. (RNA is another nucleic acid; one difference between RNA and DNA is that the thymine (T) in the latter is replaced by uracil (U) in the former.) Stanley described this achievement as 'a Rosetta Stone for the language of life'.[80] With Frederick Sanger's first sequencing of the amino acid residues of a protein, insulin, in the early 1950s, the project had output information to work with.[81] Sanger had emplyed a range of techniques: first, conventional chemical techniques were enlisted to break off amino acids of the ends of the insulin molecule and identify them; second, enzymes were used to break up the protein molecule at specific places; third, chromatography was deployed to spread the pieces out, and then radioactive tracers were used to label and identify the fragments.[82] The result, announced in 1949, was not only knowledge of the sequence of amino acids in the insulin molecule, but also an argument that the sequence of amino acids in proteins was specific.

Scientists in the 1950s began to trace the biochemistry of protein synthesis, opening Gamow's black box. They also spoke the language of information, cybernetics, command and control. The Pasteur Institute in Paris provides a clear example. The institute was home to two laboratories crucial to the growth of post-war molecular biology: Jacques Monod's laboratory of cellular biochemistry and François Jacob's microbiological laboratory within André Lwoff's Department of Bacterial Physiology.[83] Monod and Jacob began to understand genetic control of protein synthesis in cybernetic terms, rewriting the life sciences in terms drawn, as we have seen, originally from the analysis of anti-aircraft operations. Leo Szilard seems to have been the critical link in this chain of influence: he had talked at length about the ideas of Norbert Wiener and Claude Shannon with Monod.

Specifically, Monod and Jacob proposed the idea of 'messenger RNA', short-lived RNA molecules that, in their language, carried information from nucleic acid to protein. Monod had a stock of *E. coli* mutants, some of which could ('lac+') and others could not ('lac–') develop on lactose. Jacob had developed a 'zygotic technology', techniques which shook bacteria apart at different moments of bacterial sexual conjugation. This technique had shown that the genes were transferred from 'male' to 'female' bacterium in sequence, and amounted to a method for mapping the gene. Putting their resources together, and working with visiting Berkeley scientist Arthur B. Pardee, Jacob and Monod argued in the so-called PaJaMa article in the new *Journal of Molecular Biology* that their *E. coli* experiments demonstrated, first, that there must be a chemical messenger from gene to cytoplasm and, second, that a 'repressor' molecule blocked the synthesis of a lactose-eating enzyme. The cybernetic debt is clear in this reminiscence of Jacob's:

> We saw this circuit as made up of two genes: transmitter and receiver of cytoplasmic signal, the repressor. In the absence of the inducer, this circuit blocked the synthesis of galactosidase. Every mutation inactivating one of the genes thus had to result in a constitutive synthesis, much as a transmitter on the ground sends signals to a bomber: 'Do not drop the bombs. Do not drop the bombs'. If the transmitter or the receiver is broken, the plane drops its bombs.[84]

The Pasteur Institute scientists cooperated and competed with Sydney Brenner, Francis Crick and others to identify the chemical messenger. The messenger in *E. coli* was found to be short-lived strands of the nucleic acid RNA. And, as Jacques Monod said, what is true for *E. coli* is true for an elephant.[85] Joint papers announced the discovery in *Nature* in May 1961.

From the late 1950s, the National Institutes of Health had grown and attracted a galaxy of biochemical talent to well-funded laboratories. In 1961, Marshall W. Nirenberg and his post-doc Heinrich Matthaei, working at Bethesda, and inspired by the Paris techniques and the cell-free approach of Paul Zamecnik, used an *E. coli* cell-free system – ultracentrifuged fractions of *E. coli* – to synthesize a protein from a known sequence of nucleotides. The sequence of the synthetic RNA was UUU and the protein was polyphenylalanine. There followed a rush of ingenious biochemical experimentation to discover the rest of the genetic code. The experiments depended on an extraordinary array of techniques to build up and break down nucleic acids, including enzymes to assemble nucleotides into sequences, such as DNA and RNA polymerases or DNA ligase, and enzymes to destroy them again, such as DNAase and RNAase. The scientist who, more than any other, made these chemical tools was Har Gobind Khorana, an Indian émigré biochemist working at the University of Wisconsin.

The period of the completion of the code, from Nirenberg and Matthaei's announcement of 1961 to 1967, witnessed a flourishing of code talk, now packaged for popular consumption. Carl Woese wrote in *The Genetic Code* (1967) of 'informational molecules' and DNA and RNA as being like 'tapes' and 'tape readers'; Robert Sinsheimer, in his *The Book of Life* (1967), described human chromosomes as being 'the book of life' containing 'instructions, in a curious and wonderful code, for making a human being'; this was the informational discourse, devised in the early Cold War in Los Alamos, CalTech, Cambridge and Paris, now becoming a world language.[86] And if life could be read, written or copied, it could be re-edited. In this way molecular biology's rephrasing of biological specificity as information invited the imagination of the biotechnological industries of the late twentieth century.

The cracking of the genetic code was an achievement of international networks of scientists, with centres in California, Cambridge and Paris. It was the crowning achievement of this second wave of molecular biology. On a theoretical axis, molecular biologists now held that genes were made of DNA, and that DNA encoded information that determined replication and protein synthesis; the cellular machinery was described in detail, along with the biochemical pathways for replication, expression, and so on; enzymes, the protein catalysts, were isolated and related to sequences.[87] On a technical axis, new methods of manipulating DNA were devised. By 1970 Har Gobind Khorana was able to achieve the first complete chemical synthesis of a gene.

Global science (1): Plate tectonics

Another revolution of the 1950s and 1960s, the acceptance of the theory of plate tectonics in geophysics, was also the achievement of a dispersed network of scientists. It, too, was framed by the Cold War.[88] Furthermore, the data gathering justified by the Cold War contributed to a global science that would be essential to the later recognition of global climate change. I finish this chapter with a brief examination of these last Cold War sciences.

I have discussed in an earlier chapter the chilly reception of Alfred Wegener's theory of continental drift from the 1910s. After the Second World War, the context for theorizing about large-scale movements of the earth's crust was transformed, first, by new sources of evidence, paid for largely by military patrons, and, second, a disciplinary realignment in which geophysics gained in credit compared to field-based geology, largely on the back of physics' prestige after 1945.[89] An early source of new geophysical evidence was studies of land-based palaeomagnetism: the mapping of the orientation of magnetic fields frozen in rocks of different ages.[90] Patrick Blackett's group, which moved from Manchester to Imperial College, London, was active here, as was ex-radar scientist Keith Runcorn's group, which moved from Cambridge to Newcastle. Palaeomagnetic research suggested that the magnetic poles of the earth had wandered, and even reversed. Untangling the meaning of this information was challenging, but within it would be clues to past positions of the earth's rocks.

But it was from oceanography that the crucial data would be generated. Detailed knowledge of the contours and consistency of the ocean floor was essential to submarines carrying the nuclear deterrent. But the military–geophysics relationship, write historians Naomi Oreskes and Ronald Doel, 'was not simply one of increased practical application, but of vastly increased funding for geophysical and geochemical work, which spawned a greatly expanded institutional base and largely determined the priorities of the discipline.'[91] Bodies such as the Office of Naval Research therefore poured resources into oceanography, supporting institutions such as the Lamont–Doherty Geological Observatory of Columbia University, as well as research at Woods Hole on the east coast and the Scripps Institution on the west. These oceanographical investigations traced a worldwide network of oceanic ridges, for which explanations based on the presumption of both fixed and mobile crust were offered.[92] Mobile crust theories made two testable predictions. The first, independently suggested by Fred Vine and Drummond Matthews, was of the existence of alternating magnetic polarizations of rock in bands parallel to the ridge, caused when the crust spread out under conditions of reversing north and south poles. The second, by J. Tuzo Wilson, was of transform faults detectable by seismology.[93] There had been a massive increase in global seismological recording in the 1960s, including the establishment of the World-Wide Standardized

Seismograph Network, as part of the Cold War requirement to detect atomic tests.[94] These data could locate tremors theorized as being caused by moving crust.

When these predictions were borne out by oceanographic sampling of the magnetic polarity of the sea floor near ridges and by earthquake measurements, the idea of mobile components of the earth's crust gained considerable ground. It was worked up into a fully fledged theory of 'plate tectonics' – which asked how moving crust would behave on a spherical earth – by Jason Morgan at Princeton and Dan MacKenzie at Cambridge, both geophysicists. Plate tectonics provided a model of the earth of giant slow convection currents in the mantle pushing the plates into, under and over each other, driving mountain-building, causing earthquakes and, over the millennia, changing the face of our planet.

The success of plate tectonics in making such diverse knowledge 'cohere'[95] further boosted the prestige of geophysics over traditional geology. Subjects such as mineralogy, historical geology and paleontology were now squeezed out of geology curricula to make room for more physics-inspired methods teaching.[96] But also since the 1960s, there has been a shift from geology to 'earth science', as comparative data came in from missions to the moon and bodies in the rest of the solar system. 'Earth science', writes historian Mott Greene, is 'best seen as a subdivision of planetology'.[97]

Global science (2): Global warming

One of the most important effects of the Cold War for twentieth-century science (and beyond) was to encourage the globalization of data recording. We have seen examples of this in oceanography and seismology. Another can be found in the atmospheric and oceanographic recordings of past and current climate change.

The idea of a greenhouse effect, the trapping of warmth by the blanket of the earth's atmosphere, goes back at least to John Tyndall in the nineteenth century. The Swedish chemist Svante Arrhenius had made calculations of the expected warming of the earth due to added carbon dioxide, from industry, in 1896. He had been sanguine about the results:

> We often hear lamentations that the coal stored up in the earth is wasted by the present generation without any thought of the future . . . [However, by] the influence of the increasing percentage of carbonic

> acid in the atmosphere, we may hope to enjoy ages with more equable and better climates, especially as regards the colder regions of the earth, ages when the earth will bring forth much more abundant crops than at present, for the benefit of a rapidly propagating mankind.[98]

In 1938, a professional steam engineer and an amateur meteorologist, Guy Stewart Callendar, had read a paper to the Royal Meteorological Society arguing that added carbon dioxide had caused a 'modest but measurable increase in the earth's temperature'. The following year he warned of humanity conducting a 'grand experiment', and he continued to publish on the linkage until the early 1960s. Historians Mark Handel and James Risbey suggest Callendar was ignored because of the distraction of the Second World War, Spencer Weart suggests he was ignored because he was an amateur, while James Fleming, citing references to Callendar's work by professional meteorologists, argues that he was not ignored at all.[99]

Certainly what would count as evidence for global warming required a plausible claim to global coverage. This was where the Cold War was important. Consider four dimensions. First, the Cold War encouraged sophisticated, mathematical meteorology such as that pursued by Carl-Gustav Rossby's group at Chicago or the use of computers to calculate future weather patterns at the Institute of Advanced Study at Princeton. Partly this was a continuation of the historical patronage of meteorology by the military. The outcome was a concentration of computer resources, funded through bodies such as the Office of Naval Research, on weather modelling and prediction. General Circulation Models (GCMs, usually rendered now as Global Climate Models), computer calculations of changing climate, were first attempted in the mid-1950s by Norman Phillips at Princeton.[100]

Second, the question of absorption of infrared radiation in the atmosphere had direct relevance to defence research. Heat-seeking missiles or other projects to track missiles by their exhaust temperatures required an understanding of how infrared travelled and was absorbed in the air. Gilbert Plass, at the Lockheed Corporation, working with Johns Hopkins University on Office of Naval Research funds, did such missile research by day, and by night, using the same data, made calculations of the global warming caused by human activity: 1.1° per century was the estimate Plass gave in 1956.[101]

Third, the Office of Naval Research and the Atomic Energy Commission supported Roger Revelle's oceanographic research at the Scripps Institution into how fast the earth's oceans turned over (this had implications for the spread of nuclear fallout). Revelle calculated that the slowly circulating oceans could not absorb the carbon dioxide pumped into the atmosphere; the gas would linger in the air. 'Humans are carrying out a large scale geophysical experiment', he wrote, echoing Callendar, but speaking from a much more powerful position, 'of a kind that could not have happened in the past nor be reproduced in the future'.[102]

Fourth, the International Geophysical Year may be associated with the Russian coup of Sputnik, but it was primarily a programme of interdisciplinary Antarctic and atmospheric research. This included work started in 1956 at the Little America base, Antarctica, to record baseline levels of atmospheric carbon dioxide far away from specific sources of the gas. Charles David Keeling, at the end of the International Geophysical Year, moved his measuring equipment to another almost pristine location: the summit of Mauna Loa on the Hawaiian Big Island. Keeling's work was first funded through the IGY, then by the Atomic Energy Commission, and then, after a short hiatus, by the National Science Foundation. Drawn as a (now famous) graph, Keeling's data showed the clear and steady rise of levels of carbon dioxide in the atmosphere.

'Without the Cold War there would have been little funding for the research that turned out to illuminate the CO_2 greenhouse effect, a subject nobody had connected with practical affairs', writes historian Spencer Weart. 'The U.S. Navy had bought an answer to a question it had never thought to ask.'[103] I would go further: it took a global conflict to create the particular conditions to identify global warming as a global phenomenon.