the world studying it by various methods. Progress is slow, but already there are indications that connection between memory storage and genetically coded proteins is being established. Every year new discoveries of peptides active in the nervous system are described. A scotophobin-like peptide may exist, but it yet remains to be clarified what are its true role and function. On this tortuous road to truth there are always those who stumble and are left behind.

A last example concludes this chapter on error and self-deception. In 1972 Alessandro Bozzini of the Centre for Nuclear Energy in Italy found that some strains of wheat contained protein at a very high concentration. These strains lacked a piece on the short arm of the chromosome 2A. Bozzini interpreted this finding with a hypothesis that the lacking piece was the seat of a gene coding for an inhibitor of protein synthesis in the seeds of wheat. In the absence of this gene the protein synthesizing machinery is not inhibited and more protein is produced than in plants with an intact chromosome 2A.

Bozzini's findings and interpretation stimulated a burst of research activity supported by the International Atomic Energy Commission – since ionizing radiation could induce similar mutations in wheat as those observed by Bozzini. Within a few years, however, it became apparent that Bozzini's result had been due to smaller sized seeds in the strain lacking the piece of the chromosome. Production of the same amount of protein in a seed of smaller volume results in higher concentration (amount/volume) of protein. We encounter in this case a *bona fide* error in the interpretation of results.

4

In the Shadow of Doubt

We highly revere the famous scientists who were responsible for the progress of science through the ages. Among them, however, we find a small but significant number who may have 'cut corners'; these scientists seem to have presented evidence because their intuition told them something was true. I shall examine here the doubts that have arisen concerning the work of Ptolemy, Newton and Mendel.

PTOLEMY UNDER SCRUTINY

At the beginning of the nineteenth century the French astronomer Delambre accused Claudius Ptolemy, the famous astronomer of ancient times, of not actually having observed the celestial positions at around AD 135, positions which Ptolemy describes in his book *Almagest*. Delambre's accusation was later supported by an American astronomer, R. R. Newton, of Johns Hopkins University. Delambre and Newton claimed that Ptolemy's alleged observations of the equinox in Alexandria were merely extrapolations from data of Hipparchus (who some 200 years earlier had discovered the Earth precession). They thus attributed Ptolemy's work to forgery.

More recently this view was repudiated by Neugebauer[79] in his book on the history of ancient astronomy. In Neugebauer's view, Ptolemy was the first astronomer to note the constancy of the tropical year, that is the time required for the sun to return to the same position in respect to the equator. Neugebauer, as well as Owen Gingerich,[80] think it unlikely that Ptolemy's success rested on fabricated observations, but that 'in those ancient days, before error theory was understood, selected observations were adjusted for pedagogic purposes and recorded in *Almagest* in close agreement to theory . . .'.

The difficulties in obtaining reliable data in the time of Ptolemy were enormous, and it is only to the credit of the ancient astronomers that they succeeded in building a theoretic structure of astronomy. The cinematic theories of *Almagest*, backed up by better observational techniques, were essential to Newton's celestial mechanisms.

DID NEWTON FUDGE HIS DATA?

Newton, one of the fathers of modern science, continued the search for mathematical simplicity in physical phenomena, a search that began with Copernicus, Kepler and Galileo. In book III of his *Philosophiae Naturalis Principia Mathematica* published in 1697, Newton attempted to reduce the context of his philosophical principles of the System of the World to popular mathematical treatment that would be understood by those who had mastered the first two books. Till then the Pythagorean tradition represented by Kepler and Galileo was based on geometrical description of natural phenomena (movement of planets and of falling bodies), while the mechanical tradition, represented by Descartes, sought mechanical causes of these phenomena. It was Newton's genius that united these two concepts by first bringing into science the concept of action as force at a distance.

Kepler's law of planetary motion did not take into account the effects of Earth, and Galileo's calculations of the movement of planets were only ideal representations of real events. Newton was the first to take into account in his *Principia* the perturbations which differed from the ideal concepts on which the science of the time was based. He demonstrated that these perturbations could be dealt with mathematically, and he showed that theoretical calculations based on his principles would lead to results that were in accord with the experimentally measured data. Unfortunately, in striving to demonstrate how well the material events corresponded to the mathematical calculations, Newton presented his data with a degree of precision that was practically impossible to obtain in his time.

On the basis of his principles, Newton calculated the correlation between the distance to the moon and the gravity constant, the velocity of sound and the precession of the equinox.

The law of universal gravitation was derived from the correlation between the acceleration of falling bodies due to gravity on Earth, and the acceleration of the moon which moves around the Earth as if it were trying to fall. From Kepler's data Newton knew that such centripetal attraction of a planet to the sun inversely varied with the square of the distance from the sun. Newton concluded that the attraction that held the moon in its orbit around the Earth was correlated with gravity as measured by the acceleration of falling objects on Earth. He thus took into consideration the sun's effects on the moon's orbit, the moon's centripetal acceleration, the ovoid shape of the Earth (affecting the rotation of Earth around its own axis) and came to the mathematical conclusion that ½ g should equal 15 feet, 1 inch and 1.5 lines. The precision of this mathematically derived number was better that 1 part in 30,000; this precision exceeds that which was feasible in Newton's times, when, for instance, Boyle's measurements, to confirm his law were accurate to within only 1 in 100.

Newton's derivation of the velocity of sound was an achievement of a genius. Newton considered the length of a wave in water (the distance from crest to crest) and its velocity of propagation. He then proceeded to calculate how sound would behave if it were indeed a wave. He needed to know the ratio of the density of air to that of water, a value which was not known. He assumed it to be 1:850. Using Boyle's law to translate the data for density of air and of compression waves in the air, he then calculated that the velocity of sound should be 979 feet per second. In order to confirm this calculation Newton performed the famous experiment in Neville's Court in Trinity College, Cambridge, in which he measured the time of return of an echo. He used pendulums of different lengths to measure the time of sending out the sound and its return to the point of origin. His measurements provided him with a range of values of 920–1085 feet per second. Thus, his calculated value of 979 feet per second was well within that range. This value, however, was about 20 per cent lower than the experimentally derived estimates of the velocity of sound as measured in 1708 by Derham, as well as by Halley and Flamsteed of the Royal Observatory. Having learned these experimental values, Newton decided to make a further assumption that would take care of the 20 per cent discrepancy between the experimental data and his calculation. He assumed that air waves did not involve geometrical points, but real air particles (he called that 'crassitude' of air); next he assumed that the air contained water vapour at the ratio of 1:10. When these assumptions were introduced into Newton's formulae, each contributed to a 10 per cent increase and thus the now calculated velocity of sound was 1142 feet per second, a figure which coincided with the experimentally measured number to within one part in a thousand.

How did Newton arrive at the numerical values for his assumptions? He had no factual basis for them; he did not have data to show that air contained 10 per cent water vapour; he could not have known that particles of air were solid. These were unknown parameters. Though Westfall[81] bluntly states that making these assumptions was 'nothing short of deliberate fraud', my opinion is that making mathematical approximations in an intractable problem is actually the best that can be done to show that a theory is feasible at all.

Nevertheless, other questions remain. When one examines the three editions of Newton's *Principia* (published in 1867, 1713 and 1726, respectively) one notices that the corrections Newton made in his calculations were made *a posteriori*, that is, he knew what the result should have been, and then he adjusted his data until they fitted his predictions. So for instance, in his calculation of the velocity of sound he corrected the air:water density from 1:850 in the first edition of his book to 1:870 in the second. Similarly, Newton's calculation of the distance from Earth to the moon were based on the known values for the gravity constant *g*. Newton first used the data of Copernicus, Vendelin and Tycho for the distance of the moon, being 60.5 radii of earth. From this the value of ½ *g* came out to be 15 feet and 1 inch, as indeed measured by Huygens. As a matter of fact the distance to the moon was not known, so the choice of the value 60.5 fitted well the *g* value as determined by Huygens.

The third example of manipulating the data is found in Newton's calculations on Earth's precession of equinoxes, which was known to be about 50 seconds. Westfall[81] had this to say about them:

> In the case of precession . . . the correction of a faulty lemma in edition one, imposed the necessity of adjustment of more than 50 per cent in the remaining numbers. Without even pretending that he had new data, Newton brazenly manipulated the old figures on precession so that he not only covered the apparent discrepancy but carried the demonstration to a higher plane of accuracy. (p. 755)

Newton's manuscript for the second edition of *Principia* was edited by Roger Cotes. In his correspondence with Cotes about corrections Newton wrote (February 1719): 'If you can mend the numbers so as to make y^{e} [archaic 'yours'] precession of the Equinox 50″ or 51″,

it is sufficient.' Cotes made the necessary corrections and wrote to Newton: 'I am very glad to see the whole so perfectly well stated and fairly stated for without regard to the conclusion I think y^{e} distance of 18.5 degrees ought to be taken & is much better than 17.5 or 15.25 & the same may be said of y^{e} other changes in y^{e} principles from which the conclusion is inferr'd.' Thus in the second edition of Newton's book the results on precession become accurate to within 1 part in 3000.

But surely there is nothing wrong in trying to bring theory and experiment into agreement, provided that the hypothesis is further tested by experimentation, and not presented as the last word. The process of reasoning backwards from an experimental result to correct a detail in a theoretical model need not be considered dishonest.

The following story related by A. H. Boultree[82] illustrates a similar situation this century. Boultree quotes J. C. McLennan, who said during a lecture at the University of Toronto that he had once remarked to Nils Bohr how wonderful it was that his (Bohr's) equation yielded very accurate values of Rydberg's constant (a physical constant related to atomic spectra). Bohr replied: 'Of course, McLennan, I made it come out this way'.

An interesting footnote concerns Newton's jealousy of his work and of priority of his findings.[83] Newton wrote on 16 April, 1676 a letter to the secretary of the Royal Society, warning him that Boyle's invention published in the *Philosophical Transaction* of Royal Society in February of that year could do great social harm. What was this harmful invention? Boyle had noted that heat was produced when mercury was mixed with gold dust. Newton warned the Royal Society of unnamed dangers to the society if Boyle's reaction became known to the uninitiated. It is known at that time Newton himself had been carrying out alchemical experiments, and one may therefore suspect that Newton was afraid Boyle might 'steal' his own priority in discovering the Philosopher's Stone to transmute metals, and therefore used his authority to stop the dissemination of Boyle's findings.[83]

MENDEL - WHO COUNTED THE PEAS?

It took some 40 years for Gregory Mendel, the monk who, in 1865, published a paper on inheritance in the garden pea to become recognized as the father of modern genetics. His revolutionary paper was published in Brno in Bohemia (now Czechoslovakia) and was available at that time at the Linnean Society and the Royal Society in London. Nevertheless, it was largely ignored and rediscovered only in 1900.[84]

The scientists who unearthed Mendel's paper were two European botanists, students of heredity – Corgens in Germany and Tchermak in Austria. They understood that Mendel's discovery was applicable not only to plants, but also to animals and man. The essence of Mendel's discovery was that the units of heredity, later named genes, were transmitted from generation to generation unchanged, and that various combinations of these units were being reshuffled in each generation. There is no question that Mendel's contribution is one of the outstanding advances in the history of biology.

How well did experimental findings support his theory of independent inheritance of traits? Sir Ronald A. Fisher, known worldwide as an authority in biological statistics, examined Mendel's writings in detail.[85] He expressed the opinion that Mendel's talent lay in recognizing the purely mathematical and combinative properties of sets of genetic properties as expressed by two or three factors inherited independently of each other. The independence Mendel was concerned to demonstrate, according to Fisher, was 'closer to a logical than a statistical independence'. It seems that at the time of rediscovery of Mendel's writings the European botanists did not pay much attention to the actual numbers collected in Mendel's experiment, but only to the principles which Mendel derived from these numbers.

In 1936 Sir Ronald Fisher undertook a reconstruction of Mendel's data and statements as to find whether his postulates were indeed plausible. Fisher came to an amazing conclusion: though Mendel's report was to be taken literally, and his experiments could be reproduced just as he described them, some of the figures were inexplicable. The observed results were simply too good! In one of the series of experiments (a second generation of peas that was bred from hybrids or crosses) Mendel tabulated properties such as round or wrinkled seeds and yellow or green colour of their endosperm. Where Mendel expected the ratios of 2:1, the actual experimentally found ratios came out as 1.93:1 and 2.1:1. According to the laws of statistics, this agreement between the predicted ratios and those found experimentally was too good to be true. In another experiment Mendel counted 600 pea progenies. In this case the expected ratio between their traits was 3:1, that is, he should have expected 200 non-segregating plants. The actual counted number was 201,

again, too close to the predicted to obey the laws of chance. Fisher therefore concluded:

> An examination of the general level of agreement between Mendel's expectations and his reported results show that it is closer than would be expected in the best of several thousand repetitions. The data have . . . evidently been sophisticated systematically and I have no doubt that Mendel was deceived by a gardening assistant, who knew too well what his principal expected from each trial made . . . This possibility is supported by independent evidence that the data of most, if not all, of the experiments have been falsified so as to agree closely with Mendel's expectations.[85]

Fisher's conclusion indicates that prior to his reported experiments, Mendel was well aware of the independent inheritance of the seven characteristics he studied in peas. Mendel looked upon the numerical frequency ratios as a method of demonstrating the truth of his factorial predictions. The precision with which this system worked made it clear to him what to expect, and how to design an experiment so as to demonstrate the correctness of his hypotheses. The moral Fisher draws from Mendel's contribution to the history of biological thought is that all original papers purporting to establish new facts should be very carefully and meticulously examined.

Fisher's conclusions that Mendel's results were too good to be true were based on a known statistical test of the correlation between the observed frequency of traits and the theoretically expected values. Fisher charitably suggested that Joseph Marsh, Mendel's gardener, may have been the culprit; knowing what to expect, he might have counted the samples accordingly. This suggestion was supported by Sturtevant,[86], Orel[87] and Iltis.[88] There were others who insinuated that Mendel did not count *all* of his samples but stopped when he reached numbers indicating the ratio that fitted the theory.[89] Roberts[90] refuted this suggestion by showing that the number of peas Mendel counted from an indicator number of hybrid plants corresponded well with the predicted yield of such plants. Gardner,[91] Zirkle[92] and Campbell[93] interpreted Mendel's results as an outcome of what we defined in chapter 2 as experimenter bias, i.e. that Mendel saw what he wanted to see.

Discounting these last allegations, one is left with Fisher's analysis. The problem with the statistical test (the chi-square test) is that one member of the equation used is the observed trait. These traits, in order to be evaluated numerically, have to fall into discrete groups of individuals and each individual (object) has to be assigned into one such discrete group. In mathematics there exist groups known as 'fuzzy sets'. Fuzzy sets also exist in biology, when the experimenter cannot really decide whether an individual clearly belongs say to set (a) or to set (b).

Pearl[94] tested the effect of fuzzy sets on results of experiments in genetics. In 1911 he performed a crossing experiment between two varieties of maize, one yellow-starchy and the other white-sweet. In the F_1 generation he expected to obtain a distribution of nine yellow-starchy, three yellow-sweet, three white-starchy and one white-sweet. He let 15 trained scientists assign the progenies to one of these groups, each scientist counting some 512 kernels. The categorization of any particular kernel into a group had to be a result of a subjective decision. No two experimenters reported the same numbers in the respective groups. One can therefore argue that in Mendel's experiment no one but Mendel would have reported exactly the same numbers.

When an investigator encounters objects that are difficult to classify, he can either classify them into an 'indeterminate group' and decide later what to do with this group or he can ignore these individuals. In the first case the numbers in the indeterminate group can then be reclassified and assigned into one of the predetermined sets (ideal categories). It is quite clear that either of these procedures would lead to discrepancies between one observer and another.

Root-Bernstein[95] has investigated the problem further by employing undergraduate students (untrained personnel) to count various types of crosses of maize. The second procedure (when the indeterminate individuals are not counted) gave ratios closer to the ideal expected result. When the first procedure was employed (at the end of the counting the indefinites were reassigned into one of the predicted groups), the results were statistically as unacceptable as those of Mendel. Root-Bernstein therefore believes that Mendel's peas represented a fuzzy set of data which required subjective analysis to be fitted into a set of discrete categories: 'There is subjectivity in the process of inventing categories comprehending nature and there is subjectivity in the process of assigning objects to these categories.'

Mendel himself stated: 'Where one is dealing in a general way with degrees of similarity, account must be taken not only of the traits that stand out sharply, but also of those that are often too difficult to put in words' (quoted by Stern and Sherwood).[96] In another paper Mendel wrote 'seeds damaged during their development by

insects often vary in colour and shape; with a little practice in sorting, however, mistakes are easy to avoid'.

One cannot easily reconcile the fuzziness of biological reality with discrete statistical ideality. Thus, though Fisher may have been correct in his statistical criticism of Mendel, the results of the experiments by Pearl and Root-Bernstein support the notion that Mendel was a very careful experimenter and observer, and knew how to classify his material. It seems to me that all insinuations about Mendel's possible unethical behaviour should be discounted.

Fisher's suggestion that Mendel's gardening assistant might have been responsible for the over-accurate count of peas because he knew what Mendel hoped for reminds me of other cases of fraud out of respect and sympathy, which we may pause for a moment to look at here.

When I was working on my PhD thesis in the Department of Microbiology at the Harvard Medical School, a story was told to me by a Professor Eaton about Dr George O.Gey, the inventor of a new method for growing animal cells in tubes inserted in a rotating drum, a method that ensured better aeration of cell cultures.[97] Gey had claimed at that time that he could grow animal cells *in vitro* without serum. In the 1940s this was a very daring claim, because until then nobody had succeeded in growing cells taken directly from animal embryos without supplementing the media (containing salts, amino acids and vitamins) with animal serum, which provided some unknown ingredients required for good growth.

Eaton tried to replicate Gey's technique, but was not successful. He therefore invited Gey to come to Harvard to demonstrate his technique there. Gey arrived, accompanied by his faithful technician who had been working with him for many years. When experiments performed by Gey and his technician worked, while others done by Eaton did not, Eaton, knowing Gey's integrity, began to suspect that the technician was doing something that was helping the cultures along. Eaton therefore locked the incubator with the newly set up cultures every evening and had it opened the next day in his presence. As soon as this precaution was taken, no further experiments succeeded. It became apparent that the technician had helped the cultures by stealthily adding serum at night. The reason for his doing so, as explained by Eaton, was that he greatly respected and admired his boss and knew that Gey was enthusiastic when the experiments worked well, and rather despondent when they failed.

He believed that some outside intervention on behalf of the incubated cells was worthwhile to keep Gey happy.

A similar story was related to me by Professor Albert Sabin, the discoverer of the polio vaccine which bears his name. When he was a guest scientist at the Rockefeller Institute in 1935–7, the director of the Institute, Simon Flexner, consulted Sabin about a manuscript submitted for publication in the *Journal of Experimental Medicine* (the organ of the Rockefeller Institute). The article was written by Klaus Jungenblut, a bacteriologist from Columbia University. In it, Jungenblut stated that monkeys infected with poliovirus would not develop paralysis if, soon after inoculation of the virus, the animals were treated with a dose of vitamin C.[98,99] Flexner wondered whether such a result was plausible, and asked Sabin, already an expert on polio infection in monkeys, to consult with Jungenblut about his experiments. Sabin approached Jungenblut and both agreed to perform a joint experiment on some 40 monkeys, some treated with vitamin C and some left as controls, all, of course, having been first inoculated with a paralytic dose of poliovirus. Sabin insisted that he personally inoculated the monkeys with the virus.

The results clearly demonstrated that vitamin C, administered at, or after poliovirus inoculation, would not prevent the development of paralysis in 39 out of 40 monkeys. Sabin was thus convinced that vitamin C had no miraculous properties and that it was worthless as a therapeutic or preventive drug in poliovirus infection. Sabin published these results in the *Journal of Experimental Medicine*,[100] where, in a glorious understatement, he simply said: 'There is no apparent explanation to the difference between these results and those reported earlier by Jungenblut.'

Sabin later discovered that Jungenblut was an innocent victim of his technician's ruse. The technician, who had been working with Jungenblut for 25 years, knew exactly what his boss was expecting, and he helped him get the results he wanted by injecting an innocuous solution instead of virus into the animals that were about to receive the vitamin C treatment.

In the same category of fraud out of respect is this story, told by Professor Kihara[101] in his lecture on Lysenko (see chapter 5). An amateur chemist in Japan, fictitiously named 'Ito', claimed he could transform any matter into silver by incineration. He reported this discovery to a professor of chemistry who knew Ito well and believed him to be an upright and honest man. When, however, he permitted Ito to carry out the experiments under strict control, he discovered that the silver had been put beforehand into the substance to be incinerated by Ito's faithful servant, who wanted to make his master happy.

Perhaps also Blondlot's discovery of N-rays (chapter 3) that ended in the discredit of that scientist, was 'engineered' by his faithful assistant.[30]

WHY DID KAMMERER COMMIT SUICIDE?

In the early years after the First World War a Viennese biologist, Paul Kammerer, devoted his studies to the Lamarckian theory that characteristics acquired by parents during their lives can be passed on to their offspring.[102-4] An example of such an inheritance would be the acquisition of the skill to fly an aeroplane by the children of a pilot, or the development of strong biceps by the descendants of a blacksmith. In 1909 Kammerer performed experiments with a black *viviparous* salamander and a yellow-spotted *oviparous* salamander, and claimed that he could make each of these acquire the characteristics of the other. When he kept young yellow-spotted salamanders on black background they tended to lose their yellow markings. Their offspring, when held in black surroundings, were mostly black except for a row of yellow spots along the middle of their backs. When these offspring were kept in yellow surroundings, however, the yellow spots on the back fused into a single line. Such striped forms also exist in nature. A crossing of a naturally spotted salamander with a striped one produced offspring according to Mendelian segregation, but crossing of naturally spotted salamanders with experimentally striped ones did not follow the Mendelian rules.

Next, Kammerer transplanted ovaries of naturally spotted salamanders into the reproductive organs of naturally striped ones. After such a transplantation the characteristics of the offspring depended on those of the true mother. Progeny of spotted ovaries, however, resulting from transplantation into artificially striped salamanders, bore the spot or stripe property of the father. These experiments on ovarian transplantation led Kammerer to consider the possibility of inheritance of characteristics acquired through cells other than the germ cells.[102]

Another of Kammerer's experimental animals was the midwife toad, *Alytes obstetricans*, which differs from other toads by mating on land, rather than in the water. In order to be able to hold on to the slippery skin of the females, the males of those toads that mate in water develop thickened and horny swellings on their thumbs and palms. These 'nuptial pads' are pigmented. The midwife toad, which breeds on land, does not possess these nuptial pads during the mating season. Kammerer forced his midwife toads to breed in water by denying them access to dry ground; after several generations he observed that the males developed nuptial, pigmented pads. Kammerer claimed that this new characteristic was then transmitted to their male progeny.

This claim was hotly disputed by Mendelian scientists, and a prolonged controversy ensued, lasting for several years. One of the protagonists was William Bateson, who objected to Kammerer's experiments and intrepretation in several articles and letters in *Nature*.[105-7] Kammerer himself, [103] supported by another scientist, McBride,[108] defended the Kammerer viewpoint. Bateson had no opportunity to examine Kammerer's preserved specimens of the modified *Alytes* for reasons claimed by some to have been the fault of Kammerer, by others of Bateson. Eventually, an American herpetologist, G. K. Noble, arrived in Vienna in 1926 to inspect the preserved specimen of Kammerer's toad. Noble[109] found that the coloration on the thumb (nuptial pad) was due to the presence of Indian ink injected into the area. He published his findings, which were supported by Przibram.[110] Following that exposure, Kammerer admitted the fraud in a letter to the Soviet Academy of Science in Moscow dated 22 September 1926. He stated that he was personally innocent of the falsification, and that he did not know the identity of the person who was responsible.[111] A few weeks after the publication of Noble's findings, Kammerer shot and killed himself.

The obvious question is: did Kammerer himself inject the ink in order to support his Lamarckian claim, or was that done by one of his collaborators?

Arthur Koestler, in his book *The Case of the Midwife Toad*,[112] expressed his belief that the forgery had been perpetrated by an enemy of Kammerer at some time after 1923, when Kammerer demonstrated this specimen in England at a lecture. Koestler thought, though there is no direct evidence for this, that Kammerer had indeed observed genuine nuptial pads in midwife toads that were forced to breed in water.

The Kammerer affair did not end with the exposure of fraud and Kammerer's suicide. In spite of Kammerer's own admission of the falsification and the evidence provided by Noble, Kammerer's

claims and findings were used in the Soviet Union to support a Lamarckist ideology during the Lysenko period (chapter 5). At the notorious meeting of the Lenin Academy of Agricultural Science in the summer of 1948, the academician N. G. Belensky (as quoted by Zirkle)[13] described Kammerer's work on salamanders as evidence for the inheritance of acquired characteristics, never mentioning that Kammerer admitted fraud not only with regard to the specimen of *Alytes*, but also with regard to the salamanders. (Kammerer wrote in his letter that there were other objects ((black salamanders)) upon which his results had plainly been 'improved' with Indian ink). Even, as late as 1953 Western scientists like Fothergill[113] and Mason[114] described Kammerer's data on salamanders as proofs of adaptation to environmental changes. (Fothergill's opinion may be disregarded since he was a very odd mycologist, who believed that Eve was Adam's daughter!) Their opinion was probably formed in good faith and it is clear that in accepting Kammerer's discredited experiments as genuine, official doctrine on Soviet biology was influenced with regard to Lamarckian inheritance. Following the publication by J. Segal of a book on Michurin and Lysenko in 1951[115] the reviewers wrote that Segal was right in criticizing geneticists for making no effort to repeat Kammerer's experiments, but also that the author had failed to mention many investigators who had attempted to repeat the experiments of Lysenko and his followers with negative results.

Kammerer was an unconventional personality, with artistic inclinations, and great verbal skill; he was a flamboyant but dedicated worker. (This dedication to the science of reptiles was such that he named his daughter Lacerta, a lizard!) Koestler, in analysing the course of events while Kammerer was battling against the Mendelians, as represented by Bateson, tried to put the blame on Bateson for allegedly refusing to examine Kammerer's specimen when he had the chance, and for using his authority to crush Kammerer's experimental evidence.

Were Kammerer's Lamarckian views really unorthodox? In the scientific literature of that time the belief in inheritance of acquired characteristics was quite widely spread.[116] Koestler[117] himself entertained a sort of modern evolutionary theory which clashed with the new-Darwinian view: 'Neo-Darwinism does indeed carry the nineteenth century brand of materialism to its extreme limits – to the proverbial monkey at the typewriter hitting by pure chance on the proper keys to produce a Shakespeare sonnet'. Let us assume, for the sake of argument, that Kammerer had indeed successfully induced the production of nuptial pads in his male toads. Does this constitute a case of Lamarckian inheritance? It may well be possible that the gene(s) for the production of these pads, which certainly exist in this species, are inactive in the land-breeding midwife toad, but that under specific environmental stimuli they become expressed. Kammerer's experimental procedure, as described by him, was to take many hundreds of eggs from female toads and to keep them in an aqueous environment. Only a few per cent survived. He then repeated the same procedure for several generations, losing, of course, the majority of individuals in the process. He was therefore applying a strict selective pressure on the developing eggs so as to select those genetic factors that would permit the eggs to develop into adult animals in water. It is thus possible that one of the traits so selected, by keeping the toads constantly in water, was the development of a property particularly adapted to an aqueous environment – namely the production of nuptial pads (Waddington's 'genetic assimilation').

In order to be able to state that the Lamarckian rule operated in the case of the midwife toad, *all* the eggs should have survived in water in the first, as well as in the following generations, and the nuptial pads should have evolved under these conditions and remained hereditarily fixed.

It is indeed unfortunate that an inconclusive experiment, which in any case would not have proved the point Kammerer was trying to make, should have been rigged, and should have led to Kammerer's tragic death and the discrediting of his integrity as a scientist.

CARREL'S IMMORTAL CELLS

At the beginning of this century Alexis Carrel, a surgeon and biologist at the Rockefeller Institute in New York, greatly intrigued biologists by developing a technique to grow animal cells outside the body. He grew fibroblasts taken from the hearts of chicken embryos in flasks, and was able to do so continuously for 34 years. Carrel believed, and led others to believe, that these cells were 'immortal'. Careful investigation by Witkowski[118] into the events surrounding the cultivation of these cells, however, suggested that there was some manipulation of experiments involved.[119]

Alexis Carrel was a pioneer of tissue culture, a method by which pieces of organ and tissue removed from a developing animal can be cultivated outside the body in vessels containing suitable nutritional media. His techniques, based on his expertise as a surgeon, were so complicated as to appear almost magical, certainly to the layman!

Carrel was born in France and obtained his degree as Doctor of Medicine in Lyons. In 1904 he emigrated to Chicago, and from there was appointed to the Rockefeller Institute in 1906 where he worked until 1938. He was one of the few Americans to win a Nobel prize in 1912, for his surgical achievements in joining severed blood vessels.

In the Rockefeller Institute Carrel was responsible for the development of tissue culture techniques together with Montrose Burrows and Albert Ebeling. Other pioneers in this field, Willmer and Paul, criticized Carrel's surgical techniques, which involved wearing black gowns, masks and hoods (as was the current practice in operating theatres at that time to avoid contamination with bacteria, fungi, viruses, etc.) These complicated precautions dissuaded many biologists from following the methods developed in Carrel's laboratory.

Carrel's techniques involved taking small fragments of embryonic chicken heart, placing them on a coverslip in a drop of plasma diluted in water, letting the plasma clot on the coverslip, inverting the coverslip over a hollow-ground slide, and incubating the culture at 39 °C. When subculturing was required, pieces of the tissue culture were removed with a cataract knife, and transferred to a new drop of hypotonic plasma. I still remember how fascinated I was as a high school student reading about Carrel's experiments and his observation in a microscope of beating heart muscles of chick embryos in his cultures. By 17 January 1912, Carrel had established 16 cultures of such embryonic heart fragments. In the next two months 11 of them died, and of the remaining five only one, No. 726, apparently survived until September 1912. The survival of chick cells in culture, outside the body, for several months was then described by Carrel in the *Journal of Experimental Medicine* under the title 'On the permanent life of tissue outside of the organisms'[120] Carrel declared that he had determined 'the conditions under which the active life of a tissue of the organism could be prolonged indefinitely'.

Carrel's collaborator, Ebeling, claimed to have cultivated one such heart embryonic tissue culture at the Rockefeller Institute until 1938. Ebeling later moved to Lederle Co. and took the culture with him; he kept it there until 1946, when it was discarded. Carrel and his co-workers published a number of papers about the culture, one after it had been growing for 16 months, the next after 28 months. By 1919 the 'immortal' tissue had been cultured for 1390 passages. The final paper by Ebeling appeared in 1922 after 1860 transfers.[121] The 'immortalization' of a living tissue attracted public attention and many articles about it appeared in the daily newspapers and in popular scientific magazines, such as *Scientific American*.

Carrel's pioneering efforts in tissue culture had a great impact on the developing field. Albert Fischer, however, who worked in Carrel's laboratory between 1920 and 1927, wrote in his book on tissue culture, published in 1925, that the majority of biologists, morphologists and pathologists did not have much success in trying to repeat Carrel's experiments. 'Consequently, many investigators became sceptic and pessimistic in regard to the employment of the method'.[122] Nevertheless, Carrel's prominence in the field was widely recognized. At the Tenth International Zoological Congress in Budapest (1927), the president of the congress, Professor van Lenhossek, spoke of Carrel as the 'genius' who had developed tissue culture methods. As a leading figure in the field of tissue culture research, Carrel received enormous publicity, especially for the development of the 'immortal cell' strain. It is ironic that Carrel's success in tissue culture deterred many others from following his example because of the difficulties of the method.

Nobody doubted Carrel's results until 1956, when Hayflick demonstrated that embryonic human cells of the connective tissue, the fibroblasts, which kept their normal and constant (diploid) set of chromosomes and did not undergo transformation into cancerous cells, could not be cultivated in culture for more than 50±10 doublings.[123,124] In contrast, cancer cells (like HeLa) have acquired immortality and can be grown outside the body almost indefinitely. Hayflick and Moorehead[125] showed that this limitation of growth was not due to inhibitory substances being released from ageing cells or to the effects of activated latent viruses. Many theories have been proposed to explain the biochemical and biological basis of age-related decay of cells, but the fact remains that in all cases where proper precautions were taken in the cultivation process of diploid cells, their life span, that is the number of times the cells are able to divide, has been limited.

Hayflick's work, as well as the experiments that followed, focused the attention of biologists on the problem of how to explain the apparent immortality of Carrel's cells.[126] By that time Carrel was dead

(he died in France in 1944, in disgrace as a Nazi collaborator). It was, therefore, not possible to find out the details from him.

The statement that nobody doubted Carrel's results until 1956 should be qualified. One person who took special interest in Carrel's work in the early 1930s was Ralph Buchsbaum, who became Professor of Zoology in Pittsburgh in 1960 (he retired from the University of Chicago in 1972). Buchsbaum visited the Rockefeller Institute in 1930, at a time when Carrel was away on a vacation in Spain.

Dr R. C. Parker, Carrel's chief assistant was too busy to see Dr Buchsbaum, who spoke, therefore, to Ebeling. He showed him around the laboratories, but would not show him the 'immortal' cells because of the risk of contamination. Many years later Buchsbaum wrote to Witkowski describing his visit to Carrel's laboratory in 1930:

> I could not return to Chicago without seeing the famous immortal strain, so I returned to the floor where I met a young woman technician. I pleaded with her to let me see the cultures. She said Dr Carrel and Dr Parker would have a fit if they knew, but 'what harm could it do to see them?' When I looked at the cells and said they were full of fat globules and obviously on the way out, she said slyly: 'Well, Dr Carrel would be so upset if we lost the strain, we just add a few embryo cells now and then . . . We make new strains for new experiments. Dr Parker says he will retire the strain soon, it costs too much to keep it going.'[119]

Witkowski also had a letter from Dr Margaret Murray (who worked at that time in Carrel's laboratory) in which she related that one of Carrel's technicians was an anti-fascist and very much disliked Carrel's political and social beliefs. This assistant might have been the one who tried to discredit Carrel as a scientist. Other scientists who worked with the immortal cells in the period between 1930 and 1939 were not willing to confide in Witkowski. Thus we are left with Buchsbaum's account only.

Today, the well-established fact that normal, diploid cells are not immortal leaves us with the following hypothesis: Carrel, committed to the concept of the immortality of his cells, repeatedly stated that his cells could be grown indefinitely. This was part of his mystical view of the nature of life; his belief in the unending life force. His belief in the immortality of cells must have been shared by his colleagues in the laboratory, but eventually, when difficulties arose, as described by Buchsbaum, occasional replenishment of the cultures (perhaps even inadvertently by the use of embryo extracts that might have contained some living cells), would have contributed to the concept of immortality. One should not detract from Carrel's achievement to have cultivated chicken embryonic cells for prolonged periods of time in an era when antibiotics were not available to overcome any contaminating bacteria and anybody less careful and meticulous than Carrel or his close co-workers would have quickly lost the cultures by bacterial or fungal contamination.

The immortality of Carrel's cells, however, instead of being a fact, is now only a legend.

THE BURT CONTROVERSY - INHERITANCE OF INTELLIGENCE

Sir Cyril Burt, who died in 1971 at the age of 88, was an eminent British psychologist. He was the first psychologist to receive a knighthood and shortly before his death he was awarded the Thorndike Prize by the American Psychological Society. He held a Chair of Psychology at University College, London until his retirement at the age of 68.

In his numerous publications Burt presented an extensive and almost unique assembly of IQ sets to support his hypothesis that intelligence is determined by heredity. Burt collected most of his data in the period 1913–32 when he was a research psychologist in the London educational system. He was a government adviser in the 1930s and 1940s and he was partly responsible for setting up the '11-plus' system of education, in which, at the age of 11, children were tested and assigned to one of three education levels. The influence of Burt's data and theories[127,128] was such that Arthur Jensen of the University of California suggested in the *Harvard Educational Review* in 1969 that a failure of compensatory educational programme for the racial minorities in the USA might be explained by the hypothesis of dependence of intelligence on racial heredity. This idea also gained other support.[129]

Burt first reported IQ tests done on 21 pairs of twins in 1955.[127] By 1958 he had published data on 30 pairs of twins[128] and in his final paper in 1966 the tests comprised a total of 53 pairs.[130] In these papers Burt found a very high correlation (0.944) between the IQ scores of identical twins reared together, while in the case of twins who had been separated and had grown up in different environments the correlation coefficient was 0.771. (Correlation coefficient is a number expressing the degree of dependence between two variables, in this case between the IQs of parents and progeny. This number may range between 0 and 1, where 1 indicates a perfect correlation and 0 means no correlation at all.) In his final paper on the subject,[130] published when he was already 83, Burt indicated that the foster homes for the separated twins studied were chosen at random and rejected the claim of environmentalists that the high correlation for separated twins had been due to the way foster parents were chosen.

There were scientists who doubted Burt's data and conclusions. Among them was Sandra Scarr-Salapatek of the University of California. She thought Burt's data looked 'funny' and wrote asking for clarification about his procedures. There was no satisfactory answer. Nevertheless, as late as 1971, Dr Richard Herrnstein of Harvard put forward the theory that social standing was based on inherited intelligence and he supported his contention with Burt's data.[131]

In 1972 Burt's papers were brought to the attention of Leon Kamin, a psychologist at Princeton University, who at that time was mainly studying conditioned reflexes. Kamin immediately noticed that Burt's papers contained internal contradictions and lacked methodological data such as the sex of the children tested, the types of tests administered etc.[132] Kamin also noted that in his paper published in 1939,[133] Burt stated that the methods he used 'were described more fully in degree theses of the investigators mentioned in the text' or that they were, according to Burt, buried in inaccessible theses. In another paper, published in 1943,[134] Burt stated: 'A fuller account of sources, of calculations, with detailed tables will be found in her [J. Maver's] degree essay (filed in the Pyschological Laboratory, University College, London)'. In fact, as Kamin discovered, no such essay or thesis was filed or even submitted to the university. In that same paper, Burt wrote also that some of the inquiries were published in London local authority reports or existed as typed memoranda.

At the beginning of his scientific career Burt had published a paper, jointly with Moore, in which he had stated that the more important of his tests were carried out on more than *a thousand* children aged 6–14 (the Liverpool study). In 1939, however, Burt wrote: 'The value and reliability of group testing . . . were demonstrated by Moore, Davies and myself . . . These were, we believe, the first investigations in this country in which the number of children tested ran into well over a *hundred*' [my italics].[133] When Burt was challenged in 1954 in a letter to the *British Journal of Statistical Psychology* about the availability of detailed tables relating to the Liverpool research,[135] Burt answered that 'Mr Moore will himself publish a fuller account of his analysis in the forthcoming issue of this journal'[136] In fact, Moore never published anything after 1919.

The most important of Kamin's findings, in spite of his being an outsider in the IQ testing field, was that in all three of Burt's papers on twins, the correlation coefficients for different numbers of twins were exactly the same, namely 0.771 for separated twins and 0.994 for twins reared together. Amazingly, no one before Kamin had noticed this extraordinary coincidence in numbers. Kamin came to the conclusion that Burt had 'cooked' his data in order to arrive at the conclusion he wanted. Kamin's view was supported by Liam Hudson, at that time Professor of Psychology at Edinburgh University.

On the other hand, Professor Jensen, who had been a post-doctoral student of Eysenck, who in turn was Burt's pupil, did not hold such an extreme view. Jensen greatly admired Burt and considered him one of the world's great psychologists. When accusations about Burt's data were voiced in 1972, after Burt's death, Jensen visited England to collect a complete set of Burt's reprints and to write a review of his work. To his surprise, he found in Burt's papers some 20 instances of invariant correlations, although the sample size of experimental subjects changed, a finding that was almost impossible to explain as occurring by chance alone. In 1974 Jensen wrote: 'It is almost as if Burt regarded the actual data as merely an incidental backdrop for the illustration of the theoretical issues in quantitative genetics, which, to him, seemed always to hold the centre of the stage'.[137] Jensen's mentor, Eysenck, also agreed that Burt's data were unusable and unreliable, though he did not go so far as to accuse Burt of concocting them. In his obituary note on Burt, Eysenck expressed his surprise at the revelations since he knew that Burt was 'a deadly critic of other people's work when this departed in any way from the highest standards of accuracy and logical consistency.[138]

We thus face the question of whether the 'errors' in Burt's papers were due to inattention to detail on the part of a 72-year-old scientist (the view of Jensen and Eysenck), or to a deliberate attempt to deceive (Kamin). Kamin went so far as to suggest that Burt invented all his data, from the very beginning of his career in 1909.[139] Jensen, on the other hand, doubted the feasibility of this statement; he thought that if Burt had been trying to fake the data, as a person with high statistical skills, he would have done a better job.

The controversy persisted after Burt's death and seemed to demand further investigations. It was thought that the inspection of Burt's original notebooks would provide the answer, but unfortunately such notebooks and raw data were not available. After Burt's death some of his colleagues took books and reprints from his house, but left behind some six tea chests filled with test sheets and notebooks. Burt's housekeeper, acting upon the advice of some of his colleagues who had been asked what to do with this material, burned the chests with all their contents. In such a situation raw facts cannot be established and the judgement of Burt's integrity as a scientist has to depend on conjectures based on his published work.

That Burt had been inventing data appeared more probable after it was discovered that he had published his critical papers with two collaborators, Miss Margaret Howard[140,141] and Miss J. Conway.[142] These two collaborators could not be traced, and in October 1976 Oliver Gillie of *The Sunday Times* newspaper, who researched the matter thoroughly, reported that the two women did not exist. A possibility was entertained that in the early 1930s at least Miss Howard was in Burt's entourage, but it was quite certain that the women could not have collaborated with Burt in his crucial papers published in the late 1950s.

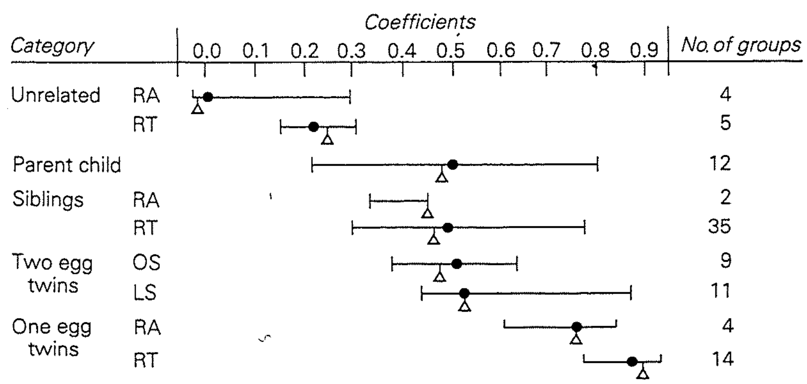
The names of Howard and Conway also appeared in the pages of the *Journal of Statistical Psychology* (edited by Burt) as authors of book reviews praising Burt's publications, indicating Burt's priority in many discoveries and criticizing the publications of his opponents. After Burt had ceased to be the editor of the journal, no additional reviews by Howard and Conway were published. Those who knew Burt well felt that the reviews were written by Burt himself, in his unmistakable style, and that he used the names of Howard and Conway as pseudonyms.

A further suspicious circumstance was that after 1950 Burt, having retired from the University, could not have administered the IQ tests himself and must have had collaborators for testing the twins, especially when travel was needed to the places where the twins lived. When asked about this in 1969 Burt wrote to a correspondent that he delegated the job to Conway and Howard.

Amazingly, during Burt's lifetime his work was never challenged, despite its shortcomings. He was a very powerful figure at the time of his retirement, and remained influential after that. More importantly, Burt's results for IQ correlations were not only in accordance with other studies (Figure 5) but fulfilled the general expectations and beliefs about inheritance of intelligence prevalent in the psychological community.

So, the question remains whether, as Kamin believes, Burt's data were fraudulent, or whether as Eysenck suggests, that Burt, now old and ill, merely carried over the correlation figures from his earlier papers in order to avoid the difficult task of recalculating the new data. Such a procedure would not constitute fraud, since it would have been done without the intent to deceive. Nicholas Wade, who reviewed Burt's story in *Science*,[143] finds it hard to believe that the combination of implausibility in Burt's results, the apparent use of pseudonyms and the failure to locate Misses Howard and Conway speak for Burt's innocence: he seems to come down on Kamin's side.

In 1978 came a very scholarly analysis of Burt's work by Dorfman of the Univeristy of Iowa.[144] He analysed in detail Burt's paper of 1961[145] on intelligence and social mobility, and concluded that Burt's data 'were fabricated from a theoretical normal curve, from a genetic regression equation and from figures published more than 30 years before Burt completed his surveys'. Dorfman also showed that Burt, instead of providing new data, copied the figures from tables



Full circles – median correlation coefficient; open triangles – Burt's data
RA = reared apart; RT = reared together; OS = opposite sex; LS = like sex

Figure 5. Comparison of Burt's data on inheritance of intelligence with data from similar studies on twins conducted by others. Median values indicated by triangles (based on Erlenmeyer-Kimling, L. and Jarvik, L. F. 1963: *Science*, 142: 1478).

published in his paper on vocational guidance in 1926,[146] which in turn were based on figures from a census taken in 1921.

Even if serious doubts are entertained about the validity of Burt's data, the fact remains that the deletion of these data from the overall picture of inheritance of intelligence, based on some 52 other studies, would have no appreciable effect. These 52 studies, reviewed by Erlenmeyer-Kimling and Jarvik,[147] comprised 1082 monozygotic and 2052 dizygotic twins, 256 siblings reared apart and 15,513 siblings reared together, 7883 parent–child pairs and 482 unrelated pairs. The authors concluded that there was a 'marked trend toward an increasing type of intellectual resemblance in direct proportion to an increasing degree of genetic relationship regardless of environmental communality'. The overall correlation coefficient for twins reared together was 0.87 and for those reared apart, 0.75. Burt's figures differ from the median (a statistical average) values obtained by other authors in an unsystematic way; Burt's corresponding median values were 0.94 and 0.77 respectively.

One can therefore repeat the statement that Burt, having once calculated the correlation coefficients for 'intelligence scores', and being familiar with the literature, seems to have published his data repeating the earlier calculated correlation. Burt's apparent forgery was detected because the variation in his results was below the likely vagaries of chance.[148] Could Burt's actions be classified as fraud? According to Edwards,[149] 'fraud occurs when the conclusions of a scientific research are blatantly at variance with the known facts; when lying is plausible, it is a misdemeanour'. Burt's action would thus be classified as misdemeanour. This is a view I shall dispute in chapter 14.

What is quite astonishing is that although the figures reported by Burt (0.944 for twins reared together, 0.77 for those reared apart) were the same for groups of 21, 'over 30' and 53 pairs of twins, figures that were recognized by Kamin as being incompatible with the known statistical rules, it took the British Psychological Society a further six years to reach this same conclusion. Estling writing in 1982[148] concludes: 'Burt's crime is the very plausibility of his fiction, which was manufactured to feed his, and our prejudices . . . for heritability.'

HOW MILLIKAN WON THE NOBEL PRIZE

Robert A. Millikan, a famous American physicist of the University of Chicago, won the Nobel prize in 1923 for his experiments of 1910 in which he determined the electrical charge of the smallest component of the atom, the electron. In his Nobel lecture Millikan said:

> My own work has been that of the mere experimentalist whose main motive has been to devise, if possible, certain crucial experiments for testing the validity or invalidity of conceptions advanced by others regarding the unitary nature of electricity . . . The success of the experiments first performed in 1909 was wholly due to the design of the apparatus, i.e. to the relation of the parts . . . scarcely any other combination of dimensions, field strengths and material could have yielded the results obtained.

What were these experiments? They essentially followed those of Regener carried out in Thompson's laboratory in Cambridge (England). Water droplets were produced in an expansion chamber between electrically charged horizontal plates; the retardation of the rate of fall of these droplets, as determined by microscopical observation, was related by Stoke's law (describing the fall of very small particles) to the electrical charge on the droplets, which they acquired in the electric field between the plates. Initial results, which Millikan reported at a meeting in Canada in 1909, gave values within an exact mutiple of the smallest charge on a droplet (this charge was calculated from the known strength of the electric field created by the condenser plates); this smallest electrical charge was calculated to be 4.65×10^{10} electrostatic units. The difficulty with experiments was that the water droplets evaporated so quickly that observation of their fall was possible for a few seconds only.

At this stage of research, a graduate student, Harvey Fletcher, joined Millikan's group. Following some discussions in the group on the difficulties encountered with the water droplets, Fletcher suggested the use of oil droplets, which would not evaporate. Within a day Fletcher had set up an apparatus consisting of an arc lamp, an atomizer for spraying oil, two brass plates (the upper one with a hole in the centre to permit the entry of the falling oil droplets), a telescope and batteries to charge the plates. Fletcher wrote in his posthumously published memoirs

> I then tried out the apparatus. I turned on the light, focused the telescope, sprayed oil over the top of the plate and then came back to look through the telescope. I saw a most beautiful sight. The field was full of little starlets, having all colors of the rainbow . . They executed the most fascinating dance, I had never seen Brownian movement before. Here was a spectacular view of it.[150]

Fletcher then applied an electrical field to produce 1000 volts across the plates and looked again through the telescope. He noticed that some droplets fell and others moved upwards. 'I knew some were charged positively and some negatively . . . I spent the rest of the day playing with these droplets and got a fairly reasonable value of *e* before the day ended.'

Millikan, who was away from the laboratory that day, only saw Fletcher's set-up the next day, and was very much excited. From then on he continued working closely with Fletcher. They made some improvements in the design of the apparatus and within six weeks the discovery was announced publicly. Fletcher thought the findings would be published in a joint paper with Millikan. Millikan, however, explained that if Fletcher wanted to use a published paper for his doctoral thesis, he must be its sole author (such were the regulations of the universities in those days), and since the oil-drop experiments were a collaborative work with Millikan, there was a problem. The problem was solved by the friendly advice (or was it demand?) of Millikan, and so the paper, entitled 'The isolation of an ion, a precision measurement of its charge and correction of Stokes' law, was published in 1910 by Millikan alone.[151] In the text of this paper, however, Millikan gave credit to Fletcher:

> . . . Mr Harvey Fletcher and myself, who have worked on these experiments since December 1909 and have studied in this way between December and May from one to two hundred drops which had the initial charge from 1 to 150 and made from oil, mercury and glycerine, and found in every case the charge on the drop to be an exact multiple of the smallest charge which we found that the drop caught from the air'.

On the issue of authorship of the paper, Fletcher wrote in his memoirs:

> I did not like this but I could see no other way out, so I agreed to use the fifth paper as my thesis . . . It is obvious that I was disappointed as I had done considerable work on it and had expected to be a joint author. But Millikan was very good to me while I was in Chicago . . . and through his influence I got into the graduate school.

Fletcher insisted that he bore no ill will toward Millikan for not letting him be a joint author on this first paper, which effectively led to Millikan's being awarded the Nobel prize.

During the period 1911–13, Millikan had a foreign rival, Dr Felix Ehrenhaft of the University of Vienna, who also measured the charges on oil droplets. Ehrenhaft's experiments, however, indicated that there were subelectronic fractional charges on the droplets, rather than the exact multiples of unit charges postulated by Millikan. How was it that the two laboratories obtained conflicting results?

Holton[152] and Franklin[153], who in 1978 and 1981 studied Millikan's unpublished data on his oil drop experiments during that period, found that Millikan graded his results in his notebook from 'best' to 'fair'. A paper published by Millikan in 1913 was based, according to the notebook, on 140 observations, but he had excluded 49. Nevertheless, Millikan stated in the published paper that the results 'represented *all* the drops experimented upon . . .' It seems therefore that, contrary to his statements in the paper, Millikan was rather selective in the use of his experimental data. Thus, in distinction from the data of Ehrenhaft, which contained a wide range of fractional values, Millikan's data came out in his 1913 paper as very elegant and clear. It was this paper that put an end to the controversy with Ehrenhaft, who ended his life in disillusionment while Millikan had the recognition afforded by the Nobel prize.

It is ironic that in a paper published in *Science* in 1923, Millikan wrote '4/5 of all experiments which we make in our physical laboratories in the hope of developing new relations, establishing new laws, or opening up new avenues of progress, are found to be directed along wrong lines and have to be abandoned.'[154]

There are two issues involved in this story. I should like to separate the conduct of Millikan as the thesis supervisor of Fletcher, from his later act of omission of some results in his published papers.

The mentor–student relationship is a very complicated one. The attribution of credit for a new idea or discovery made by a professor and student, or by the student under guidance of his supervisor, or by the student entirely on his own, cannot be viewed as a black and white situation.

Let us recall the famous Waksman–Schatz discovery of streptomycin. At that time the only known and clinically used antibiotic was penicillin, which had been discovered some 14 years previously. Selman Waksman, at that time Professor of Microbiology at Rutgers University in New Brunswick, New Jersey, had been studying a class of bacteria-like ubiquitous soil fungi, the Actinomycetes. Knowing that penicillin was produced by a mould, Penicillium, Waksman assumed that other classes of moulds or fungi might also be producing antibiotic substances that would enhance their survival in soil. He

therefore set up his students to isolate as many Actinomycetes as possible from soil and screen them for antibiotics against disease-causing bacteria. One of his doctoral students, Albert Schatz, made the momentous discovery that a fungus called Streptomyces actually produced an antibiotic that would kill bacteria causing tuberculosis. This antibiotic also proved effective against other human and animal diseases. The paper describing the discovery of streptomycin was signed by both Waksman and Schatz. Nevertheless, Waksman alone was awarded the Nobel prize for this discovery.

When Waksman patented streptomycin and obtained substantial royalties from the manufacturers (which incidentally were mostly used to build and finance the Waksman Institute of Microbiology), Schatz sued Waksman and demanded a share of royalties from the sale of streptomycin. The matter was eventually settled out of court, but the precedent that a student may sue his professor incensed the scientific community to the extent that Schatz was ostracized in the USA and could not obtain a research position there. (He went to South America and became engaged in education.) The Nobel Prize committee recognized the fact that behind Schatz's discovery was a general concept based on the arduous studies of Waksman, and that this background, which Waksman provided, was essential for Schatz to make the discovery. The Nobel Prize was, however, given to Waksman only.

In the Millikan–Fletcher case there was, after all, an amicable settlement between the two: Fletcher, though feeling wronged, agreed out of his feeling of respect and gratitude to Millikan, to let his mentor to reap the glory. Here was a situation where a young student joined a team that was set up to attack a certain physical problem. The basic idea to measure the charge of the electron on droplets was already there when Fletcher arrived. True, the idea to use oil rather than water was a real breakthrough and one would expect that Fletcher be given credit for this idea and appear as a co-author in the article. Unfortunately, the strict rules applying at that time to PhD students (in some universities they still apply now), that the thesis has to be the student's own work, caused difficulties. Thus a situation developed in which Millikan, as the supervisor, reaped the harvest alone.

I should like to think that the problem would be solved differently today, although I personally know of cases where graduate students are not permitted to pursue their own ideas and have to toe the line set for them by their supervisor. There are still many laboratories (mainly in the biological and medical sciences) where the rule applies that any paper coming from a student should be signed by the head of the laboratory (usually as the last author). I have hardly encountered any comments about this procedure that would classify it as unethical. On this basis, then, Millikan's behaviour toward Fletcher would not be considered unethical; nevertheless, I believe that taking away the credit from a student who has had an innovative idea and executed it should be censured.

What of the other charge against Millikan, that he left out some of the experimental data assembled in the course of measuring the charges on the electrons? Is it improper to leave out data? In the case described, Millikan left out one-third of his measurements from the published report. Salvador A. Luria, also a Nobel prize winner, expressed his conviction that 'leaving out data that inexplicably conflict with the rest of scientists' data or with the proposed interpretation is an anathema'.[16] From this point of view, which I share, Millikan's selective reporting of his results would not conform with the norms and ethics of science, and would be branded as misconduct.

5

Lysenko – Science and Politics

Trofim D. Lysenko, a Soviet agronomist active during the period between 1929 and 1965, won the highest Communist Party support for an effort which, in essence, was based on the annihilation of the science of genetics. The Marxist ideology was founded on the concept of the malleability of human nature. In support of this theory Lysenko applied his energy and drive to show that the nature of plants can also be moulded by the environmental conditions, notwithstanding their genetic character.

Lysenko rebelled totally against modern-day science. His multifaceted activities involving the top levels of Soviet agriculture stemmed from his arrogant belief that he knew better than the academic scientists how to increase the yields of agricultural products. This led to a situation where Soviet agriculture was abused for 35 years.

Through him many leading scientists lost their jobs, and sometimes their lives, after having been accused of 'wreckage of Soviet agriculture'.

No true scientist, Lysenko never addressed any genuine scientific problem which he and his followers considered a scholastic obstacle to quick solutions in agriculture. Lysenko attended a secondary school of gardening (a type of horticultural college) and subsequently worked at a rural experimental station belonging to the Kiev Agricultural Institute. He had no postgraduate training or higher degree.

Lysenko was assigned to a remote agricultural station in the North Caucasus, and there set about trying to find a good winter crop. During that period he re-discovered so-called 'vernalization', a process that involves the moistening and chilling of seeds or seedlings during the winter. When such plants are sown or planted in the spring, they complete their life cycle in a shorter period of time; in regions where the summer is short, such vernalized plants can be harvested before autumn[155*]

N. A. Maksimov, a leading Soviet plant physiologist at that time, said that the results obtained by Comrade Lysenko did not represent anything new in principle: they were not a scientific 'discovery' in the precise sense of the word.[156] The transformation of winter to spring habit by moistening and chilling the seeds was known in the USA in 1857, and was even reported in a Russian agricultural paper in 1885.[157] A German plant physiologist, Klebe, wrote about the phenomenon in a book *Willkuerliche Entwicklugsaenderungen* published at the beginning of this century; this book was then translated into Russian by Timiriazev. Chouard[157] has reported that other studies on vernalization were made in 1918 by Gassner, who did not think the method had any practical advantage.

Lysenko, incensed by the criticism that he had made no 'discovery', and being unable to explain how his vernalization method actually worked, insisted, from 1923 onwards, that *all* varieties of wheat, both winter and spring, would respond to chilling or soaking by hastening the onset of earing, and believed that this would obviously increase the yields. The spring varieties were soaked and kept under controlled temperatures and humidity. They were thus sown in a swollen condition. The procedure was supposed to shorten the vegetative stage. In fact, the exact conditions required for this process could not easily be reproduced on collective farms and in some cases the yields of the vernalized wheat were actually lower.

In 1929 Lysenko was transferred to the Odessa Institute, and within a year, from there to the Moscow Institute of Genetics. From there he reported his work not in established scientific periodicals, but rather in interviews with reporters from mass-circulation newspapers. Lysenko would not submit his articles and data to the scrutiny of journal reviewers.

Lysenko applied vernalization not only to seeds, but also to tubers and cuttings, and claimed success in his methods. He never acknowledged the accumulating data on the influence of plant hormones, claiming that he had disproved these phenomena. (In fact, we now know that hormones control plant growth and the reproduc-

* The references in this chapter are taken from the source books *The Rise and Fall of Lysenko* by Zhores Medvedev (1969) and *The Lysenko Affair* by D. Joravsky (1970) and they are presented as they appear in these books.

tive process of plants. There exists today a multi-million pound industry for the production of synthetic hormones for many uses in agriculture, ranging from ripening fruits to inducing rooting of cuttings.) In 1939 Lysenko wrote:

> The speeded up development of such plants (sprouted) we explain basically not by the fact that the eyes of the tubers are sprouted before planting, but by the fact that the sprouts are subjected to the influence of certain external conditions, namely the influence of light (a long spring day) and of a temperature of 15–20°C. Under the influence of these external conditions (and that precisely is vernalization), in the potato tuber's eyes, as they start to grow, there occur those quantitative changes which, after the tubers are planted, will lead the plant to more rapid flowering, and hence to more rapid formation of young tubers.[158]

This represents an explanation without substance. From Lysenko's many publications and pronouncements one would understand that vernalization was the initial stage of development in any plant or its part, and that certain conditions of air, moisture and temperature were essential for the onset of the next stage and for flowering.

As the years went by, Lysenko became more and more powerful and reached the peak of his influence in the Soviet establishment in the years 1948–52, when he was unequivocally supported by Stalin himself. At the 1948 session of the Lenin All Union Academy of Agricultural Sciences (LAAAS), Lysenko made this chilling statement: 'The question is asked in one of the notes handed to me: What is the attitude of the Central Committee of the Party to my report? I answer: '"The Central Committee of the Party has examined my report and approved it."'[59] Lysenko received a standing ovation for that statement. By that time he had succeeded in destroying the science of genetics in the Soviet Union completely, and had had the most important geneticists removed from their posts; in some instances they had been arrested and even executed.

Consider the fate of Maksimov, for example. Lysenko's paper (with Dolgushin) on vernalization was first presented at the Congress of Genetics, Selection, Plant and Animal Breeding, held in Leningrad in 1929. At that meeting Maksimov also presented a paper on physiological methods of regulating the length of the vegetative period in plants. Maksimov, who at the time was the head of the Physiological Laboratory at the Institute of Applied Botany, had used the method of germination in the cold since 1923; he obtained crops of winter varieties in the first year without damaging