



Contents

Editorial		2
News		3
Features		
Six months at Bart's: Szilard's 1934 neutron experiments		
	<i>by Francis Duck</i>	7
1820: Ampère's "failed revolution"?	<i>by Jim Grozier</i>	21
A sketch of Mihajlo Pupin (Part 1)	<i>by Malcolm Cooper</i>	28
The colour and polarisation of light from the sky (Part I)		
	<i>by Edward A Davis</i>	39
Sir Francis Bacon: Parallels in time between 1620 and 2020		
	<i>by Alison McMillan</i>	53
Book review		
Unravelling The Double Helix	<i>Reviewed by Malcolm Cooper</i>	66
Committee and contacts		70

Editorial

The noughts just roll off!

As has been mentioned before in these pages, anniversaries are a very effective way of focussing on an event or a person of interest in the history of physics. So it was good to recall the discovery of electromagnetism by Hans Christian Oersted and the birth of John Tyndall, in 1820 some 200 years ago. We might also recall that 200 years ago Julius Plücker demonstrated that cathode rays could be deflected by a magnetic field and it is 100 years since Frederic and Irene Joliot-Curie show that uranium fission can result in a chain reaction.

But why the slavish interest in 00s - why not any number of years? Why should we be pinned to periods of time with those zeros? Well, I think you will have guessed by now that I've been a little disingenuous here and that in my last two examples I should have written 200_9 and 100_9 to show these dates were calculated using numbers to base 9. But it offers some wonderful opportunities. Nothing much was happening 300_{10} years ago or for that matter 300_9 and 300_8 but 300_7 years ago came '*A Treatise on Electricity and Magnetism*' - an event well worth celebrating!

And then - what about non-integer bases? The possibilities are endless...

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News



The History of Physics Group of the Institute of Physics is delighted to congratulate Vincent Smith on the award of the Phillips Prize.

Vince was elected to the committee of the History of Physics Group a few years ago, and he has served first as Secretary and now as Treasurer. He has organised conferences in Bristol for the International Year of Light in June 2015, and on the history of particle colliders in April 2017. He is presently organising a meeting on the two hundredth anniversary of Hans Christian Oersted's discovery of the connection between electricity and magnetism. During several of our meetings he has organised 'picnic lunches' which combine convenience, culinary pleasure and economy.

Previously he has served on the Nuclear Physics Division committee, as Secretary, then Chair from 2005 to 2009.

He served on the committee of the SW Branch, as ordinary member, Secretary, Treasurer and Chair from 2001 to 2005. As Chair, he introduced the six-monthly Festival of Physics, which brings together speakers, workshops and demonstrations in a full-day event in Bristol or Bath in the Spring, and Exeter or Plymouth in the Autumn. He re-joined the SW committee recently as Treasurer from 2016 to 2018 and Chair from 2018 to present. .

More widely he has supported the activities of the Institute in very many ways. He was appointed to the Nations and Regions committee, serving as Chair from 2006 to 2008. He also served on the Institute's Council from 2004 to 2008, and on its Education and Public Affairs Board from 2002 to 2004.

Vince is a member of the panel who consider applications for Fellowship, he was a member of the Degree Accreditation committee from 2004 to 2009 and serves on panels who visit university departments seeking accreditation. He was a founding member of the Lab in a Lorry steering committee, and he served on the Advisory committee for the Supporting Physics Teaching project.

However probably his major contribution to the Institute's aims is his many school visits delivering talks on 'What Time is it on Mars?', 'Dark Matter' and 'Einstein's Revolutionary Ideas from 1905'.

In his day job Vince spent 40 years in the Physics Department at Bristol University, finishing as Reader. He researched in elementary particle physics, taking part in projects at CERN and Fermilab, and he was a member of the team that discovered the Higgs Boson.

As an Honorary Senior Research Fellow at Bristol, Vince still visits CERN, acting as a Shift Leader, but also, as a trained CERN guide, taking school and other groups on guided tours. He also visits the University of Jaffna in Sri Lanka every two years and gives unpaid lectures on Advanced Electromagnetism.

In 2004 Vince was awarded the MBE for services to physics.

Andrew Whitaker

Joule bicentenary



A memorial pavement plaque was unveiled (April 2019) in Worthington Park, Sale, to commemorate the bicentenary of JAMES PRESCOTT JOULE (born 24 December 1818). The plaque is close to the pedestal and bust of the Salford-born scientist erected many years ago by public subscription. Fortunately, the weather was fine, a fact appreciated by the sizeable crowd that comprised council dignitaries, local schoolchildren and residents, some scientists and even a ‘look-alike’ Joule suitably dressed in Victorian garb.

Sale (formerly in the historic county of Cheshire) has many links with Joule. It is where, in Wardle Road, he lived out his last years, where he worshipped (St Paul’s church) and where he is buried. There is also a local hostelry that bears his name (the J P JOULE) a fitting tribute to a man descended from brewing stock.

The plaque was designed by local ceramicist Gordon Cooke, who claimed that its manufacture expended much heat and work. It records aspects of Joule’s life and achievements (for example, the paddle-wheel experiment) but, most prominently, it displays the figure 772.55 foot pounds. This represents the result that Joule gave in 1849 for the mechanical equivalent of heat. The same figure is inscribed on Joule’s headstone in nearby Brooklands Cemetery.

Sale is proud of its association with Joule and it is fitting to note that the memorial plaque was sponsored and partially financed by several local organisations and a local resident. Anyone with an interest in the history of Victorian science could do worse than spend an afternoon visiting the ‘Joule’ sites of Sale.

4th International Conference on the History of Physics

Trinity College Dublin

Covid-19

This conference, which was to have been held in June 2020, had to be cancelled on account of the Covid-19 pandemic. The plan was to reschedule it for June 2021. However, the current uncertainties about how the disease may develop, the possible availability of vaccines and so on, lead us to conclude that we have to consider the possibility of yet another delay. We sincerely hope that such a delay will not be necessary but further announcements will be made in the next couple of months via our website:

<http://hop2020.iopconfs.org/Home>

The first three conferences in the series were held at Trinity College, Cambridge UK in 2014, Pöllau, Austria in 2016 and San Sebastian, Spain in 2018. Their aim was to bring together physicists interested in the history of their subject and professional historians of science in the belief that proponents of the two disciplines, with their different perceptions and methodologies, can benefit from interaction and discourse.

Inspired by the recent centenary of two major landmarks in modern physics - nomination of the proton as a fundamental nuclear particle and discovery of the bending of light in a gravitational field - the leading theme of the present conference will be:

‘On the road to modern physics’

The conference will include presentations on the history of particle physics, general relativity, cosmology and astrophysics. However, there will be *invited and contributed papers on other topics related to physics history.*

Professor Dame Susan Jocelyn Bell Burnell will give the keynote lecture.

Six months at Bart's: Szilard's 1934 neutron experiments

Francis Duck

Retired but formerly of the University of Bath and Royal United Hospital, Bath.

Szilard in London

When the Hungarian physicist Leo Szilard arrived in London in April 1933, escaping from the increasingly aggressive Nazi onslaught on the German Jewish community, he had no plans for his future. He was by then extremely well connected in the Berlin physics community. He had left secretly by train to Vienna on 30 March, carrying bundles of bank notes in his suitcase, one of the last to cross the German-Czech border before police began questioning every traveller.¹

A nomadic existence suited his personality. Writing to his brother Bela from London he told him 'I intend to stay in a hotel. I do not intend to rent a house; I like mobility'. He was 35, free, and believed that he had enough savings to last a year without a steady income. There followed an intense period during which he bent the ears of a wide range of senior scientists, academics and politicians, lobbying to create safe havens for other scientists who were being forced to leave Germany. His efforts underpinned the creation of the Academic Assistance Council (AAC) in May, and he helped to organise its office in London as a clearinghouse to match refugees to offers of positions in universities and industry. By the autumn, the AAC had a list of over one thousand refugees from German universities, seeking placements in Britain.

Busy helping others, Szilard remained uncertain of his own future. He gathered testimonials, from von Laue, Wigner, Volmer and Ehrenfest. Schrödinger considered that Szilard was 'an absolutely trustful and altruistic person'. For Einstein he belonged 'to the group of people who create an intellectual environment for others'. Szilard considered opportunities at Liverpool and Manchester, with Bose at Dacca and at University College in London (UCL). In truth, his academic CV was thin, and he had not published anything of significance since his two papers on entropy in 1925 and 1929, developments of his 1922 doctoral thesis. He thought about changing discipline, receiving an offer from Archibald Hill to be a demonstrator in physiology at UCL.

¹ Several details are taken from William Lanouette's well-documented biography of Szilard, *Genius in the Shadows*, Macmillan 1992.

It was Ernest Rutherford's lecture at the 1933 British Association meeting in Leicester, given with Einstein sitting at his side that redirected Szilard's thoughts back to physics. Suffering from a cold, he was not able to attend the meeting himself, but read a summary of Rutherford's lecture on atomic transformations in *The Times* on 12 September and in *Nature* on 16 September. *Nature* reported:

*'Beryllium, of mass 9 and charge 4, when bombarded, captures an α -particle of mass 4 and charge 2, giving rise to a structure of mass 12 and charge 6 and emitting a neutron of mass 1 and charge zero. It is not difficult to picture the changes which ensue when neutrons are fired into oxygen or nitrogen with the consequent emission of an α -particle, and indeed it is certain that future experiments will show that the neutron is a very powerful weapon of research.'*²

Szilard had spent his last years in Berlin lecturing on, and speculating about, future directions in physics. The report on Rutherford's lecture served to remind him of a world that he had temporarily relinquished. In particular, it led him to the thought that an atomic nucleus, split by one neutron, might in the process release two, so initiating a nuclear chain reaction. He later recalled that this particular thought occurred to him as a traffic light on Southampton Row turned green, a detail that adds colour to the story, but without apparent significance.

Szilard tested the idea of a nuclear chain reaction on a couple of London scientists, Patrick Blackett, then working at Birkbeck College, and George Thomson at Imperial, but neither responded with any enthusiasm. Fritz Lange, a physicist friend from Berlin, visited London in December. Together they visited the General Electric (GE) research laboratory at Wembley, where Lange was interested in comparing his own work with Arno Brasche on very high voltage discharges with progress at GE. This first link with British industry set Szilard in a new direction, deciding to patent for his ideas first, and then approach commercial partners. On March 12 1934, he filed his first patent application for a nuclear chain reaction with the UK Patent Office. Taking his lead from Rutherford, Szilard named beryllium as the chosen element to sustain a nuclear chain reaction, although also identifying uranium and thorium as possible alternatives.

Eugene Wigner, another of the brilliant German-educated Hungarian/Jewish physicists, who had by this time secured a half-time lectureship at Princeton, visited Britain in the spring of 1934. Together, they went to see Rutherford in the Cavendish Laboratory in Cambridge. Rutherford had

² A.F. Nuclear transmutation. *Nature* Sept 19 1933;132(3333):432-433.

become the president of the AAC when it was formed in May 1933 and strongly promoted the accommodation of German refugee scientists. Szilard was now in urgent need of a post himself as his savings dwindled. It appeared to be a perfect match. Szilard proposed yet another experiment, this time for an alpha-particle chain reaction. Not only was Rutherford unimpressed with this idea, as with his proposals for improved particle accelerator design, he also took a dislike to Szilard, particularly when he learned that he was promoting his ideas through patents. Moreover, as David Wilson's biography notes, Rutherford hated the 'impressionistic mode of research of men who "sprayed out ideas" but left it to others to pick them up and work them out'. Whilst this was used to describe JD Bernal it equally applied to Szilard, whose subsequent letter asking to work at the Cavendish remained unanswered. Whilst Rutherford was pleased to recognise, in a letter to the Times, that recent political revolutions had displaced scientists 'whose talent and experience could be effectively used', he regretted that, 'To incorporate the services of these wandering scholars is more difficult today than in the Middle Ages when the 'community of learners' were less hampered by administrative formalities, restrictive endowments, and incipient nationalist tendencies'. These were convenient loopholes to select only those with amenable personalities.

No matter how fertile his imagination was, Szilard's separation from experimental colleagues was becoming a severe limitation. He came up against a similar brick wall when trying to persuade his new contacts at General Electric to take an interest in investing in his 'rather romantic enterprise', but was repeatedly rebuffed, even after having submitted to them a 'Memorandum of possible industrial applications arising out of a new branch of physics'.

The work of Fermi's group, and of Joliot-Curie, had demonstrated that an array of new radioactive isotopes could be created when stable elements were bombarded by alpha particles. Szilard speculated that if this could happen with alpha particles it might also be possible with high-energy photons. In March, he wrote to Lange suggesting that he could use his 2MV X-ray system to irradiate each element in turn to look for evidence of induced radioactivity, the X-ray analogy of Fermi's experiments. Now the door to the Cavendish had been so firmly closed, Szilard had no way of testing his ideas in Britain.

An opening at last

Finally, in July, he made progress in a most unlikely manner. In a letter to Harry Railing at GE on July 20, Szilard was able, for the first time, to refer to his own work on medical radionuclides. He had finally gained agreement

to use radium in a physics laboratory in London, which offered the possibility of testing some of the ideas that were whirling in his mind.

But why seek radium? Szilard had an agile and imaginative mind. The newspaper headlines on 5 July announced the death of Marie Curie, a name forever associated with radium and, moreover, with its medical use. Historians should not speculate, but it is tempting, nevertheless, to imagine that the reports of Marie Curie's death triggered new connections in Szilard's fertile imagination, that led him away from seeking science in industry or in a university laboratory, and to find physics in a medical school. He had already proposed testing new radionuclide production using high energy X-rays. Perhaps he could test his ideas initially using the much lower photon flux from radium.

By the 1930s, radium therapy had become an established therapy in the battle with cancer. Clinical trials, such as the multi-centre study into the treatment of cervical cancer by the London Association of the Medical Women's Association that led to the opening of the Marie Curie Hospital in Hampstead in 1929, set standard methods of treatment and included a physicist as a recognised part of a multi-disciplinary clinical team. Radium was still extremely expensive, however, in spite of the discovery of rich deposits in the Belgium Congo, a source that broke the previous near-monopoly of United States. The *Union Minière du Haut Katanga*, Belgium, had started production in December 1922 and soon became the main world supplier of processed radium, successfully supporting a rapidly growing medical demand.³

In March 1929 the Radium Sub-Committee of the British Committee of Civil Research, under the chairmanship of Robert Strutt, 4th Baron Rayleigh, had reported that only about 25 g was available for medical use, half the estimated national requirement. Most of this stock had been privately purchased, and less than 10% belonged to the government, recovered from the illuminated gun-sights and instruments of military aircraft after the end of the war, and placed in the custody of the Medical Research Council. These were the days of private medicine, before the establishment of the NHS. The purchase of radium offered a rich benefactor considerable scope for personal gratification and public thanks. Thus, many medical establishments found themselves in receipt of gifts of radium, private funding making available much more radium than the government scheme. The national distribution was very uneven, some well-endowed medical schools in London were awash with radium while provincial

³ The medical use of radium comprises about half of the content of a handbook published c. 1929 by the Haut Minière du Haut Katanga.

hospitals had limited stocks. In addition, academic laboratories were struggling to obtain even small amounts for experimental studies. Even Rutherford had to go cap-in-hand to his old student Sidney Russ, professor of physics at the Middlesex Hospital Medical School, to request the release of 1 g of radium sulphate for physics research. Russ was the leading radium physicist in London and had been appointed scientific secretary of the Radium Commission from its inception.

So there remained, for Szilard, only the need to identify someone with the authority and interest to accept him into their medical laboratory, willing to cut through the administrative formalities and who did not share any ‘incipient nationalist tendencies’. We do not know whether Szilard approached Russ or not. In any case, any approach that may have occurred coincided with a substantial hiatus in Russ’s domestic life, during which his wife Mary developed an obsession for the artist and writer Wyndham Lewis.⁴ It would have been difficult or impossible for Russ to give any serious attention to Szilard’s ideas and plans. However, given Szilard’s network of contacts, it would have been easy for him to identify the one other physics professor in a London medical school, Frank Lloyd Hopwood at the medical school of St Bartholomew’s Hospital (Bart’s) at Smithfield. By the end of July 1934, Szilard had gained the agreement of Hopwood to work in his laboratory.

Frank Lloyd Hopwood

Frank Lloyd Hopwood (1884-1954) was the son of a Welsh mining engineer from Flintshire, graduating in 1905 with a creditable second-class physics degree from the University of North Wales at Bangor. Ambition, or adventure, took him to London, where he soon secured a post as demonstrator in physics at St Bartholomew’s Medical School, commencing a lifetime applying physics to medicine.

Physics had become a mandatory component of pre-clinical medical training in 1895. Most universities responded by inviting senior physicists to teach medical students. JJ Thomson, Ramsey, Schuster, Lodge, Larmor, Lord Kelvin, all taught physics in medical schools at this time.⁵ On the other hand, the London medical schools, by and large, assigned physics teaching to one of the medical lecturers. St Bartholomew’s Hospital Medical School appointed Frank Womack MD, who had a scraped a 3rd

⁴ Duck F. The Radium Boss. The life and times of Sidney Russ (1879-1963). IPEM Scope. 2020;29(2):32-35. On-line at ipem.ac.uk

⁵ Duck FA. Physicists and Physicians: A history of medical physics from the Renaissance to Röntgen. IPEM York. 2013.

class degree in experimental physics in 1880. When Hopwood arrived, he was expected to deliver the necessary lectures, manage the laboratories, but no more. Womack was never motivated to create any career opportunity for this young physicist, especially one without a medical degree. By 1914, now supporting his young wife and daughter in a terrace house in North London, Hopwood's career was in the doldrums. Lecturing gave little time for research, and he had no scientific sponsor. On his own initiative, perhaps stimulated by interest in the new Coolidge X-ray tubes, he started to investigate the physical and electrical behaviour of heated filaments. This was soon followed by research on X-ray crystallography with William H Bragg, Quain Professor at UCL.

Through Bragg, Hopwood was recruited in 1916 to work at the Admiralty Experimental Station, perfecting a design for a practical, ship-borne, directional hydrophone to detect submarine engine noise. On his return to Bart's in 1919, Womack was on the point of retirement, and Hopwood was appointed as physics lecturer. He set about replacing Womack's mediocre, unimaginative physics department into one appropriate for a revived, increasingly technological, medical service.



Figure 1: Frank Lloyd Hopwood. The hanging threads from the source suggests that it consists of multiple radium capsules. Hopwood appears to be counting scintillations. This experiment is not described in the literature.

In 1924 Hopwood was appointed by the University of London as Professor of Physics at Bart's Medical School. His mind was open to any new and emerging technology that could be applied to medicine. In his 1933 Presidential Address to the British Institute of Radiology he cited high-frequency RF therapy, thermography, X-ray microscopy, artificial radionuclides, and MV X-rays as examples of such technologies. His own work on the biophysics of ultrasound, stimulated by reports of Paul Langevin's wartime developments in submarine detection, was the first in Britain.⁶ Szilard's ideas about neutrons opened yet another exciting area for investigation.

Nevertheless, the main focus for physics in medicine was radium therapy. When Hopwood took over at Bart's he inherited just three radium sources. By 1929 he was the radium custodian in charge of over 1000 mg radium, worth at the time about £15,000. Half the activity was in solution and the rest in the form of radium sulphate contained in several hundred needles and capsules made of platinum or platinum-iridium alloy, ranging in content from 50 mg to 0.5 mg. By the time of Szilard's visit, most sources had 1.0 mm platinum filtration, sufficient to stop all alpha emission and most of the betas also. Managing its use needed strict safety rules, and Szilard became adept at dealing with strong-minded medical colleagues who thought they knew best. Even so he was unable to prevent a number of sources from being damaged by careless handling. There was frequent transport of sources during the day: 36 % of cases were outpatients, typically skin lesions. But there was a strict rule that, at the end of each working day, any radium not in clinical use must be returned, to be inspected, recorded and locked in the night safe in the Curator's room overnight.

Szilard at Bart's

Szilard had never experienced such a professionally managed department before, and it was immediately apparent that there was going to be a clash between his expectations and the needs of the hospital service. He had imagined that he could access radium as he wished, working late into the night when spurred to do so. Hopwood, now 50 years old, had plenty of experience in dealing with the large egos and focussed obsessions of senior doctors. He may have been non-confrontational but he was also remembered as being 'a big man in all ways, in mental and physical stature, voice, 'presence' ' and one suspects that it was Hopwood, not Szilard, who got the better of this argument. Furthermore, remembering his own

⁶ Hopwood FL. Ultrasonics: Some properties of inaudible sound. Nature Oct 31 1931. 748-751.

struggles to establish a CV, Hopwood insisted that Szilard must involve his staff, and must publish in the open literature, in spite of Szilard's 'absolute refusal of publishing on a subject he has not really thoroughly studied in all its details'.⁷ Under these conditions, Hopwood agreed to release up to 150 mg radium for Szilard's experiments, perhaps 10% of the stock he now held. He left his new guest in the capable hands of a practical and competent young colleague, Tim Chalmers, and went off on holiday.⁸

Szilard was looking for a simple means of releasing neutrons in the laboratory and believed that high-energy gamma photons could act in the same way as alphas to release neutrons from beryllium. He also wanted an easy way to confirm that neutrons had been released. The experiment they devised was simple and elegant. Several radium needles were enclosed in 25 g beryllium to create neutrons. Neutrons were confirmed by inducing radioactivity in iodine in the form of 100 ml ethyl iodide encasing the beryllium. Radioactivity was confirmed using a Geiger counter.

Where did the materials apart from radium come from? Beryllium is not a material associated with a medical laboratory, and one may assume that Szilard managed to borrow this from one of his contacts. The ethyl iodide was probably in Hopwood's laboratory. By this time, iodine was well established as a contrast agent for medical X-ray studies, for arteriography and renal investigations. Reginald Payne, a young Bart's surgeon, was investigating new organic iodinated contrast agents for urography for his MD at this time. Whilst ethyl iodide was unsuitable itself, it is the chemical from which several contrast agents could be made, and it would not be surprising if Hopwood's department just happened to have some in a bottle.

On 29 September 1934, Szilard and Chalmers announced, in a letter to Nature,

*'We have observed that a radiation emitted from beryllium under the influence of radium gamma rays excites induced radioactivity in iodine, and we conclude that neutrons are generated from beryllium by gamma rays'*⁹

They were not, just, the first to report induced radioactivity by photons. Only a month before, Chadwick and Goldhaber had reported atomic disintegration from gamma radiation when they exposed deuterium (which

⁷ Lanouette p 126 quoting a letter from Schrödinger to Frederick Donnan at UCL.

⁸ Chalmers had been recruited from National Physical Laboratory in 1932, where he had spent five years after graduating from Battersea Polytechnic.

⁹ Szilard L, Chalmers TA. Detection of neutrons liberated from beryllium by gamma rays: a new technique for inducing radioactivity. Nature 1934;134:494-495.

they named diplon) to gamma rays from thorium C. But, as Szilard and Chalmers pointed out, neutron emission could only be inferred in this case from the detection of emitted charged nuclei.¹⁰

Szilard had written to Hopwood on 28 August while he was still on holiday. By this time they had measured the induced radioactivity, but were challenged by how to separate the radioactive from the stable iodine isotope. In his letter to Hopwood he speculated ‘whether we can separate the radioactive Iodine from the bulk of the bombarded Iodine’ adding ‘this sounds ‘blasphemous’ to the chemist, but I hope it can be achieved’. Mark Oliphant’s electromagnetic method for the separation of lithium isotopes had been submitted for publication in June, and Szilard may have learnt of it during his earlier abortive attempts to interest Rutherford in his ideas, but it was not published until October. Anyway, Szilard sought a simpler method.

The separation method was published in a letter to Nature on 22 September.¹¹ They recognised that recoil following neutron capture would also displace the iodine from its molecular bonds, allowing free iodine to be precipitated as ¹²⁸I silver iodide, leaving the stable ¹²⁷I as ethyl iodide. This process of separation became known as the Szilard-Chalmers reaction, and later became one method used in the separation of cyclotron-generated radioisotopes.

The authors ended with the prediction that ‘it will be possible to have very much stronger sources of neutrons and to produce thereby larger quantities of radioactive elements by using X-rays from high-voltage electron tubes.’ Szilard had sourced ‘gamma rays of sealed radium containers, which are available in many hospitals for therapeutic purposes’. Now he needed to persuade someone to lend him their X-ray equipment. Hopwood was not in a position to help at Bart’s, at least not in 1934. In his Presidential address to the Congress of the British Institute of Radiology in Central Hall Westminster the previous year he noted that the NPL had a million-volt installation and that ‘radiologists all over the world have been hankering over’ this type of equipment. However, he said, ‘If we tried to install such an apparatus at one of our hospitals it would mean that the hospital would become an adjunct to the X-ray department!’. He spoke too soon. By 1937, Bart’s became the first hospital in Britain to operate a very high voltage X-

¹⁰ Chadwick J, Goldhaber M. A ‘nuclear photo-effect’ : Disintegration of the diplon. Nature Aug 18 1934;134:237-238.

¹¹ Szilard L, Chalmers TA. Chemical separation of the radioactive element from its bombarded isotope in the Fermi effect. Nature 1934;134:462.

ray unit, with a huge, continuously-evacuated tube powered by a Cockcroft-Walton generator. But, in 1934, Szilard needed to look elsewhere.

Berlin

Lisa Meitner was one of a number of German scientists with whom Szilard had worked before moving to England, and was one of the few scientists who recognised the importance of the Szilard and Chalmers papers. She immediately acted on this new insight, and by October had submitted her own results for publication in *Naturwissenschaften*.¹² She found that the gamma+beryllium neutrons were captured by iodine, silver and gold but not by lighter elements such as sodium, aluminium or silicon. This led her to conjecture that neutrons emitted by photon capture were of lower energy than those released by alpha particle bombardment.

The next letter in *Nature* from Bart's, submitted on 26 November and published on 8 December, acknowledged Meitner's 'kind assistance in the Berlin experiments'.¹³ Lange's visit to London in the spring had given Szilard a clear idea of his work with Arno Brasche on high energy X-rays in the *Allgemeine Elektrizitäts-Gesellschaft* (AEG) High Tension Laboratories in Oberschönweide, Berlin.¹⁴ They had developed a discharge tube containing alternating conducting and insulating rings which could be operated at voltages up to 2.4 MV, pulsed up to 0.1 ms with maximum currents of 1000A. Szilard persuaded the German scientists to confirm his prediction that high-energy X-rays could replace radium gammas to release neutrons from beryllium. Working with an Egyptian colleague, Adnan Waly, they exposed bromoform, an organic bromine compound, with X-rays from the tube operated at between 1.5 MV and 2.0 MV. The solution was then sent by air to Bart's where weak activity of a radioisotope of bromine with a 6-hour half-life (⁸²Br) was detected. This initial experiment was followed up by further exposures of both bromine and iodine solutions in which much higher activities were achieved. This was accomplished after Meitner had received a preprint of Fermi's latest paper at the end of October showing the considerable increase in activity that arose when the neutron-

¹² Sime RL. *Lise Meitner: A life in physics*. University of California Press. 1997. 166.

¹³ Brasch A, Lange F, Waly A, Banks TE, Chalmers TA, Szilard L, Hopwood FL. Liberation of neutrons from beryllium by means of X-rays: radioactivity induced by means of electron tubes. *Nature* Dec 8 1934;134:880.

¹⁴ Brasche A, Lange F. Experimentell-technische Vorbereitungen zur Atomzertrümmerung mittels hoher elektrischer Spannungen. *Z Phys* 1931;70(1-2):10-33.

irradiated material was surrounded with substances containing hydrogen.¹⁵ This approach, slowing the neutrons to increase the probability of capture, was immediately used in the Berlin experiments, resulting in a considerable increase in radioactivity. Measurements were carried out using an electroscope both in Bart's and in the Kaiser Wilhelm Institute of Chemistry in Berlin, where the co-operation of Kurt Philipp, Meitner's chief assistant and Otto Erbacher, Otto Hahn's chief assistant, were noted.

All this was being carried out at a time of considerable political turmoil in Germany. All three AEG scientists left Germany shortly thereafter. Lange went to the USSR the following year. Brasche emigrated to the USA in 1936 to work at the Jewish Hospital at Brooklyn. Waly returned to Egypt. Lisa Meitner stayed longer: her dramatic escape to Sweden via Holland in the summer of 1938 is well known. Philipp, an active Nazi party member, was promoted to fill the vacancy she left.

In a matter of three months, Szilard's work with the Bart's physicists had demonstrated the release of neutrons from beryllium using high-energy gamma radiation from radium and high-energy x-radiation. They had used these neutrons to create radioisotopes of iodine and bromine, confirming the importance of hydrogenated materials to increase the probability of neutron capture. And they had reported a new chemical means to separate the radioisotope of iodine from its stable parent.

What happened next

Szilard continued to use the Bart's laboratory until the end of the year, but there was less overlap now between his motives and those of the Bart's team. In one final experiment he worked with Chalmers to investigate multiple isotopes induced by neutron irradiation of indium.¹⁶ By contrast, in the same supplement to *Nature* on 19 January, a second letter was published on the Bart's neutron work but without Szilard's co-authorship. Hopwood had involved another young colleague, Tim Banks, suggesting that he should repeat the earlier radium irradiations, but substitute heavy water for beryllium, because of Gilbert Lewis' belief in its biological importance.¹⁷ Radioactivity was induced in both ethyl iodide and bromoform.¹⁸

¹⁵ Fermi E, Amaldi E, Pontecorvo B, Raasetti F, Segré E. Azione di sostanze idrogenate sulla radioattività provocata da neutroni I. *Ric Sci* 1934 5(2) 282-283.

¹⁶ Szilard L, Chalmers TA. Radioactivity induced by neutrons. *Nature* (suppl) 19 Jan 1935;135:98.

¹⁷ Lewis GN. The biology of heavy water. *Science* 16 Feb 1934;79(2042):151-153

¹⁸ Banks TE, Chalmers TA, Hopwood FL. Induced radioactivity produced by neutrons liberated from heavy water by radium gamma-rays. *Nature* (suppl) Jan 19 1935;135:99.

Hopwood had provided the disciplined environment that allowed Szilard's natural volatility to briefly crystallise. But Szilard was a bird of passage. He had now gained all he needed from Hopwood and his colleagues, and moved on. Indeed, the success of the experiments in Hopwood's department finally gave him the credibility he sought in Britain. His next port of call was the Clarendon Laboratory in Oxford, where he worked on neutron absorption and scattering.

Meanwhile, interest was awoken in the possibility of clinical applications of neutrons. Only two years earlier, in the 1932 Mackenzie Davidson memorial lecture to the British Institute of Radiology on 'The Neutron and its Properties', James Chadwick had said that the neutron 'has at present no professional applications for the radiologist', possibly wondering why he had been invited to address a group of medical men on the subject. Now, Hopwood's team were being presented with a huge opportunity for new medical research. They now knew of two methods to generate neutrons, one using radium, which they could exploit immediately, and one using X-rays that might be available to them in a few years. Might neutron therapy find a place in the growing range of physical therapeutic agents? If so, how might it best be exploited? And, in addition, they had identified one means by which an artificial radioisotope might be separated from other isotopes of the same element, and this could lead to the development of both therapeutic and tracer uses of radionuclides. Neutron therapy and medical uses of artificial radionuclides did indeed develop, but slowly, and with a number of false starts. Nevertheless, stimulated by these first steps, Hopwood's team started to explore neutron physics in a medical context.

Chalmers investigated how neutrons might be harnessed for therapeutic work. Before Szilard departed he had suggested that it might be possible to exploit the reduction of neutron speeds by using hydrogenated materials as a method of directional control. This they proceeded to investigate.¹⁹ For this work they also produced neutrons by the action of alpha particles on beryllium, sealing 100 mCi (3.7 GBq) radon in a glass tube with powdered beryllium. Slow neutrons were produced by enclosing the neutron source in paraffin. These were then emitted into an air-filled channel surrounded by wax, with a target at the end of silver, rhodium or iodine. After a few minutes exposure the targets were removed to measure their activity with a Geiger-Müller tube. Chalmers was able to demonstrate that thermal neutrons diffused down the air-filled cavity more readily than in free air or in a carbon cylinder. The work constituted a very early investigation into the exploitation of neutrons and, whilst this device never led on to any

¹⁹ Chalmers TA. Canalisation of neutrons – I. *Br J Radiol* 1935;8(87):163-169.

useful clinical application, it demonstrated an eager wish to find a place for neutrons in therapeutic medicine. Hopwood also continued to explore the biophysical properties of neutrons, with J.T. Phillips, for several years. During the next decades numerous centres explored the potential of neutron therapy. However, whilst there remain today a few specialist centres, neutron therapy has never offered serious advantages over treatment with ionising radiations, and has largely disappeared from use. The most serious contender emerged in the form of boron capture therapy, which aims to exploit the very high neutron absorption of thermal neutrons by targeting cancer cells with a boron compound before irradiation.

Postscript

I am writing this article at a time when the world remembers the first use of an atomic bomb to destroy a civilian target, 75 years ago. In 1938, Szilard emigrated to the USA. His voice was one of the strongest to warn the authorities that Germany must be creating an atomic weapon of mass destruction, a belief that was later discovered to be unfounded. With his compatriot émigré Edward Teller, he was responsible for drafting Einstein's persuasive letter to President Roosevelt that initiated the Manhattan project to create an atomic weapon in the USA. In 1945 he argued in favour of a demonstration of the bomb on a Japanese military target.

Tim Chalmers stayed in medical physics. He left Bart's the same year as Szilard went to America, to become the first physicist to be appointed at the Liverpool Radium Institute, where a large stock of radium had been bought as the result of a local appeal. He remained active with the medical use of radionuclides as they became increasingly available from cyclotrons and reactors after the war. In particular, he published on ^{40}K as a calibration source and helped to introduce ^{24}Na as a tracer.

Hopwood retired in 1949. His replacement, Joseph Rotblat, was, like Szilard, a refugee physicist who had been recruited to work on the Manhattan Project. Unlike Szilard, Rotblat turned his back on atomic weapons. He had been recruited by James Chadwick, in spite of American security concerns about his Polish citizenship and claims that he was a communist sympathiser. Rotblat stayed less than a year at Los Alamos, returning to Liverpool in December 1944 having learned that there was no threat from a German atomic bomb after all. By 1948, back in the George Holt Physics Laboratory at the University of Liverpool, he was seeking a way to devote his deep knowledge of atomic physics for the direct benefit of mankind. His first medical collaboration was with a young radiologist, George Ansell, with whom he introduced the diagnostic use of ^{131}I for goitre (abnormally enlarged thyroid) in patients where the mass was hidden

behind the sternum.²⁰ By this time they could list eleven known radioisotopes of iodine, of which ^{131}I was the most widely available, although still imported to Britain from the USA by the National Institute of Medical Research. Nowadays, ^{131}I is no longer used diagnostically, but remains important in the treatment of overactive thyroid and for thyroid cancer. ^{123}I is used diagnostically, most commonly for the investigation of Parkinson's disease, tagged to the cocaine analog Ioflupane and also for thyroid imaging and neuroendocrine investigations.²¹

Tim Chalmers' name is listed among those whose assistance was particularly acknowledged by Rotblat and Ansell. No doubt Chalmers was also able to encourage Rotblat to apply for the Bart's chair, established for Hopwood 25 years before. Szilard had used his knowledge of atomic physics to encourage the development of nuclear weapons. By contrast, Rotblat had chosen to use his to the benefit of medical science.²²

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²⁰ Ansell G, Rotblat J. Radioactive iodine as a diagnostic aid for intrathoracic goitre. *Br J Radiol* 1948;21:552-558.

²¹ I am grateful to Martyn Evans, from the Medical Physics Department Royal United Hospital Bath NHS Trust, for this information.

²² Joseph Rotblat became a leading scientist in the Pugwash Conferences, sharing in the 1995 Nobel Peace Prize for their efforts on nuclear disarmament.

1820: Ampère's "failed revolution"?

Jim Grozier

Two hundred years ago, in July 1820, Hans-Christian Oersted discovered the magnetic field due to an electric current. Shortly afterwards, André Ampère demonstrated the force between current-carrying wires, and announced that a magnet was "only an assembly of electric currents".

Now, in the 21st century, that explanation of the action of a magnet is part of mainstream scientific understanding. We might, therefore, be tempted to view Ampère's statement as evidence of a "scientific revolution" that occurred in 1820, along the lines of Thomas Kuhn's description of the progress of science as occurring through a series of revolutions.

The hallmarks of Kuhn's model do indeed seem to be present. He described the early years of a science, before a consensus emerged as to how the observed phenomena should be explained, as a "pre-paradigm period". Eventually, consensus is achieved in the form of a *paradigm* – a "coherent tradition of scientific research". There follows a period of what he called "normal science", in which scientific work takes place within that tradition. Eventually a crisis occurs when a discovery is made that can't be explained in terms of the paradigm. An alternative paradigm may then be proposed, which can explain the observed phenomenon, and may also make further predictions. The new paradigm will typically represent a completely different way of describing the phenomena, so that the old paradigm and the proposed new paradigm are simply *incommensurable*. There is then a revolution, in which the new paradigm may replace the old, or it may be discarded; the outcome is contingent on all kinds of factors that exist inside and outside the science itself, and may simply be decided on the basis of "who shouts loudest", or in other words, whoever possesses the greater power of persuasion. There may be "Kuhn losses" – in other words, there may be some phenomena which are harder to explain under the new paradigm than the old.

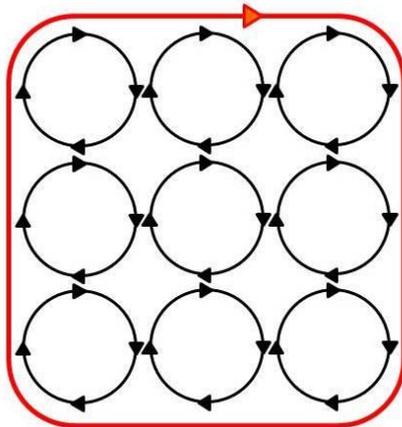
Following the revolution, the science settles down into another period of "normal science", now under the new paradigm, until the next crisis and the next revolution.

So far, so good, so Kuhnian. The old idea of magnetism as due to poles was replaced by Ampère's theory of molecular currents. The problem is that this

revolution, if revolution it was, took rather a long time. How long? Well, in order to answer that we must have some way of judging when the revolution ended. One obvious criterion is to look for the date when the new paradigm enters the textbook. And here we may be surprised; for it would seem that the textbook explanation of magnetism only changed fairly recently, when viewed on a timescale of two centuries. Personally, I can remember (just!) being taught physics at school in the 1960s and being introduced to magnetism in terms of a permanent magnet, which, we were told, consisted of a pair of poles; like poles repel, unlike poles attract. We drew “lines of force” representing the direction in which a unit pole would move when placed at a point near the magnet – in other words, the direction of the magnetic field at that point. If we were lucky, we would be shown the pattern produced by a magnet held under a sheet of paper on which iron filings had been scattered.

The written record bears this out: Morley & Hughes’ *Principles of Electricity* (published 1958) and Nelkon’s *Advanced Level Magnetism and Electricity* (1961) both introduce magnetism as a property of magnets. You have to go several decades further on before the idea of magnetism as primarily due to electric currents becomes the default – see, for instance, Grant & Phillips’ *Electromagnetism*, published in 1990, which is typical of degree-level textbooks in that it describes permanent magnetism as due to “magnetisation currents” which are macroscopic “currents” produced as a result of the alignment of the atomic dipoles.

Fig. 1: Magnetisation currents. Individual atoms are shown with their electron “orbits” represented as circular currents (black). These atoms are aligned (there are a few more than 9 of them!) The atomic “currents” cancel everywhere except on the surface, producing the red line. If the magnetisation is non-uniform, there may also be macroscopic “currents” inside the material.



So, what took the physics community so long? Well, according to Kuhn we mustn't necessarily think of a revolution as something that happens all that quickly; after all, he based much of his theory on "the" Scientific Revolution, which could be said to have lasted from Copernicus in 1543 to Newton in (say) 1666 – well over a century. Nevertheless, we still need an explanation of why the adoption of the "current" paradigm was so slow.

A clue can be found in Sir Edmund Whittaker's classic *History of the Theories of Aether and Electricity*, in which the author contrasts James Clerk Maxwell's eulogising of Ampère's theory with a somewhat different response to it from Oliver Heaviside in 1888:

"It has been stated, on no less authority than that of the great Maxwell, that Ampère's law of force between a pair of current elements is the cardinal formula of electrostatics. If so, should we not be always using it? Do we ever use it? Did Maxwell in his Treatise? Surely there is some mistake" [Whittaker p88].

The confusion is compounded, to the modern reader, by the fact that most of us, in our studies or in our work, will have encountered a formula known as "Ampère's Law" which is actually a completely different law, being the relationship – often expressed in the symbolism of vector calculus – between an electric current and the *magnetic field* it produces. That's odd, because, as you will recall, the magnetic field is defined in terms of the distance from a magnetic pole, yet the whole thrust of Ampère's argument was that we don't need to think in terms of poles at all, because magnets can be represented by currents. Yet this law is so famous that it has even had a song¹ written about it, describing how Ampère first wrote it down and later Maxwell modified it by adding the displacement-current term.

For Oliver Darrigol, however, there is no mystery here, because he tells us that, in fact, "*Maxwell* was the first to enunciate this result, which is *improperly* called the Ampère law (or theorem) ... I avoid the expression "Ampère's law", which is even more misleading" [my emphasis] [Darrigol p142]. And this should come as no surprise to the seasoned historian of science, as one of the first things one learns when delving into the subject is that we should not assume that a law, or a unit, or anything else, that bears the name of a particular scientist is actually anything to do with that

¹ Broadside Electric, *Ampère's Law Song*:
<https://www.youtube.com/watch?v=XyrulQOC82g>

scientist. Heaviside, in fact, was all in favour of naming *another* law after Ampère, namely that “expressing the mechanical force on an element of a conductor supporting current in any magnetic field” – which we would nowadays think of as the Lorentz Force Law, after Hendrik Lorentz – a contemporary of Heaviside, who clearly lost that argument. And just to compound the confusion, the aforementioned textbook by Nelkon names a Law of Magnetic Intensity after Ampère, which, since it concerns the magnetic field at a point a distance r from a current i , would probably nowadays conjure up the names of Biot and Savart rather than that of their hapless contemporary.

Does Heaviside’s confusion suggest that Ampère’s ideas had fallen into disuse, along with his formula, in the intervening 60-odd years? Did Ampère’s revolution, then, “fail”? And if so, why? Well, remember Kuhn’s comments on the various factors governing the success or otherwise of a scientific revolution – eloquently summed up by Imre Lakatos as “a bandwagon effect ... in Kuhn’s view scientific revolution is irrational, a matter for mob psychology” [Lakatos & Musgrave p. 178].² The “mob” in this case included contemporaries of Ampère, such as Jean-Baptiste Biot and Michael Faraday. Faraday, of course, had his own ideas about magnetism: not so much its *source*, as how its effect was transmitted, apparently across empty space. He explained this in terms of “lines of force” which permeated the aforementioned empty space, thus getting round the tricky problem of “action at a distance”.³ The idea persisted; even today, astrophysicists talk about such things as though they were real physical entities.⁴

² Sophie Osiecki disputes this charge of irrationality, however [Osiecki p100].

³ Faraday, despite being a giant of 19th century physics, and having two units, a law and a cage named after him, did not have it all his own way as regards lines of force. One dissenter was the Astronomer Royal, George Biddell Airy (who could only claim a ring and a disc), who was quite happy with “action at a distance” and found Faraday’s lines of force “vague”. [see Whitaker p 172]. Personally I am with Airy here: in a quantum world, how can there be any action that *isn’t* at a distance?

⁴ “The moving medium avoids crossing the field lines by dragging them along with it” [Open University astrophysics textbook, 2002]. Anyone for teleology?

Other giants of 19th century physics were busy with magnetic problems that didn't lend themselves to the concept of magnetisation currents. Poisson was preoccupied with correcting the errors of navigational compasses in ships containing iron, "a problem of increasing economic and military significance" at the time, as Norton Wise points out; while Gauss was busy "mapping the earth's magnetic field over its entire surface" [Wise, in Olby *et al.*, p345]. And so, by the time Heaviside came along, and despite the undoubtedly true claim in the song that Maxwell "thought of Ampère as a saint", the die was cast.

One problem with Ampère's idea of "molecular currents" was, of course, that at the time no-one had much idea of what atoms and molecules actually consisted of. It was not until the emergence of the Bohr atom in the early 20th century that the idea began to resonate. Soon after that, Einstein found experimental evidence of Ampère's currents and some time later, they duly found their way into the textbooks.

End of story? Well, no, actually. Because those lines of force, and those magnetic fields, which plot the course of a non-existent unit pole in a magnetic field, are still very much with us. The \mathbf{B} and \mathbf{H} vectors still point in the direction of the force on such a non-existent pole, and require physicists to make potentially rude 3-fingered gestures in order to work out the direction of the force on things that do actually exist, such as currents. And, while, as all the textbooks tell us, magnetic problems can be solved entirely in terms of currents, it is still very much easier to perform complex magnetic calculations by using the boundary conditions on those fields, rather than modelling magnetic materials as currents – something I discovered, the hard way, when working on my PhD, having tried in vain to do this in Ampère's paradigm. And, to be honest, this should come as no surprise to the reader; because we are quite used to holding several mutually contradictory theories in our heads at once, and using whichever one appears appropriate to the task in hand. Think, for instance, of the constant-acceleration "suvat" equations we all learnt at school to solve problems involving projectiles; think of Newton's theory of gravity. Both have supposedly been consigned to the dustbin of history, and we are supposed to say that our "best" theory is general relativity. But, to quote Heaviside out of context, unless we happen to be cosmologists, do we ever actually use that theory?

The Kuhnian may object at this point that electromagnetism is just a bad example – perhaps the exception that proves the rule. Fair enough – let the

Kuhnian choose her favourite theory and demonstrate how it works, in that case. The “favourite theory” here is likely to turn out to be the phlogiston theory, overthrown during the Chemical Revolution of the late 18th century. This is, if anything, a “textbook” Kuhnian revolution, and one on which Kuhn based many of his own ideas. We are supposed to be convinced by this example, because phlogiston is as dead as a doornail, isn’t it, and deservedly so, having held back the progress of science for so long. But wait! Here is Hasok Chang on the phlogiston theory:

“On balance, in all honesty, I cannot see that there were good enough reasons for a decisive rejection of the phlogistonist system. I am convinced that the death of phlogiston, however slow it might have been, was premature” [Chang p44].

He goes on to point out that several 18th century scientists “postulated a close relationship between phlogiston and electricity” and one of them, John Elliott, even proposed that phlogiston should be re-named *electron* – and that was in 1780, many years before even Dalton’s atomic theory, and over a century before the discovery of the particle that adopted that name. Chang’s point is that, if phlogiston had not been abandoned when it was, recognition of this link could have led to a much earlier discovery of the photo-electric effect, and hence actually accelerated the progress of science in the 19th century. That would not have been to the detriment of our understanding of the role of oxygen or the structure of water, because Chang is not arguing against Lavoisier’s version of events but simultaneously embracing both Lavoisier and the phlogistonists – pluralism, in other words, which, when you think about it, is the same way in which we simultaneously embrace Newtonian gravitation and general relativity today, using whichever theory suits our purpose. It is helpful to think of these theories, not as rival contenders for “the truth”, but rather as *models* with which to describe and explain phenomena.

So, to conclude, I would not describe Ampère’s theory as a “failed revolution”, because I am not convinced that revolutions are a useful way to describe the progress of science. I’d prefer to view the birth of a new theory as simply an addition to the community of scientific ideas, rather than a “replacement” for an “outdated” one.

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A sketch of Mihajlo Pupin

Physicist, Engineer, Entrepreneur

Malcolm Cooper**Introduction**

Mihajlo Pupin was a flamboyant, politically passionate yet spiritually driven man who was born into a poor farming family in the wilds of Serbia in the middle of the 19th century but who emigrated to the USA where he achieved eminence as President of the New York Academy of Sciences, Professor of Physics at Columbia College, the holder of many degrees, over 30 U.S. patents and numerous honours and tributes.

Here is a man who lists among his friends, acquaintances and teachers not only the scientists Tyndall, Stokes, Rayleigh, Helmholtz, Kirchhoff and Lorentz, but also U.S. President Woodrow Wilson and industrialist - philanthropist, Andrew Carnegie, and yet little is known of him outside his native country.

Part 1



The village of Idvor - birth place, left



The house today

Early life.

Mihajlo Pupin was born on October 9th, 1854, in the tiny village of Idvor, to Constantin and Alimpiada Pupin. The village was located in the southern part of the Banat region of the Austro-Hungarian Empire and had been populated, in the early 18th Century, by tens of thousands of Serb families brought from the south specifically to defend the Habsburg Empire against the Ottoman Turks.

The village elders would gather in the meeting room and recount ancient tales of their forefathers, with the young Mihajlo sitting by the stove listening intently to these stories which were to draw in the first lines of his lifelong pride in the land of his birth. Then came the first blow to disturb the steady stream of tradition. The threat of the Turks had diminished by then and following the ‘Austrian-Hungarian Compromise’ of 1867 it was decided, in 1869, to abolish the military frontier and pass control of the area to Hungary. This, and other punitive measures, infuriated the Serbs and were to mould the young Pupin’s political outlook. He later wrote: *‘...I always felt that this treacherous act of the Austrian Emperor was the beginning of the end for the Austrian Empire.’*¹

However, life was not all politics! He had, along with his boisterous friends, tasks that were to prove, for him, equally formative. The community had the care of several dozen oxen which grazed in local pastures and it fell to him and his friends to watch over them, which was no easy job. But, at night

¹ Pupin, *‘From Immigrant to Inventor’*, Charles Scribners’ Sons, 1925;

there were moments when he could gaze at the stars and ponder on the nature of light. Sometimes the group got separated but they had an intriguing method of communication - this was to stick a knife in the ground and set it vibrating. His friends could hear the vibrations but, as he noted, only when the ground was firm - a ploughed field was no good. It was on these occasions that the first hints of scientific wonder came to him; how did the vibrations travel through the earth? And what was light - how did it travel?

His mother, (right), an extraordinary woman, who could neither read nor write, exerted a powerful and yet beneficent influence on him, epitomised by her many encouraging aphorisms promoting the search for truth. This, combined with her deeply felt spirituality, proved a driving force that was to remain with him all his life. He records his mother saying: *'Knowledge is the golden ladder over which we climb to heaven; knowledge is the light which illuminates through this life and leads to a future life of everlasting glory'*.²



He had attended the village school in Idvor to learn the basics of reading, writing and arithmetic but where he also showed some of the promise to come, although no answers were forthcoming to the questions he was asking on the nature of light. So, in 1867, at the age of 13, at the instigation of his mother, he was sent to the school in the town of Pančevo some 25 miles away.

There is little information on his academic life at the secondary school in Pančevo but two teachers he remembers with affection. One, Simon Kos, his physics teacher introduced him to scientific experimentation by telling him the story of Benjamin Franklin flying his kite in a storm. The other, father Vasa Živković, proved to be very crucial to Mihajlo's future. As

² Ibid.

already mentioned the military frontier had been abolished, but worse, learning the Hungarian language was made compulsory and the beginnings of the suppression of the Serbian language were instituted. This was all too much for the intemperate young Pupin and when a firebrand of Serbian nationalism, Svetozar Miletić, visited the town Pupin was quick to lend support. After a more serious incident when Pupin was caught stamping on the Austrian flag, they threatened to send him packing. It was only through the intervention of Živković in consultation with Pupin's parents that it was decided he should go to Prague. In truth, a return to Idvor would probably have meant an abrupt end to his academic future.³

He arrived in Prague in the autumn of 1872. But it didn't work out; he did not do well and so felt guilty about those who had supported him, including his family. Thus it was that in early 1873 he received the news that his father had died - quite unexpectedly. He was determined to give it all up and return to Idvor and his family. But, as has been noted, his mother was a remarkable woman and insisted he stay in Prague.

We need to go back in time a little here because events on that journey turned out to have been very influential on his decisions.

On the journey from Pančevo to Prague he should have changed trains at Gänsendorf, just outside Vienna, but falling asleep he woke to find himself in Vienna. On asking the advice from a station official he was berated for such stupidity and lack of respect, threatening him with his return to Serbia. He was not one to take such censure lying down and the altercation (somewhat abbreviated) is instructive:

Pupin: *'Your gracious majesty will pardon my apparent lack of respect to my superiors but this is to me a world of strangers'*

Official: *'You are not in the savage Balkans, the home of thieves but in Vienna, residence of the Emperor'*

Pupin: *'Yes, but my father told me that all rights and privileges of the military frontier were stolen here in Vienna'*

Official: *'Do you expect to get free transport back to Gänsendorf? Restrain your rebellious tongue or I'll send you back to the frontier where you ought to be behind lock and key!'*

³ DA Cucić, *Mihajlo Pupin and father Vasa Živković*, (2002). Letter to the Parish Council p.5

The threat of return may well have been enacted but for the intervention of a couple who had overheard this extraordinary exchange and took pity on the young Pupin paying for his onward journey to Prague. To his excitement his benefactors turned out to be American citizens so he proceeded to speak of Benjamin Franklin and Abraham Lincoln. He cannot have known too much about these icons of American history but it seems likely that this incident was to prove very influential.

He was faced with a dilemma - stay in Prague or go back to Idvor. Neither felt right so he took the momentous decision to quit both and go to America.

Grappling with a new world

Mihajlo Pupin sailed from Hamburg on 12th March 1874 aboard the SS Westphalia and landed in Castle Garden, New York on March 26th, an almost penniless 19 year old.

So here he was in a strange city in a strange country, no money, no work, but off he goes to explore his new surroundings. His Serbian clothes complete with a red fez, not surprisingly, set him apart and attracted considerable attention, but also that of a prospective employer - a Delaware farmer. Could he look after a couple of mules and load the carts? Yes, came the confident reply and thus began his first job which lasted a few months and provided his first earnings. But perhaps more importantly here is where he began to learn English.

After a number of short lived jobs labouring he got his first 'real' job in a biscuit factory. He made friends with Jim the boilerman and a fellow employee by the name of Bilharz. Quite by chance helping Jim out led him to ponder on the thermodynamics of heating systems and Bilharz (curiously for a manual worker was well versed in Latin and Greek) encourage Pupin to attend the Cooper Union Library as a first step in his academic career. Thus it was that in late 1879, Pupin was accepted as a student at Columbia College, New York.

The mathematics course presented no problems but science teaching at Columbia was less than satisfactory, to say the least. The first hint of this inadequacy came when he posed the question which had long concerned him 'what is light?' The professor of physics was unable to answer except to say that it was some kind of vibration in the aether. This must have been something of a disappointment to Mihajlo and in discussing the question with his tutor he first learned of James Clerk Maxwell. It is interesting to

note that better teaching in physics might not have been as effective as one might imagine. He had watched in awe as Professor Rood demonstrated Faraday's electromagnetic induction - the galvanometer needle twitching as the professor deftly moved the magnet and then waiting with bated breath for the explanation - but nothing. It was probably at this moment when Mihajlo knew he had to seek answers further afield.

So finally, in 1883, after four years of study, he graduated Bachelor of Arts.

He had come a long way since early days of being a stranger in a foreign land burdened with the task of assimilation; he spoke the language fluently, was fluent in its history and so was ready to apply for citizenship. There was no difficulty and he was sworn in the day before he attended his graduation ceremony.

From now on he was to be known as Michael Idvorski Pupin. (Idvorski meaning 'of Idvor'.)

In pursuit of Maxwell

It had been eleven years since he was last in his native village of Idvor and was now in a position to fulfil his wish to make the trip, so on a beautiful June afternoon of 1883 he set sail on the *State of Florida* bound for Greenock, in the United Kingdom. His journey was very indirect however calling at Cambridge, Lucerne, Budapest and finally, Idvor.

The intense conversations on his new experiences and his plans for further education gradually gave way to the inward looking yet satisfyingly content way of life in the village where he was born. His mother, noticing a wistful air of doubt for the future told him:

'You will wake up and see this was all a pleasant dream only ... in your restful hours in drowsy Idvor. The real things are waiting for you at Cambridge. The blacksmith softens his steel before he forges it into a chain; you are just right for the blacksmiths of Cambridge'.

An extraordinary woman.

The blacksmiths of Cambridge

So, in the October of 1883, Michael began his 18-month stay in Cambridge, advised by William D. Niven. He had read books including Tyndall's book on 'heat', but he was otherwise quite ill prepared. This, not surprisingly, troubled him a good deal but Niven advised him to study mathematics under J.E. Routh but warned it would be an uphill struggle.

Michael was no stranger to fighting and caught up with his fellow students, but something was not quite right. He increasingly found the concept behind the mathematical tripos sterile⁴. He had no wish to become a 'wrangler' - he wanted to know about underlying physical concepts - 'what is light?' for example. It did, however, provide the good grounding to tackle Maxwell's 'Treatise on Electricity and Magnetism'.

Needing a break he returned to Idvor - via Paris where he acquired a copy of La Grange's 'Mechanique Analytique'. . But he was not idle, spending his leisure time studying La Grange and Campbell's 'Life of Maxwell'; or as he put it: *'The company of La Grange and Maxwell kept me a prisoner in my mother's garden'*

When the time came for him to return to Cambridge his mother was no less eloquent, saying:

'Go back, my son and may God be praised forever for the blessings which you have enjoyed and will continue to enjoy in your life among the saints of Cambridge'

Return to Cambridge and John Tyndall

The spiritual teachings of his mother had given him the mind-set to revere all those great names with which he was familiar at that time. He thought of them as the saints of science which illuminated the path to the Eternal Truth. Indeed, he even felt these icons of physics - Copernicus, Galileo, Newton, Faraday, Maxwell and Helmholtz⁵ - should be honoured with the equivalent of Saints Days. He was passionate about these concepts, bringing an almost religious fervour to his views on the development of science, and one which he held for his entire life.

⁴ It is interesting to note that he was in good company in questioning the 'Tripos'. Lord Kelvin and others had suggested reshaping the structure.

⁵ These names were quoted in his autobiography but for sure could have added many more.

By now he was comfortable with Maxwell but as he puts it: *'I could handle the mathematics of Maxwell's theory of electricity with considerable ease; but I did not understand his physics'*.

He worried about the adequacy of his preparation; his lack of experimental experience. He considered asking to study at the Cavendish Laboratory under Lord Rayleigh.

But then, quite out of the blue he received a letter from President Barnard of Columbia College, New York, informing him of a recently endowed fellowship for a Columbia graduate, funded by the generosity of John Tyndall. A letter of introduction was enclosed and Michael lost no time in seeking a meeting with the Irish physicist and director of the Royal Institution. The meeting proved a turning point in his life.

Michael Pupin could not have had a better advisor. On his second visit to Tyndall (right) he announced his intention to study under Helmholtz, a decision which was heartily received by Tyndall who strongly supported him to apply for the Tyndall Fellowship for a young American philosopher. Tyndall had already given Michael a copy of his lectures on 'light' and on his last visit presented him with a copy of 'Faraday as a Discoverer'. These volumes along with Faraday's 'Electrical Researches' were to keep him occupied for some considerable time.



Berlin and Helmholtz

After all these profound events he desperately needed some quiet time on his own to consolidate his tripos studies with these new exciting ideas. So, in the summer of 1885 he decided to go to Scotland to the seclusion of the Isle of Arran where he might pursue Tyndall's suggestion of interpreting Maxwell through Faraday.

So after this ‘break’, Michael set off for Berlin to gain some much needed experience in experimental physics under Hermann von Helmholtz at the Friedrich Wilhelm University of Berlin. But even there he was still haunted by Faraday, devoting much study (in his spare time) of Faraday’s perplexing ‘lines of force’ and the fact that Maxwell had drawn them in to his theory of electromagnetism.

Thus he began his studies beginning in that first year of 1885 with attending Helmholtz’s lectures on experimental physics. Michael was clearly captivated by these lectures and, always a man with a penchant for the poetic, commented:

‘Helmholtz threw the searchlight of his intellect upon the meaning of his experiments, and they blazed up like the brilliant colors of a flower garden when a beam of sunlight breaks through the clouds.’

But Faraday and Maxwell continued to trouble. It was with great anticipation then that he attended lectures by the renowned Gustav Kirchhoff on ‘Theoretical Electricity’; here, surely would he finally receive enlightenment. But to his dismay it was not to be; Kirchhoff hardly mentioned these two giants of electromagnetism. How could this be so he asked himself?

The question posed to Prof

König failed to yield the answer and led to a somewhat heated discussion between the two, a discussion which was interrupted by Helmholtz himself. Smiling, he referred Michael to a recent address he’d given entitled ‘Recent Developments in Faraday’s Ideas Concerning Electricity’.



Hermann von Helmholtz 1881

The searchlight beamed, the mists lifted; finally it became clear through the writings of a German physicist how an English experimentalist and a Scottish theoretical physicist had revolutionised the understanding of electromagnetism leading to technologies that changed the world. For Michael it was a revelation

So finally having laid the ghost of the concepts of Faraday and Maxwell, he once again returned to his native and beloved Idvor in the summer of 1886 and, as always, he was welcomed by his mother. It was to be the last time he would visit in such happy circumstances.

Michael enjoyed many conversations with his mother about his work and studies and the joyous triumph of his recent revelations. With her extraordinary divine view of life reiterated her oft spoken words *'Knowledge is the golden ladder over which we climb to heaven.'* He writes:

'Her religion taught her how to catch the spirit of science, and I was always certain that science can teach us how to catch the spirit of her religion.'

She died just a few months later.

Doctoral research and other distractions

Almost two years had passed since he had arrived in Berlin and yet no work was attempted towards his avowed aim of post-graduate research; all his time was spent; all had been preparation to find the path and for the final assault.

The path opened up when a new branch of science was moving over the landscape: physical chemistry. Helmholtz was much interested in this and suggested to Michael there could be great opportunities for new research in this little explored territory. Thus he began work on his thesis: 'Osmotic pressure and its relationship to free energy', finally becoming, in 1889, Dr. Pupin.

It is interesting to note that on the title page he signs himself as: 'Michael Pupin from Idvor in the k.k. Austrian Military Frontier.' (k.k. stands for kaiserlich und königlich - Imperial and Royal). Written, I suspect, with tongue firmly in cheek.

But before that momentous time, in the spring of 1888, another event intervened. Michael had visited his friend and Columbia fellow student - though not of physics - AV Williams Jackson in Halle, Germany, where they were joined by Williams' sisters one of whom he took special note. Later, the Williams sisters and their mother returned the visit, travelling to Berlin. But then one of the sisters, Sarah, decided to take a trip to Italy. Michael describes his courtship thus:

'One of Jackson's sisters went to Italy and I followed; she returned to Berlin and I followed; she went to the island of Nordeney...and I followed. The Faraday-Maxwell electromagnetic theory and the Hertzian experiments, my research in physical chemistry...disappeared from my mind as if they had never been there.'

By a happy coincidence Columbia College were establishing a new department of electrical engineering to open the following year in September 1889. In true globetrotting style Michael left for the US to help with its organisation and was rewarded with the offer of the post of 'Teacher of mathematical physics in the new department.

He returned to Europe, specifically the UK, where, in London he married his betrothed, Sarah Catherine Jackson in October 1888, the ceremony taking place in the Greek Orthodox Church, happily attended by his wife's family.

Thus it was in the summer of 1889 he began the sea voyage and that greater voyage, with his bride, his doctorate and his sights set firmly on new horizons ahead.

(Part 2 to follow in the next newsletter, 2021- Editor)

This is a condensed version of 'Mihajlo Pupin, Physicist, Engineer, Entrepreneur'

My thanks to D Cučić and D Martinović. Also thanks to Jelena Kalin, curator of the Pupin Foundation, Idvor, Serbia.

The colour and polarisation of light from the sky: Part I

Edward A Davis
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The Third Baron Rayleigh is rightly credited for explaining in detail why the sky is blue and the light from it polarized. A succession of papers in the *Philosophical Magazine* – the first published in 1871 and the last in 1918 – developed the theory of scattering of light by particles of size small in comparison to its wavelength, a theory that now bears Rayleigh's name. However, the narrative leading up to this explanation has its beginnings in the musings of Greek philosophers, the development of these by the insights of Arab Scholars and the observations of painters, and more latterly in the theories and experiments of scientists. An historical study of the topic provides a fascinating insight into how scientific discoveries and explanations evolve with the passage of time and with the continual acquisition of new knowledge.

Note: Part I covers the period up the work of John Tyndall. Part II, starting with John William Strutt (later the Third Baron Rayleigh), will be published in the next issue of the Newsletter.

Greek Philosophers

When considering early explanations for the sky's colour, it is important to note that from the time of Aristotle (350 BC) until Newton's famous prism experiment (1666 AD), colours were considered to arise from mixtures of black and white present in different proportions. Thus, according to Aristotle's *On the Senses*, yellow was deemed to consist of about one-sixth black and five-sixths white; red, one-third black and two-thirds white; purple, equal proportions of black and white; green, two-thirds black and one-third white, blue, five-sixths black and one-sixth white. Black and white themselves were considered by Aristotle to be invisible. Greek philosophers frequently referred to brightness rather than colour (luminosity rather than hue) when describing the appearance of objects: for example, blue is considered to be a dark colour, while yellow is a light one.

An early reference to the colour of the sky was made by one of Aristotle's students, Theophrastus, who is believed to be author of the work *On Colours*. In this account Theophrastus wrote:

'Air seen close at hand appears to have no colour, for it is so rare that it yields and gives passage to the denser rays of light, which thus shines through it; but when seen in a deep mass it looks practically dark blue. This again is a result of its rarity, for where light fails the air lets darkness through.'

Of importance here is the realisation by Theophrastus that the sky is not a fixed canopy lying at a great height above our heads – a concept that one might have thought early civilisations would have considered to be almost self-evident – but instead is associated here with the *'air seen close at hand'* and extending upwards from the Earth's surface. The blue arises because darkness overcomes the light, which is an extension of Aristotle's idea that blue is a mixture of black and white with the former (darkness) predominating in this case.

The works of classical Greek philosophers were for the most part lost during the Dark Ages but rediscovered by Arab scholars in the ninth century. One of these scholars, Abu Yusuf al-Kindi, who was born in present-day Iraq around 800, modestly exclaimed that he and his contemporaries could not hope to achieve anything like the teachings of the Greeks. Nevertheless, he went on to write what is the oldest known and preserved account addressed solely to the matter of *'the cause of the azure-blue colour which is considered to be the colour of the sky'*. Perhaps based on his familiarity with sandstorms, he proposed that *'earthly particles'*, are stirred upwards, creating a haze in the atmosphere. In al-Kindi's own words:

'The air surrounding the Earth gets weakly lighted by the earthly particles dissolved in it and changed into fiery ones due to the heat which they have accepted from the reflection of rays off the Earth. The shadowy air above us is visible because the light of the Earth and the light of the stars intermingle into a colour in the middle of shadow and light and this is the azure-blue colour.'

It was in 995 that the first scientifically significant observation of the sky was made. It involved nothing more than climbing a mountain. Abu Rayhan al-Biruni, an early astronomer in today's Afghanistan, climbed the highest mountain in Persia, Demavend at 5,610 metres. He reported that the clear sky was darker than it was when seen in the plains below. This important observation was consistent with the earlier idea that the colour of the sky had somehow to involve darkness as well as light. This idea was articulated by a 13th century teacher of Islamic law at a Quranic school in Cairo and amateur scientist, Ahmed al-Qarafi. In a treatise, he wrote:

'One does not see the sky at all, and what we don't see, we see as dark. Ask a blind man, what is it that you see, and he will answer "black darkness". And so the sky is also black, and under it is the air, which is transparent and luminous. Our gaze penetrates the air and sees it against the sky so to speak..... Thus the blue colour results from the purity of the air and the darkness of the sky, for we are dealing here with the mixture of the black and the pure.'

Note that al-Qarafi appears to invoke the Aristotelian theory of colours, namely as all colours being a *mixture* of black and white in different proportions, but there is a subtle difference in that in this account the blue arises from observing a black background *through* a white (pure) one. Also implicit here, although not referred to by al-Qarafi, is an explanation for the darkening of the sky with altitude as observed by al-Biruni. Although al-Qarafi did not propose any explanation for the air's luminosity during daylight, his basic concept of a black background seen through a mass of essentially transparent but illuminated air is a fairly accurate description of what is in fact seen when looking skywards.

Roger Bacon

The first Englishman to contribute to an understanding of the sky's colour was Roger Bacon (1220-1292), who became a Franciscan monk at the age of 27. Bacon's enthusiasm for the teachings of Aristotle was curtailed by the prevailing doctrine of the church, which forbade teaching of Aristotelian philosophies on account of them being regarded as contradictory to Christian beliefs in miracles, the creation, and other matters of doctrine. Building on the works of Aristotle and Arab scholars, which had suggested that the colour of the sky depended on a competition between black and white, and hence implicitly on the amount of air through which the dark background is viewed, Bacon's writings (which were secretly copied and circulated in Europe) included the proposal that the colour of air depended explicitly on its depth. He likened the air to water, with both media being transparent over short depths but getting progressively darker with distance. In his *Opus Maius* [1], Bacon writes

'I say here that the air, or the sphere of fire, or the heavens, near and remote, is of similar rarity as far as perception is concerned; but it has, however, some density of its own nature, and this density is able in a great distance to terminate the species of vision, which it cannot do in a short distance, and therefore it will be quite visible at a distance, but not near at hand.'

Bacon attributes the blue colour to traces of particles in the air. However, unlike al-Kindi, he does not invoke foreign bodies, but instead attributes the colouration to the nature of air itself. Nevertheless, he did not depart from Aristotle's theory of colours in which blue is considered close to black. He writes:

'Why a colour appears approaching black, namely blue, is explained in the same way as in the case of deep water, where in a like manner that colour appears owing to shadows projected by particles. Darkness is caused by these shadows, which is similar to blackness. This is what takes place in the air or medium between us and the last heaven.'

In the 13th century, the colour blue was not readily available as a pigment for artists to use on their canvases. Blue pigments were normally obtained by grinding minerals such as lapis lazuli or azurite and mixing them with oil. On account of their rarity and cost, their use was often limited, for example in religious paintings, to the robe of the Virgin Mary. A trick that some painters employed was to apply a translucent black pigment over a white base coat. Simply *mixing* black and white merely produces grey, whereas the technique of 'overlying' can produce visual effects that are often blue in colour, contrary to expectations. A contemporary of Bacon was Ristoro d' Arezzo, who, drawing an analogy between the colour of the sky and the painter's pallet, wrote:

'Clever painters who paint in colour – when they want to get the colour blue – mix two different colours together: light and dark, and from this mixture, blue results. When I look at the sky, I see two opposite colours mixed, namely light and dark, and this is due to the air's depth.'

The argument used by d' Arezzo to refute the suggestion by Bacon that the air itself might be tinged blue – at first sight a convincing one – was that anything we see beyond the atmosphere, such as the sun, moon, planets and stars, should all take on a blue colouration, which, he exclaimed, is obviously not the case.

Leonardo da Vinci

Leonardo da Vinci was both a painter and a natural philosopher. In about 1510, he completed one of his codices – manuscripts containing, in tiny scribbled mirror writing, his thoughts on a wide variety of natural phenomena and mechanical inventions, including, for example, water pumps and flying machines. The one that contains his theories, and even the results of experiments, on the nature of the sky is known as the Leicester

Codex, named after the first Lord of Leicester, Thomas Coke, who purchased it in 1717 from an Italian painter. It was re-sold in 1980 to an American, Armand Hammer, and is currently owned by Bill Gates (founder of the Microsoft Corporation) who purchased it at auction for over \$30 million in 1994.

Leonardo's interest in the accurate representation of landscapes in paintings led him to prescribe (as an artist):

'...in painting, to make one (object) seem more distant than the other, it would be necessary to represent the air as a little thick, for you know that in air like this, the farthest things seen in it, such as mountains, seem blue – almost the colour of the air when the sun is in the east – because of the great quantity of air that is found between your eye and the mountains.'

In addition, he proposed (as a scientist):

'I say that the blue that is seen in the air is not its own colour, but is caused by the heated moisture having evaporated into the most minute imperceptible particles, which the beams of the solar rays attract and cause to seem luminous against the deep intense deep intense darkness of the region of fire that forms a covering above them.' (see Footnote 2)

'As a further example of the colour of the air, we may take the case of the smoke produced by old dry wood, for it comes out of the chimneys it seems to be pronounced blue when seen between the eye and a dark space, but as it rises higher and comes between the eye and the luminous air, it turns immediately to an ashen grey hue, and this come to pass because it no longer has darkness behind it.'

Leonardo also found that water blown in the form of spray into a dark space, through which solar rays pass, produces the same blue colouration. Another experiment (with strong connections to his main profession as a painter) was to stain a board with very strong black and overlay this with a thin transparent white. He who does this will, in his own words in the Codex, *'perceive that the lustre of the white will nowhere display a more beautiful blue than over the black, but it must be very thin and finely ground.'*

Isaac Newton

Winding forward by about 150 years to the time of Newton, we reach his ground-breaking experiment with a glass prism which he performed in 1666. Utilising sunlight entering through a hole in the shutter of a window at his home in Woolsthorpe, he positioned the prism to refract the light onto the opposite wall. The resulting oblong pattern contained all the colours of the visible spectrum from red to violet. Newton had, at a stroke, demolished Aristotle's theory that colours are mixtures of black and white by demonstrating that white light itself can be decomposed into all known colours. Although others before Newton had observed coloured fringes generated by glass and by thin layers of liquids, these had been dismissed as chromatic flaws or artefacts, without their significance being realised. Likewise, of course, rainbows had been observed since the dawn of man. Aristotle himself had a fairly detailed theory of rainbow formation, which (correctly) involved reflection from raindrops but which (incorrectly) described its colours (of which he recognised only three: red, green and violet with yellow being dismissed as an 'illusion') in terms of combinations of light and dark.

In his book *Opticks*, the 2nd edition of which was published in 1718, Newton presented his colour circle in which the seven basic colours of the spectrum are arranged around its circumference. From this it is possible to predict the colour (hue) that results from mixing any two colours, in addition to its brightness (purity or saturation), with white being at the centre of the circle.

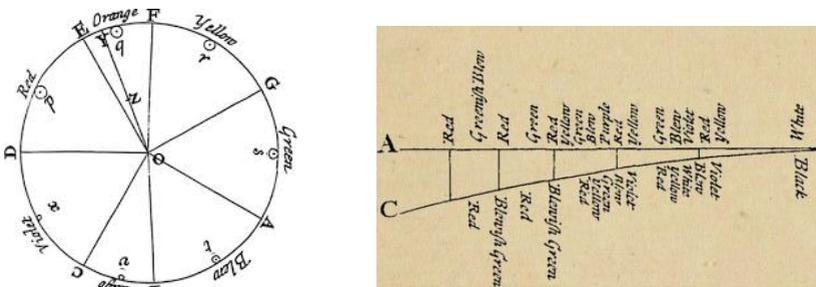


Figure 1. On the left, Newton's colour circle. The interior letters denote musical notes as Newton sought an analogy between these and the light spectrum. The right diagram shows various orders of colours observed in Newton's rings and recorded in detail in his book *Opticks*.

Even more consequential than his prism experiment for an understanding of the nature of light were Newton's observations on the coloured rings seen when two convex lenses were pressed together. Knowing the focal lengths of the two lenses, Newton calculated their radii of curvatures and hence their separation as a function of distance from their central point of contact. He thereby determined the difference in the gaps between the lenses corresponding to two adjacent coloured bands to be as small as $1/89,000$ inch. The regularities of the rings and their appearance in both reflection and transmission suggested to Newton that light must have an intrinsic periodic property, the characteristic length of which was related to the refrangibility of the light rays, i.e. the amount by which they were refracted or, alternatively, their colour. It would seem that observations of these rings caused Newton to question his belief in the corpuscular theory of light. It also led him to suggest that the colour of objects could depend on the size of particles of which they were composed. The idea was that particles either reflect, refract or transmit light of a particular colour depending on their size.

In addition to using this concept to account for the colours of foam, paper, gold and copper, Newton applied it to the colour of the sky. The innermost circle of Newton's rings (as they are now called) is blue, which he regarded as the colour of least refrangibility. He called this 'the blue of the first order' and associated it with the smallest of particles. In his own words:

'The blue of the first order, though faint and little, may possibly the colour of some substances; and particularly the azure colour of the skies seems to be of this order. For all vapours when they begin to condense and coalesce into small parcels, become first of that bigness, whereby such an azure must be reflected before they can constitute clouds of other colours.'

So, Newton's explanation for the blue colour of the sky was, in essence, reflection from small water droplets in the atmosphere – a proposal that, as mentioned earlier, had been first suggested by Leonardo da Vinci.

Leonhard Euler and Horace Bénédict de Saussure

The Swiss mathematician and scientist, Leonhard Euler, wrote in 1760 that *'the cause of the visibility of objects is a motion of vibration extremely rapid, by which the minuter particles of their surfaces are agitated, and that the frequency of their vibrations determines the colour.'* Euler's study of meteorological phenomena led him to assert that the higher one ascends from the earth's surface, the rarer the air becomes and the blue of the sky fades away into utter blackness. He then concludes that the air cannot be a

perfectly transparent medium. He espoused the idea that *'the air is loaded with a great quantity of small particles which are not perfectly transparent, but which, being illuminated by the rays from the sun, receive from them a motion of vibration, which produces new rays proper to those particles; or else they are opaque and become visible to us from being illumined'*. A few years later he contended that the particles were not impurities but *'minute parcels of air'* itself.

This description of the light from the sky anticipates, rather remarkably, the explanation to be offered by Rayleigh over 100 years later.

Another Swiss scientist, Horace Bénédict de Saussure, was both a geologist and Alpine explorer. He is credited with the invention of several instruments, including a hygrometer based on a single human hair, an anemometer, an electrometer (possibly the first) and what he called a cyanometer, a device for comparing the various degrees of blueness of the sky. The cyanometer was a circular card divided into 52 segments, each numbered and coloured with a different shade of blue varying from very pale to very dark [3]. It was simply held up to the sky and the closest colour match found by visual inspection (see Figure 2). As an experienced mountaineer, de Saussure was able to confirm Euler's darkening in the colour of the sky with altitude. He also chronicled variations in the degree of blueness with angle of observation from the zenith to the horizon and changes associated with atmospheric purity and meteorological conditions.

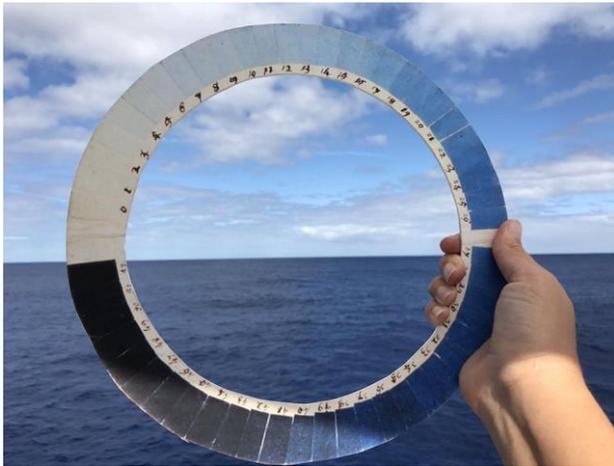


Figure 2. Replica of de Saussure's cyanometer

Alexander von Humboldt

Humboldt was a German naturalist and explorer. In 1802 he and a colleague climbed the mountain Chimborazo in Ecuador where a cyanometer was used in an attempt to quantify the darkening of the sky with altitude and determine the possible influence of oxygen on its depth of colour. The composition of the atmosphere had been determined about 30 years earlier by Joseph Priestley, Antoine Lavoisier and Henry Cavendish but it was not clear whether this changed with altitude. In 1804, Humboldt analysed samples of air obtained by Biot and Gay-Lussac during a balloon ascent to 23,000 feet. He obtained values of 21 percent oxygen, 78.7 percent nitrogen and 0.3 percent carbonic acid, the latter not having been found earlier. Furthermore, he found essentially no change in these values with altitude.

The wave nature of light and polarisation

Thomas Young was a physician and scholar who was appointed professor of natural philosophy at the Royal Institution in London in 1801. His proposal that light propagated in the form of periodic waves was demonstrated by displays of analogous phenomena with water waves in a so-called ripple tank. With this apparatus he was able to display interference phenomena, in particular those generated by waves after passing through apertures. His famous optical double-slit experiment was conducted using light of various colours and, by noting that the spacing of the interference fringes changed with the colour of the light, he came to the conclusion that different colours were associated with different wavelengths. Young went on to determine these wavelengths and established that the visible spectrum ran from about 400 nm for violet light to 750 nm for red light. Furthermore, in contrast to Huygens who earlier had suggested light propagated as waves similar to longitudinal sound waves, Young was the first to propose that light waves were transverse in nature.

This significant proposal was vital for understanding the polarisation of light, a phenomenon discovered by a French physicist, Louis Malus, in 1808. The capability of crystals of Iceland spar to split a ray of light into two paths, named by Huygens an *ordinary* ray which obeyed the laws of refraction and an *extraordinary* one that did not, had been known for over 100 years before Malus's discovery. Using another double-refracting crystal, tourmaline, Malus was surprised to find, on viewing the reflection of sunlight from a pane of glass, that, at a particular orientation of the crystal and viewing angle, there was only one image, instead of the

expected two. The result suggested that light itself was capable of having a particular orientation associated with it. Malus coined the word polarisation to identify this property but, as an adherent to the corpuscular theory of light, found it difficult to conceive how particles could exhibit it. Augustin Fresnel, a contemporary of Malus, recognised that Young's wave theory of light easily allowed for different orientations because the waves could, in principle, vibrate in any specified plane.

The experiments of Malus had not only provided insight into the nature of the two rays produced by double-refracting, or birefringent, crystals, but had also demonstrated that light reflected at a particular angle from glass was completely polarised. The angle of incidence at which this occurs is known as Brewster's angle, named after the Scottish physicist, David Brewster. It depends on the refractive index of the material; for glass it is 56 degrees and for water 53 degrees.

It was soon discovered that the light from the daytime sky is polarised too. Francis Arago noted how the polarisation pattern in the sky changed according to the sun's elevation and found that at 90° to the sun's direction it has a degree of polarisation that is as high as 75%. Arago also discovered that in a direction near to the horizon opposite to the setting sun the polarisation vanished – the so-called Arago point. Subsequently three other similar 'neutral' points have been identified (see Figure 3).

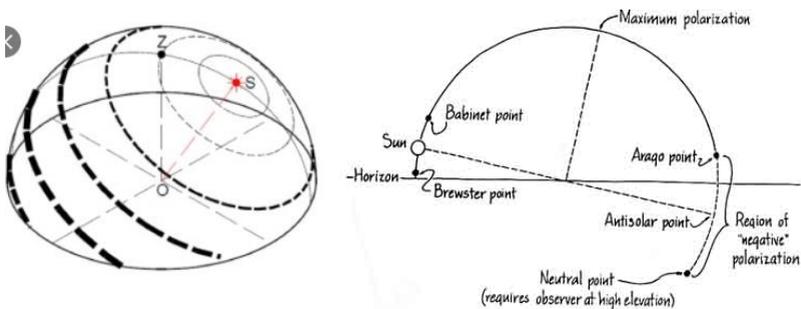


Figure 3. The light from the sky is polarised to varying degrees depending on the angle relative to the sun. It is most heavily polarised along an arc at 90° to the sun's direction, with the electric vector along that arc (thickest line in diagram on the left). The degree of polarisation is zero at the neutral points as illustrated on the right diagram.

That the maximum polarisation of the daytime sky occurs along an arc subtending the observer and forming a plane at an angle of 90° from the sun's direction provided a challenge. The observation was not unrelated to the blue colour because it was soon discovered that, when the sky was cloudy and white, the extent of polarisation decreased substantially. It was clear that any explanation of the blue colour of the light from the sky must also account for its polarisation.

Newton's proposal that the blue colour of the sky arises from reflection from water droplets or ice crystals could be dismissed on the grounds that application of Brewster's law to reflection from water with a refractive index of 1.33 leads to the maximum polarisation occurring at an angle of 74° to the sun (see Figure 4). In 1840, the astronomer, John Herschel, inverted the argument and pointed out that for polarisation to occur in reflected light at right angles to the light source requires a Brewster angle of 45° , which can only occur if the medium from which the reflection occurs has a refractive index of unity. Since air has a refractive index of 1.0003, this essentially rules out reflection from anything except from a boundary between air and air itself which is nonsensical. Herschel exclaimed at the time *'the colour and polarisation of the sky are the two great standing enigmas of meteorology.'*

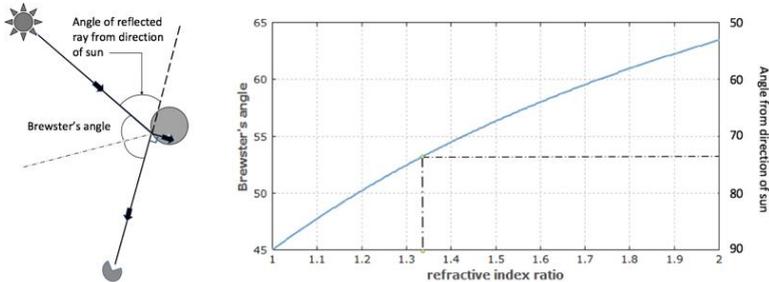


Figure 4. The ray received by external reflection of the sun's ray from a raindrop is 100% polarised when the reflected ray is at 90° to the refracted ray as shown on the left. The angle of incidence is then Brewster's angle θ_b and the angle of the reflected ray with respect to the sun's direction is $(180^\circ - 2\theta_b)$. In terms of refractive indices, Brewster's angle is given by $\theta_b = \tan^{-1}(n_2/n_1)$. For an air/water interface with $n_1 = 1$ and $n_2 = 1.33$, $\theta_b = 53^\circ$ and the angle of the reflected ray from the sun's direction is then 74° . For this angle to be 90° , θ_b would have to be 45° and hence n_1/n_2 would need to be unity as can be seen from the graph on the right. The same conclusion can be reached for reflection from the internal surface of a raindrop.

John Tyndall

The Royal Institution (RI) in London has been the laboratory of many great scientists, for example Humphrey Davy, Michael Faraday and Thomas Young who has been referred to earlier. John Tyndall was another professor at the RI, an appointment that ran from 1853 to 1887. Using the apparatus shown in Figure 5, Tyndall studied the optical appearance of various vapours of liquids mixed with air when precipitated by the action of light [4].

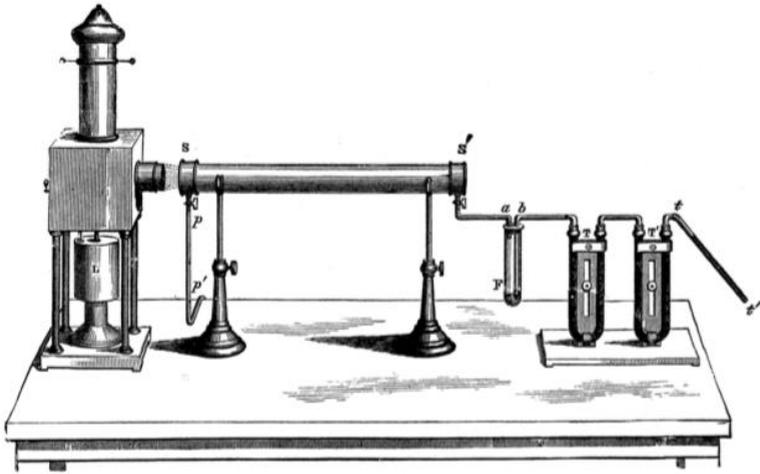


Figure 5. Apparatus used by Tyndall to study ‘chemically active’ clouds. Air from the right passes through filters to remove floating matter and water vapour, and then over a volatile liquid in F before entering the central observation tube SS’ about one yard in length. The clouds were illuminated by the lamp L on the left. The narrow tube pp’ was connected to a vacuum pump.

By selection of the vapours, which were, for example, carbon bisulphide, amyl nitrate, and isopropyl iodide, and, controlling their quantities, he was able obtain precipitated particles of various sizes some of which were beyond the reach of the ‘highest microscopic powers’ and ‘whose diameters constitute but a very small fraction of the length of a wave of violet light’. In all such cases, and irrespective of the chemical nature of the substance, a blue cloud appeared, the colour of which in many cases ‘rivalled that of the purest Italian sky’. Of significance is the fact that the cloud was totally invisible in daylight. To be seen it needed to be surrounded by darkness and illuminated only by a powerful beam of light directed from the side.

Furthermore, Tyndall found that the blue light emitted at right angles to the illuminating light beam was highly, and in some cases perfectly, polarised with the plane of polarisation also being at right angles to the beam. When the precipitated particles grew in size, the blue colouration turned to white and the emitted light was then no longer polarised.

The parallel beam of light used in these experiments tracked its way beyond the experimental tube and was visible for a length of 18 feet in the laboratory air, which Tyndall likened to *'sun-beams seen in the dusty air of London'*. The beam imparted a bluish tint to the air and when viewed laterally the light was polarised. Furthermore, observations at different angles revealed neutral points where the polarisation changed direction, just as Arago and others had observed in the sunlit sky.

After extending these investigations using tobacco smoke, condensed steam and fumes of various acids, Tyndall came to the conclusion [5] that *'the colour of the sky is due to the action of finely divided matter, rendering the atmosphere a turbid medium, through which we look at the darkness of space'*. He acknowledges that this proposal dates back to Leonardo da Vinci and Newton. In his paper, Tyndall also refers to earlier laboratory experiments using suspensions conducted by Professor M. Govi in 1860, by Rudolf Clausius, and by Ernest Brücke. However, his own experiments were the most comprehensive and convincing demonstration in the laboratory that particles of dimensions smaller than the wavelength of light reproduce the blue colour of the sky as well as its polarisation pattern. Tyndall makes the candid admission that the undulatory nature of light is unable to explain the reason for either of these related phenomena, echoing John Herschel's earlier pronouncement.

Referring to private communication with George Gabriel Stokes, Tyndall states *'I would say, if it can be demonstrated that when the particles are small in comparison to the length of a wave of light, the vibrations of a ray reflected by such particles cannot be perpendicular to the vibrations of the incident light; then assuredly the experiments recorded in the foregoing communication decide the question in favour of Fresnel's assumption.'* Fresnel, it will be recalled, had proposed that light is characterised by transverse, not longitudinal, vibrations.

The scattering of light by particulate matter is often referred to as the Tyndall effect.

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1. The *Opus Maius* of Roger Bacon, translated by Robert Belle Burke, Philadelphia: University of Pennsylvania Press, 1928.
2. The reference to ‘the region of fire’ refers back to Aristotle’s four elements making up nature, namely earth, water, air and fire, which he proposed around 340 BC to exist in concentric spheres, in that order, around the centre of the earth, which was at that time considered to be the centre of the universe.
3. See article by Peter Tyson, *A Laboratory in the Clouds - Horace-Bénédict de Saussure (1740-1799)* in History of Physics Newsletter No.35, November 2017
4. John Tyndall, *On the action of rays of high refrangibility upon gaseous matter*, Philosophical Transactions of the Royal Society of London 160 (1870) pp.333-365.
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(Part 2 will follow in the first newsletter of next year - Editor)

Sir Francis Bacon: Parallels in time between 1620 and 2020

Alison McMillan

Introduction and a Personal Motivation

If Sir Francis Bacon were here now what would he think, and what would he be writing as a new *Novum Organum* for the 21st century?

I am not a historian, but I am a scientist. I think the scientist's perspective is very important to understanding Bacon's intentions when reading his works. The scientist's point of view informs that understanding in two ways.

Firstly, the scientist, versed in today's scientific method, is already an actuality of Bacon's method.

Secondly, in terms of interpretation, I believe the scientist can see the process of thought the Bacon was applying.

My starting point

I should start by making it clear, that at the current point in time, I am writing without a conclusion, but am describing the process of discovery and penetration in which I am currently engaged. I was born in 1965 and observed the twilight of the 20th century educational system, watching opportunities closing as I passed through them: the introduction of student fees being perhaps the most significant of these.

During the same period, Equal Opportunities legislation has gradually increased and expanded, so that now we recognise the "*equality*" of those from the "*protected characteristics*" groups, but at the same time it is hard to see that any real progress in opportunities and representation for "*diversity*" has really happened at all. The women on top of the glass ceiling seem to be guarding the trapdoors and ladders more assiduously than any of the men.

I cannot comment on the LGBT+ experience, except as to say that to study the life and times of Francis Bacon one must surely consider the nature of concealed deep personal relationships. Likewise, one might consider what is now considered to be "*cognitive or neuro-diversity*": not merely being an avoidance of "*group-think*" but being a recognition of cognitive traits like autism and dyslexia as being talents rather than handicaps. Groucho Marx is famously quoted as saying, "*I refuse to join any club that would have me as a member*", and as a self-denial of type, perhaps it is as fitting for Bacon then, as I would assume is still true for many today. Perhaps for me, it is the act of boxing oneself into a fixed "type" that I find so difficult to deal with, why I find it so hard to accept the "*protected characteristics*" approach to diversity equality, and why I find it unpalatable to hear people apply such fixed, 21st century judgements to people from history.

As far as science is concerned, I have all the usual qualifications and experiences, and probably a few others that are more unique to me. After a 15-year career in the

aerospace industry, I changed track and returned to academia. This was, to some extent, enabled by being awarded a Royal Society Industry Fellowship, which paid for me to spend part-time of the latter four years of my time at Rolls-Royce on academic research. During this time, I became much more aware of the history of The Royal Society, the history of science in general, and of Sir Francis Bacon in particular. I have been completely blown away by the prescience of his vision and its relevance even today.

As I continued with my academic research, begun during the Industry Fellowship, but continuing now, I came to realise that I have been instinctively approaching science discovery in pretty much the same way that Bacon describes in *Novum Organum*. This probably requires some explanation: I have been trying to relate the process of material fatigue to known facts and theories in elasticity and fracture mechanics. This is a “*science*” within the domain of mechanical engineering which is messy, contains many contributing factors and for which at the current point in time, machine component validation is carried out by testing or through empirical models built from test data. The state of knowledge is as messy and incomplete as thermal physics was in Bacon’s day. The main difference between his approach and mine is my use of computational models as proxies for impossible-to-manufacture perfect test specimens.

Over the past two years, my focus has shifted from merely contemplating Bacon’s writings, to envisaging what a present day *Novum Organum* should say. There are similarities in features of the time around 1620, when *Novum Organum* was published, and the present time. Here in 2020, humanity is facing a global pandemic, climate change, various locations of the world have shortages of resources (water, clean air or access to power), and mass extinctions are threatened. At the same time, the economy is under pressure and will have to reform and nation state democracies are losing legitimacy – we face a change in governance in scale not unlike that of the reformation. Mankind’s exploration reaches from Earth orbit and contemplates manned missions to Mars – in 1620, the Mayflower set out to Virginia.

While Bacon writes that science is for “... *the relief of man’s estate*”¹, the range of our influence has grown, and with it our responsibility. Perhaps now, for “*estate*” we should read “*Solar System*” and for “*man*” we should read “*all life here*”?

Brief sketch of Bacon’s life

Francis Bacon was born in 1561, to Sir Nicholas Bacon, the then Lord Keeper of the Great Seal to Queen Elizabeth I, and Lady Anne Bacon, neé Cooke. Anne Bacon was Nicholas’ second wife, Francis was the second of her two sons; the only two offspring to survive infancy. Francis had six older half-siblings from Nicholas’ first wife.

¹ Francis Bacon, *The Advancement of Learning (Book 1)*, contained in *The Major Works*, Ed. Brian Vickers, Oxford University Press, 1996.

One might be tempted to think that Nicholas Bacon was born into a rich and powerful family, but his background seems to have been modest. He attended Corpus Christi College, Cambridge; spent time in France, and then entered Gray's Inn. By the time Francis was born, Nicholas was 51 years old and had reached the pinnacle of his career. The family were at that time living at York House, in London, which was in those days the residence of the person holding the role of Lord Keeper – I suppose this has a similar parallel with the use of numbers 10 and 11 Downing Street today. He held the motto "*Mediocria firma*"² – presumably, this was his own personal motto, which subsequently became the family motto, on his establishing the rise in status of his heirs. He died in 1579, when Francis was only 18. At that point in time, he had not made provision for Francis' future – so Francis' career followed a very similar pattern to his father's: working his way up in Queen Elizabeth's court whilst developing his legal career at Gray's Inn.

Anne Bacon is an interesting character, and a scholar in her own right. She was a linguist and is known for her translations of religious texts. She was very religious, and it appears, quite controlling. After Nicholas' death, she, and the family had to vacate York House. Most of siblings were already settled in lands of their own, but the settlement for Anne and her older son, Anthony, was the estate at Gorbambury. Anthony left England for France and began a career in intelligence gathering and diplomacy: he received letters from servants begging him to come home and manage the estate because Anne's management was erratic. He was away from England for 17 years.

Meanwhile Francis was living in the family rooms at Gray's Inn, pursuing his legal career and struggling to make much progress. He wrote to his uncle, Lord Burghley, (his father's long-standing friend and colleague, and his mother's elder sister's husband), to ask for help in developing his career to enable him dedicate his life to scholarship. The usual and rather pat explanation for this seems to be that Burghley was more interested in supporting the career prospects of his own son, Robert Cecil (1563-1612), later to become Secretary of State for England, Lord High Treasurer and 1st Earl of Salisbury. Clearly, Robert Cecil was a son worthy of that effort, and perhaps engineering his entrance into court took all of his father's diplomacy and skill; however, at first sight it seems cruel for him not to have made some attempt for his nephew, Francis. Two other aspects should be considered: the probable one-up-man-ship of the two mothers, and the letter that Francis sent to Burghley. It is rather a long letter, and not particularly personal, so they probably did not have a close relationship. After a paragraph concerned with his age (at that time 31), health and his good intentions, his second paragraph starts as follows³:

"Lastly, I confess that I have as vast contemplative ends, as I have moderate civil ends: for I have taken all knowledge to be my province... This ... is so fixed in my mind as it cannot be removed..."

² https://en.wikisource.org/wiki/Dictionary_of_National_Biography,_1885-1900/Bacon,_Nicholas

³ Francis Bacon, *Letter to Lord Burghley*, contained in *The Major Works*, Ed. Brian Vickers, Oxford University Press, 1996.

One assumes that Francis was well-known, at least within his family and close associates, for his precocious talents, but the letter reads a baldly arrogant. In the first paragraph, had Francis started the letter with an enquiry into the health and wellbeing of Burghley and his family, and a congratulation and acknowledgement of the advancement of cousin Robert, he might have been more endearing to his uncle. In the second paragraph, had he stated a more modest goal than “*all knowledge*”, then perhaps his uncle would have thought it more credible, and more likely to lead to self-sustaining outcome. One would think he would want to see his nephew established, but not to support him for his entire life on some wild-goose-chase. Further, the words “*fixed in my mind*” indicate a lack of flexibility, and one who cannot bend and adapt does not present a good prospect in a period of turbulent times. From our perspective of posterity, of course we can recognise the need that Francis envisaged, and which began to be addressed when The Royal Society was formed; but it has to be said that that letter to Burghley was not only misjudged, but also premature in terms of setting out the case.

Although we are left in no doubts as to his intellectual abilities, one might wonder about Francis’ communication skills and human relationship management. At age 31, he is still making tactical blunders with his interpersonal relationships. In later years, he seems more able to manage such relationships. Possibly, this might have something to do with the timing of his brother Anthony’s return from France? At this point, Francis had developed a plan to build a relationship with Robert Devereux, 2nd Earl of Essex. Anthony, not having been adequately paid for his intelligence services overseas, was in serious financial difficulties, and since Francis depended on his brother’s financial support, this was a mutual problem for them both. Essex was the young and inexperienced favourite of Queen Elizabeth I, and the Bacon brothers befriended and advised him, and Anthony worked for Essex in intelligence gathering. According to the popular histories, Essex betrayed the Queen in a military campaign in Ireland, but the truth is more complex than that. He took on the campaign despite advice from the Bacons not to, he was ill-equipped to fight against guerrilla warfare tactics, and inadequately supplied. He bargained for truce instead, and returned to England, where his actions were not understood, and sent to the Tower and later placed under house arrest. After reacting, ill-advisedly to provocation, he was accused of treason and executed.

Anthony Bacon would have been implicated in this, so it is perhaps unsurprising that Francis did exactly as ordered by the Queen, taking a prosecution rôle in Essex’ trial. Later historians have cited this of evidence Francis Bacon’s being hard-hearted and betraying his friend, but the reality is probably that there would have been little he could do for Essex, he was under orders from the Queen, and if he made any sort of waves then his brother would have been forfeit. At this point in time, Anthony was bedridden and in poor health, and he died shortly after Essex’s execution.

Queen Elizabeth I died in 1603, and was succeeded by King James VI of Scotland, who then became King James I of England. During the years in which Anthony had been intelligence gathering for Essex, he had been corresponding with King James, and this probably gave Francis an “in”. At this point, Francis’ career progressed

faster. He married Alice Barham in 1606, when she was just 14. In 1607 was promoted to the office of Solicitor General, in 1613 to Attorney General, and in 1618 at Lord Chancellor, when he was also created Baron Verulam. In 1621 he became Viscount St Alban.

I have alluded above to Bacon's lapses in judgement with regard to relationships, and I imagine that while he was exceedingly astute with cause and effect in regard to political policy, he was less astute at watching his back and recognising when he was making enemies. One of the principle problems with governance during the reigns of both Elizabeth I and James I was in controlling the flow of money. In those days, the economy assumed that the money retained the same value. In the absence of ready money at the crown, the flow of payments to the lower tiers was hindered, causing undeserved debts, and leading to high levels of usury. At the lower ends of society, there was less impact: those people traded in produce and manufactured goods. There was no stable and equitable form of taxation, and so the crown rewarded people through titles, marriages, and the grant of monopolies.

Bacon's lack of career progression under Elizabeth was likely a result of his opposition to one of her taxation policies: he evidently acted according to what he thought right, rather than quietly going along with the inevitable. Under James' rule, by 1621 the royal finances were again in serious trouble, and the actions that Bacon had taken to shore them up, and also to put right systematic abuses – controlling monopolies – were not popular with his colleagues in Parliament. Rather than an all-out coup d'état, parliamentarians settled on Bacon as one man to oust to destabilise the balance of power and retain the status quo. Again, the popular histories state that Bacon was found guilty of taking bribes, but the reality is much more complex. At that time, any man in his position would find it difficult to avoid being found guilty this way: money and gifts were given to those in high places, and such people were in need of that money to fund the costs associated with high rank which were inadequately and erratically paid by the crown. While his accusers dug for dirt on Bacon, they found surprisingly little, and much of that was attributable to poor record keeping or dishonesty with those in his employ⁴. In the end, when he realised the circumstances that he found himself in, Bacon chose to accept the charges against him, because he knew he could not win.

After his fall from office, he was barred from London and from serving at court or in parliament. The restriction on London was eventually lifted. Again, payments that he should have received did not come through, and he turned to writing to make an income. He was very ill during the period 1624/5 but recovered towards the end of 1625. In 1626 he caught a chill while out in the snow, famously trying an experiment in freezing a chicken to preserve the meat and died a few days later of pneumonia.

⁴ Nieves Mathews, *Francis Bacon: The History of a Character Assassination*, Yale University Press, 1996.

Good or Bad Scholarship?

The historian writing at any point in history, has his own perspective. In some cases, historians writing about Bacon, have an agenda which is in complete opposition to the intentions Bacon's political, educational, historical, philosophical, literary, and scientific messages. As a naive scientist, trying to play the part over historian of a historian, I find myself applying principles of science to the principles that I think should be part of good historic practise good historian practise. It seems to me, that accurate recording and diligent research are essential features of the academic process. This should be independent discipline area. So when I confront history, and I see the works of historians, I make an assumption factual or contextual information gleaned by historians is represented in an unbiased manner in the text the historian presents to the reader. Over the course of the last six months or so, I have become rather jaundiced about the quality of historical reporting.

Thomas Babington Macaulay (1800-1859)

Macaulay⁵ was a Whig politician, holding various senior appointments, but between 1834 and 1838 he served on the Supreme Council of India, in India. As an indication of the man, consider these two quotations:

“I am quite ready to take the Oriental learning at the valuation of the Orientalists themselves. I have never found one among them who could deny that a single shelf of a good European library was worth the whole native literature of India and Arabia.”

“It is, I believe, no exaggeration to say that all the historical information which has been collected from all the books written in the Sanskrit language is less valuable than what may be found in the most paltry abridgments used at preparatory schools in England.”

While in India, he was pivotal in the introduction of English as the language of education there, relegating Sanskrit and Persian to the side-lines, and leading to the term “Macaulayism” as of the educational system introduced into the colonies.

Macaulay wrote a series of “historical” essays for The Edinburgh Review, between the years 1825 and 1844. The essay about Bacon was published in 1837, it is ill-informed and did a great deal of damage to Bacon’s reputation.

James Spedding (1808-1881)

One tends to pass over the historic in favour of the modern. Surely, if there be gaps in the scholarship, or evidence not originally turned up, then a more modern historian would have access to it. It is now 180 years since Spedding set out collect Bacon’s complete works, and over the course of the next 40 years to produce his famous 15 volume edition. These volumes are readily available to study⁶.

It should be emphasised that this was true, scholarly work. Spedding collected original sources. He was thorough. He worked carefully on this for the main part of

⁵ https://en.wikipedia.org/wiki/Thomas_Babington_Macaulay

⁶ https://en.wikipedia.org/wiki/James_Spedding

his life. To be fair and honest, as the volume of Spedding's work is enormous, and my time spent studying Bacon so far very limited, I cannot make any informed comment on Spedding's contribution to the understanding of Bacon's circumstances and his contribution to the development of science.

Daphne du Maurier (1907-1989)

Well might you wonder that I include an author of novels as a serious historian of Anthony and Francis Bacon. Towards the end of her life, she took an interest in biography, and in 1975 wrote "*Golden Lads: Sir Francis Bacon, Anthony Bacon and their Friends*"⁷, which is mainly focussed on the life of Anthony Bacon, and ends with his death shortly after the death of Essex. The book, "*The Winding Stair: Francis Bacon, His Rise and Fall*"⁸ was published in 1976. Both are written in a very readable style, and supported by copious notes, and references to original sources.

The most notable aspect of her scholarship here is in her research into the details Anthony Bacon's life, but it has to be said that she brings the brothers alive and portrays them as real people with real motivations and character.

Nieves Mathews (1917-2003)

Mathews' book title, "*Francis Bacon: A History of a Character Assassination*" says it all. This is a near 600-page tome, written in very dry language. I might complain that the book is difficult to read, but I do not complain as to the quality of the scholarship. The main issue addressed is that of the reputational damage done to Bacon's memory, and therefore to the reduction in the impact that his contributions in various fields of scholarship should have had in subsequent years.

To be honest, until I started to read this book, it had not occurred to me to question that Bacon's reputation was anything other than that which it ought to have been. I had considered my relative ignorance of Bacon's life and contribution to science as symptomatic of my being too busy learning science to question science history. Now, I question everything.

Lisa Jardine (1944-2015): "the pre-eminent historian of the scientific method"

So, let us now turn to the example of a historian of our own times. Lisa Jardine's Wikipedia entry is full of glowing praise for her stellar career. Listed among her publications is an English translation of Bacon's *The New Organon*, edited by Jardine and Michael Silverthorne⁹: this is a recent work from 2000. The 21-page Introduction to this volume, written by Jardine herself, seems overtly feminist, taking Bacon's mother's part to criticise his dealings with his male colleagues, servants and friends.

⁷ Daphne du Maurier, *Golden Lads: Sir Francis Bacon, Anthony Bacon and their Friends*, Doubleday, 1975.

⁸ Daphne du Maurier, *The Winding Stair: Francis Bacon, His Rise and Fall*, Virago Modern Classics, 1976.

⁹ Francis Bacon, *The New Organon*, Ed. Lisa Jardine and Michael Silverstone, Cambridge University Press, 2000.

In the 3rd paragraph of the Introduction, sitting at the bottom of the 1st page, where even if one might decide to pass over the introductory blurb and move straight into the main text, one cannot but notice it, Jardine writes:

“Given his volatile relationship with the queen, Essex was a poor choice of backer, and in the aftermath of the Essex rebellion (following which Essex was beheaded), Francis was lucky to survive politically. But then, Bacon was never a good judge of the men in his life. Under the next monarch, James I, he made an equally unwise choice when he threw in his lot with the Duke of Buckingham just prior to his disgrace. Bacon’s mother complained frequently that he indulged his male servants and turned a blind eye to their petty thieving.”

Let us unpack this.

In the first place, like any other person, Bacon required an income. Because Bacon’s father had died before having made financial provision for his youngest son, Bacon was obliged to find employment, and he chose to find it in the legal profession. At this time, he leaned financially heavily on his family, and particularly his brother Anthony for income and support, as well as managing some of Anthony’s financial and state affairs whilst Antony was in France developing his career in diplomacy and intelligence gathering. Queen Elizabeth one, and the leadership team who should have been contributing to the costs work did not pay up, so by the time Anthony returned to England his financial affairs were very difficult. This made it impossible for Anthony to continue working directly for the Crown and between them Anthony and Francis decided but they could do good by supporting Essex, who was at that time young, wealthy, the Queens favourite and evidently a talented leader. Their Association with Essex at that time was strategic, and well-reasoned.

In Jardine’s first sentence, she immediately launches into an attack on Essex without giving context to the events that led to his execution. On reading more detailed histories, it is clear that Essex was strongly provoked, and that his actions prior to his incarceration were motivated by the possibility of creating peace in Ireland, and not treasonous as they were recorded to be. So, the history books have done us a gross disservice in recording many different fantasies about why it was necessary to execute Essex.

Jardine’s second sentence is unjustified. She says come up without providing any evidence or any context, “... *never a good judge of the men in his life*”. “Never” is a very strong word to use, without evidence, justification or even a set of convincing examples. Which men were supposedly a poor influence, or had a negative effect on the course of Bacon’s life? Should that include all the men in Bacon’s life including his father and brothers? Or should those simply be the men of whom his mother did not approve? It is Jardine’s own opinion, paraded here as an unqualified statement of fact?

In Jardine’s third sentence she uses the phrase, “*he threw in his lot*”. This is pejorative and suggests thieves, bandits, or petty criminal relationships. Jardine is tweaking our emotional response to Bacon by using such words. Now to say something about George Villiers, who was to made 1st Duke of Buckingham by

James I in 1623. Like Essex, he was a young man when he became the King's favourite, and Bacon's association with him began shortly after in about 1616. It is very much through Buckingham's interventions the Bacon rose to the position of Lord Chancellor.

I struggle to parse the final clause of that sentence, "...*just prior to his disgrace*". Does the pronoun "*his*" refer to Buckingham or to Bacon? One assumes from context, Bacon, although the suggestion that Buckingham was an "*unwise choice*" implies that "*disgrace*" could be applied to either man. Bacon's fall took place in the spring of 1621. So, if one is happy to stretch think that a period of five years can be described as "*just prior*", then it seems likely Jardine was writing of Bacon's "*disgrace*". Buckingham made a number of blunders and poor judgements, though he never did actually fall from grace, even under Charles I. He did become unpopular with Parliament and the public, and was assassinated in 1628, three years after Bacon's death. Perhaps this is what Jardine meant by "*never a good judge of the men in his life*"?

Let's move swiftly on to the final sentence of that perfidious paragraph: "*Bacon's mother complained...*" as well any mother might, but are we seriously to judge Bacon by his mother's judgement of his choice of household staff, and "facts" that she could only know by hearsay? Is it really just a one woman quoting another woman in order to make some sort of feminist statement? If that were the intention, then it failed, and seems to me more to incite and justify contempt for Bacon for his (presumed) homosexual tendencies. Lady Anne Bacon was disappointed not to have grandchildren, but in fairness to Bacon, was this an unwillingness or inability of his or of his wife, Alice Barnham? She re-married 11 days after Bacon's death, and would still have been young enough, but there was no issue there either.

As I turned the pages of the Introduction, I was filled with a growing distaste and distrust. A person who presents a biased argument in such an overt manner can hardly be trusted with opinions on interpretation of Bacon's writing, and hence understanding, of the science principles he sets out in *Novum Organum*¹⁰.

Latin: Bad Authorship or Bad Translation?

In book I of *Novum Organum*, Bacon sets out the idea of prejudice, starting in aphorism XXIII, and then developing to describe his famous four "Idols of the Mind", aphorism XXXIX and onwards.

Bacon wanted that his *Instauratio Magna*, of which *Novum Organum* was intended to be one part, would be an important and influential work. He wrote in Latin, not only to make the work accessible to foreign scholars, but also to ensure its

¹⁰ James Spedding, *The Works of Francis Bacon*, Volume 1, p194-539, Houghton and Company, <https://archive.org/details/worksoffrancisba001bacoiala/page/194/mode/2up> (digitized by the Internet Archive in 2007 with funding from Microsoft Corporation).

longevity. This might seem laughable to us, for whom Latin is no longer in common usage, but for him, looking about over two millennia in which Latin was used to record all the most significant documents, it was a logical choice.

There are some of Bacon's detractors who said that his Latin was poor, incompetent, or unintelligible. Having tried to read it myself, I would prefer to say that it is difficult and lay the blame at my door for my inadequacy. To be sure, I have found other texts to be easier and more obvious, but there is also a question as to the complexity of the message, irrespective of the language in which it is cast. Alternatively, we might lay at Bacon's door the mistake of not realising that the message should be boiled down and simplified for the easy consumption of the less gifted or more idle student.

Another problem in considering translation, is to fail to take into consideration the fads of the day. I imagine, many of you reading this now, might – if you ever did – recall studying Latin at school, at a time when languages were learnt through mastery of the grammar, and reading and translating of original texts. The 1938 *Teach Yourself Latin* by F Kinchin Smith¹¹ suggests that one might make a “literal” translation, and then a more natural paraphrase. Perhaps it was the fad of the post war period, in the dying years of a Latin grammar school education, to adopt the progressive paraphrase approach? Personally, I was born too late for the Latin class, and my approach to language learning has always been to choose a self-teaching book, preferably with badly foxed pages and smelling of mould.

Aphorism XLI

Let us put this discussion into the context of one very particular Latin text: Aphorism XLI of book 1 of *Novum Organum*. The original text reads:

“Idola Tribus sunt fundata in ipsa natura humana, atque in ipsa tribu seu gente hominum. Falso enim asseritur, sensum humanum esse mensuram rerum; quin contra, omnes perceptiones tam sensus quam mentis sunt ex analogia hominis, non ex analogia universi. Estque intellectus homanus instar speculi inaequalis ad radios rerum, qui suam naturam naturae rerum immiscet, eamque distorquet et inficit.”

Let us consider only the final sentence and look to a number of other translations:

Jardine and Silverthorne renders it as:

“The human understanding is like an uneven mirror receiving rays from things and merging its own nature with the nature of things, which distorts and corrupts it.”

Urbach and Gibson¹² give:

¹¹ F Kinchin Smith, *Teach Yourself Latin*, The English Universities Press Ltd, 1938.

¹² Francis Bacon, *Novum Organum*, trans & ed. Peter Urbach and John Gibson, Open Court, 1994.

“... and the human understanding is like an uneven mirror that cannot reflect truly the rays from objects, but distorts and corrupts the nature of things by mingling its own nature with it.”

Jonathan Bennett¹³ gives:

“The human intellect is like a distorting mirror, which receives light-rays irregularly and so mixes its own nature with the nature of things, which it distorts.”

Spedding et al.¹⁴ give:

“And the human understanding is like a false mirror, which, receiving rays irregularly, distorts and discolours the nature of things by mingling its own nature with it.”

And finally, my own literal translation, 2020:

“Thus, human understanding is the image generated from the rays from things that strike an imperfect mirror, which mixes Nature [or Reality] with its own character thereby distorting and discolouring it.”

Note that here I render the image as a metaphor for human understanding, whereas most translations liken the human understanding to the mirror itself. Bacon’s intention was to relate understanding to image – it seems obvious to me, that a thought or an idea is a mental image, something held in the mind’s eye. A mirror is a piece of metal or a silvered glass: an object which can reveal images but is not an image itself. In short, I find this a fundamental misunderstanding of the author’s intent.

The other common distortion in translation is of the word *“inficere”*, which most translations give as “corrupt”, but the more simple and obvious translation *“discolour”* makes more physical sense. The surface of an imperfect mirror not only misaligns the direction of the rays, but also selectively reflects or absorbs at different wavelengths. Bacon, thinking like a physicist, is telling of his observation of images in mirrors: the translators see the word *“inficere”* and immediately think of corrosion or of mirrors from old wardrobes where the silvering has faded.

I also posed the translation problem to Google Translate, which gives:

“The understanding is like a false mirror receiving rays irregularly, and it is the same thing, that is the nature of things by mingling its own nature, and distort and disfigure them.”

Neither Babelfish nor Microsoft will carry out machine translation of Latin.

¹³ Francis Bacon, *The New Organon: or True Directions Concerning the Interpretation of Nature*, trans. Jonathan Bennett, Early Modern Texts, 2017. <http://www.earlymoderntexts.com/authors/bacon>

¹⁴ Francis Bacon, *Aphorisms concerning the Interpretation of Nature and the Kingdom of Man*, contained in Volume 8 of *The Collected Works of Francis Bacon*, ed. Spedding. <https://archive.org/details/worksfrancisbaco08bacoiala/page/76/mode/2up>

So, did Bacon do any real Science after all?

Over the last nine months or so, I have been running weekly or fortnightly discussions about Bacon, his life, his works, and what a similar line of approach might mean to us today. One of the repeating themes is the claim that despite *Novum Organum* being a prompt to the development of science and one of the motivating factors behind the founding of The Royal Society, Bacon himself did not generate any new science understanding.

This is a view that I do not share. At this point in time, I feel hardly ready to marshal a defence. My Latin is too poor to attempt a thorough reading of *Novum Organum* in its original form; however, my reading of it in English translation seems to have led to an understanding and impression which is somewhat different to that of the others with whom I have been discussing. I have to ask myself, am I projecting my modern understanding on Bacon's reasonings? Or am I picking up on nuances of his method that are cited as examples of how one might play one factor off against another to discern the common features?

Another difference in my perception to that of others is that I regard the style of reasoning employed by Bacon to mark him out, principally, as being a physicist. His interests in topics such as heat, metallurgy, magnetism, light propagation, and gravitational forces would also suggest his being a physicist. Nevertheless, some of my discussion circle regard his methods and his thought processes to bear more in common with the modern biological sciences.

Does it matter if I do not know all the facts?

As a physicist or engineer pretending to be a historian, this is a question that worries me. In the physical sciences, one's lack of knowledge can be tested: one makes a prediction, and then carries out a fair test. If the prediction matches the test, then understanding is confirmed. If it does not, ignorance of something significant is diagnosed.

In history, at least in my view, there are two types of historical fact: those which can be confirmed by trustworthy documented evidence, and those that cannot. One hopes that the facts about the most important events are recorded in the form of evidence that can be found, while the day-to-day trivia likely to be lost ephemera. At difficult times in history, documents can become evidence in court trials: evidence that can lead to the imprisonment or execution of document author or document recipient. It is likely that such communications would have been passed by word of mouth, by letters that were burned, or written in code. Code breaking is difficult or impossible if the text containing the code does not appear to be written in code, and if the method of the code is not recorded. This paragraph answers the question of general ignorance of facts.

The more particular ignorance is that of myself. Bacon spent 65 years being himself and living through his own life and times. Spending dedicated about 40 years of his life to studying Bacon and his works. I am not Bacon, but myself, and I have invested only a tiny fraction of my life in studying his. Nevertheless, I live in these

modern times, and perhaps then I am better placed than Bacon to determine the goal for the future?

*"It is not possible to run a course aright when the goal itself has not been rightly placed."*¹⁵

What purpose is the study of history, and reflection of the work of those who have gone before, if it be not to help guide us towards a better future, and to avoid some of the pitfalls along the way?

Joining the Journey into the next 400 Years...

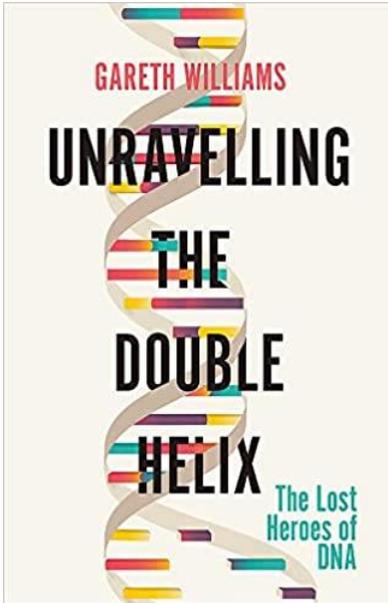
The present report for the History of Physics Group newsletter is but a staging point, and it might be some time before I form a definitive opinion on this topic. If any of you readers have opinions to share or you have facts and evidence to convince me with, I would be delighted to hear from you. The Bacon2020¹⁶ discussion fora will continue until into the New Year, and information about participating can be found on-line. One part of the plan is to produce a book reflecting the thoughts and ideas of the participants of the discussions.

¹⁵ Francis Bacon, *Aphorisms concerning the Interpretation of Nature and the Kingdom of Man*, contained in Volume 8 of *The Collected Works of Francis Bacon*, ed. Spedding. Second sentence of LXXXI.

<https://archive.org/details/worksfrancisbaco08bacoiala/page/112/mode/2up>

¹⁶ <https://ajmcmillan.co.uk/Bacon2020/bacon2020.html>

Book review



Unravelling The Double Helix
The Lost Heroes of DNA

Gareth Williams

Weidenfeld & Nicolson, UK, 2019

ISBN 978-1-4746-0935-7

494pp Hardback £20

Reviewed by Malcolm Cooper

‘Not another one’ are the opening words to the preface - which is likely to be the reaction of many on reading the title. This book, unlike many others though, encompasses the first 85 years of the history concerning DNA - a monumental task, and is complete with its heroes and a few villains, who the author does not shirk from exposing. Misunderstandings, personality clashes or just downright duplicitous behaviour - they’re all here - and rightly so, to give as accurate an account as possible.

Content

The contents page of 26 chapters is followed by a ‘timeline’ ranging from 1833 to 2001; then comes a ‘who’s who’ - both of these features - not so frequently encountered - I found very useful in helping understand just who and how were the many people involved in this labyrinthine story. Apart from an extensive index, notes and bibliography there is a glossary of terms which will be very useful to the general readership at which this book is aimed. It’s a rollercoaster story of great successes and missed opportunities.

After a brief resume of the DNA structure and its discovery, he begins the story proper in 1868 with Friedrich Miescher, who was working with pus-soiled bandages obtained from the nearby hospital. Miescher extracted a greyish material from the cells which he called nuclein - later known by its abbreviated name DNA. There follows a flashback to the work of botanist Robert Brown in the 1830s, famous for the effect named after him - Brownian motion, and for naming that small blob at the centre of each cell - the 'nucleus'. Pausing briefly to record the prophetic statement by Ernst Haeckel on the function of the nucleus, he moves on to describe the careful studies by Walther Flemming on the action of chromosomes in cell mitosis. Williams devotes a couple of chapters to the experiments of Gregor Mendel on hybridisation of pea plants and what followed on into the early years of the 20th Century. The story of Mendel's dominant and recessive elements is, of course, well known and yet the author breathes new life into it with nice portraits of the man himself, with his highs and lows. The path that led to evocatively described TH Morgan's fly room at Columbia, NY, was a rocky one strewn with obstacles and a good many villains hiding in the undergrowth.

Chapters 6 and 7 deal with the uphill struggles by such people as Albrecht Kossel and Phoebus Levene to tease out the vital components from nuclein and to identify their structures. They succeeded identifying the four bases and sugars but had to make inspired guesses as to structure. New techniques were desperately needed. That new technique was, of course, x-ray crystallography.

Williams takes the reader swiftly through from Max von Laue's discovery to the penetrating combination of William Henry Bragg and son William Lawrence using the X-ray spectrometer and Bragg's Law to determine the crystal structure. The results tumbled out but the year 1914 was approaching. This was a serious interruption to their work but the author deals with this tactfully and informatively describing some wartime activities on both sides of the divide.

Here comes a slight pause when Kossel and Levene declare nucleic acids as largely irrelevant but we are soon pitched back into action with WH Bragg at the Royal Institution and the dynamic trio of Kathleen Yardley (later Lonsdale), JD Bernal and William Astbury. With Lawrence Bragg setting up in Manchester focussing on inorganic crystals, Bragg senior's team concentrated on carbon-containing materials even to his almost capricious suggestion that Astbury tackle a human hair.

In chapters 11 and 12 the action goes back to the bacteriologists in their study of the pneumococcus. Friedrich Neufeld discovered that there were several types but thought them unchanging. Williams describes the shock that fell on Fred Griffiths working in his 'grim' path lab over a post office in Covent Garden to discover that they could change by some unknown mediation. William Astbury had - somewhat reluctantly - set up shop in Leeds, and pursued by X-ray diffraction studies of natural fibres which led him to be the first to suggest a 3-dimensional model of DNA. It wasn't correct but it was another piece in the fascinating jigsaw.

Then follows the darkest chapter in the book, relating the tragic story of the Russian botanist, Nicolai Vavilov, and giving a brief history of eugenics and its appalling consequences. It does, however, include the heart-warming story of how George Hevesy hid Max von Laue's Nobel medal from the Nazis. The author deals with other wartime events and their impact, e.g. John Randall's invention of the cavity magnetron at Birmingham, and Wilkins' involvement in nuclear weapons research, which whilst not directly germane to the story are very important in appreciating the backdrop to it.

Over at the Rockefeller Institute for Medical Research, NY, Oswald Avery, Colin Macleod and Maclyn McCarty are working on identifying the mechanism first noticed by Fred Griffiths - work which culminated in their triumphant paper of 1944 identifying DNA as the transforming agent of pneumococcus. The story so far has involved many people of different disciplines and Williams takes a pause in chapter 17 'tidying up' and a recap of the situation as at 1947. Biochemist Alfred Mirsky still clung to the idea that proteins were the genetic material but, the cracks were beginning to show.

The end was in sight even if no one could quite yet see it. But with Masson Gulland's suggestion of hydrogen bonding as a mechanism to hold the DNA molecule together, Erwin Chargaff's discovery of equal amounts of the base pairs and Roy Gosling's X-ray photographs indicating a helical structure, the scene was set for the final pieces to be popped into place. Although the history of that final 'popping' is well known and much written about, with its central cast of Maurice Wilkins, Rosalind Franklin, Francis Crick and James Watson, Williams brings a fresh look and detail to the account - warts and all. As ever, he paints all those involved as three dimensional as the double helix itself.

But it doesn't end there - the author adds two more chapters describing what happened in the years following the uncovering of the DNA structure and finishes with biographical details of the final years of the main characters.

Conclusions

This book is a complex but fascinating account from Miescher's isolation of DNA in 1868 to Crick and Watson's paper of 1953. It adopts a chronological approach to the main events but back tracks to outline biography and/or a history of what led to that point. At first I found that a little disconcerting but soon got used to it. I was most impressed with the way Williams gives evocative details - often not directly concerned with the story but gives solidity to the relevant action. I will give one example right at the beginning about Miescher's work with pus soaked bandages, he writes: 'It is a bitterly cold morning in December, 1868. We are in Tübingen, Germany...' He goes on to describe the high speed surgery of that time with colourful imagery. None of this is strictly necessary but it plunges the reader right into those early times and in my view adds considerably to the appreciation of the work being undertaken. There are 29 people listed in the 'Who's Who' but there were undoubtedly many omitted in order to keep the narrative within reasonable bounds. Williams succeeds magnificently - a 'rattling good yarn' which I thoroughly recommend.

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Published by the History of Physics Group of the Institute of
Physics (UK & Ireland)

ISSN 1756-168X

Cover picture: Top - Leo Szilard, John Tyndall. Bottom - Michael Pupin, André
Ampère