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Dorothy Wrinch (right) with her model of protein structure in 1938.

X-RAY CRYSTALLOGRAPHY

Symmetry wars

Philip Ball is gripped by the life of a remarkable scientist whose flawed theory dented her reputation.

X-ray crystallography and the study of biomolecular structure was one of the first fields of modern science in which women scientists came to the fore. Dorothy Crowfoot Hodgkin, Rosalind Franklin and Kathleen Lonsdale are the best known of the women who made major contributions in the face of casual discrimination and condescension. In *I Died for Beauty*, Marjorie Senechal suggests that there was nearly a fourth: Dorothy Wrinch, a name that few now recognize and that is often derided by those who do.

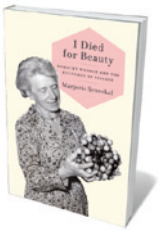
The late protein chemist Charles Tanford, for instance, poured scorn on Wrinch's best-known work, the 'cyclol theory' of protein structure, proposed in the 1930s. It was, he said, "not really worth more than a footnote ... a theory built on nothing". Of Wrinch herself, he proclaimed "she was arrogant and felt persecuted when criticized, but in retrospect her miseries seem self-inflicted".

In a bid to rebalance such attacks, Senechal, co-editor of the journal *The Mathematical Intelligencer* — for whom Wrinch was a mentor at Smith College in Northampton, Massachusetts — has written a sympathetic apologia. And whatever one feels about Wrinch and her research, she is a fascinating subject. Her circle of friends, colleagues and correspondents included many of the luminaries of early twentieth-century science and philosophy.

Wrinch, a Cambridge-trained mathematician, was the first woman to earn an Oxford DSc, in 1929. A student of Bertrand Russell, she was championed by D'Arcy Thompson and Irving Langmuir, worked alongside Robert Robinson, and knew Niels Bohr, G. H. Hardy, Kurt Gödel and John von Neumann. Several of them considered her brilliant. Calling for mathematicians to interest themselves in biology, Thompson wrote in 1931, "I do not know of anyone ... so well qualified as Dr Wrinch." The polymathic mathematician and geophysicist Harold Jeffreys developed some of his ideas on statistical reasoning in collaboration with Wrinch at Cambridge, and wrote in *Nature* in 1976 of "the substantial contribution she made to this work, which is the basis of all my later work on scientific inference".

"Wrinch never really made important contributions to one area before flitting to another."

Senechal's central question is: what went wrong? Why did an apparently so promising figure, a member of the pioneering Theoretical Biology Club that included Joseph Needham, J. Desmond Bernal and Conrad Waddington, end up relegated to obscurity?



I Died For Beauty: Dorothy Wrinch and the Cultures of Science
MARJORIE SENECHAL
Oxford Univ. Press: 2012. 312 pp. $34.95

The too-easy answer is: Linus Pauling. When, in a 1939 paper, Pauling comprehensively destroyed Wrinch's cyclol theory — which argued that globular proteins are polyhedral shells, in which amino acids link into a lattice of hexagonal rings — he also finished her career.

But a more complex reason for Wrinch's downfall emerges as the story unfolds. Part of her undoing was her magpie mind. Seemingly unable to decide how to use her substantial abilities, Wrinch never really made important contributions to one area before flitting to another — from Bayesian statistics to seismology, topology to mitosis. Warren Weaver, the astute director for natural sciences at the Rockefeller Foundation in New York, which funded Wrinch for some years, described her as "a queer fish, with a kaleidoscopic pattern of ideas, ever shifting and somewhat dizzying. She works, to a considerable extent, in the older English way, with heavy dependence on 'models' and intuitive ideas."

Senechal presents a selection of opinions that the foundation collected on Wrinch while assessing her funding application, many deeply unflattering: she is a fool; she is mad or 'preachy'; she dismisses facts that don't fit and poaches others' ideas. Frustratingly, we're left to decide for ourselves how much of this is justified, but the evidence for a problematic personality piles up.

She had a talent for making enemies. "Everyone in England in or near the protein field is more than antagonistic to her," said one of the Rockefeller interviewees. Bernal was incensed when Wrinch tried to argue that the diffraction data obtained by his student Hodgkin supported her cyclol theory — an assertion that was sloppy at best, and perhaps dishonest. In retaliation, Wrinch called Bernal "jealous, brutal and treacherous". (Hodgkin was charitably forgiving.)

Underlying all of this is the position of Wrinch as a female scientist. Like many educated women of the 1930s, Wrinch felt motherhood as a burden and barrier that only extreme measures could relieve. Her eugenic inclinations and call, in her pseudonymous *The Retreat from Parenthood* (1930),

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Georgina Ferry discusses Dorothy Hodgkin:
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for a state-run child-rearing service that farmed out children to professional carers, reinforce the fact that Aldous Huxley was only writing what he heard. Alarming though her approach to parenting now sounds, it is shameful that the professional structures of science have hardly made it any easier for mothers some 80 years on.

Wrinch's central problem, it seems, was that, working at a time when most male scientists assumed that women thought differently from them, she seemed to conform to their stereotype: headstrong, strident and reliant on intuition rather than facts. But those complaints could also be made of Wrinch's arch-enemy Pauling: Senechal rightly observes that "Dorothy and Linus were more alike than either of them ever admitted". She sees injustice in the way Pauling's blunders, such as the denial of quasicrystals, were forgiven, whereas Wrinch's were not.

Did sexism play a part here? I think not. Unlike Wrinch, Pauling hit more than enough bullseyes to compensate for his errors. Nonetheless, Senechal's imagined scene of braying men and their snickering wives poring over Pauling's devastating paper has a depressing ring of truth.

Senechal's prose is mannered, but pleasantly so — a welcome alternative to chronological plod. Yet, primarily a mathematician herself, she doesn't always help the reader to understand what Wrinch was trying to do. Her interest in "the arrangement of genes on chromosomes" sounds tantalizingly modern, but it is impossible to figure out what Wrinch understood it to mean. Neither can one easily infer, from Senechal's criticisms of Pauling's attack, that the cyclol theory was way off beam even then. Tanford has pointed out that it predicted protein structures that were "sterically impossible" — the atoms just wouldn't fit (although cyclol rings have now been found in some natural products).

Fundamentally, Wrinch was in love with symmetry — to which the book's title, taken from the 1924 Emily Dickinson poem of the same name, alludes. It was this that drew her to crystallography, and her 1946 book *Fourier Transforms and Structure Factors* is still esteemed by some crystallographers today. But such Platonism can become a false refuge from the messiness of life, both in the biochemical and the personal sense.

It is tremendous that Senechal has excavated this story. She offers a gripping portrait of an era and of a scientist whose complications acquire a tragic glamour. It is a cautionary tale for which we must supply the moral ourselves. ■

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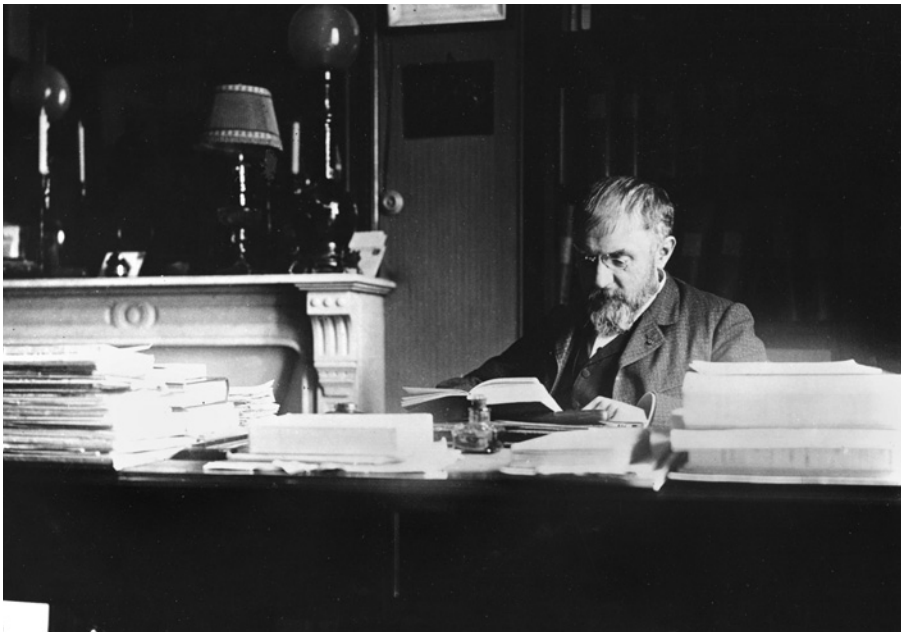


DORNAC (PAUL FRANCOIS ARNOLD CARDON)/ARCHIVES LAROUSSE, PARIS/GIRAUDON/BRIDGEMAN

Henri Poincaré posed a puzzle that remained unsolved for 99 years.

MATHEMATICS

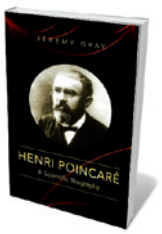
Poet of the infinite

George Szpiro celebrates a biography of the multifaceted mathematician, physicist and philosopher Henri Poincaré.

Were it not for the Poincaré conjecture, it is doubtful whether many non-mathematicians today would know of Henri Poincaré. His vexed question in topology was solved only in 2003 — nearly a century after it was published and some years after its conqueror, Russian mathematician Grigori Perelman, began to unpick it. Perelman has vanished from public view. Poincaré remains a household name.

He was hardly unknown in his day. As John Gray recounts in his masterly *Henri Poincaré: A Scientific Biography*, Poincaré was one of France's great intellectuals in the late nineteenth and early twentieth centuries. When he died at just 58 in 1912, the French Minister of Education called him "a kind of poet of the infinite, a kind of bard of science", and his funeral cortège was a veritable who's who of the world's intellectual elite.

Poincaré was also a prodigiously versatile thinker. A brilliant mathematician, equalled in his time only by David Hilbert in Göttingen, Germany, Poincaré was also rightly considered a physicist and philosopher of science of the first order. Gray encapsulates Poincaré's multiple dimensions; his intellectual biography is both a tour de force and a triumph of readability. He leads us through Poincaré's life, and the vast array of subjects he touched on, covering practically the entire corpus of what interested mathematicians and physicists at the turn of the twentieth century — from topology and algebraic geometry to Lie groups.



Henri Poincaré: A Scientific Biography
JEREMY GRAY
Princeton Univ. Press: 2012. 616 pp. $35

The field that Poincaré spawned is algebraic topology, which explores surfaces in higher-dimensional spaces using techniques from abstract algebra, the discipline concerned with mathematical structures. And, within topology, he formulated his conjecture.

Poincaré in fact posed a version of the conjecture four years before the one for which he is remembered: a theorem he decided to publish to "avoid making this work too prolonged", as he put it, with the promise of a proof to follow. Instead, he proved himself wrong by providing a counterexample. In 1904 he was much more cautious, and published the puzzle as a question. In essence, he asked whether a three-dimensional surface is equivalent to a three-dimensional sphere if rubber bands, wound around it, can be contracted, lasso-like, to a single point. He