"Pauling's Boys" and DNA Structure: Collaborative Failure in the Transition from Structural Chemistry to Molecular Biology

Pnina Geraldine Abir-Am

WSRC, Brandeis University, USA

Introduction: Pauling's failed DNA structure as a historiographical challenge

In February 1953, Linus Pauling, (1901-1994) a most accomplished and versatile structural chemist[1] who contributed, often decisively, to solving the structure of over 200 inorganic and organic compounds; author of best-selling textbooks, most notably the influential *Nature of the Chemical Bond: An Introduction to Modern Structural Chemistry* which ran into several editions since its publication in 1939; Chairman of the Department of Chemistry and Chemical Engineering at Caltech; (1937-1958) one of youngest members of the US National Academy of Science at his election in 1931; and soon to become a Nobel Laureate[2], published a profoundly flawed model of DNA structure.[3] Pauling's model was disproved only two months later, in April 1953, when seven scientists from two labs sponsored by the British government's Medical Research Council (hereafter MRC) in London and Cambridge-UK, published three back-to-back papers supporting a totally different structure for DNA, a structure which would become famously known as the DNA double helix, or just the "double helix",[4] because it revolved around two intertwined helical strands of paired polynucleotides.

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[1] For a dozen or so fields of chemistry to which Pauling contributed, by himself as well as by training students turned leading scientists, see Alexander Rich and Norman Davidson, eds. *Structural Chemistry and Molecular Biology*, (volume honoring Pauling's 65th birthday, San Francisco: Freeman, 1968) Each section includes 4-10 chapters written by over 100 former associates. Pauling's 90th birthday was marked by a volume in which the contributors were Nobel Laureates, see Ahmed H. Zewail, ed. *Linus Pauling. The Chemical Bond: Structure of Dynamics.* (Boston, Academic Press, 1992) For the range of Pauling's contributions, both scientific and public, see Paulingblog, *https://**paulingblog**.wordpress.com/* maintained by the Oregon State University Libraries, Special Collections & Archives Research Center. (hereafter SCARC)

[2] For the Nobel Foundation's justification of the 1954 Prize in chemistry to Linus Pauling and for the Laureate's Nobel Lecture see http.//nobelprize/1954/chemistry. For Pauling's nominations as a potential laureate prior to 1954 see the archives of the Nobel Foundation, Swedish Academy of Sciences, Stockholm. For the nomination background of the Nobel Prizes in chemistry in the 1940s and 1950s which explains in part why Pauling received his Nobel in 1954 see Robert Marc Friedman, *The Politics of Excellence, Behind the Nobel Prize in Science, (New York: Times Books, 2001)*

[3] Linus *Pauling* and Robert B. Corey, "A Proposed Structure For The Nucleic Acids", *Proc Natl Acad Sci U S A. 1953* Feb; 39(2): 84–97. See also Linus Pauling, "Fifty Years of Physical Chemistry in the California Institute of Technology", *Annual Review of Physical Chemistry, 16* (1965) 1-15; DOI: 10.1146/annurev.pc.16.100165.000245

[4] See the section "A molecular structure for deoxyribonucleic acid", Nature, April 25 (1953) 731-740; it includes a paper by Rosalind Franklin & Raymond Gosling, a paper by James Watson & Francis Crick, and a paper by Maurice Wilkins, William Seeds, and Robert Wilson. For the confusion over the conceptual and historical relationships between these papers as a result of their publication order (which placed the Cambridge lab paper by Watson & Crick ahead of the two papers from King's College, London, as well as a result of asymmetric references of these papers to each other, especially between the first and third papers, see Aaron Klug, *Journal of Molecular Biology, 335*, (2004) 3-26; Pnina G. Abir-Am, "DNA at 50: Institutional and Biographical Perspectives", *Minerva, 51* (2004), 167-193; idem, "Photo 51 – A Recent Addition to History-of-Science-Inspired Theatre", *History of Science Society Newsletter*, July 2012, 30-35; idem, "Setting the Record Straight: Review of *My Sister Rosalind Franklin, Une Vie a Raconter, and Genesis of the Salk Institute", Endeavour* (2015) http://dx.doi.org/10.1016/j.endeavour.2014.10.011; idem, *DNA at 50: History, Memory, and Politics in Scientific Discovery*, forthcoming. See also Robert Olby, *The Path to the Double Helix*, Section V. (London: Macmillan, 1974; New York: Dover, 1994) which was however written prior to the opening

By contrast, Pauling's structure revolved around three helical strands, though the wrong number of strands was only the first, most obvious and best known of several flaws and differences between what would emerge as the correct structure of DNA and a symbol of the new field of molecular biology, and Pauling's largely forgotten "blunder". A second key difference pertained to the location of the phosphate backbone in the center of the macromolecular assembly in the structure proposed by Pauling, as opposed to being at its periphery as discovered by Rosalind Franklin, and adopted by Crick and Watson in their second effort at a DNA model.[5]

A third difference pertained to the uncharged nature of the DNA structure proposed by Pauling, even though under physiological conditions DNA is a negatively charged acid. (DNA stands for deoxyribonucleic acid) The charges in Pauling's structure were balanced, as in a salt, largely, though not entirely, because he vastly underestimated the water content of the macromolecular assembly, being primarily concerned with the structural-chemical challenge presented by DNA, as if such a challenge could be considered apart from DNA's biological environment.[6]

A fourth difference between Pauling's proposed structure and the one proposed by the teams from the two British labs pertained to stereochemical feasibility and stability, or the requirement that certain distances obtain between different atoms in a molecule. Pauling's proposed structure was overly "crowded", i.e. it did not satisfy the minimal Van der Waals distances between various atoms, especially the larger phosphate ones. This aspect was particularly baffling since Pauling was a world expert in stereochemistry. His proposed structure for DNA thus amounted to no less than disregarding the key role of stereochemistry, which he himself had done so much to establish, for the sole purpose of securing priority in the topic of DNA structure, a topic which evidently had been and remained profoundly unfamiliar to him despite a five week "blitz" of a focused effort.

Last but not least, the fifth flaw was that Pauling's structure had no functional implications even though it was by then increasingly accepted that DNA was most closely involved in heredity.[7] This feature was also rather odd especially since Pauling discussed the type of structures that might be suitable to carry out the function of genetic duplication five

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of the archives of the above DNA co-author protagonists, and hence offers a limited understanding of this confusion.

[5] Prior to becoming "acquainted" with Franklin's decisive work, in the fall of 1951, Crick and Watson built a failed DNA model which, much as Pauling late in 1952, put the phosphate backbone in the middle of the molecule. See Olby, Klug, and Abir-Am. (note 4)

[6] This approach reflected Pauling's philosophy of chemistry's supremacy over biology, or as he put it while offering guideline to his post-doc Alexander Rich, "Do the chemistry first, and look for biological ramifications, if any, later" in Rich, "Linus Pauling's approach to biomolecular structure" in John T. Edsall, *Selected Topics in the History of Biochemistry and Molecular Biology*, (Boston, MA.: American Academy of Arts and Sciences, 1973), 71-77.

[7] Alfred D. Hershey, and Martha Chase, Independent functions of viral protein and nucleic acid in growth of bacteriophage. *The Journal of General Physiology*, 36 (1952) 39–56; this paper was discussed at the International Congress of Biochemistry held in Paris in the summer of 1952, which Pauling attended. However, Pauling spent his time in Paris as a political hero, who prevailed over the US State Department's decision to revoke his passport as a result of international pressure. Pauling did not attend Congresses of Biochemistry but was specifically invited to the 1952 Congress so as to provide him with a good reason to travel outside the US.

years earlier,[8] while specifically highlighting the role of complementary molecular templates of the sort proposed for the DNA double helix by others in 1953.[9]

Yet, during the late 1940s and early 1950s when the interest in DNA grew steadily worldwide, not only did Pauling not gravitate toward DNA but on several occasions he missed distinct opportunities for a closer encounter.[10] His failed effort at solving the structure of DNA late in 1952 and early in 1953 thus cannot be understood without a detailed clarification of those missed earlier opportunities which eventually converged into a five dimensional, colossal failure.

The historical background of Pauling's belated move toward DNA

Linus Pauling's failed attempt to solve the structure of DNA has long baffled scientists, historians of science, and others interested in great scientists and the making or missing of great discoveries. The Center for Research Integrity has even established a blog in which over 60 scientists have debated whether Pauling's DNA paper should be retracted, given the fact that it has remained incorrect on all five key counts for more than half a century.[11]

This paper departs from previous attempts to justify Pauling's failure with DNA by invoking his own busy life in both science and politics, a life which has already received considerable attention from biographers and historians.[12] This paper accepts that Pauling's many responsibilities in science management at that time, such as serving as department chairman, supervising twice the average number of graduate students per adviser in his

[8] Linus Pauling, "Molecular Architecture and the Processes of Life", Sir Jesse Boot Foundation Lecture at the University of Nottingham, May 28, 1948. The author thanks Mathew Meselson for a copy. Pauling's lecture in 1948 is also discussed in Olby. (note 7) and Crick. (note 9)

[9] For the role of Pauling's ideas on complementary molecular templates in stimulating Crick's outlook on DNA structure and its duplication function see Francis Crick, "Linus Pauling's contributions to molecular biology" in Zewail, ed. (note 1), p. 33-44.

[10] See below pp. 10-11; for greater detail see Abir-Am. (note 4)

[11] http://retractionwatch.com/2012/06/27/should-linus-paulings-erroneous-1953-model-of-dna-be-retracted/. For the stance of Paulingblog, a website dedicated to disseminating research on Linus and Ava Helen Pauling's life and work, which is maintained by the Special Collections and Archive Research Center (hereafter SCARC) at Oregon State University in Corvallis, the home of the Paulings' Personal Papers as well as of the Linus Pauling Research Institute, see https://paulingblog.wordpress.com/2009/04/28/the-pauling-corey-structure-of-dna/. This site attributes Pauling's failure to "insufficient data and an overloaded research schedule": "As a result of insufficient data and an overloaded research schedule, Pauling's structure turned out to be incorrect. However, it is interesting to see the ways in which one of the world's leading scientists went wrong with his approach to the structure of this hugely-important molecule", ibid, first paragraph. The PaulingBlog concludes in a way common among scientists: "For Pauling, this event was a single failure in a sea of successes. In fact, the very next year, he would win the Nobel Prize in Chemistry – the first of his two Nobel Prizes. Despite his embarrassing mistakes, Pauling was to remain in good standing with the scientific community", ibid. As recent as August 2012 when the author interviewed former Pauling associates Ken and Lise Hedberg at Oregon State University in Corvallis, they refrained from attempting to clarify Pauling's approach to DNA on the ground that Pauling was successful so many times (having solved over 200 structures with his many collaborators) that his isolated failure with DNA was of no consequence.

[12] Ramesh Krishnamurthy et al (eds.) "The Life and Work of Linus Pauling (1901-1994) : A Discourse on the Art of Biography", video and transcripts of a symposium held 2/28-3/2/1995, http://scarc.library.oregonstate.edu/events/1995paulingconference/index.html; Goertzel, Ted and Ben Goertzel (1995). *Linus Pauling: A Life in Science and Politics* . New York: Basic Books, Thomas Hager, *Force of Nature: The Life of Linus Pauling*. (New York: Simon & Schuster, 1995) Mary Jo Nye, "Pauling, Linus Carl (1901-1994) *New Dictionary of Scientific Biography*, ed. Noretta Koertge, (New York: Scribner's Sons, 2007) Vol. VI, pp. 36-44; John W. Servos, Physical che*mistry from Ostwald to Pauling: The Making of a Science in America*, Princeton, N.J. : Princeton University Press, 1990; Mina Carson, *Ava Helen Pauling: Partner, Activist, Visionary*, Corvallis, OR: Oregon State University Press, 2013; Pnina G. Abir-Am, "Nobelesse Oblige: Lives of Molecular Biologists*", ISIS, 82* (1991) 326-343. (which includes an essay review of the 1989 Pauling biography by Anthony Serafini)

department, updating best-selling textbooks, and being constantly in demand for talks, both professional and lay, greatly constrained the span of his attention for any topic, including DNA. Pauling's growing involvement in Cold War politics[13] also took a toll.

For example, Pauling's passport difficulties at the time[14], stemming from his political activism in causes perceived by the US State Department to be sympathetic to Communism during the McCarthy era,[15] were invoked to justify his failure with DNA in terms of his being prevented to travel to a major conference in London in May 1952. Presumably, once there, Pauling would have seen the DNA X-ray work done at King's College; and presumably he would have solved the structure of DNA once he would have been able to glance[16] at the sharpest X-ray photo obtained by Rosalind Franklin and Raymond Gosling, now widely known via books, plays and films as Photo 51.[17]

Pauling's own belated assessment that he took his time and his chance with DNA because he felt entitled to do so since in his opinion no one else could have solved it,[18] though very telling of Pauling's excessively confident state of mind, it also remains as a rather limited and misleading explanation, some sort of "wishful thinking", since Pauling continued to ignore critical work pertinent to DNA structure which had been accomplished in several

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[13] Goertzel and Goertzel; (note 12) Hager; (note 12) Carson. (note 12)

[14] See for example the Editor's note to Linus Pauling's chapter "Fifty Years of Structural Chemistry", in Stephen Graubard (ed.) *Intellectuals and Change*, summer issue of *Daedalus* on *The Making of Modern Science: Biographical Studies* **(**Boston: American Academy of Arts and Sciences, 1972) p. 787; see also Hager, (note 13) and https://paulingblog.wordpress.com/2011/03/30/pauling-obtains-his-fbi-file/

[15] Though Pauling was involved in many hearings as a result of his political activism, the passport issue stemmed from his carelessness in becoming listed as an officer of an organization viewed as a communist front by the State Department. Even though he was too busy to be so active, Pauling's sense of self importance and his admiration for politically involved scientists such as his friendly rival in the pursuit of protein structure, J.D. Bernal, who served at the time as Vice-President of the Association of Scientific Workers, led him to accept his listing as more than a regular member; he did not know that the latter category was not subject to passport revocation. Indeed, his passport was reinstated after he resigned from his otherwise empty of content position as an unusually busy officer.

[16] This assumption reflects a belief that Pauling's experience with proteins two years earlier will repeat itself with DNA, a belief which informed Pauling who may have shared it with others. Obviously, it is impossible to know what might have happened had Pauling been able to travel to UK in April & May 1952. He was eventually allowed to travel to both UK and other countries in July and August 1952, but that travel made no difference in preventing Pauling from embarking on his failed approach to DNA structure in December 1952. The author posed this very question to Pauling at a special session of the History of Science Society at its annual meeting in Seattle, WA. in 1990, a session attended by hundreds of HSS members and organized by former Caltech archivist Judy Goodstein. By then Pauling took the view that he planned to eventually tackle DNA but saw no rush in so doing. This reply sheds light on why he did nothing on DNA during 1951 and 1952, but does not clarify why he published after five weeks of work only.
This excuse revolving around the incorrect assumption that a mere glance at the best diffraction photo would have been sufficient to deduce the structure appears to have been further influenced by James Watson's account, *The Double Helix, A Personal Account of the Discovery of the Structure of DNA,* (New York: Atheneum, 1968) which remains the most widely disseminated account despite its author's various retractions, as well as a growing awareness that such an account remains not only partial but very misleading. Though that account mentions Pauling's failed attempt as a reason Crick and Watson were allowed to have a second try on DNA structure, it does not contain any information, let alone insights, into Pauling's take on DNA. For the social and historical context of Watson's account see Abir-Am (note 4) On why the DNA X-ray data was necessary but not sufficient to deduce the structure see Klug. (note 4)

[17] Abir-Am. (note 4) Lynn O. Elkin, "Rosalind Franklin and DNA", *Physics Today, 23* (2003), 42-48.

[18] "In 12/1988, Linus was the keynote speaker at the UCLA winter school on molecular evolution… As we were leaving the lecture…he suddenly asked my wife and me in his uniquely direct way if we ever wondered "why he hadn't solved the structure of DNA" …He said that one day his wife asked his question. It had made him think and he replied something to the effect of "I don't know, I guess that I always thought that the DNA structure was mine to solve, and therefore I didn't pursue it aggressively enough". Quoted in James Lake, "Why Pauling did not solve the structure of DNA?", *Nature,* 2001, 558.

major labs in both US and UK in the late 1940s.[19] He did so because much as other scientists who came of scientific age before WW2, Pauling retained a fixation on the pre-WW2 macromolecule of utmost interest, the proteins.[20]

Furthermore, as the first "winner", in 1951, of a long raging international debate on protein structure, when Pauling's discovery of the alpha-helix as a major structural principle of protein structure was published; Pauling had a huge vested interest in disseminating his major success with the alpha-helix.[21] He further saw the alpha-helix as a triumph validating not only 15 years of effort against the best structural minds at the time, but also his "indirect" approach of prioritizing structural chemistry over X-ray crystallography.[22]

Collaborative failure as the root cause of Pauling's strategy toward DNA

Under these circumstances, to which one must add the then rising political activism on the part of both Paulings,[23] due to intensification of the Cold War in the international arena with the outbreak of the Korean war in 1951, Pauling persisted in overlooking the dramatic changes in DNA research in the late 1940s and early 1950s. What remains to be addressed is why Pauling did not delegate the challenge posed by DNA structure to some of his many research associates who abounded in his lab, department, institution, and beyond.

Even more so, since other, equally busy, department chairs who had a structural interest in biomolecules, did invariably deploy such research associates in addressing the new challenge of DNA structure. Indeed, all the seven co-authors of the three back-to-back DNA structure papers published in April 1953, papers which demolished Pauling's then only two months old proposed structure, were research associates to whom the DNA challenge was delegated by their lab directors and/or department chairs.[24]

This paper thus regards Pauling's failure with DNA as an "interactive failure" or an issue of failed collaboration between him, an accomplished, senior and powerful scientist and his gifted, junior, research associates during a period of transition from structural chemistry to molecular biology.[25] Such a transition was triggered after WW2 by new solutions of biomolecular structures, most notably those of proteins and DNA. Since the complex

[19] Abir-Am. (note 4) Recent research suggests that Pauling's deliberate ignoring of most DNA labs was also influenced by a fair amount of social prejudice which he held against major DNA scientists who were women and "racial refugees"; as well as prejudice against disciplines he considered to be beneath chemistry, most notably biochemistry, and biology at large, Abir-Am, ibid.

[20] P. Srinivasan, Joseph Fruton, and John T. Edsall, eds. *The Origins of Biochemistry, A Retrospect on* Proteins. (New York City: New York Academy of Science Press, 1979) Abir-Am, "Toward a Historical Ethnography of Science: The 50th Anniversary of the First Protein X-ray Photo", *Social Epistemology, 7,* 1992. (special issue in December devoted to this topic)

[21] Linus Pauling and Robert B. Corey, "Atomic Coordinates and Structure Factors for Two Helical Configurations of Polypeptide Chains", *PNAS, 37,* (May 1951) 235-240. The travel to London in April 1952 which Pauling was prevented from taking due to the revocation of his passport, was meant to present Pauling's discovery of the alpha-helix to other protein workers, many of whom were based in UK. When eventually he reached Europe and UK in July and August 1952, Pauling still spent the bulk of his time on proselytizing for the alpha-helix. For details see Abir-Am. (note 4)

[22] For debates on the "direct" versus the "indirect" approaches to protein structure see J. Desmond Bernal, "Linus Pauling's Pattern of Work in Molecular Biology" in Rich and Davidson, (note 1) 345-356. Bernal championed the "direct" approach but despite their methodological divergences, Bernal and Pauling remained friendly since both shared a rival who anticipated both in proposing the first theory of protein structure. Abir-Am. (note 4 and note 20)

[23] Carson; (note 13) Goertzel and Goertzel; (note 13) Hager. (note 13)

[24] Five of them, organized in two teams, one team led by Rosalind Franklin and another team led by Maurice Wilkins, belonged to the Biophysics Lab and Sub-department chaired by John T. Randall (later Sir John) at King's College, London. The other two, Crick and Watson, belonged to the Molecular Structure of Biological Systems Lab, housed in the Physics Laboratory at the University of Cambridge, and directed by Sir W. Lawrence Bragg. Abir-Am (notes 4, 20)

[25] On this transition see Rich and Davidson. (note 1)

structures of such macromolecules required a wide range of instrumentation, as well as transdisciplinary team efforts, the locus of success or failure can no longer be sought in the conduct of a single individual, however accomplished, but must be pursued in connection with the rise, duration, management, and eventual dissolution of such teams.[26]

Furthermore, since Pauling's institution, California Institute of Technology (hereafter Caltech) did not accept women until the mid- and late-1950s,[27] the relevant social structure of a team or teams around Pauling in the early 1950s revolved by necessity around "Pauling's boys", i.e. a contingent of former students turned research associates who served as an easily available reservoir of labor, strategic information on scientific advances and other scientists, and specialized expertise.[28]

The question thus persists as to how "Pauling's boys" were deployed to meet the DNA challenge, as well as whether their eventual limited deployment stemmed from the fact that both Pauling and his "boys" remained captive of outdated forms of hierarchical social organization and asymmetric power relations. As a result, the paper argues, both sides were precluded from engaging in a more egalitarian collaborative effort and credit sharing, of the sort that would have been required for scaling the transdisciplinary frontier of molecular biology, or a frontier which built upon but extended well beyond Pauling's brand of structural chemistry.[29]

Pauling's rapport with his "boys" can be best understood in comparative terms, i.e. in light of similar relationships between lab directors and their respective research associates in other DNA labs. Though such a systematic comparison is beyond the scope of this paper, it is useful to remember that the more limited aim here, i.e. to clarify not only why Pauling and his many gifted "boys" failed but why they failed so badly, or why they were not even close to solving the structure of DNA, remains informed by the author's parallel study of pertinent "boys" in other DNA labs.

The question thus persists as to why, if Pauling himself has remained fixated on the pre-WW2 problem of protein structure[30], further remaining slow to seize upon the shift in the

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[26] On this issue with special reference to three major chemists and their respective collaborators and co-authors, including Pauling, see Mary Jo Nye, "Mine, Thine and Ours: Collaboration and Co-authorship in Material Culture of the mid-20th Century Chemical Laboratory", *Ambix, 61,* August 2014, 211-235; Joseph S. Fruton, Contrasts in scientific style: Emil Fischer and Franz Hofmeister, Their research schools and their theories of protein structure", *Proceedings of the American Philosophical Society 129* (1985) 313-370.

[27] For the story of a woman physicist whose gender identity was hidden by her post-doctoral adviser, the noted physicist Thomas Lauritzen, at Caltech, see Fay Ajzenberg Selove, *A Matter of Choice, Life of a Woman in Physics*. (New Brunswick/NJ: Rutgers University Press, 1994) Undergraduate women students were accepted in the 1970s only, as a result of affirmative action legislation in 1972.

[28] Mathew Meselson, "We were all Pauling's boys", in his oral history, Cold Spring Harbor: Cold Spring Harbor Laboratory Library & Archive, 2007; also as speaker on "The origins of the Meselson-Stahl experiment" at Caltech" in a session on "DNA at 60: New sources, new methods, new perspectives" organized by Pnina G. Abir-Am at the annual meeting of the History of Science Society in Boston, November 22, 2013, http://50.87.139.59/wp-content/uploads/2014/04/Apr2014-Newsletter.pdf.

[29] For details of the argument that the transdisciplinary nature of molecular biology required a restructuring of the Comptean, positivist, hierarchical and reductionist relationships between biology and the exact sciences see the theoretical, second part of Pnina G. Abir-Am, "The Biotheoretical Gathering, Transdisciplinary Authority and the Legitimation of a New Discourse in Molecular Biology: New Perspectives in the Historical Sociology of Science", *History of Science*, *25* (March 1987), 1-70.

[30] For the international dimensions of this problem prior to WW2 see Srinivasan, Fruton, and Edsall, eds. (note 20) Abir-Am 1992; for evidence in favor of the argument that Pauling's ongoing fixation upon protein structure was justified by the unenthusiastic British response of Pauling's discovery of the Alpha-helix at the famous May 1, 1952 meeting in London, as well as afterwards, see Edward Hughes, Oral History by Caltech Archives. Hughes, one of Pauling's longest serving "boys" to whom Pauling delegated the reading of his paper at the London conference once it became clear that he won't be able to attend, recalled that in Pauling's absence, all the protein worker attendees overtly expressed doubts; Ibid.

biological frontier from proteins to nucleic acids,[31] his "boys" who as members of a younger generation were not so fixated, still proved so useless in his quest for DNA structure? After all, the seven DNA scientists who co-authored the DNA structure papers which disproved Pauling's were also someone else's scientific "boys".

For example, one of those scientists, Maurice Wilkins who called himself "the third man of the double helix",[32] was a former Randall student turned long term protégé and heir[33] whose eventual place in the DNA story hinges on his role as a dual "boy" serving two masters, Randall's in London and W.L. Bragg in Cambridge.[34] Equally crucial was the "boys" status of JD Watson (1928-) & F. Crick (1916-2004) known at the time (and in science policy quarters until the late 1960s)[35] as "Bragg's boys", so as to signal their being part and parcel of the agenda of their Lab director, W. L. Bragg. (1890-1971) Holder of the most prestigious Chair of science in the British Empire, the Cavendish Chair at the University of Cambridge,[36] which he held between 1938-1954, Bragg continued to refer to these two as his "boys" into the late 1960s, when DNA structure became widely known as a major discovery, and his "boys" had become household names.

The only one among the seven authors of the simultaneously published three DNA papers in April 1953 who declined the "privilege" of being the "boy", or the "gal", of a lab director was Rosalind Franklin who left Randall's lab, relocating to Bernal's a mile away, where she was able to find the independence she so cherished. However, declining a "boy" status at a time the publication of the three DNA papers was being negotiated between the lab directors (i.e. the above mentioned J.T. Randall and W.L. Bragg) was not without consequence. The lab directors gave priority to their respective boys, as that was the only way to credit themselves at a time the work of "boys" was habitually attributed to their lab directors. As a result of this "boy effect", for half a century or roughly until 2003, neither Pauling nor other lab directors, let alone their beneficiary "boys", admitted to or revealed Rosalind Franklin's key role.

Who were Pauling's boys and why did they prove unable to prevent his DNA blunder?

Pauling's "boys" (Figure 1) were part of Pauling's accumulation of major symbolic and material assets prior to his involvement with DNA. He established key signposts in the post-WW2 biomedical frontier by putting together a team which discovered in 1949 that sickle cell anemia was a molecular disease[37]. Another team of his discovered in 1951 that the alpha-helix was a major principle of protein structure.[38]

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[31] For this shift see Abir-Am, "The Molecular Revolution in 20th Century Biology: The Impact of Three post-War Phases" in John Krige & Dominique Pestre, eds. *Science in the 20th Century,* (London, Harwood, 1997) 495-520.

[32] Maurice Wilkins, *The Third Man of the Double Helix, The Autobiography of Maurice* Wilkins. (Oxford: Oxford University Press, 2003)

[33] On Randall see Wilkins; (note 33) Abir-Am. (note 4)

[34] For details on Wilkins' concern with the succession of Randall's position as Director of the Biophysics Unit see Wilkins, note 32.

[35] Abir-Am. (note 4)

[36] Graham Hunter, *Light is a messenger; The Life and Science of William Lawrence* Bragg. (Oxford: Oxford University Press, 2004)

[37] Pauling, Linus; Harvey A. Itano; S. J. Singer; Ibert C. Wells (1949-11-01). "Sickle Cell Anemia, a Molecular Disease". *Science 110* (2865) (1949) 543–548.

[38] Linus Pauling, Robert B. Corey, and Howard R. Branson, " The Structure of Proteins, Two Hydrogen-Bonded Helical Configurations of the Polypeptide Chain", *Proceedings of the National Academy of Science, USA, 37,* (1951) April, 205-211.

Under these conditions, the category of Pauling's "boys" grew to include students,[39] post-docs, junior faculty, domestic and foreign visitors. Most often arrived on personal fellowships but were able to stay on grace to Pauling's many grants and contracts. Pauling's "boys" were often scientists who were impressed by his scientific prowess and calculated that the advantages of working in his company (access to his drive, many ideas, some of which turned out to be wrong but more often they were right, lab resources, his scientific influence and contacts, opportunity to delegate for Pauling when he was too busy, the institutional prestige of Caltech, and the life style and climate of Southern California) outweighed the disadvantages of working and living in the shadow of a giant, while having to "stand by" so as to "serve" on Pauling's many projects. (thus making it more difficult to establish one's own independent line of work)

Above all, the "boys" had to consent to a skewed distribution of scientific credit since Pauling believed that he should put his name first on any project that he had initiated.[40] Some Pauling "boys" were former graduate students, who remained in research positions in Pauling's Department for years[41]; others were former Pauling Ph.Ds who became faculty in the Chemistry Department chaired by Pauling[42]; or in other institutions[43] and provided occasional input, both solicited and unsolicited. Others were foreign visitors to Pauling's lab, e.g. Jack Dunitz who came for several years (1948-51; 1953-54) from Dorothy Hodgkin's (1910-1994) lab at Oxford, a Pauling friend and comparable colleague.[44] Dunitz suggested to Pauling to use the term "helix" rather than "spiral" as more appropriate for its three-dimensional reference.[45]

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[39] The students were mostly graduate but an occasional brilliant undergraduate such as Mathew Meselson, Pauling's last student, and the would-be "Mozart of molecular biology" was also included. Pauling handpicked him as a graduate student but also delayed his involvement with the Meselson-Stahl experiment which proved the semi-conservative mode of DNA duplication, see Pnina G. Abir-Am, "Mathew Meselson", "Franklin Stahl" and the "Meselson-Stahl experiment", *Electronic Encyclopedia of Life Sciences, eLS*, published May 2014. Meselson remained at Caltech for all the 1950s, mostly in research positions, while transferring from the Division of Chemistry from which he graduated in 1956 to that of Biology, until he left in 1961 for the Department of Biology at Harvard.

[40] On Pauling's attitude to sharing scientific credit see Nye. (note 29)

[41] For example, Ken Hedberg stayed for 8 years (1948-1956) until he left for a position at Oregon State University in Corvallis, eventually becoming its Chairman. Hedberg & Pauling shared a cultural affinity due to their origins in the State of Oregon. Ken Hedberg and his scientist wife Lise were friends of the Paulings and became instrumental in the transfer of their Personal Papers to OSU. Ken Hedberg, Oral History at OSU-SCARC; Ken Hedberg and Lise Hedberg, (separate) conversations with the author in their respective offices at OSU, August 21 and 24, 2012.

[42] E.g. Verner Shomaker, a 1938 Pauling Ph.D. who was full professor by the early 1950s and was considered to be the only one whom Pauling consulted frequently.

[43] E.g. David Harker, a 1936 Ph.D. who became director of a large scale, also in the $1 million range, project on protein structure at the Brooklyn Polytechnic Institute in New York, in 1950; or David Shoemaker, a Pauling Ph.D. in 1948 who became an assistant professor at MIT.

[44] For comparisons between Pauling and Dorothy Hodgkin see *Newsletter* of the *American Crystallographical Association,* joint memorial issue, 1994; Nye. (note 29)

[45] Jack Dunitz, "Linus Carl Pauling, 1901-1993", Biographical Memories of Members of the National Academy of Science/ USA, Washington D.C., National Academies Press, 1997, 221-261. Dunitz, Professor Emeritus of Chemical Crystallography at ETH-Zurich, left Pauling's lab twice, in 1951 and 1954, because he sought to return to his native UK. Despite several offers in the US, Dunitz returned to the Royal Institution, London, in 1956 after two years with Alex Rich, another Pauling "boy" at NIH. Dunitz was more theoretically minded than most other associates of Pauling so one can only wonder whether Pauling's effort with DNA might have fared better had Dunitz not left for UK in 1951. Though he returned on February 1, 1953, at Pauling's specific invitation to work on DNA monomers, by the time he arrived the DNA episode was over. Pauling, To Whom it may concern, December 3, 1952 (terms of J. Dunitz's appointment as research fellow: "…He will, under this appointment, carry on research on the structure of nucleoside, nucleotide, and related substances"), Pauling Folder, Jack Dunitz Personal Papers, OSU-SCARC.

In addition to their specialized skills, the relevance of Pauling's "boys" was also determined by their geographical distribution. During the key period of 1951-1953, Dunitz was away at Oxford, Edward Hughes, who served as a jack of all trades, running a wide variety of errands for Pauling including reading his paper at conferences when Pauling could not travel, and teaching sections, was away at Leeds for the year 1951-1952; and Jerry Donohoe was away in Cambridge/UK for 1952-1953. Hence, the only "boy" whom Pauling could recruit for his sudden interest in DNA was Alexander Rich[46], an MD who arrived in 1949 at the suggestion of his Harvard tutor John T. Edsall.[47] (1902-2002) Edsall shared Pauling's interest in protein structure, spent a sabbatical in Pauling's lab in 1941, remained friends with Pauling and some of his associates, and played a role in the US reception of Pauling's alpha-helix.[48]

Though Rich (who later became a central figure in molecular biology, having discovered Z-DNA, among other key discoveries in both DNA and RNA) stayed with Pauling as a post-doc for five years, (1949-1954) he was prone to be called anytime into service as a military physician, especially during the Korean War in the early 1950s. Possibly for this reason, i.e. that Rich might not be able to continue in the field of X-ray diffraction, Pauling decided not to train him in experimental crystallography, instructing Dunitz to limit Rich's participation to observing, and assisting with interpreting the X-ray photos.[49]

It is thus strange that after he had limited Rich's training, despite the fact that an X-ray crystallographic expert such as Dunitz overlapped with Rich for two years, (1949-1951) Pauling sent Rich a memo in December 1952 (when Dunitz was still in Oxford) to take better X-ray photos of DNA than those available in literature which dated to 1947 and 1939.[50]

Given the fact that it took Rosalind Franklin, a particularly gifted physical chemist with much longer research experience than Rich, (a former medical student with only three research years and little if any experimental experience with X-ray diffraction) 18 months to get Photo 51; (the sharpest in a long series which eventually led to the interpretation of DNA structure as a double helix) Pauling's cavalier memo to an inexperienced Rich in December 1952 suggests that Pauling not only underestimated the critical role of mastering the X-ray technique for obtaining fully interpretable photos but further missed the key role of collaboration between a boy and a lab director in addressing the challenge of DNA structure.

Pauling's lack of interest in investing in X-ray studies of DNA, whether at Caltech or elsewhere, is also evident from his correspondence with two of his "boys" located at other US institutions. David Shoemaker, (1923-1997) who completed his Ph.D. with Pauling in 1948 and moved to MIT, had a student who wrote to Pauling early in 1952 that he wanted to work with him on DNA. Though Pauling admitted that he had funds for such research, he did

[46] Rich and Davidson, eds. (note 1) Rich passed away on 4-28-2015, as this paper was being finalized, see https://newsoffice.mit.edu/2015/obituary-alexander-rich-dies-90-0428, downloaded 4/40/2015.

[47] Howard K. Schachman & Cyril M. Kay, "John Tileston Edsall, 1902-2002", Biographical Memories of Members of the National Academy of Science/ USA, Washington D.C., National Academies Press, 2010, 3-21.

[48] Abir-Am, "J.T. Edsall (1902-2002) biochemist and moral leader of post-WW2 American bioscience". (work-in-progress, 2012-present)

[49] Pauling to Dunitz, (cc to Robert B. Corey) January 30, 1950: (Jack Dunitz Personal Papers, Pauling folder, OSU-SCARC) "…not worthwhile to train AR (Alex Rich) in experimental technique of X-ray diffraction since he is not planning to continue in this field". Pauling was further concerned that X-ray equipment might be misused or damaged, because they are planning to do "so much X-ray work during the coming year".

[50] W.T. Astbury "X-Ray Studies of Nucleic Acids." 1947, Symposia Soc. Exp. Biol. 1: 66–76. Astbury, W. T., & Bell, F. O. (1939). X-ray data on the structure of natural fibres and other bodies of high molecular weight. Tabulae Biologicae, 17, 90–112.

not respond in a timely fashion, so the MIT student, upon completing his Ph.D. with Shoemaker, took a teaching job in a small college.[51]

David Harker[52], (1906-1991) a 1936 Ph.D. of Pauling and since 1950 the director of a large scale project in protein structure at Brooklyn Polytechnic Institute in New York, hosted in 1953 two foreign visiting post-docs intimately involved with DNA research, Vittorio Luzzati of Paris and Francis Crick of Cambridge/UK.[53] Though Crick arrived in the second half of 1953, or only after he completed his Ph.D. thesis, as well as co-authoring several papers on DNA structure; Luzzati arrived in December 1952 and told Harker that his former colleague in Paris, Rosalind Franklin, by then at King's College, London, where she continued to consult with Luzzati, worked out a 3-dimensional contour map of DNA and was willing to collaborate with Pauling on its interpretation. Harker passed this critical information to Pauling but to his surprise, Pauling was in no rush to meet with Franklin.

Pauling still hoped for a repetition of his experience with the alpha-helix, when an experimental piece of data which delayed his publication of the alpha-helix for two years, turned out to be irrelevant because it was not due to the basic structure of the protein but to supercoiling. Pauling published only when industrial or artificial fibers of polypeptides were shown to lack that feature of supercoiling.[54]

Pauling continued to avoid a meeting with Franklin during the critical months of February and March 1953, at a time he was planning a trip to Europe with a stop over in England to see his second son Peter, a first year research student at Cambridge, UK, in Bragg's lab where he was befriended by his boys, Crick, Watson, and Wilkins. Instructing Peter on how to organize his visit in England, Pauling again gave low priority to a potential meeting with Franklin.[55]

The question persists as to whether Harker (or Shoemaker beforehand) could have pressed Pauling to meet with Franklin sooner, or to start the work on DNA sooner. Harker already knew what may happen in such a case. When he sided with another woman scientist who contested Pauling's opinion, Dorothy Wrinch, (1894-1976) on the key issue of protein structure in the late 1930s,[56] Harker was threatened by Pauling in no uncertain terms.

By the time Pauling arrived in England, in the first week of April 1953, the double helix model had already stood up at Cambridge, where Pauling had to admit, before WL Bragg and his various boys, a coterie of rivals who prided themselves at beating him at his own game; as well as at the prestigious Solvay meeting in Brussels in mid-April 1953, to which he travelled in WL Bragg's company, that Bragg's "boys" were right and he, Pauling,

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[51] Correspondence of Pauling and David Shoemaker, David and Clara Shoemaker Personal Papers, OSU-SCARC, Folder "L. Pauling".

[52] Herbert A. Hauptman, "David A. Harker, 1906-1991", *Biographical Memoires of Members of the US National Academy of Science, 74* (Washington D.C.: The National Academies Press, 1998) 1-19; http://www.nap.edu/catalog/6201/biographical-memoirs-v74

[53] Vittorio Luzzati, *Une Vie a Raconter*, Paris, Editions HB Temoignage, 2011; Abir-Am 2015, (an essay review of Luzzati's autobiography jointly with other books including one on Rosalind Franklin) note 4.

[54] Pauling and Crick solved the problem of supercoiling in the fall of 1952; on this episode see Hager, (note 6) chapter 15; Olby, (note 4) Abir-Am. (note 4)

[55] This sordid episode of Pauling avoiding a meeting with Franklin, the only person who could have saved him from major embarrassment, is analysed in detail in Abir-Am (note 4) Peter Pauling did not know Franklin but heard his friends complaining about her lack of interest to cooperate with them even though they had nothing to offer, so he informed his father that Franklin was seen as difficult. Pauling seemed reluctant to contact Franklin because he did not meet her earlier, so she remained an unknown quality with whom he did not know how to deal.

[56] Abir-Am. (note 10)

was wrong.[57] Still, it took Pauling longer to digest such news, since at a meeting at Caltech in May 1953 he was still championing his own model as if the outcome was still undecided.

Even in September 1953, when he had to modify the program of an international meeting he had long planned at Caltech to celebrate his success with the alpha helix as the long sought solution to the structure of proteins, so as to add a session on DNA, Pauling still devoted the bulk of the meeting to protein structure. The transition from structural chemistry to molecular biology was not quite the one he had envisaged, as DNA was not about to remain very long in the shadow of proteins. Pauling's "vision" fell short of sustaining the molecular revolution in biology which had DNA at its center.[58] Jack Dunitz observed that chemistry was about "mental discipline, adventure, and aesthetic experience"[59]; yet, neither attribute quite captured Pauling's wishful thinking that biology in general, and DNA in particular, were merely derivative, or just an afterthought for a very busy structural chemist.

Though Pauling's prioritizing of proteins and politics over DNA in the period 1951-1953 suggests why he was nowhere near a solution of DNA structure, his inability to treat his own "boys" as potential partners also played a role in the ultimate irony of Pauling's not only being beaten by a top rival's "boys", but being beaten with help from Pauling's own "boys". During the "finale" of the DNA saga, between January and April 1953, Pauling's "boy" Jerry Donohoe[60] (1920-1985) shared an office with Bragg's boys at the Cavendish, while Pauling's biological boy, the scientifically inclined second son Peter, had also been there as a first year research student since the fall of 1952, also in WL Bragg's lab.

These two were sent there in part because Pauling hoped they would enable him to keep an eye on his competition, but they were handicapped not only by the clever strategy of Bragg's boys but also by the fact that Pauling would not deploy his "boys" as equal partners. Since Pauling greatly appreciated the shock value of surprise, that is to say a bad and shocking surprise for his rivals, he concealed his purpose from his own "boys".

Pauling cherished the alpha-helix not only because of its scientific value but also because at the same time it exposed the scientific nakedness of his rivals; all their prior models of protein structure were demolished by the alpha-helix in one masterful stroke. Little did Pauling know that he will share that experience of being similarly exposed in less than two years. Perhaps, it was a matter of some consolation that he did not invest as much time in DNA (five weeks) as his rivals, WL Bragg and his various boys, had invested in proteins. (over a decade)

So, what went wrong with Pauling's boys and their encounter with DNA?

Long accustomed to accept Pauling's supreme power as lab director, department chairman", and public figure, the "boys" lost (if they ever had) the ability to argue with him. Though some "boys" voiced objections (e.g. Verner Shomaker, a full professor regarded as brilliant but one who did not publish much and could not match Pauling's standing) Pauling ignored them since in the aftermath of recent big successes, especially the alpha helix, he was no longer seeking or listening to advice. Pauling kept the boys in the dark re: his interest in DNA since he sought to shock rivals and "boys" alike with his latest trick. So, what could Pauling's "boys" have done?

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[57] Linus Pauling to Ava-Helen Pauling, April 1953, in Ava-Helen & Linus Pauling Personal Papers, SCARC-OSU.

[58] Abir-Am. (note 31)

[59] Jack Dunitz to E.G. Cox, April 18, 1951 while quoting Cyril Hinshelwood's Centenary address to the Chemical Society in UK as published in its journal in 1947, p. 1271; Jack Dunitz Personal Papers, OSU-SCARC, Folder, C-misc.

[60] Mark Ladd and Sidney Kettle. "In memoriam: Professor Jerry Donohue". *Journal of Chemical Crystallography* ***15*** (4) (1985) 303.

On the issue of improving Pauling's timetable, i.e. of starting earlier to work on DNA, rather than the last five weeks of 1952, which turned out to be both "too late and too little" even for a master structurist who tackled the structures of 225 compounds; none of the "boys" surrounding Pauling knew of his tacit interest in DNA. Even Shoemaker and Harker who could mediate between Pauling and their own acquaintances who had an interest in DNA were careful not to try to influence Pauling's priorities. Corey and Shomaker had no interest in DNA, Rich and Peter Pauling was too inexperienced, while Donohue, ended up helping Pauling's rivals.

On the issue of collecting better X-ray data, Pauling could have suggested that Rich, the only post-doc with a biological background, be trained by Dunitz as soon as Pauling learned of the existence of high quality DNA X-ray photos at King's Collge, London. Even if Dunitz had no interest in either proteins or DNA he could have trained Rich. But again, Pauling did not suggest that Dunitz train anyone in X-ray crystallography of biological compounds, further limiting the training Dunitz was asked to give to Rich.

As to collaboration in DNA model building, Pauling left it in the hands of Robert Corey, his sole co-author of the DNA structure paper, who however had no interest in DNA. Corey was also afflicted with health issues due to his work on explosives during WW2, but Pauling did not provide him with help other than students he supervised directly, and who again were not taught to develop an interest in DNA. Though Corey was shown by Franklin her great X-ray photos in mid-1952, he apparently did not pay attention since at that time Pauling was still fighting his old battles with protein structure.

As to the issue of help with calculations, though Pauling habitually used help from old timer colleagues such as Stephen Weinbaum, he left the DNA calculations to Corey who had no interest in DNA. Even Shomaker criticized the strereochemical features of their proposed DNA structure Pauling did not listen since he remained confident that he could fiddle with the stereochemical parameters.

On the issue of delegating responsibility for presenting Pauling's papers at major conferences, again Pauling relied on close associates such as Corey and Hughes but neither (they presented the alpha-helix paper in Pauling's absence at the May 1952 critical conference in London) could offer a suitable substitute for Pauling's impressive lecturing style, rhetorical abilities, and show business tenor of his public talks. As a result, Hughes noted that the attendees came out missing the importance of the alpha-helix.[61]

On the issue of collecting information on Pauling's rivals while visiting their labs, neither Donohue, an advanced "boy" and expert in hydrogen-bonding, nor Peter Pauling, an inexperienced first year graduate student socializing with leading scientists because of the latter's interest in his father, were technically or mentally capable of such a demanding task, especially since Pauling did not sufficiently guide them as to what to specifically collect,[62] further overwhelming them with frequent demands for general information of all sorts.

As to the issue of betraying Pauling by passing information on him to his rivals, both Donohoe and Peter Pauling ended up sending faulty information to Pauling on his rivals, intentionally or otherwise, to some extent because Pauling offered nothing in return. Pauling's, and their own belief in Pauling's omnipotence may have obscured from them the fact that they were actually serving Pauling's rivals.[63]

[61] Hughes, Oral history. (note 35)

[62] Abir-Am. (note 4)

[63] Both Donohoe and Peter Pauling remained obsessed for the rest of their lives with their inability to reveal what they witnessed at a time they were forced to pass information from and to Pauling as well as from and to his rivals. For details on their role see Abir-Am. (note 4)

Conclusions

Pauling and his boys were nowhere close to solving DNA structure, despite the many assets at their disposal. Key scientific (a-d) and personal character issues played a key role in malpositioning them as a collaborative endeavour addressing the challenge of DNA structure.

a) Pauling's ongoing obsession with protein structure issues even after the alpha-helix was published and became a peak of his achievements, led him to focus on consolidating his gain rather than opening new frontiers. The "boys" were deployed to enhance old victories rather than seek new ones.

b) Pauling's relationship with the Division of Biology at Caltech remained shallow, merely social, (Ken Hedberg , Jack Dunitz, others' testimony) or a scheme to get big money. This meant that Pauling missed the rising interest in DNA among biologists, because his Caltech Biology main contacts whom he helped hire (George Beadle, the Chairman and Max Delbruck, the bacterial virus geneticist) also failed to do so, because they long aligned with Pauling's own preference for proteins.

c) Pauling's refusal to meet with Rosalind Franklin at the informed advice of his mature "boy" David Harker suggests gender bias, as befits someone whose entire scientific career was spent in an institution which denied women the right to an engineering and scientific education. Long accustomed to pliant "boys", Pauling could not see that his only salvation (i.e. rescuing him from a terrible DNA model which would taint his reputation, despite various efforts to "cover up" the sheer magnitude of his blunder) might come from a "gal", or just a colleague of the other gender.

d) Pauling's failure to follow up on Erwin Chargaff's discoveries of the base-ratios in DNA in the period between 1947-53, reflects a combination of scientific bias against biochemistry as presumably a lesser discipline than structural chemistry. But it also reflects Pauling's cultural and racial bias against émigré biochemists, whose cultivated demeanor he could not match thus leaving himself indifferent to their plight as Central, Eastern and Western European refugees from the totalitarian onslaught of European fascism. Having ignored Chargaff's base-ratios, Pauling had no chance to come close to a solution of DNA structure, even if he had overcome his gender bias so as to deal with Rosalind Franklin, as an equal.

e) Pauling's habit of taking advantage of others, or of engaging in unilateral transactions always beneficial to himself but not to other parties, (as with Randall, Todd, among others) a habit stemming from his déclassé background and need to struggle to support his widow mother and siblings, precluded collaborations on an equal footing with others, whether lab directors and research associates who had pursued DNA earlier; or even his own "boys".

Since Linus Pauling has long been a scientist hero, having won two (sole) Nobel Prizes, scientists, historians of science, biographers, commemorating colleagues, and other authors chose to focus on his many achievements, often dismissing his failure with DNA as an aberration of no consequence. By contrast, this paper has argued that Pauling's failure with DNA is uniquely instructive for better understanding not only his lack of proximity to an eventual solution of DNA structure, but also his role as leader of many teams, yet a leader so steeped in bias against other scientific disciplines, as well as in social prejudice on all counts of race, class, gender, and their intersectionality that he entirely missed the transition from structural chemistry into molecular biology, a transition which was to largely revolve around the ramifications of DNA structure.

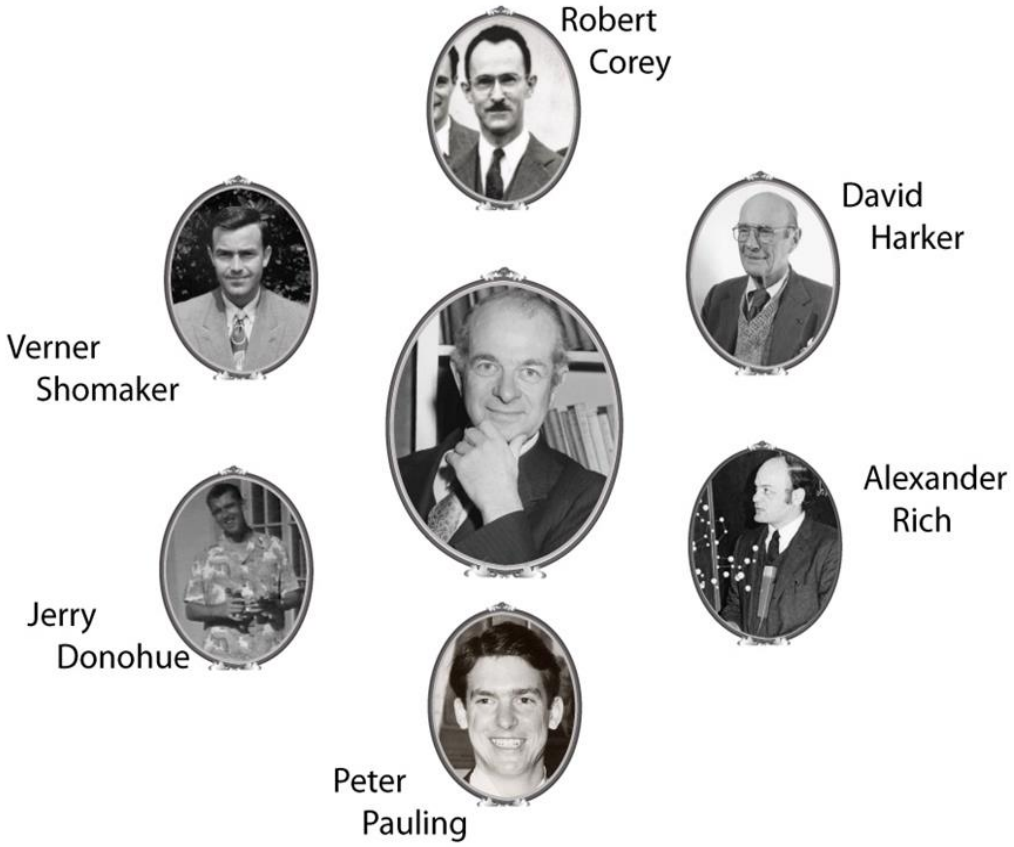

Figure 1: "Pauling's boys" most related to his quest for DNA structure, by Christy Turner, Special Collections & Archives Research Center, Oregon State University Library

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