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Academia Americana de Mecánica ● American Academy of Mechanics

Volume 33, Number 7-8 **July-August 2004**

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mechanics (ISSN 0076-5783) **POSTMASTER**: Send address changes to *mechanics*, Subscription and Membership, ESM, MC 0219, Virginia Tech, Blacksburg, VA 24061 (Tel. 540-231-6871: Fax 540-231-2290). Editorial and Advertising: Horacio D. Espinosa, Northwestern University, 2145 Sheridan Rd., Evanston, IL 60208-3111 (Tel. 847-467-5989, Fax 847-491-3915). Membership in the American Academy of Mechanics includes the subscription to *mechanics*.

American Academy of Mechanics
Academia Americana de Mecánica

LETTER FROM THE EDITOR

Dear AAM members,

I hope you are enjoying the new format of *Mechanics*. The electronic publication of table of contents from mechanics journals has been discontinued. This was a board decision after a careful examination of the value and cost of such publication. I would like to express a special thank to those that submitted articles and opinions to facilitate the transition and make the magazine more relevant. I hope you all appreciate and value the fact that *Mechanics* is a voice within our community and that, as such, your participation and commitment will increase in the future.

In this issue, an Op-Ed to the "Opinion" written by Professor Mohamed Gad-el-Hak, March-April, 2004 issue of Mechanics, is published. The author of the Op-Ed is Professor Emeritus Gerald Wempner from the Georgia Institute of Technology. You will find his comments very revealing and provocative. The author hopes the community will examine ideas and courses of action to ameliorate the problem of excessive publication of irrelevant articles and what is probably more important the lowering of standards in doctoral dissertations. Professor Wempner asked: "Is money the root of all evil?" In my opinion, "money is not necessarily the problem." Resources are a key component towards the advancement of science and engineering. In this regard, US institutions of higher education are the envy of many others in the world. We have vibrant and extremely dynamic institutions supported by diversity, absence of hierarchical structures of power deciding what research should be pursued, truly opened competitions in which the best people are hired, etc. Nonetheless, because of the variety and number of opportunities that such system offers, we are not very effective in achieving a balance between obtaining resources and being truly innovative and creative. As highlighted by Professor Wempner, this is in part because of a university system that promotes fund raising without a proper check and balance of associated accomplishments. In this regard, it is interesting to read the obituary of Francis Crick, published in the NY Times and reprinted in this issue, and ask ourselves what were Cambridge University measures of academic accomplishment at the time Crick and Watson made the DNA discovery.

While resisting excessive administrative greed would seem a formidable and difficult task, for the reasons hinted by Professor Wempner, each one of us *can and must* make a difference! Firstly, we can reassess measures of academic achievement within our own research groups and in our capacity of mentors of students and post-docs. Secondly, those in administrative positions can resist the commercialization of higher education by implementing more relevant criteria for promotion and tenure as well as hiring. Thirdly, those serving as editors or as members of editorial boards in a variety of journals can deal with the problem of publication of irrelevant materials head on.

In closing, I would like to highlight that there had been examples of fine leadership within our mechanics community. One I had frequently heard of is the contribution made by Dan Draker as Dean of Engineering at the University of Illinois, Urbana-Champaign. Our generation must learn from such examples and have the courage to change the *status quo*.

Sincerely,

Horacio D. Espinosa, Editor

Op-Ed

Dear Professor Espinosa:

You could call this letter an addendum to the "Opinion" written by Professor Mohamed Gad-el-Hak in the March-April, 2004 issue of *Mechanics.*

I certainly agree with the concerns expressed by Professor Mohamed Gad-el-Hak, but, if we are to change this most unhealthy situation, we must identify the origins and the causes. In my opinion, the roots go much deeper than the author has indicated; indeed, the trend originated at least one-half century ago, in the wake of World War II, and it started in the United States. It is perhaps not coincidental that the United States emerged from that conflict as the strongest and wealthiest country in the world; it appears that many of our traditional values began to erode at that time. Since I am addressing the history of the questions and, of course, only from my personal observations, let me cite a few personal anecdotes.

When I was a graduate student at the University of Wisconsin in 1953 I had the occasion to speak with the dean of the graduate school. That dean was a renowned professor of chemistry in the twilight of his academic career. He subsequently acceded to the chancellorship of the entire university system. Some years later (1962) I had an opportunity to speak with the dean of engineering at the University of California in Berkeley. That dean was a prominent professor of electrical engineering; the position was a temporary one which rotated among the senior faculty. He spoke of *serving* his colleagues and his eagerness to return to his academic pursuits. These experiences suggest that such positions of leadership at some of the best institutions were occupied by persons of academic repute and mindful of scholarly values. In recent years I have noted that similar positions at many of our institutions are filled by career administrators; most have acquired a doctoral degree, but moved quickly into the administrative ladder. I will address the question of these graduate degrees, but when I have raised additional questions about the prevailing mechanisms of recruiting, supporting and rewarding our graduate students.

Let me turn to the matter of academic research and critical changes which followed in the wake of World War II. It was at this time in our history when the various governmental agencies began to funnel funds into higher education, *but* under the guise of research contracts. Not surprisingly, this had profound effects on faculties, students and administrators (Is "money the root of all evil"?). Gradually, colleges of engineering became increasingly dependent on this new-found source of funding. Values shifted. The measure of research became the quantities of dollars; the best professors had the fat contracts. Of course, these same professors required a labor force to implement the *research;* this, in turn, required the recruitment of graduate students. A possible consequence was the lowering of requirements. We must note too that many of the funding agencies had specific engineering problems which required quite restrictive solutions and procedures; moreover, the agencies placed certain time restrictions on the contracts. Now, one must ask several questions: If one anticipates a solution in a specified period, then is one doing research? If a graduate student is assigned a specific task and, perhaps, given the procedures, then is he/she pursuing original research. We know that many of these efforts are eventually accepted as doctoral dissertations.

Finally, we must combine the shift in administrative mentalities with the emphases on these new measures of *academic* achievement. Of course, one must have some system of evaluation to acknowledge the achievement of students and faculties. As important, the governmental agencies and their political appointees want means to judge the administrators of the academic institutions. The politicians seldom have histories of academic or scholarly achievement. (It has been many years since we have had a Thomas Jefferson in the white house.) This mix has led to the prevailing measures: The *number* of dollars, the *number* of students and the *number* of publications. From the top (the politician), to the next level (the academic administrators) and finally (it seems) to some faculties who have few (if any) of the traditional measures of scholarly achievement, i.e., critical searches of the literature, new concepts, original solutions, etc.

I realize that my diatribe serves only to present my historical perspective. I have offered no solutions. Indeed, it seems to me that it will require powerful economic forces, or some vary courageous leadership at the highest level. I do not foresee the latter. I do foresee the former, since no society can continue to enjoy our comforts and prosperity without a stronger foundation of intellectual effort and consequent achievement.

Sincerely yours,

Gerald Wempner
Professor Emeritus
Georgia Institute of Technology

NEWS

US-South America Workshop
Mechanics and Advanced Materials Research and Education

Sponsored by
the National Science Foundation and the American Academy of Mechanics

The US-South America Workshop on Mechanics and Advanced Materials: Research and Education was held during August 2nd to 6th, at the Caesar Park Hotel in Ipanema, Rio de Janeiro. It was co-organized by Profs. Glaucio H. Paulino (UIUC), Horacio Espinosa (Northwestern University), Fernando A. Rochinha (COPPE/UFRJ), Lavinia Borges (COPPE/UFRJ), and Ney A. Dumont (PUC-Rio).

The workshop provided first hand information from leading experts regarding the latest technology in areas such as *nano, smart, graded, composite and bio-* materials. The researchers, who delivered lectures in this workshop, are renowned world experts in their respective areas. Seven graduate students from the U.S. also attended the event, who received a fellowship through competition coordinated by the awards committee of AAM. This workshop combined research, development, technology transfer and training activities. The meeting promoted integration of researchers around joint work-programs and contributed to strengthening the scientific and technological basis of both USA and South America.

The workshop consisted of an opening ceremony, a short course on "Mechanics of Materials at Nano and Micro scales" (delivered by Prof. Nasr Ghoniem, from UCLA), a two-day symposium with presentations by North- and South-American researchers, one-day guided technical site visits and networking, students' presentations and poster sessions, and a post-conference short course on "Multiscale Multiphysics Computational Solid Mechanics" (delivered by Prof. Jacob Fish, from RPI).

This event was supported by the National Science Foundation (NSF), the American Academy of Mechanics (AAM) and by several important state and federal government agencies in Brazil (e.g. MCT: Ministry of Science and Technology; AEB: Brazilian Aerospace Agency; CNPq: National Council for Research and Development; CAPES: Coordination for Improvement of Academic Personnel; FAPERJ: Foundation for Research in the State of Rio de Janeiro). The event was very successful! An interesting aspect is that we got a lot of coverage in the news (newspaper, radio and TV). There is a web-site dedicated to the event, which can be found at the following URL address: http://cee.uiuc.edu/paulino/workshop2004

ANNOUNCEMENTS

ECF16
16th European Conference of Fracture

Failure Analysis of Nano and
Engineering Materials and Structures

Alexandroupolis, Greece, July 3-7, 2006

http://ecf16.civil.duth.gr

Under the auspices of the European Structural Integrity Society Sponsored by the American Society for Testing and Materials, The Society for Experimental Mechanics of USA, The British Society for Strain Measurement

General Information

Started in 1976, the European Conference of Fracture (ECF) takes place every two years in a European country. Its scope is to promote world-wide cooperation among scientists and engineers concerned with fracture and fatigue of solids. The conference is under the auspices of the European Structural Integrity Society (ESIS).
ECF16 will focus in all aspects of structural integrity with the objective of improving the safety and performance of engineering structures, components, systems and their associated materials. Emphasis will be given in the failure behavior of nanostuctured materials and nanostructures.
ECF16 will comprise invited lectures together with contributed oral and poster presentations covering all aspect of fracture and fatigue. During the conference special symposia covering major areas of research activity will be organized. The language of the conference will be English.

Conference Chairman

Emmanuel E. Gdoutos, Democritus University of Thrace, Greece

Location

The conference will take place in the city of Alexandroupolis located in Thrace which occupies the northeastern corner of Greece. Thrace has a unique scenic beauty with areas of historical interest and archeological importance. The nearby islands of Thasos and Samothrace offer ideal locations for pre or post conference vacations. A daily excursion to Samothrace will take place during the conference.

Conference Tracks

Track 1
Nanomaterials and Nanostructures

- Fracture and fatigue of nanostructured materials
- Failure mechanisms
- Nanoindentation for fracture of nanostructured materials
- Fatigue and fracture of MEMS and NEMS
- Failure modes and failure analysis of nanodevices
- Fatigue and fracture at atomistic and molecular scales
- Micromechanisms in fracture and fatigue
- Thin films
- Electronic materials
- Failure of nanocomposites

Track 2
Engineering Materials and Structures

- Physical aspects of fracture
- Brittle fracture
- Ductile fracture
- Nonlinear fracture mechanics
- Fatigue and fracture
- High temperature fracture
- Fretting fatigue
- Polymers and composites
- Ceramics
- Ice
- Fracture mechanics analysis
- Surface treatment technologies
- Probabilistic approaches to fracture mechanics
- Computational fracture mechanics
- Experimental; fracture mechanics
- Creep fracture
- Environment assisted fracture
- Dynamic, high strain rate, or impact fracture
- Damage mechanics
- Residual stress effects
- Concrete and rock
- Sandwich structures
- Novel testing and evaluation techniques
- NDE
- Mixed-mode fracture
- Structural integrity
- Scaling and size effects
- Mesofracture mechanics
- Smart materials and structures
- Fracture of biological materials
- Geophysical and tectonic problems

Important Dates

Second Announcement: **December, 2004**
Submission of Abstracts: **June, 2005**
Notification of Acceptance/Rejection: **September, 2005**
Submission of Complete Papers: **January, 2006**
Conference: **July, 2006**

Conference Proceedings

All invited and contributed oral and poster papers will be published in camera ready form in the conference proceedings.

Measuring, Monitoring and Modeling Concrete Properties
in honor of Surendra P. Shah

Organized by
Maria S. Konsta-Gdoutos
Democritus University of Thrace

A special symposium within the context of ECF16, in honor of Surendra P. Shah, Walter P. Murphy Professor of Northwestern University will take place **(http://ecf16.civil.duth.gr)**
.

Symposium Topics

- Fracture mechanics of cementitious composites, concrete, stone and masonry
- Micromechanics of cementitious composites
- Advanced sensing techniques for structural health monitoring
- Engineering performance and modeling for early age concrete
- Degradation, repair and rehabilitation of concrete structures
- FRP's and textiles in cement composites

OBITUARIES

Professor emeritus and mechanics of collision expert Werner Goldsmith dies at 79

By Patti Meagher, College of Engineering, University of California at Berkeley

BERKELEY – Werner Goldsmith, professor emeritus at the University of California, Berkeley, an international authority on the mechanics of collision, and a pioneer in the biomechanics of head and neck trauma, died Aug. 23 after a brief illness. He was 79.

He died peacefully in his Oakland home, surrounded by his wife and children.

Best known for his classic textbook "Impact," Goldsmith had a 55-year academic career as a mechanical engineer and educator at UC Berkeley. He served as an expert consultant in several high-profile actions, including the two trials evolving from the beating of Rodney King. Recently, he devoted much of his energy to research on shaken baby syndrome, mounting a campaign in 2001 to caution physicians and prosecutors to use biomechanics in assessing apparent cases of child abuse.

Werner Goldsmith

(Peg Skorpinski photo)

"My father had a brilliant mind and tremendous energy," said his daughter, Andrea Goldsmith, an associate professor of electrical engineering at Stanford University, who credits her father with inspiring her to go into engineering.

"He was working on a paper, proofreading and editing the week before he died. He always finished things with perfection." The paper, "Brain injury in infants and children," co-written with John Plunkett, M.D., is scheduled for publication this year in the American Journal of Forensic Medicine and Pathology.

"Werner was a great man, with friends all over the world," said Professor Emeritus Jerome Sackman of UC Berkeley's Department of Civil & Environmental Engineering, a friend and longtime colleague. "Post docs and graduate students came from Asia, from Europe, from all over to work with him, and many of his students are now holding leading positions in government, industry and first-class universities all over the world."

Goldsmith's 1960 monograph, "Impact: The Theory and Physical Behaviour of Colliding Solids," was the first textbook to systematize the mechanics of collision. Reprinted in 1998 by Dover Publications, the book analyzes the mechanics of colliding solids in everything from car crashes to refinery explosions and remains the premier textbook in the field.

A registered mechanical and safety engineer for the state of California, Goldsmith was sought after as a consultant for nearly 50 years in the areas of impact, vehicle collisions, head and neck injuries, and the effectiveness of protective devices such as sports and armed forces helmets. In May 1992, he was consulted by the Los Angeles District Attorney's Office to provide expert testimony in the prosecution of police officers charged in the beating of Rodney King. He only accepted four to five such cases a year, everything from accidents caused by the tipping of all-terrain vehicles to quadriplegic or paraplegic injuries resulting from football and other sports.

"He testified sometimes for the defense, sometimes for the prosecution, whatever side he thought was correct," said George Leitmann, a Professor in the Graduate School at UC Berkeley and Goldsmith's second Ph.D. student. "He was a person of very strong convictions that were very firmly held and very firmly expressed."

After the publication of "Impact," Goldsmith moved from wave propagation and impact phenomena in military and other applications to rock mechanics and biomechanics. Although primarily an experimentalist, he always accompanied his results with analytical and numerical models. His seminal contributions included developing the foundations of the boundary element method (BEM), a powerful calculation tool used in numerical analysis when purely analytical methods of evaluation will not suffice.

Goldsmith was born in Dusseldorf, Germany, on May 23, 1924. An only child, Goldsmith emigrated in his early teens to the United States, the only member of his family to escape Nazi Germany. His parents, Siegfried and Margarethe Goldschmidt, died in Auschwitz.

He attended high school in New York City and Mount Vernon, New York, then the University of Texas, where he received his undergraduate degree in just three years and a master's degree in mechanical engineering in 1945, the same year he became a U.S. citizen. He supported his education by working as a country club bellboy, a newspaper boy, a typist at the university library, and a reader.

After two years as an engineer at Westinghouse Electric Corporation and an instructor at the universities of Pittsburgh and Pennsylvania, Goldsmith came to UC Berkeley in 1947. He completed his Ph.D. in mechanical engineering in just two years, simultaneously holding an appointment as instructor. He earned his degree in 1949 and was then appointed assistant professor, becoming a full professor by 1960.

Goldsmith's research was supported by 40 grants and contracts, and he produced more than 225 publications and invited surveys. He generated a combined total of 80 master's theses and Ph.D. dissertations among his students. He retired in 1987, but was immediately recalled as a Professor in the Graduate School to continue his research and supervision of graduate students. In 1997, he wrote "Mechanical Engineering at Berkeley: The First 125 Years," tracing the history of mechanical engineering on campus.

In the early 1970s, he co-founded the Graduate Group in Bioengineering, which would later evolve into the departments of bioengineering at both UC San Francisco and, in 1998, at UC Berkeley. Goldsmith's significant role in the genesis of the department very recently was recognized by his appointment as professor emeritus of bioengineering, an appointment that required approval from the chancellor's office. Since 1951, Goldsmith served as a consultant and mechanical engineer to the U.S. Naval Weapons Center at China Lake, Calif., investigating trajectory of missiles, susceptibility of materials to explosives, and other aspects of ballistics. In 1966, he was invited to chair the Head Injury Model Conference of the National Institutes of Health, working with neurosurgeons to pioneer modern medicine's understanding of the biomechanics of head and neck injury. He also served as a member of a National Research Council Committee, evaluating the efficacy of the Materials and Weapons Directorate of the Army Research Laboratories.

Goldsmith's many awards included a Guggenheim fellowship and two Fulbright fellowships. He was a member of the National Academy of Engineering, a fellow of the American Academy of Mechanics, and an honorary member of the American Society of Mechanical Engineers. In honor of his 70th birthday, an entire issue of the International Journal of Impact Engineering was devoted to his work. In 1995, he received UC Berkeley's prestigious Berkeley Citation and, in 2001, he was honored by the UC Berkeley College of Engineering with its Distinguished Engineering Alumni Award.

An avid traveler, Goldsmith once boasted about using all of every sabbatical available to him. In addition to his native German and English, he also spoke French and Greek, and collected maps as well as stamps depicting maps. As a younger man, his interests included playing classical piano and tournament bridge.

Goldsmith is survived by his wife, Penelope Goldsmith of Oakland; daughters, Andrea Goldsmith of Menlo Park and Remy Margarethe Goldsmith of Oakland; son, Stephen of Santa Rosa; and four grandchildren.

Donations in his memory may be made to the Berkeley Engineering Annual Fund, College of Engineering, University of California, Berkeley, 208 McLaughlin Hall (1722), Berkeley, CA 94720-1722; or Bay Area Holocaust Oral History Project, P.O. Box 1597, Burlingame, CA 94011-1597.

Francis Crick, Co-Discoverer of DNA, Dies at 88*

By NICHOLAS WADE

Francis H. C. **Crick**, co-discoverer of the structure of DNA, the genetic blueprint for life, and the leading molecular biologist of his age, died on Wednesday night in a hospital in San Diego. He was 88.

He died after a long battle with colon cancer, said Andrew Porterfield, a spokesman for the Salk Institute, where he worked.

Dr. **Crick** laid the foundations of molecular biology in a sustained burst of creativity that began in 1953 with the discovery of the structure of DNA, the hereditary material, in Cambridge, England, and ended about 13 years later with the subject's primary problems solved, most of them either by Dr. **Crick** or by scientists in his circle.

The discovery of the structure of DNA resolved longstanding questions about the nature of the hereditary material and the manner in which it is copied as one generation succeeds another. The proposal for the structure, almost immediately accepted, was electrifying to scientists not only because of its inherent elegance but also because it showed how biology, evolution and the nature of life itself could ultimately be explained in terms of physics and chemistry. Indeed, the desire to replace religious with rational explanations of life was a principal motivation of Dr. **Crick**'s career.

So central is DNA to biology that the names of **Francis** H.C. **Crick** and James D. Watson, his American colleague in the discovery, may be remembered as long as those of Darwin and Mendel, the architects of the two pillars of modern biology, the theory of evolution and the laws of genetics.

Some consequences of understanding the structure of DNA are already familiar, from linking suspects to crime scene evidence to manipulating it to make genetically engineered crops. But these are just foretastes of a gene-based medical revolution that is expected to unfold in the years ahead now that the human genome -- about three billion units of DNA, encoding all the biological information needed to generate and maintain a living person -- has been deciphered.

Dr. **Crick** was a scientist with a thirst to understand and a talent for productive friendships. It was his two-year collaboration with Dr. Watson that made possible the discovery of the structure of DNA, a feat that each has said he would not have accomplished without the other. After Dr. Watson returned to the United States, Dr. **Crick**'s close collaborator for many years was Sydney Brenner, with whom he solved the nature of the genetic code.

Dr. **Crick** occupied a rarely paralleled position of intellectual leadership in the early years of molecular biology. In intense efforts to explore beyond the door opened by the discovery of DNA, biologists from Paris to Pasadena, were drawn into a pursuit that at every stage was shaped by **Francis Crick**.

"By brain, wit, vigor of personality, strength of voice, intellectual charm and scorn, a lot of travel, and ceaseless letter-writing, **Crick** coordinated the research of many other biologists, disciplined their thinking, arbitrated their conflicts, communicated and explained their results," the historian Horace Freeland Judson wrote in "The Eighth Day of Creation."

The French biologist Jacques Monod told Mr. Judson: "No one man discovered or created molecular biology. But one man dominates intellectually the whole field, because he knows the most and understands the most. **Francis Crick**."

A unforgettable portrait of **Francis Crick** was drawn by Dr. Watson in "The Double Helix," his best-selling account of their discovery. Mr. **Crick** was unknown at the time, pursuing his Ph.D. at the advanced age of 35. But the lack of this credential did not diminish his confidence in his own abilities.

"I have never seen **Francis Crick** in a modest mood," Dr. Watson wrote in the first sentence of his book.

He described Mr. **Crick**'s animated conversation, his manic laughter, his habit, infuriating to colleagues, of pumping them for their data and showing them what it meant.

"Conversation with **Crick**," Dr. Watson wrote, "frequently upset Sir Lawrence Bragg," the director of the Cavendish Laboratory in Cambridge, where Mr. **Crick** then worked, "and the sound of his voice was often sufficient to make Bragg move to a safer room."

Yet Dr. Watson's vivid portrait held elements of caricature. Mr. **Crick**'s immodesty did not extend beyond the realm of intellectual argument.

"Rather than believe that Watson and **Crick** made the DNA structure, I would rather stress that the structure made Watson and **Crick**," Dr. **Crick** wrote diffidently in a memoir, "What Mad Pursuit."

On the day of the discovery, Dr. Watson asserted, "**Francis** winged into the Eagle," the dingy Cambridge pub where they lunched every day, "to tell everyone within hearing distance that we had found the secret of life."

Dr. **Crick** did not remember that incident, he has written, but he did recall going home and telling his wife, Odile, that he seemed to have made a big discovery. Years later, he continued, Odile told him that she had not believed a word of it, saying, "'You were always coming home and saying things like that, so naturally I thought nothing of it."

Rejected Tenured Position

Francis Harry Compton **Crick** was born on June 8, 1916, in Northampton, England, where his father and uncle ran a boot and shoe factory founded by their father. He studied physics at University College, London, and after a short period researching the viscosity of water under high pressure (in his view "the dullest problem imaginable"), he was drawn by World War II into military research, working on the design of magnetic and acoustic mines. He did so well at this job that after the war, Dr. R. V. Jones, the head of Britain's wartime scientific intelligence, wanted Mr. **Crick** to succeed him. But Mr. **Crick** chose research.

"Looking back, it was absurd because I had a tenured job," he said in a recent interview. Finding himself at loose ends after the war, he decided the most interesting research problem lay in trying to understand the physical basis of life, the division between the living and the nonliving. The choice eventually drew him to the Cavendish Laboratory in Cambridge, one of the world's leading centers for studying the structure of proteins by X-ray analysis. At 35, he started working for his Ph.D. on the structure of proteins.

Proteins were already understood to be the cell's working parts, and Mr. **Crick** began with studying the structure of hemoglobin, the oxygen-carrying pigment of the blood. He worked in a branch of the Cavendish, the Medical Research Council unit, headed by Dr. Max F. Perutz. Well before his thesis was finished, however, he realized that a far more interesting problem was the structure of deoxyribonucleic acid, or DNA. A classic experiment of 1944 had pointed to DNA as the genetic material. But biologists had made almost no progress since then in understanding how DNA might store hereditary information and few were actively working on the problem.

Mr. **Crick**'s life was changed one day in October 1951 when a Dr. Watson, a 23-year old American biologist, walked into his life. Dr. Watson also understood that the structure of DNA was the key to everything. Neither was supposed to be working on DNA, but they at once fell into discussing how the problem might be approached, in conversations so sustained that the pair were given their own small office at the Cavendish laboratory so their voices would not disturb everyone else.

"Jim and I hit it off immediately," Dr. **Crick** later wrote, "partly because our interests were astonishingly similar and partly, I suspect, because a certain youthful arrogance, a ruthlessness and an impatience with sloppy thinking came naturally to both of us."

Their approach, copied from the great chemist Linus Pauling, then at the California Institute of Technology, was to build exact scale models of the DNA that would be compatible with the limited information available from X-ray crystallography, a method of probing a chemical's structure with X-rays. A political difficulty was that the problem of DNA's structure had been assigned to another scientist, Maurice H. F. Wilkins of King's College, London. Under the etiquette of British science, to Dr. Watson's amazement, no one else was supposed to muscle in on it.

But Dr. Wilkins, a wartime friend of Mr. **Crick**'s, said he did not object to his trying a model. Dr. Watson and Mr. **Crick** soon had one ready. It was based, in part, on X-ray data about DNA obtained by Dr. Wilkins's colleague, Dr. Rosalind Franklin. Dr. Watson had heard Dr. Franklin describe these data in a public lecture but had misunderstood them.

For their model, Mr. **Crick** and Dr. Watson constructed the backbone of the DNA molecule in the form of a spiral, or helix, with the winding chains held together in the middle by metal ions. The bases, the four chemical subunits that spell out the genetic information, pointed outward from the chains because the two researchers could see no way that the necessarily irregular sequence of bases would match together neatly if they pointed inward.

With the model completed, Mr. **Crick** invited Dr. Wilkins and Dr. Franklin to Cambridge to inspect their progress. Dr. Franklin instantly recognized a glaring error, and a few days later Dr. Bragg, embarrassed by the debacle, ordered Mr. **Crick** to do no more work on DNA.

Nonetheless, Mr. **Crick** and Dr. Watson kept thinking about the problem and a few months later were able to reverse Dr. Bragg's prohibition. The precipitating event was the announcement by Linus Pauling, who was Dr. Bragg's peer and rival, that he had found the solution to the structure of DNA. Mr. **Crick** and Dr. Watson knew that Pauling's solution was wrong, but believed it might be only days before Pauling realized his error and seized on the solution.

In their second attempt, Mr. **Crick** and Dr. Watson picked up several important clues. As part of a reporting system designed to share information among laboratories supported by the British Medical Research Council, Mr. **Crick** came to see a correct version of the X-ray data that Dr. Franklin had reported at the lecture attended by Dr. Watson. Although Dr. Franklin had insisted that these data proved DNA could not be a helix, Mr. **Crick** understood that they proved the opposite and that the two chains were antiparallel, in other words that the head of one was always laid against the tail of the other.

The two biologists had also belatedly learned of Chargaff's rules, named for Erwin Chargaff, a longtime student of DNA at Columbia University. The four bases that occur in DNA are known as adenine, guanine, thymine and cytosine, or A, G, T and C for short. Chargaff had discovered that from whatever organism DNA was isolated, A and T were found in roughly equal quantities, as were G and C.

Ingredients in Place

From Jerry Donohue, an experienced American chemist who happened then to be sharing their office, Mr. **Crick** and Dr. Watson also learned the true chemical structures of the DNA bases and the fact that the structures shown in current textbooks were incorrect.

The ingredients for the discovery were now all in place. With the right structures in hand, Dr. Watson was one day playing with cardboard cutouts of the four bases when he noticed that an A-T pair on his small desk was identical in shape with a G-C pair. He immediately perceived how the bases could point inward, holding the spiral staircase together with steps of always equal width, provided that adenine always paired with thymine, and guanine with cytosine.

The pairing rule at once explained the equivalences of Chargaff's rules and, more critically, how one DNA chain could serve as the template for building another, the essential requirement for any molecule that embodied hereditary information.

"That morning," Mr. Judson wrote in "The Eighth Day of Creation," "Watson and **Crick** knew, although still in mind only, the entire structure: it had emerged from the shadow of billions of years, absolute and simple, and was seen and understood for the first time."

In his memoir, Dr. **Crick** said: "It's true that by blundering about we stumbled on gold, but the fact remains that we were looking for gold. Both of us had decided, quite independently of each other, that the central problem in molecular biology was the chemical structure of the gene." No other scientists were pursuing the structure with such single-mindedness.

It took only a few days to build the model dictated by their new concepts. This time it convinced everyone because it explained everything.

"It has not escaped our notice," Mr. **Crick** wrote in a lapidary conclusion to their report of April 25, 1953, in the journal Nature, "that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material."

After making the discovery and completing the requirements for his Ph.D., Dr. **Crick** plunged into the problems now made accessible by the new structure. How did the sequence of bases in DNA determine the sequence of amino acids in the ribbon-like structure of each protein molecule? How was the information copied from DNA and transferred to the cell's protein-synthesizing centers?

Though many scientists played important roles in solving this array of problems, the guiding intelligence at almost all points was Dr. **Crick**'s. It was he, for example, who first realized there could be only a specific number of amino acids, the building blocks of proteins. Scanning the confused biochemical literature, he drew up the canonical list of the 20 acids.

With his colleague Sydney Brenner, Dr. **Crick** eventually proved, in an experiment of remarkable elegance, that the genetic code was a comma-less, triplet code in which a set of three bases determines an amino acid unit and a string of triplets thereby specifies the full sequence of amino acids in a protein chain. The **Crick**-Brenner experiment essentially consisted of deleting bases, one by one, in the DNA of bacteria and showing that only after three bases had been eliminated in close proximity did the DNA-transcribing system come back into correct phase.

In a conversation in 1960 with the French biologist Francois Jacob, Dr. **Crick** and Dr. Brenner recognized the long-puzzling identity of the messenger chemical, now known as messenger RNA, that distributes copies of the genetic information in the cell's nucleus to the protein-making apparatus in the cell's periphery.

In another insight of remarkable power, Dr. **Crick** in his "adaptor hypothesis," divined that there must exist both a class of carrier molecules that recognize triplets of bases on the messenger and adaptor enzymes that link each kind of amino acid to its appropriate carrier. Biochemists ridiculed the idea, saying that if the adaptor enzymes existed, they would already have found them. But both the transfer RNA's and the adaptor enzymes proved to exist, as Dr. **Crick** had predicted.

Dr. **Crick** derived several sweeping theories that have stood the test of time. He assumed from the start that the genetic code was universal to all forms of life, as indeed with trivial exceptions it has proved to be. His "central dogma" formulated the view that once genetic information had passed into protein, it could not get out again. The dogma meant that the genetic message was impenetrable by information from outside the cell, thus excluding the Lamarckian thesis that acquired characteristics could be inherited.

Nobel Prizes for Three

In 1962, Dr. **Crick**, Dr. Watson and Maurice Wilkins received the Nobel Prize in medicine for their work on DNA. Dr. Wilkins and Dr. Franklin had contributed the X-ray data that suggested and confirmed the structure of DNA, but Dr. Franklin died of ovarian cancer in 1958.

The discovery of DNA brought unwelcome attention, too. In 1967 Dr. **Crick** read a draft of Dr. Watson's account of their discovery, "The Double Helix." The memoir, which then bore the working title "Honest Jim," was a startling departure from the usual staid accounts of laboratory life. After its opening declaration about Dr. **Crick**'s lack of modesty it adroitly portrayed the participants' feelings as the helter-skelter pursuit of DNA wound to its resolution. Dr. **Crick** viewed the gossipy narrative as a betrayal of their friendship, a violation of his privacy and a distortion of their methods and motives. He was unsuccessful in efforts to prevent the book's publication.

Dr. **Crick** later came to terms with his colleague's account.

"In those days there was a different convention, at least in Britain, about writing about your friends," he said in an interview in 2003. "But I came out of the book quite well, apart from the first sentence. As Peter Medawar said, the person who comes out worst is Jim."

One of the problems caused by the book was Dr. Watson's implication that the pair of them had obtained Dr. Franklin's data on DNA surreptitiously and hence had deprived her of due credit for the DNA discovery. Dr. **Crick** believed he obtained the data fairly since she had presented it at a public lecture, to which he had been invited. Though Dr. Watson had misreported a vital figure from the lecture, a correct version reached Dr. **Crick** through the Medical Research Council report. If Dr. Franklin felt Dr. **Crick** had treated her unfairly, she never gave any sign of it. She became friends with both Dr. **Crick** and Dr. Watson and spent her last remission from cancer in Dr. **Crick**'s house.

Dr. **Crick** gave his younger colleague no equivalent cause for complaint. Dr. Watson acknowledged the selflessness of Dr. **Crick**'s motives.

"**Francis** was always so kind to me," he said in an interview in 1998. "He never tried to promote himself. He was just interested in solving problems."

By 1966, the first era of biology at the molecular level was complete. Though many details of enormous interest remained to be discovered, the foundations had been well and truly laid. Dr. **Crick** and Dr. Brenner decided to move on to another vast field of biology, the manner in which a whole organism develops from the fertilized egg.

In 1977 Dr. **Crick** left Cambridge, and his well-known house on Portugal Place, with its golden helix above the front door, where he and Odile had held many high-spirited parties. The Cricks moved to the Salk Institute in San Diego. There he took on another challenging unsolved problem of biology: the nature of consciousness.

He had little expectation of producing any radically new ideas at age 72, he wrote in 1988, "but at my time of life I had a right to do things for my own amusement." Never one to let his mind lie fallow, Dr. **Crick** produced a stream of papers about aspects of the brain and a well-regarded popular book in 1994, "The Astonishing Hypothesis," which summarized his ideas.

Another diversion that Dr. **Crick** allowed himself was a bold speculation about the origin of life. Only the most eminent and secure of scientists would dare flirt with the idea that earth may have been seeded with life by a rocket ship from another planet. But that possibility, a thesis Dr. **Crick** termed "Directed Panspermia," was aired in an article he published in the journal Icarus (1973) with his Salk Institute colleague Leslie E. Orgel and in a popular book by Dr. **Crick** alone, "Life Itself" (1981).

Dr. **Crick** in no way rejected the orthodox scientific thesis that life evolved in some way, yet to be specified, from the chemicals present on the early earth. But he was impressed by the unexplained universality of the genetic code and uncomfortable with the narrow window of time between the date the earth cooled enough to be habitable and the first appearance of life in the fossil record. With "Directed Panspermia," he prepared, in effect, an intellectual escape hatch, an alternative explanation for life should scientists in fact find it too hard to account plausibly for the remarkably rapid emergence of earth's first life forms.

Dr. **Crick**'s style of practicing science was unusual. Most biologists do experiments; he did so very rarely, being one of biology's few theoreticians. He did not take graduate students, preferring instead to work with a single colleague. His scientific interlocutors were, after Dr. Watson, Dr. Brenner during the Golden Age of molecular biology, and Dr. Christof Koch, for his work on the brain.

"**Francis** essentially works alone but likes to have a colleague to play against, so to speak," Dr. Brenner said recently.

Dr. **Crick** wrote little about his own life and, despite his fame, remained a surprisingly private person. His first marriage, to Ruth Doreen Dodd, ended in divorce in 1947. He is survived by his wife, the former Odile Speed, an artist; a son from his first marriage, Michael F. C. **Crick** of Seattle; and by two daughters from his second marriage, Gabrielle A. **Crick** and Jacqueline M-T **Crick** Nichols, both of England; and four grandchildren.

What is the nature of scientific genius? Dr. **Crick** was perhaps offering an answer in his response to a different question, that of whether he enjoyed his life.

"I cannot do better," he said, than to quote from a lecture by the painter John Minton "in which he said of his own artistic creations, 'The important thing is to be there when the picture is painted.' And this, it seems to me, is partly a matter of luck and partly good judgment, inspiration and persistent application."

RETROSPECTIVE: Francis Crick (1916-2004)*

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In the latter half of the 1950s, I had the good fortune to be accepted by Francis Crick and his co-workers as an observer of, and occasional verbal contributor to, their efforts to understand DNA replication, protein synthesis, and other aspects of classical molecular biology. Francis and I became good friends, so I have had the opportunity to observe his mind at work in Cambridge, England, and later at the Salk Institute, where he served in an advisory capacity until 1977, and then as a faculty member until his death.

I will not attempt to summarize Francis' scientific achievements in detail; that is a task for historians of science. My list of favorite papers that he authored or coauthored would include those on diffraction by a helix, coiled-coils, the adaptor hypothesis, wobble pairing, the three-letter code, the structure of collagen, the prediction of an

"RNA world" and, of course, the two short papers on the structure of DNA that launched many thousands of manuscripts. I would include selfish DNA but, since I was a coauthor, I realize that I may be prejudiced. Success in science may depend on many factors: imagination, intellectual power, experimental skill, persistence and, of course, luck. The series of important contributions that Francis made to structural and molecular biology rules out luck as a major factor in his case.

If luck didn't come into it, what explains Francis' extraordinary achievements? His intellectual power and remarkable intuition in all matters structural and biological are by now legendary. Watching him in action, I was always amazed at his ability to get his mind around a set of disparate and sometimes contradictory facts and in very little time force them to order. He seemed to know instinctively which facts he should take seriously and which he could ignore. He often advised that one should not abandon a good theory because of a few contradictory facts--not good advice for most of us, but it seemed to work for Francis.

I never saw Francis Crick in a pompous mood. He was always confident in public debate and, at the beginning of his career, he was sometimes assertive, but he never resorted to reputation or seniority to further his point of view. He had no interest in becoming part of the power structure of science, but was generous with his time when he thought his advice might be useful. The Salk Institute benefited greatly from his numerous suggestions.

Francis did not suffer fools gladly. In his younger days he may have dismissed them a little harshly, but he became gentler as he grew older. He liked new ideas, and he didn't care where they came from. Surprisingly, he was always prepared to give careful consideration to ideas that seemed lunatic fringe to most of us, if he thought that they might possibly contain even a grain of truth. If he decided that they didn't, he would patiently explain to the authors what was wrong--but rarely more than once. He had a nose for any results that "smelled fishy" and would make an appropriate facial gesture when describing them.

At the Salk Institute, Francis switched from molecular biology to the neurosciences. I heard him say on a number of occasions that he did not expect to make a major contribution himself, but that he hoped to point younger scientists in the right direction. He was convinced that understanding consciousness, or at least its neural correlate, was the most important goal in neuroscience and that the time was ripe for an experimental approach. I am not competent to judge the importance of the contributions that he and his longtime collaborator, Christof Koch, have made; I suspect that the jury is still out. However, there is no doubt about his success in attracting other scientists to the field. When Francis began writing about consciousness, mention of the subject would probably have doomed a grant application. Nowadays, conferences on consciousness attract thousands.

The last few months of Francis' life were among the most striking. He was suffering serious discomfort from the side effects of chemotherapy and was sometimes slowed down mentally by the effects of painkillers. Knowing that time was short, he concentrated almost entirely on his work. He became interested in the role that a relatively little understood part of the brain, the claustrum, might play in consciousness. Within a few months he had mastered the literature to the point that he was writing a paper that included a lengthy review section. The last time we talked about science, 2 weeks before his death, he was as excited as a schoolboy about two new ideas that had occurred to him in the past day or two. On the last day of his life he was correcting the manuscript on the claustrum. Francis died as he had lived, striving to understand how the biological world works.

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