History of Physics
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The Author in Dialogue:

Jimena Canales's *The Physicist and the Philosopher: Einstein, Bergson, and the Debate That Changed Our Understanding of Time*

by Robert P. Crease
Stony Brook University, Stony Brook, NY

Over 150 people attended the March meeting session devoted to Jimena Canales's book, *The Physicist and the Philosopher: Einstein, Bergson, and the Debate That Changed Our Understanding of Time*. It was the second of an annual series of FHP-sponsored sessions in which the author of a recent book on history of physics meets critics and commentators. The 2017 session was held on the third floor of New Orleans' huge convention center. The atmosphere was relaxed and genial, with the papers and conversation directed, not at professional physicists or professional philosophers, but to those interested in the general subject of the nature of time, and assumed a modest shared knowledge of both physics and philosophy. Canales, from the University of Illinois at Urbana-Champaign, and four other people spoke.

Canales's book was built around a 1922 episode in which Einstein and Bergson clashed about the nature of time; she discussed both the buildup to and the aftermath of the event, in which physicists and philosophers continued to display a lack of understanding for each others' position. "The book has been more successful than I ever imagined," Canales began. Part of the controversy, she continued, was fueled by physicists' negative attitude towards philosophy, and she cited numerous examples. Then she noted that most if not all physicists have implicit philosophies, but these are so prevalent and taken for granted that they are not usually recognized as philosophical at all. She also pointed to negative reviews of her book, associating a defense of Bergson with postmodernism, fake news, and specious thinking. The value of reviving interest in the controversy, she said, was in reviving discussion of the difference between psychological and philosophical time -- between, say, measuring the length of melodies and being able to hear them as melodies at all.

Jean Bricmont, of the Université catholique de Louvain, spoke next, giving a talk entitled "Bergson vs. Einstein: is there really a philosopher's time?" His answer seemed to be in the negative. He explained the scientific issues of relativity that he said had led Bergson into error. Relativity, he said, had nothing to do with consciousness, as Bergson seems to have thought, but with clocks. Bricmont declared, though, that

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Jimena Canales of the University of Illinois at Urbana-Champaign

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History *of* Physics
NEWSLETTER

The Forum on History of Physics of the American Physical Society publishes this Newsletter biannually at *http://www.aps.org/units/fhp/newsletters/index.cfm*. Each 3-year volume consists of six issues.

The articles in this issue represent the views of their authors and are not necessarily those of the Forum or APS.

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March 2017 Meeting Session Reports:
“The Pais Prize Session for Mary Jo Nye”

by Joseph D. Martin
University of Cambridge

Mary Jo Nye of Oregon State University

Nowhere is better suited than New Orleans to honor the work of Mary Jo Nye, winner of the 2017 Abraham Pais Prize for History of Physics. The city blends artistic, musical, linguistic, and culinary influences from a vibrant collection of cultural traditions, and so presented a fitting backdrop for a celebration of Nye’s contributions to history of physics, which have often examined how methods, theories, and practices blend in the spaces where scientific disciplines meet. The Forum on the History of Physics–sponsored prize session at the APS March meeting showcased work that followed Nye’s spirit of exploring boundaries between disciplines and discussing the interfaces between science and politics.

Nye herself spoke on one of the issues that has defined her career, asking how the boundaries of physics have evolved since the beginning of the twentieth century. She used the broad perspective her work provides to examine how the history of physics has responded—or not—to the prevailing winds of the physics profession. Nye’s analysis showed that historians of physics have largely reproduced the prestige hierarchies that exist in physics itself, focusing their attentions on relativity, quantum mechanics, and nuclear and high energy physics. But she also identified recent trends that show the field waking to the wider collection of investigations that often move fluidly across the boundaries of the physics,

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Session Report:
“60 Years Since BCS and 30 Years Since Woodstock”

by Brian Schwartz
Brooklyn College and The Graduate Center of the City University of New York

Since the session dealt with physics history and was sponsored by the APS Forum on the History of Physics (FHP), it seems worthwhile to present some background as to how the session and its speakers came about. For the March 2017 meeting Alan Chodos, the FHP program chair (currently chair of the FHP Executive Committee) called me and indicated that the forum was interested in having a session celebrating the 60th anniversary of the 1957 BCS theory and needed an organizer. I accepted and proposed the title for the session, and some potential speakers.

In the past, for the 50th anniversary of BCS, the 2007 APS March meeting had two invited sessions: “50 Years of BCS Theory,” sponsored by the DCMP, and a second invited session, “20th Anniversary of High Tc Superconductivity ‘Woodstock’ Session.” Later that

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Superconductivity experts figuring out the projector

FHP Invited and Contributed Events at the 2018 March and April Meetings

Note: Times and Titles subject to change

March 5-9, 2018 Meeting Los Angeles, CA

Monday, March 5, 8 am - 11 am
Room 5
"Historical Perspectives on Soviet Physics"

Monday, March 5, 11:15 am - 2:15 pm
Room 5
"Pais Prize Session: Peter Galison"

Tuesday, March 6, 8 am - 11 am
Room 5
"The Author in Dialogue: A. Douglas Stone's *Einstein and the Quantum*"

April 14-17, 2018 Meeting Columbus, OH

Sunday, April 15, 10:45 am - 12:33 pm
"Dark Matter & Galaxies: The Legacy of Vera Rubin" (co-sponsored with FPS)

Sunday, April 15, 1:30 pm - 3:18 pm
"The History of Numerical Relativity" (co-sponsored with DCOMP)

Monday, April 16, 1:30 pm - 3:18 pm
"The Legacy of Richard Feynman"

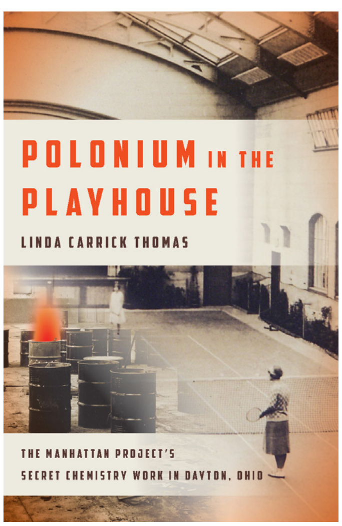
Monday, April 16, 3:30 pm - 5:18 pm
"History of Women Pioneers in Astronomy" (co-sponsored with DAP)

Tuesday, April 17, 1:30 pm - 3:18 pm
"The Chapel Hill Conference and its role in the Renaissance of General Relativity" (co-sponsored with DGRAV)

Book Review:

Polonium in the Playhouse, The Manhattan Project's Secret Chemistry Work in Dayton, Ohio by Linda C. Thomas

by Cameron Reed
Alma College



Polonium in the Playhouse, The Manhattan Project's Secret Chemistry Work in Dayton, Ohio. Linda C. Thomas, 247 pp. Trillium, Columbus, Ohio. 2017. Price $29.95 (hardcover). ISBN 978-0-8142-1338-4.

Even after more than 70 years, details of some important aspects of the Manhattan Project remain only partially known to scholars of the Project, let alone the public at large. Linda Thomas's *Polonium in the Playhouse* fills a gap in Manhattan history with a very readable, fast-paced, well-illustrated, and carefully documented treatment of a highly-secret Project facility operated by the Monsanto Chemical Company in an upscale residential neighborhood of Dayton, Ohio. This facility was devoted to extracting and purifying polonium, a key ingredient in the neutron-emitting "initiators" used in the Trinity, Hiroshima, and Nagasaki atomic bombs. Details of the Dayton project were not declassified until 1983, and some aspects of the associated chemical research still remain under wraps. As the Dayton work was not mentioned in Henry Smyth's 1945 *Atomic Energy for Military Purposes* public report on the bomb project, it has been largely overlooked by Manhattan scholars. (Disclosure: This reviewer served as a reviewer for some of the chapters of this book while it was in preparation.)

In effect, this volume is two books in one: A history of the Dayton project, and a biography of the author's grandfather, Charles Allen Thomas, a Monsanto executive who was appointed to coordinate all Manhattan Project chemistry research. With access to family papers complemented with interviews and documents accessed through numerous Freedom of Information Act requests, the author is uniquely positioned to relate this story. The Manhattan Project was immensely complex, but Thomas does an excellent job of describing its scientific and organizational background and the work of its major facilities and laboratories in just enough detail to set the stage for how the Dayton project fit into to the overall picture.

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In Memory of Martin M. Block

by Silvio Bergia, University of Bologna and National Institute for Nuclear Physics, Bologna (Italy); bergia@bo.infn.it
and Giorgio Dragoni, University of Bologna, and National Institute for Nuclear Physics, Bologna (Italy); dragoni@bo.infn.it

Physicists Block and Puppi at work in Bologna during the Sixties
Courtesy of Prof. Giovanna Puppi, Bologna (Italy)

Recollections of studies and discoveries by American physicists in the last century have been widespread, one example being those cited in David Kaiser's 2011 book [1]. We believe that certain authors, however, have not received sufficient attention, one being Martin M. Block, who passed away at the age of 90 on July 22, 2016. Martin is fondly remembered in Italy because between 1961 and 1964 he collaborated there on an interesting set of research projects with Italian authors. Various physicists, particularly in Bologna and Pavia, remember his research projects, and that he and his wife and children struck up friendships with their peers. In this article, we try to provide an account of his life and studies in Italy.

Life and early research activity

Martin M. Block was born on November 29, 1925 in Newark, New Jersey. He pursued his studies of physics at Columbia University in New York, and received his doctorate there in 1951. Between 1958 and 1959 he received a John Simon Guggenheim Fellowship and, in 1964, the Unesco Fellowship, as well as one given by NATO during the years 1964-65. As an assistant professor at Duke University, in Durham, North Carolina, he guided a Cosmic Ray High Energy Group that, after the introduction of the bubble chamber invented by Donald Glaser in 1952, built a liquid helium bubble chamber with the collaboration of W. M. Fairbank (1958).

While Block was an experimental physicist, he participated in the famous Rochester Conferences and retained an interest for theoretical physics. A subject of great interest for various physicists was a reflection about the validity of the conservation law in the phenomena of the weak interactions. That validity was generally accepted, but not experimentally confirmed. The situation was clearly described by Chen Ning Yang, on the occasion of his acceptance of the Physics Nobel Prize (11 December 1957), entitled "The Law of Parity Conservation and other Symmetry Laws of Physics." In it, one reads the following: "The situation that the physicist found himself in at that time has been likened to a man in a darkroom groping for an outlet. He is aware of the fact that in some direction there must be a door, which would lead him out of this predicament, But in which direction? … The fact that parity conservation in the weak interaction was believed for so long, without experimental support was very startling. …" [2]. Fascinated by these issues, Block proposed that in the weak interactions parity was not conserved, which would then explain the tau/theta puzzle, a subject of great actuality in those days, but he did not dare to formally transmit his view to the participants at the conference. Richard Feynman, however, communicated Block's idea to the participants, as mentioned in his 1985 book [3], *Surely You're Joking, Mr. Feynman*: "Anyway, I was sharing a room with a guy named Martin Block, an experimenter. And one evening, he said to me: 'Why are you guys so insistent on this parity rule? Maybe the tau and theta are the same particle. What would be the consequences if the parity rules were wrong?' I thought a minute and said: 'It would mean that nature's laws are different for the right hand and the left hand, that there's a way to define the right hand by physical phenomena. I don't know that that's so terrible, though there must be some bad consequences of that, but I don't know. Why don't you ask the experts tomorrow?' He said: 'No, they won't listen to me. You ask.' So the next day, at the meeting … I got up and said, 'I'm asking this question for Martin Block: What would be the consequences if the parity rule was wrong?' Murray Gell-Mann often teased me about this, saying I didn't have the nerve to ask the question for myself. But that's not the reason I thought it might very well be an important idea."

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2010 *continued*

The 50th Anniversary of the Prediction of Superfluidity of He3

Secrecy and Physics

Remembering Enrico Fermi

Origins of Research and Teaching at Selected Physics Departments

The Laser: Historical Perspectives and Impact on Precision Measurements

2009

Origins of Silicon Valley

Centenary of Lev Landau

50 Years of Anderson Localization

The Scientific Legacy of John Wheeler

History of Telescopes

Science Policy: Yesterday, Today, and Tomorrow

History of MURA, Fermilab, and the SSC

2008

50th Anniversary of Physical Review Letters

Industrial Physics History

Triumphs of 20th Century Astrophysics (2 sessions)

Manhattan Project and Beyond

Los Alamos and the Manhattan Project

80 Years of Quantum Mechanics: A New International Project

2007

20th Anniversary of High Tc Superconductivity Session

Condensed Matter Physics at Synchrotron Facilities: History as Prologue

Building the Elements: 50 Years of B^{2}FH Nucleosynthesis (2 sessions)

Changing Role of Nuclear Weapons in Foreign Policy

History of Gravitational Waves and General Relativity

Sputnik 1957: Its Effect on Science in America

2006

Low Temperature Physics, A Historical Perspective

A Century of Critical Phenomena

(April meeting abstracts apparently not available)

2005

Einstein and Friends (3 sessions)

Quantum Optics Through the Lens of History

The Rise of Megascience

2004

History of Physics in Canada

History of Physics in Industrial Laboratories

Monolayers and Multilayers: Agnes Pockels and Katharine Blodgett

The Discovery of Black Holes

The Sun as a Physics Laboratory

Science Advising

Mossbauer Spectroscopy: various Historical Perspectives

2003

J. Willard Gibbs and His Legacy: A Double Centennial

The Early Days of Solid State Physics

History/Methodology and Career Development

2002

Tunneling, From Alpha Particle Decay to Biology

Synchrotron radiation: From Stepchild to Star

EPR to Entanglement

Eugene Wigner Centennial

History of Los Alamos

2001

NIST at the Millennium: Condensed Matter and Measurement Science

History of Electronic Structure Theory in Atoms

2000

Twenty Years of the Quantum Hall Effect/ George Pake Prize

1999

Women and Men Inside the Atom: A Historical Look

Physics in the 20th Century: World War II, Accelerators, and the Rise of High Energy Physics

1999 *continued*

I.I. Rabi: Physicist and Citizen

Physics in the 20th Century: The Revolution: Quantum Mechanics and Relativity

20th Century Developments in Instrumentation & Measurements

History of Physics in National Defense

1998

The History of Critical Phenomena

Science and Its Critics

Teaching Physics a Century Ago

1997

One Hundred Years of Electrons

Joseph Henry Bicentennial

The Electron Centennial

100 Years of Electron-Photon Interactions & Rivalry (Electron Centennial II)

1996

Topics in the History of Radioactivity

History of Computing in Physics

Contributions of Women to Physics

1995

New Studies of Isaac Newton's Works

The Centennial of X-Rays

1994

The Solid-State Roots of Silicon Valley

Milestones in the History of Astrophysics

Science Advice to the Government

Scientists and National Security: Washington's Four Governments ■

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physicists could do philosophy a favor by giving up certain ways of speaking that seem to lead philosophers to think that relativity does have to do with consciousness. The take-home lesson of the Bergson-Einstein controversy, he concluded, is that (as Bertrand Russell had warned) to understand the world, we cannot trust our intuitions.

"I am a scientist -- an astrophysicist," said Adam Frank of the University of Rochester, the third speaker in the session, "and I loved this book." The reason, he said, was that it highlighted a different dimension to time, what philosophers call "lived time," than the one that physicists are used to discussing. Bergson, he said, was trying to grasp lived time and distinguish it from the way physicists encounter time. Frank proceeded to describe the approach to time of phenomenologists, who study the conditions for the possibility of any experience altogether -- again, the difference between what is involved in measuring the length of a piece of music and what is involved in hearing it in the first place. It is difficult to describe this to physicists, who are interested in structures and foregrounded objects of research, but it is part of being human and nothing subjective. You have to use a different kind of language to describe it. "I want to turn the usual question on its head," Frank said. "The usual question is, 'How do we situate experience within physics?' I want to change it to, 'How do we situate physics within the totality/unity of experience?'" Such a question can only be answered by admitting that there is a time that is not the physicist's time -- not clock time. Frank also noted that one major difficulty to reconciling the physicists' and the philosophers' view of time is that the practitioners of each field have an "ontological mania," regarding themselves as the wardens of the ontological primacy of things like time.

Jean Bricmont of the Université catholique de Louvain (top left), Adam Frank of the University of Rochester (top right), Joseph Martin of the University of Cambridge (bottom left) and Alberto Martinez of the University of Texas at Austin (bottom right)

Joseph Martin's talk was entitled, "What if Bergson Won?" Being a good historian, he immediately apologized, admitting that counterfactual history is out of favor with historians. It's hard enough, he said, to evaluate statements about what did happen, let alone about what would have happened. It's also hard even to get a fix on what it would mean for Bergson to win. Still, he thought that the exercise might be a good thought experiment that would give a "different and helpful perspective on the history that we do have," and in particular on the split between physics and philosophy.

What Martin intended was not that, in envisioning a win by Bergson, it would mean that Bergson's account would replace accounts of objective time, or that all objections to Bergson's account would vanish. Rather, the disagreement, he noted, was not really about time itself, but "about who has the authority to speak on behalf of the phenomena." If Bergson had succeeded in this counterfactual thought experiment, Einstein and his followers would have come to appreciate that clock time was derived from lived time. But we also have to be somewhat charitable to Bergson and imagine that the victory was not entirely complete, but that Bergson ultimately had to realize that he had to stop defending his time as the opposite of clock time, and abandon the rivalry between his views and Einstein's. If this all happened, Martin was implying, several good things might have occurred.

First, it would have affirmed the right of philosophers to engage with the physicists in the European intellectual community, and perhaps the American one as well, about the nature of scientific phenomena. This would have meant that "philosophers (even those without positivistic leanings) would have gotten better than Bergson was at responding to the leading edge of theoretical knowledge." It would have encouraged at least some philosophers to keep up with their technical competence of science enough to carry philosophy of science forward. It would have meant that the philosophical community would have taken philosophy of science more seriously as a mainstream discipline rather than as a specialty best left to technicians or even to scientists who have read a bit of Kant and Popper. Second, Martin said, if that happened, "scientists would have gotten better at communicating those ideas to philosophers, if only to fend off potential philosophical critiques, which would have been important for their own standing." Furthermore, one might add, it would have meant that physicists would have been more likely to notice and respond to what Bergson and other philosophers were noticing and responding to. A world in which Bergson had won, Martin said, would be "one in which physics doesn't split quite so abruptly from its natural philosophy roots." As a result, Martin said, had Bergson "won," whatever that would mean, it would have encouraged greater communication between the physics and philosophical communities.

The final talk in the session was by Alberto Martinez, who tried to explain Einstein's allergic reaction to Bergson. Martinez cited a lot of pro and con remarks by Einstein about philosophy, noting that Einstein liked Hume-like clarity, and said that "from the point of view of logic, all of our concepts are freely chosen conventions." Martinez also listed problems with Bergson's various remarks about relativity, calling one remark by Bergson about the twin paradox "stunningly inept." In the question and answer period, Frank objected to that remark, saying that Bergson's error was not the point. Bergson was pointing out that each twin had its own experience. The point is not to work out the details mathematically, it's to emphasize the role of lived experience, whose root is different from the functioning of the apparatus. Martinez responded that "Bergson needed more courage," and was making a straw man out of Einstein. ■

Session Report: "The Pais Prize Session for Mary Jo Nye"

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such as solid state and condensed matter physics, chemical physics, and plasma physics. The challenges of dealing with the richness and complexity of late-twentieth century physics, and especially those activities that push up against disciplinary boundaries, Nye suggested, should make historians of physics more open to the types of multi-author collaborations now standard in physics itself.

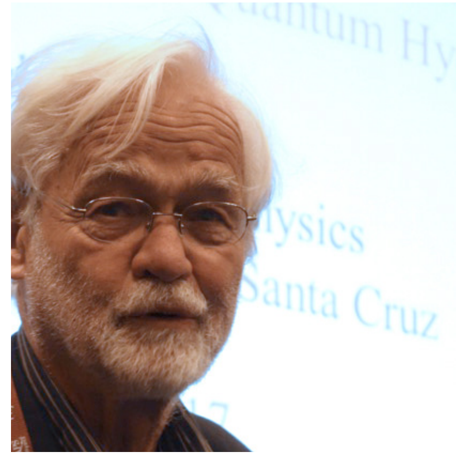
Two previous Pais Prize winners joined the panel to speak in Nye's honor, the first being Allan Franklin. Franklin was awarded last year's prize for his trailblazing work on the history of experiment, and his talk furthered in that line of research, tracing the difference between direct and indirect experimental detection through a number of examples from twentieth-century physics. This history has become particularly important in light of the recent gravitational wave observations at LIGO—which directly recorded a phenomenon that had previously been indirectly inferred, leading the physics community to mull how to apportion credit for the discovery, and the accolades likely to accompany it. Franklin's case studies suggest that the distinction between direct and indirect observation is not always so powerful an arbitrator of these questions as we might expect it to be. In some cases, direct observations carry greater weight, but in others, the indirect inferences are what we remember as the groundbreaking discoveries. If therefore behooves historians to consider what other factors might guide our instincts about which is more important for a particular episode.

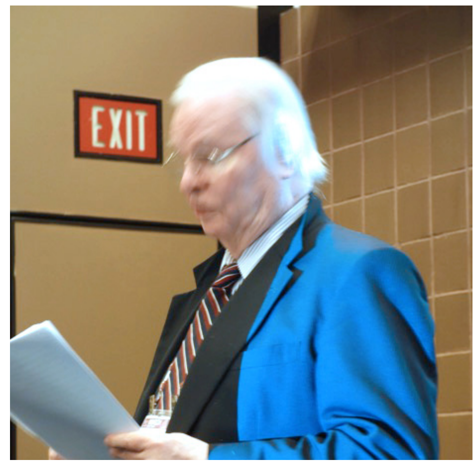
Richard Staley, following Franklin, paid tribute to Nye's perceptive work on boundaries by considering the interfaces between physics and physiology. In a professional world so thoroughly compartmentalized by specialization, it is easy to forget that many of those we remember for their contributions to physics were also fascinated by physiological phenomena. For figures like Ernst Mach and Hermann von Helmholtz, understanding physical phenomena like light, space, and motion meant probing the bodily mechanisms governing sight, hearing, and balance. The same thinkers willing to map out the boundary between physics and physiology were often also those who were likely to navigate into politics. Machism, Marxism, and mechanism became intertwined in the second half of the 1800s for those natural philosophers who, like Mach, were as skeptical of absolute boundaries between areas of knowledge as they were of absolute space, absolute despots, and absolute notions of selfhood.

"What Kuhn has been dishing us is nonsense" was the take-away from Michael Nauenberg's talk on the origins of the quantum. The nonsense in question refers to a long and contentious debate about the conceptual origins of the quantum. Thomas Kuhn suggested that historians withhold the blue riband from Max Planck and award it instead to Albert Einstein. The former might have nominally quantized the energy spectrum in 1900 in the course of his efforts to explain black-body radiation, Kuhn argued, but it was Einstein who fully committed to the idea when he introduced light quanta in 1905. Nauenberg quoted Einstein's advice to examine what physicists do, rather than what they say they did, when tracing their histories to argue that Kuhn did Planck an injustice.

Rounding out the session was the symposium's second former Pais Prize winner, Roger Stuewer, who began his career in the history of science alongside Nye in the University of Wisconsin's graduate program. Stuewer shared his renowned expertise in the history of nuclear physics. He juxtaposed his narrative account of the evolving understanding of the nucleus and nuclear fission with a discussion of the concurrent political developments that led to World War II. Scientific understanding of the disintegration of the nucleus marched in lockstep with the disintegration of Europe, and the political fissions of the 1930s scattered the European scientists responsible for knowledge of nuclear fission across the globe. Coming to rest in more stable political environs, they sparked new interest in nuclear physics worldwide.

Stuewer, in closing, noted the injustice of denying Lise Meitner a Nobel Prize, echoing Nye's observation of the dearth of women among physics laureates. Nye observed that the stark gender disparity in physics has been cast in relief by the recent deaths of Vera Rubin and Mildred Dresselhaus, both of whom achieved just about every honor physics has to offer except its highest. A session devoted to work that crosses boundaries therefore offered ample opportunity to reflect on the boundaries that remain, in both physics and the history of physics. The physics profession opened up to boundary-crossing physical investigation in the late twentieth century, but significant boundaries, built on race, class, and gender, persist for physicists themselves. And engaging the boundary-crossing topics and approaches that define recent physics will necessarily require breaking down similar boundaries for those studying its history. ■





Allan Franklin of the University of Colorado (top left), Michael Nauenberg of the University of California, Santa Cruz (top right), Richard Staley of the University of Cambridge (bottom left) and Roger Stuewer of the University of Minnesota (bottom right)

Session Report: "60 Years Since BCS and 30 Years Since Woodstock"

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same year a comprehensive conference, "BSC@50" was held at the University of Illinois and a book based on the conference was published: "BCS: 50 Years," edited by Leon N. Cooper and Dmitri Feldman, World Scientific (2010). In 2011, two APS March meeting sessions were held to celebrate the 100th anniversary of the discovery of superconductivity by Kammerlingh Onnes: a session called "The History of Superconductivity from its Discovery by Kammerlingh Onnes in 1911," sponsored by the FHP, and another session, "The Kavli Foundation Special Symposium: Nobelist Perspectives on 100 Years of Superconductivity," sponsored by the DCMP and DMS.

Alan Chodos suggested that I consider starting off with a talk by Andrew Zangwill (Georgia Tech), who had just finished a historical article on Walter Kohn and was in the middle of a book project on Phil Anderson. I agreed with Alan and invited Andy who, after checking with Phil, got Phil's personal "top five" list of contributions to superconductivity. Andy suggested his talk title: "Phil Anderson and Superconductivity: An Appreciation of his Contributions." Organizing the rest of the session was straightforward, choosing Paul Grant (W2AGZ Technologies and formerly IBM), to cover Woodstock, Richard Greene (University of Maryland), to review of experimental high temperature superconductivity, Doug Scalapino (University of California, Santa Barbara), on the status of the theory of both BCS and high Tc superconductors, and Neil Ashcroft (Cornell), to report on the latest high temperature experiments and theory involving hydrogen, especially the high Tc sulfides.

After I finalized the program and speakers for the session "60 Years since BCS and 30 Years since Woodstock," two of the original speakers had to cancel suddenly, and fortunately two excellent substitutes generously agreed to speak. For the talk on Anderson, Piers Coleman (Rutgers) agreed to speak on Anderson's contributions to superconductivity. Piers had recently coedited a book for Phil's 90th birthday in 2013, "PWA90: A Lifetime of Emergence," World Scientific (2016). For the talk on hydrogen and superconductivity, Isaac Silvera (Harvard) agreed to speak on his recently published experiment results claiming to have produced and observed metallic hydrogen. The APS meetings staff was exceptionally cooperative in that they were able to include the new speakers and talks in the Bulletin even though the abstract deadline had long passed.

Almost all of the session speakers were old enough to be active in research in the 1950 and 60s, during the so-called "golden age of superconductivity' and thus directly participated in its history. Alan Chodos and I noted that it seems unlikely that one will be able to have a BCS session with eyewitness researchers available to speak at the 75th anniversary.

Piers Coleman in his talk, "Phil Anderson's Magnetic Ideas in Superconductivity," discussed Anderson's work on superconductivity, from his pseudo-spin formulation of the BCS theory, to the Anderson Higg's mechanism and the resonating valence bond RVB theory of cuprate superconductivity. He divided Anderson's contribution into different time periods. The 1950s included antiferromagnetism and the pseudospin formulation of superconductivity. The period from 1958 to 1963 was most productive and included: dirty superconductors, the theoretical and experimentally observed Josephson Effect, flux creep in type 2 superconductors and a prescient paper on "Plasmons, Gauge Invariance and Mass," known as the Anderson Higgs Effect. In 1973 Anderson presented his theory of resonant valence bonds which showed that in copper oxide lattices, electrons from neighboring copper atoms interact to form a valence bond. With doping, however, the bonded electrons can be unlocked and form mobile superconducting Cooper pairs. This RVB concept was used by Anderson to explain the high Tc results first discovered by Bednorz and Muller in 1986.

Paul Grant's talk, "The Woodstock of Physics: The Hyped Future Then (1987)…The Actual Situation Now (2017)," reviewed the experimental history of high temperature superconductors starting with the initial 1986 experiments of Alex Muller and Georg

Piers Coleman of Rutgers University (standing) with session organizer Brian Schwartz (seated)

Bednorz on cooper oxide perovskites followed by the 93 K transition temperature detected in yttrium, barium, copper perovskites by Paul Chu. Their results were immediately confirmed leading to pandemonium at the March 1987 meeting at the New York Hilton termed ``The Woodstock of Physics.'' Many speakers at the Woodstock meeting were forecasting that the discovery of high Tc superconductors (HTCS) would result in "the energy deliverance of mankind." The immediate period after the discovery of high Tc was filled with optimistic proposed applications, but few practical uses followed. The bottom line then and now seems to be that despite the many successes of HTCS wire technology and the prototype testing of applications to transmission lines and other uses, there seems to be no significant commercial applications. Grant speculated that in the future power application might be one of the HTSC applications.

Richard Greene's talk, "The Current Experimental Status of the High Tc Problem," started by noting that over 50,000 experimental papers had been published since 1987 on the copper oxide (cuprate) high Tc superconductors. He summarized the experimental properties that are understood presently and those not yet understood. He then listed the many improved experimental measuring techniques such as ARPES, SI-STM(QPI), RIXS, Quantum Design MPMS and PPMS and the availability of higher magnetic fields. In addition, there are improved and available high Tc materials. He presented the layered crystal structure of cuprate superconductors and illustrated the frustration of antiferromagnetism with doping. Ultimately, he presented a phase diagram of the cuprates constructed from many separate experiments (figure 1). Greene ended his talk with the big question remaining, "What causes the Cooper pairing in HTSC?"

Doug Scalapino in the title his talk asks the question, "Why did it take

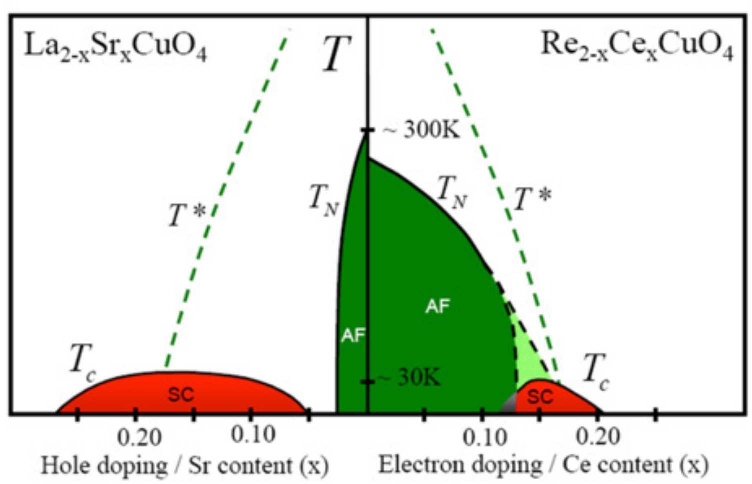


Paul Grant of EPRI (left), Richard Greene from the University of Maryland (middle), and Douglas Scalapino of the University of California, Santa Barbara (right)

over 40 years from the experimental discovery of superconductivity to the BCS theory and will it take this long to understand the high Tc superconductors?" He summarized the experimental advances and theoretical clues from the discovery of superconductivity in 1911 until 46 years later with the BCS theory in 1957. He also described the many failed attempts to explain superconductivity by such noted physicists as Albert Einstein, Felix Bloch, Lev Landau, Walter Heisenberg and others. Hints to the solution were implied in the phenomenological theory of Heinz and Fritz London, indicating a likely phase transition with long-range order. In 1950, through the isotope effect, electron-phonon interactions were implicated in the mechanism for superconductivity, leading to the attractive force for pairing of electrons calculated by Cooper and then followed by the BCS theory. However, even after more than thirty years of intensive theoretical and experimental research, the origin of HTCS is still not clear. It seems that for HTCS, one is dealing with more exotic electronic mechanisms (e.g. by antiferromagnetic correlations), and instead of conventional, purely s-wave pairing, more pairing symmetries are thought to be involved (d-wave in the case of the cuprates; primarily extended s-wave, but occasionally d-wave, in the case of the iron-based superconductors). Scalapino listed the many theoretical ideas for high Tc and discussed Anderson's RVB approach, spin fluctuation pairing, nematic pairing, loop currents and more. Scalapino stated his belief that

Figure 1: Phase diagram of the cuprates

*The T*green dotted line on the right is AFM fluctuations and the T* on the left is unknown*
N.P. Armitage, P. Fournier and R.L. Greene, Rev. Mod. Phys. 82, 2421 (2010)

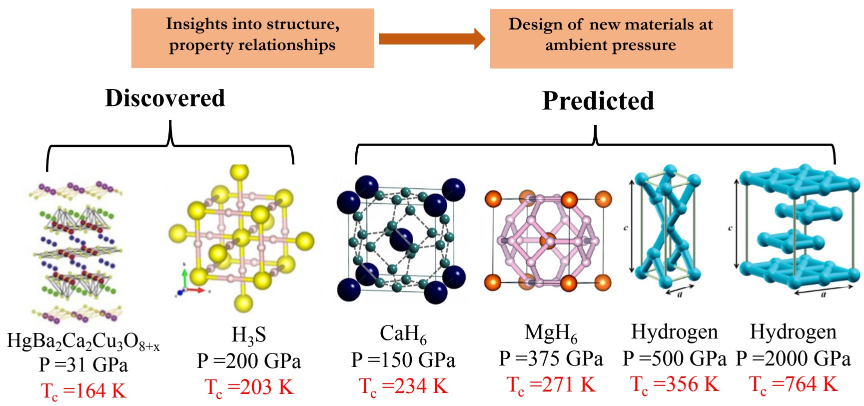


for the HTCS materials, we are close to understanding the high Tc problem and that the Hubbard model plays an important role in the understanding of HTCS. He concluded that: 1) the pairing is mediated by short range AF spin fluctuations, 2) the pairing emerges from a delicately balanced phase dependent upon near neighbor hopping and 3) the theory includes the many different HTCS materials.

The final paper in the session was by Isaac Silvera on "Pressing Hydrogen into an Atomic Metallic Phase: Implications for Superconductivity." In 1935 physicists Wigner and Huntington predicted that hydrogen would become a metal at very high pressure. That prospect was made even more enticing when, in 1968, physicist Neil Ashcroft showed that metallic hydrogen might become superconducting at room temperature (figure 2). Silvera described his experiment in which a hydrogen sample at a temperature of around 5.5K was squeezed between the teeth of a diamond anvil cell that created multi-megabar static pressures. Based on his calibration of the pressure scale, Silvera claimed that metallization took place at 495 gigapascals (GPa). At this pressure point, the sample – which they could not identify definitively as solid or liquid – change from black and opaque

Figure 2: Hydrogen and Poly-Hydrides under Pressure

N.W. Ashcroft, Phys. Rev. Lett. 92, 187002 (2004)



Isaac Silvera of Harvard University

to the highly reflective characteristic of a metal. His result, claiming to have observed metallic hydrogen, remains controversial and thus he had to counter the criticism of the experiment in that the pressure was overestimated. A further complication was the fact that his sample, in which he claimed to observe metallic hydrogen, broke after the measurements were made. Silvera concluded his talk by listing his plans for the future: to repeat the experiment, to use X-ray to determination of ground state structure, (liquid or solid), to measure the conductivity vs T to test for superconductivity, and to show if the state is metastable, and to produce metallic hydrogen at lower pressures. ■

Book Review: "Polonium in the Playhouse, The Manhattan Project's Secret ChemistryWork in Dayton, Ohio by Linda C. Thomas"

Continued from page 4

Design of the bomb initiators is still one of the most closely-classified elements of the Manhattan Project. These devices, each about the size of a golf ball, were designed to create a brief torrent of neutrons to initiate the nuclear chain reaction by what are known as "alpha-n" reactions, wherein alpha-particles emitted by a naturally-radioactive material strike a light-element material, resulting in the emission of neutrons. Polonium (Po) is ideal as the alpha-emitter because of its short (138-day) half-life; a mere 0.24 milligrams of Po emits as many alpha particles per second as a full gram of radium (which defines 1 Curie). More recently, polonium was the poison involved in the infamous murder of former KGB agent Alexander Litvinenko in 2006. Manhattan Project initiators were of the so-called "Po-Be" type, which meant that they contained polonium as the alpha-emitter and beryllium as the light element. The two elements were contained within the initiator but were initially separated; when the initiator was crushed by the assembling bomb core they mixed, producing the initiating neutrons. Because of the short half-life of Po, these initiators had limited half-lives, and so it was necessary to establish a dependable supply chain of this otherwise rare element. Only two methods of sourcing Po are available: By extracting it from waste lead-dioxide ores from uranium and radium-mining operations, or by breeding it synthetically via neutron bombardment of bismuth within a reactor. In the latter case, nuclei of Bi-209, by capturing neutrons, become nuclei of Bi-210, which transmute to Po-210 via beta-decay with a half-life of about 5 days. The cross-section for this process is small, however, so it is necessary to irradiate hundreds of pounds of bismuth to produce Curie-level amounts of Po. Manhattan Project initiators each used about 50 Curies of Po, equivalent to about 10 milligrams. The task of the Dayton project was to research and develop the chemical processes necessary to realize both methods, using polonium-bearing ores from Canadian mines and slugs of bismuth irradiated in the Oak Ridge and Hanford reactors.

The central figure of the story, George Allen Thomas, was born near Lexington, Kentucky, in 1900. His interest in chemistry was apparent at a young age, and after skipping two years in high school he enrolled in Transylvania University, graduating in 1920. He then went on to graduate school at MIT, from which he graduated with an MS in 1924. Through a classmate who was a member of the DuPont family, Thomas landed a job at the Ethyl Corporation (a subsidiary of General Motors) in Dayton, where he researched motor fuels. Popular, extroverted, and a natural leader, he began to move easily in upper-crust Dayton social circles, and in 1926 married into the very wealthy Talbott family, whose patriarch, Harold Talbott, was a part owner of GM. At about this time, GM decided to move Ethyl operations to Detroit; not wishing to move, Thomas and a co-worker, Carroll Hochwalt, decided to start their own chemical consulting business, Thomas & Hochwalt Laboratories. Their first major product was a revolutionary new fire extinguisher; during Prohibition they devised a method to analyze the safety of bootleg liquor and also developed a means of artificially ageing raw liquor in anticipation of Repeal.

As Thomas and Hochwalt were building their business, Thomas's mother-in-law, Katherine Talbott, built a recreation center for social and sporting events dubbed "Runnymede Playhouse" on the grounds of the family estate. The glass-roofed structure, built at a cost of $100,000, was at the time the largest free-standing private hall in the country, boasting a stage, dressing rooms, tennis and squash courts, a greenhouse, and a swimming pool; the main dining area could accommodate 1200 bridge players at once. Runnymede would play a crucial role in the Dayton project.

Thomas & Hochwalt thrived; by 1935 it was the largest independent consulting laboratory in the country, and the next year they were bought out by Monsanto, which made the facility the nucleus of the company's Central Research Division with Thomas as Director. When the National Defense Research Committee and later the Office of Scientific Research and Development were formed under Vannevar Bush and James Conant, Monsanto received numerous contracts for liquid fuels and synthetic rubber work. Thomas became well-connected to those agencies; at the time of the Japanese attack at Pearl Harbor he was deputy chief of the NDRC's explosives division.

When the Los Alamos Laboratory was established in the spring of 1943, it became clear that the Manhattan Project's need for chemical and metallurgical research, which was spread among sites at Los Alamos, Berkeley, and the University of Chicago, was going to be much greater than initially appreciated, and required a experienced science administrator. General Groves offered Thomas a position as an associate Director of Los Alamos; he declined the offer, but did agree to coordinate Project chemistry while remaining in Dayton. Over the next two years travel to Project sites would keep him away from home for two weeks of every month.

In May 1943, Monsanto was awarded a contract to produce polonium, and Thomas set up operations in Dayton. This work was regarded as so sensitive that materials could not be ordered through normal Manhattan Engineer District procurement channels; rather, they were sourced through existing Monsanto contracts. From an original estimate of requiring only 12 chemists, the Dayton project grew to a staff of 200 spread among four buildings by the end of the war. Many of these people were be involved in developing new radiation-counting instruments and in staffing an extensive health physics program. One of the health physics staff members, George Koval, would later be revealed as a Soviet agent.

Chemists will enjoy Thomas's descriptions of the processes involved in extracting minute amounts of polonium from ores and bismuth slugs. The scale of the ore work and the need to handle 110-pound bismuth slugs demanded an expansion of laboratory space, and the Army Corps of Engineers seized Mrs. Talbott's Playhouse (she had died in 1935) and entered into a lease with the family to renovate the structure and use it as a production facility. The cover story for neighbors was that it was a laboratory for producing training films for the Army Signal Corps.

The ore-extraction process was inefficient and was eventually dropped in favor of the bismuth process exclusively, although it did contribute about 40 Curies of Po extracted from 37 tons of ore, enough for about one initiator. Fifty tons of bismuth would be processed; by June, 1945, Dayton was sending 35 Curies of Po per week to Los Alamos, delivered by couriers driving trucks over a 53-hour route which deliberately circumvented cities. Thomas witnessed the Trinity test; his letter to his mother describing that event is worth the cost of this book alone.

After the war, Thomas served on numerous corporate, academic, and government committees and boards, including the one that drafted the 1946 Acheson-Lilienthal report, although he later became skeptical of efforts by intellectuals to influence political events. He became President of Monsanto in 1951, and remained at the company until his retirement in 1970. He passed away in 1982, having never spoken to his family of his connection to the Manhattan Project. The cost of the Dayton project ran to about $3.9 million, a fraction of a percent of the Project's overall cost of about $2 billion, but without it there would have been no functioning bombs.

As the need for polonium production grew during the Cold War, the Dayton Project outgrew its facilities, and the Atomic Energy Commission established the Mound Laboratory, a purpose-built facility about 10 miles southwest of Dayton, to take on the work. Mound began processing operations in February, 1949; work there also included preparing radioisotopes to power satellites. The Mound Laboratory was decommissioned in 1993. Operations at the Playhouse continued until late 1948; early the next year, the Playhouse, which had become contaminated, was dismantled and the site remediated and returned to the family. The book includes a number of photos of the Playhouse as it was being cleaned up and dismantled.

Appendices to this book include a brief primer on the science underlying the bombs, a log of travels by Dayton personnel among Project sites, and brief biographies of several members of the Dayton staff, a valuable contribution as these names would probably otherwise be lost to history. Books on the Manhattan Project can be notorious for reiterating technical or historical errors, but only one such glitch caught my eye, a statement on page 99 that neutrons are emitted directly by polonium.

Polonium in the Playhouse brings to light a little-known aspect of the Manhattan Project, the work of Charles Allen Thomas, and the underappreciated role of chemistry in the Project. This book should be in the collection of any serious student of the Project.

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According to another version, quoted by Martin Gardner in "The Ambidextrous Universe. Left, Right and the Fall of Parity", Feynman would later speak of making "a fifty dollar bet with a friend that parity would not be violated" [4].

At any rate, the idea, initially not quite understood, if not actually rejected outright, was developed by Lee and Yang, who presented it in a famous article [5]. Later on, the question posed by the Chinese-American physicists as to whether parity was preserved in the weak interaction was shown to have a negative answer. Madame Chien Shiung Wu of Columbia led a team that found experimentally parity violation in the beta decay of Cobalt 60 [6]. A number of other experiments on the subject were carried out near-simultaneously. We recall one of the first papers in 1957 by an international collaboration: "Demonstration of Parity non Conservation in Hyperon Decay" [7]. The experiment was carried out thanks to the collaboration of four groups of physicists, two Italian (Bologna and Pisa) and two from the US (Brookhaven National Laboratory and the University of Michigan), consisting of some twenty physicists in all. This work contributed to the background of the award of the 1957 Physics Nobel Prize to Tsung-Dao Lee and Chen Ning Yang for their theoretical analysis of the phenomena related to weak interactions.

These two winners of the Prize did not mention Martin Block. Nor was a Nobel Prize was award to Madame Wu. The episode evidently weighed heavily on Block. During the years he spent in Bologna he often used, in his accented Italian, the expression "Maledetti teorici!" to dismiss theorists.

Block's activity as an experimental physicist was mainly characterized by the set-up of advanced instruments for the research in elementary particles physics. In particular, he contributed to the realization of the magnet for the accelerator, then of the highest energy, the Nevis Cyclotron of the New York Columbia University.

Block remained at the Duke University until 1961, when he got a professorship as professor of physics at Northwestern University. Financial support allowed him to visit European laboratories and universities occasionally, including the Physics Institute at Bologna directed by Giampietro Puppi.

Block and Puppi at Durham Sept 23, 1960. (Martin Block is the first at left, then Mrs Puppi; Professor Puppi is in the fifth seat) - Courtesy of Prof. Giovanna Puppi, Bologna (Italy)

Block carried out research in Italy, frequently with Italian colleagues of the research group directed by Giampietro Puppi in Bologna, which was engaged in studies of elementary particle physics with particular attention to the determination of quantum numbers, spin, parity and isotopic spin of particles as well as of resonances.

Martin Block worked actively with Italian colleagues, aside from Puppi also R. Gessaroli, L. Grimellini, L. Lendinara, L. Monari, S. Ratti. Among other things this work achieved the observation (perhaps the first) of the decay of the Sigma Minus as well as the Lambda Zero, using for the first time a liquid helium bubble chamber [8]. Another run of the same bubble chamber photographed 100.000 interactions of the K minus [9]. A systematic study of K minus interactions in liquid helium followed in 1964 [10].

Other work that deserved mention include "Helicity of the proton from a Decay" [11], "Lifetime of the Lambda 0 Hyperon" [12], "The Decay Modes of the He 4 Hypernucleus" [13], "The Lambda 0 Decay Asymmetry Parameter and the Kappa-Lambda Parity" [14]. These works belonged to a package of papers worked out by groups of authors who, besides Block and colleagues at Bologna, also included American physicists.

After returning to Duke, where he would remain for the rest of his career, Block widened his technical background, and built large spectrometer counter and spark chamber systems. He had studied these while a member of the Ford Foundation at CERN in 1964-65, and in 1972-73, when he was a Fellow of the Giuseppe Cocconi's laboratory. In this work, he made use of heavy liquid chamber in order to measure neutrino interactions, the first experiment of this kind. Block was also the first to measure the relative parity of two strange particles, determining that the parity of the Lambda 0 and the Kappa 0 is odd. In

1970, his interests moved towards the technique used for measuring cross sections of high energy scattering that he studied at Brookhaven National Laboratory.

Recognitions

In 1985, in Block's honor, Pavia University organized a symposium, "On Weak Interactions," and that same year awarded him for his contribution to the particle physics the "Medaglia Teresiana," which was only the third time it had been given out since the end of the Second World War. In 1985, Block obtained the medal of the Italian Physical Society, for his researches in high energy physics. That same year as well, Block initiated, in Aspen, a Winter Conference at the Aspen Center for Physics in elementary particle physics. The first conference was so well received that it has been repeated annually ever since, with topics including astrophysics, biophysics, and condensed matter physics. Ten years after its founding, the Center celebrated Block for these specialized conferences, including a public event at the Wheeler Opera House.

Giorgio Giacomelli and others organized a meeting entitled, "Thirty Years of Bubble Chamber Physics" (Bologna, Italy, 19 March 2003 at the Science Academy) attended by Block himself. Block was also honored by the dedication of a symposium in 1996 for his forty-five years of physics at the Northwestern University (Evanston-Chicago), where he was named Emeritus Professor.

In 2005, to celebrate his eightieth birthday, Block and others founded the Block Prize to be awarded to promising young physicists on the occasion of the Aspen Winter Conferences. Block subsequently retired from academic life and remained with his wife Beate in a lodge in his beloved Aspen. Notwithstanding this retirement, he continued to produce several important scientific contributions.

In 2010 he gave rise to a series of papers of which one, "Hadronic cross sections: from cyclotrons to colliders to cosmic rays," was presented at the International Symposium on the high energy cosmic rays at the Fermilab. He also wrote a brief note on the high energy scattering of neutrinos that appeared on Physical Review D. In 2011, he published two other articles, about sixty years after the publication of his first papers. The Aspen Centre for Physics announced the death of Martin Block, at the age of ninety, in Los Angeles on July 22, 2016.

Concluding Considerations

The list of discoveries and results obtained by Block in several years with bubble chambers and other instruments, is truly considerable: new particles, new resonances, high energy hadronic interactions, neutronic interactions, and so forth, not to mention his remark about parity violation in weak interactions.

Grateful Acknowledgments: particular thanks to Professor [Mrs] Lella Grimellini, who gave us the Bologna Martin Block papers and Professor [Mrs] Giovanna Puppi for giving us photographic images of Block together with Gianni Puppi. We also thank for useful suggestions Professor Attilio Forino of the Bologna Department of Physics and Astronomy and the friend Doctor Paolo Cinti of the same Department

Martin Block

Bibliography:

[1] D. Kaiser, How the Hippies saved Physics, W. W. Norton & Company (2011); tr. It., "Come gli Hippies hanno salvato la Fisica", Castelvecchi Ed., Roma, (2012)

[2] C. N. Yang, "The Law of Parity Conservation and other Symmetry Laws of Physics," in Nobel Lectures, Physics 1942-1962", published for The Nobel Foundation by the Elsevier Publishing Company, New York, (1964) 393-405, 398

[3] R. Feynman, R. Leighton, Surely You're Joking, Mr. Feynman, W.W. Norton, New York, (1985); tr. it. Zanichelli, Bologna, (1988)

[4] M. Gardner, The Ambidextrous Universe. Left, Right and the Fall of Parity, Basic Books Inc., New York, (1964); tr. It. L'universo ambidestro, Zanichelli, Bologna, (1984)

[5] T.D. Lee, C.N. Yang, "Question of parity conservation in weak interactions", Physical Review, 104, 1, (1956) 254-258

[6] C.S. Wu et al., "Experimental test of parity conservation in beta decay", Physical Review 105, 4, (1957) 1413-1415

[7] Eisler, F. et al., "Demonstration of parity non conservation in hyperon decay", Phys. Rev. 108 (1957) 1353; we remind that among the usual "et al." were included various italian physicists: Bassi, Conversi, Puppi…

[8] L. Grimellini, T. Kikuchi, L. Lendinara, L. Monari, M. M. Block, "Observation of the Decay Sigma-", Proceedings of 1962 Int. Conf. on High-Energy Physics at CERN, Ginevra, (1962) 457

[9] M.M. Block, C. Meitzer, S. Ratti, L. Grimellini, T. Kikuchi, L. Lendinara, L. Monari, "Lifetime of the Hypernucleus H3", Proceedings of 1962 Int. Conf. on High Energy Physics at CERN, Ginevra, (1962) 458-459

[10] L. Grimellini, L. Lendinara, L. Monari, M. M. Block, R. Gessaroli, "Interazioni di K- a riposo in He 4", Supplemento al Nuovo Cimento, 2, 1, (1964) 735-736

[11] J. Leitner, L. Gray, E. Harth, S. Lichtman, J. Westgard della Syracuse University di New York; M. Block, B. Brucker, R. Gessaroli, A. Kovacs, T. Kikuchi, C. Meltzer of the Duke University, Durham; H. O. Cohn, W. Bugg, dell' Oak Ridge National Laboratory, Oak Ridge; A. Pevsner, P. Schlein, M. Meer of the Johns Hopkins University, Baltimore; N. Tomasini Grimellini, L. Lendinara, L. Monari, G. Puppi of the Bologna University in Physical Review Letters, 7, n°6, 15 (1961) 264-268

[12] M. M. Block, R. Gessaroli, S. Ratti, Nothwestern University, Evanston; L. Grimellini, T. Kikuchi, L. Lendinara, L. Monari of the Bologna University; E. Harth, W. Becker della Syracuse

University, New York; W. M. Bugg, H. Cohn del Oak Ridge Natiional Laboratory, Oak Ridge in Physical Review, 150, 2, (1963) 766-768

[13] M. M. Block, R. Gessaroli, s. Ratti, Northwestern University, Evanston; L. Grimellini, T. Kikuchi, L. Lendinara, L. Monari, Physics Institute, Bologna; E. Harth, Syracuse University, Syracuse, Il Nuovo Cimento, 10, 28, (1963) 299-308

[14] L. Gray, E. Harth, J. Leitner, Syracuse University; J. Auman, M. M. Block, R. Gessaroli, T. Kopelman, S. Ratti, Northwestern University; L. Grimellini, T. Kikuchi, L. Lendinara, L. Monari, Physics Institute, Bologna in Il Nuovo Cimento, 10, 28 (1963) 179-183 ■

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