

confinement imply the experimentally observed color singlet nature of hadrons? This converse can actually be proved within this theory.²

This theory also allows exactly relativistic phenomenology in a natural way to explain observed hadron mass spectra with some success.³

References

1. T. Biswas, *Nuovo Cimento A*, **107**, 863 (1994)
2. T. Biswas, *Nuovo Cimento A*, **104**, 995 (1991).
3. T. Biswas, *Nuovo Cimento A*, **88**, 145 (1985).

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Drell Clarifies Nuclear Stance

I wish to correct Irwin Goodwin's "Washington Reports" story on nuclear testing (October, page 51), which discusses the Jason panel that I chaired. At issue is Goodwin's assertion that "As recently as 1990 Drell had advocated low-yield underground tests, but he now says his views have been affected. . . ."

In my 1990 congressional testimony, when I presented the results of my report on nuclear weapons safety to the House Armed Services Committee, I testified that the US arsenal meets the official US safety requirements. But I also said certain safety improvements could be achieved and should be considered, most notably by replacing the high explosive now used in the Trident warhead with an insensitive high explosive used in most of the other modern US nuclear warheads. Replacement, though, could be undertaken only after a limited series of underground tests at full primary yield or higher—that is, not low-yield tests. The importance of the resulting improvement in safety, I testified, would have to be weighed against a political judgment as to the importance of a comprehensive test ban in achieving an effective worldwide nonproliferation regime (which I support, now as then), with the highest priority going to the effort to reduce nuclear danger.

The US is now committed to negotiating a comprehensive test ban treaty (CTBT) by the end of 1996. This commitment was made in order to gain the support of 175 nations for the indefinite extension of the nonproliferation treaty this past May at the United Nations in New York.

The remaining issue addressed by

this year's Jason study on nuclear testing was the importance of retaining the option of low-yield (subkiloton) tests as permitted activities under a CTBT. Based on our technical analysis, we concluded that such low-yield tests were less important than the other actions we described, as reported in Goodwin's story. Our conclusions support President Clinton's decision to call for a true zero-yield CTBT. I fully endorse that decision.

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Eugene Wigner Remembered

I would like to mention three chance encounters I had with Eugene Wigner that have led me to rank him among the great "true gentlemen" of the world and to lament his leaving us. [See the articles on pages 40 and 46.]

After receiving my PhD in physics from the University of Nebraska, where I worked with Joseph Macek on an extension of some of Wigner's work, I received a postdoctoral appointment to Louisiana State University. There I discovered that the office next to mine was occupied by Wigner. We met briefly one day when he was standing next to my car, talking with my wife and two daughters, who were waiting for me. He said, "Ah, so these are your charming daughters, Dr. Wooten," and walked off after politely shaking my hand. I had seen him in the hall but had never actually been introduced to him and was amazed that he would even be aware of me. This was the first Nobel laureate I had met; I thought them to be gods and I the dust at their feet.

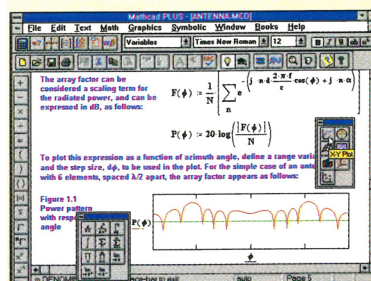
Later in the semester, during an atomic physics seminar on the effects of temperature and pressure in solids, I became confused at what seemed to be a discrepancy between the effect pressure would have on a substance and what the speaker's equation seemed to imply. When I explained that I was confused and asked him to clarify one point of his work, the speaker haughtily replied that it should be obvious to anyone and began to move ahead in his talk. At that point Wigner raised his hand and, upon being recognized, stated very quietly and politely, "I'm sorry, but like Dr. Wooten, I too do not understand." This, of course, produced an immediate, if somewhat nervous, detailed and satisfactory explanation.

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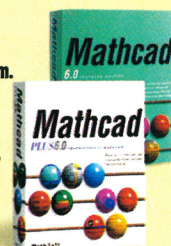
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LETTERS *(continued from page 15)*

Wigner's action was that of a gentleman, quietly reminding the speaker that the goal of physics is understanding, not the inflation of one's ego at another's expense.

My last remembrance of Wigner came almost two years later, after I had obtained a position at Oak Ridge National Laboratory. Returning along the sidewalk from the cafeteria, I saw Wigner talking to Herman Postma, our laboratory director at that time. Never for a minute thinking Wigner would remember me, I was amazed to hear him say to Postma, "Excuse me, but I'd like to say hello to a colleague of mine," whereupon he came over, shook my hand and said: "Dr. Wooten, I'm so glad to see you here. This is such an excellent place for young physicists. How are your two lovely daughters?" He took the time to chat with me briefly and then returned to Postma, who must have been somewhat puzzled about who Wigner would take the time to speak to. I believe that Wigner greeted me not because I was so memorable but because he took the time to be interested in young people and in their interests in physics.

These several actions by Wigner convinced me that there can be truly great men who are not snobs, who take the time to talk to new people and who encourage them.

JOHN W. WOOTEN

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Correction: Taking the Pressure Off

Concerning your report (September, page 9) detailing Gabrielse *et al.*'s method for trapping and cooling positrons, we were intrigued to discover that 100 gas atoms per cubic centimeter can exert a pressure of 5×10^{17} torr.

Should we congratulate the researchers on discovering ultraheavy gas atoms—and a new species named Talltaleium?

PHILIP MCGUINNESS

LIAM A. DOYLE

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[Editor's Note: The report should have read 5×10^{-17} torr.]

Getting a Fix on Origins of GPS

The letter on the history of the Global Positioning System by for-

mer Aerospace Corp president Ivan A. Getting (October 1994, page 13) could have corrected some very minor mistakes in Daniel Kleppner's "Reference Frame" column, "Where I Stand" (January 1994, page 9). Instead Getting's rewrite of history adds to the propagation of errors on the origins of GPS.

I worked for the Naval Research Laboratory for 37 years before retiring in 1980. The idea of using satellite-carried precise clocks for three-dimensional navigation came to me out of the blue in 1964. Ground tests at NRL were used to demonstrate the principle to Bureau of Naval Weapons personnel later that year. The idea is covered by US patent 3 789 409, entitled "Navigation System Using Satellites and Passive Ranging Techniques." I was in charge of the NRL organization that developed satellites for demonstrating the Time Navigation (Timation) concept for the Naval Air Systems Command. Getting introduces an unsubstantiated name change when he writes, "As the decade [of the 1960s] wore on, the Air Force with GPS, the Navy with improved TRANSIT concepts and the Naval Research Laboratory's Timation concepts, and the Army with SECOR competed for the role of the Department of Defense's navigation system."

Actually the only real competition was between the Air Force-Aerospace Corp 621B concept and NRL's Timation. (The name Navstar GPS came later.)

The 621B concept in its original publication consisted of three or four constellations of 24-hour satellites. Each constellation would have had a single geostationary satellite and either three or four satellites having eccentricities and inclinations such as to trace near-circular ground tracks about the subsatellite point of the stationary unit. According to reference 1, these satellites were to use transponders continually controlled by ground stations.

The Timation concept consisted of an array of satellites carrying high-stability free-running clocks updated by ground stations as required. All the satellites were to be in circular orbits in several planes, all with identical high inclinations.

GPS consists of an array of satellites carrying high-stability free-running clocks updated by ground stations as required. All the satellites are in circular orbits in several planes, all of them with identical high inclinations.

Is the GPS description more like that of 621B or of Timation? You be the judge.

NRL placed four Timation navigation satellites (some of the names were changed later) in orbit. The first two were low-altitude secondary payloads. These units used crystal oscillator clocks. The third unit carried two rubidium oscillators obtained from Efratom in Munich through Frequency and Time Systems, a company started by Robert Kern and Arthur McCoubrey. The fourth satellite contained two cesium beam clocks developed for NRL by Kern at FTS. All the results and development plans for Timation were furnished to the Joint Program Office upon its adoption of this concept.

Of the other concepts developed at the time, the one developed for the National Aeronautics and Space Administration by Roy Anderson of General Electric was the closest to Timation. Anderson's concept envisaged 24 satellites in four planes of 5600-nautical-mile circular orbits. Anderson's idea, however, used the satellites as transponders rather than as clock carriers.

I find Kleppner a bit too prompt with his regrets in his reply to Getting's letter. Also, in writing that "the GPS was initiated by the Navy in the 1970s and taken over by the Air Force in the early 1980s," he should have used "1960s" rather than "1970s," "1970s" rather than "early 1980s" and "the Joint Program Office managed by the Air Force" rather than "the Air Force." Even so, his description was much closer to the facts than were the assertions in Getting's letter.

Reference

1. B. W. Parkinson, S. W. Gilbert, Proc. IEEE **71**, 1177 (1983).

ROGER EASTON

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GETTING REPLIES: Roger Easton declares that "Getting's rewrite of history adds to the propagation of errors on the origins of GPS." Well, I am not a historian, but as a scientist I try to be accurate and give credit to others where it is deserved. In my October 1994 letter I tried to recognize the excellence of the Naval Research Laboratory and the frustrations its scientists encounter in getting recognition. I also gave credit to the Johns Hopkins Applied Physics Laboratory and the US Navy for the development of the TRANSIT satellite navigation system. And I referred to NRL's work on Timation in the 1960s.

Unfortunately I also wrote, "As the decade [the 1960s] wore on, the Air Force with GPS. . . ." My choice of "GPS" was unfortunate; I should have