Letters

Sulfur Dioxide Pollution

Philip H. Abelson's editorial "Control of sulfur dioxide emissions from coal" (25 July, p. 253) correctly identifies one of the major environmental problems arising from the rapidly changed international petroleum situation. However, he paints a far bleaker picture of the nation's ability to turn to coal for electrical power generation than really exists.

First, we must recognize the consensus in the scientific community that sulfur oxides pollution—which comes primarily from coal-fired power plants—does represent a serious health hazard and that present SO_2 ambient standards should not be reduced.

Both the National Academy of Sciences report cited in Abelson's editorial (*1*) and a recent (13 March 1975) report (*2*) by the Environmental Protection Agency's Science Advisory Board have not favored any relaxation of the present SO_2 standards. Quantitative assessment of the health effects of increasing sulfur oxide emissions is difficult because of the uncertainties involved in the scientific information base. However, present estimates strongly suggest that substantial excess adverse health effects may be expected each year if national ambient air quality standards are not met: increased premature deaths, increased illness among susceptible segments of the population, increased acute lower respiratory illnesses in otherwise healthy children, and increased chronic respiratory disorders among adults.

Quite frankly, Abelson's phrase, "... the public will not stand for drastic curtailment of electricity" is quite misleading, since it implies that the public must choose between clean air and electricity. No such choice is necessary. By an intelligent choice of SO_x control options, including the use of flue gas desulfurization (FGD) technology where needed, a given utility can meet its responsibilities to produce electricity without unacceptable environmental damage.

The Environmental Protection Agency has been working with the states to ensure that the maximum amount of coal—both high sulfur and low sulfur—can be burned consistent with protection of public health. Through relaxation of "overkill" requirements in some of the state implementation plans, about 50 million tons of high sulfur coal which might have otherwise been precluded by the plans can be used. We anticipate that an additional 50 million tons will be made available by further revision of state plans during the coming year. There are, however, areas where the air quality has deteriorated so severely that sulfur oxides control is required as soon as possible.

Abelson alleges that "EPA has been pushing hard for installation of flue gas desulfurization systems" and points to a number of problems with scrubbers. In particular, he indicates that scrubbers have not proved reliable, produce a sludge which cannot be easily disposed of, and cost between $8 to $30 or more per ton of waste produced.

Most of the operating scrubbers (at power plants producing a total of 3340 megawatts) are now achieving a high degree of reliability, following the solution of problems encountered during shakedown—the usual pattern associated with the introduction of any new technology.

Abelson's reference to the magnitude and severity of the sludge disposal problems associated with FGD systems is also misleading. It is important that more representative production rates and acreage estimates be used. Assuming that 90,000 Mw of limestone FGD capacity is installed, EPA staff estimate that about 120 million tons of sludge would be produced each year, not 300 million as Abelson suggests. To put this in perspective, the sludge production (wet, exclusive of ash) from a coal-fired power plant represents less than 20 percent of the total solid waste generated in the coal fuel cycle. Other wastes associated with the coal fuel cycle include surface mining wastes, processing wastes, and coal ash. To put land usage areas in perspective, it is also noteworthy that land affected by sludge disposal can be as little as 3 percent of the total land affected by the total coal fuel cycle. Land uses such as surface mining, rail transport, and transmission generally affect much larger areas than sludge ponds.

Several techniques are available to minimize the impacts from FGD sludge. These include (i) using pond liners in closed-loop systems employing well-engineered disposal sites to eliminate water pollution problems; and (ii) employing commercially available sludge fixation processes to convert the sludge into a more desirable landfill material with acceptable structural properties and decreased permeability and leachability.

Abelson states that "There must be and there are better solutions." Direct combustion of low sulfur coal offers one alternative; unfortunately the nation's current production of low sulfur coal is limited. In addition, even the highest quality coals cannot meet air quality standards in certain metropolitan areas with severe pollution problems. At the present state of coal technology, FGD is the only alternative to the burning of scarce low sulfur fuels in highly impacted areas and in new power plants. Advanced technologies which have potential for SO_x control include fluidized bed combustion, chemical coal cleaning, low-Btu gasification, and coal liquefaction. However, these technologies do not appear any less costly from FGD, and all have potential residual and disposal pollution problems which have not been adequately studied. Due to their early stage of development, these processes, under the most favorable assumptions, cannot make a major impact on SO_x emission control until the mid-1980's.

One alternative which may have limited but important utility for Northern Appalachian coal is advanced physical coal washing, which removes the pyritic sulfur from coal. Through such treatment, much of the lower sulfur Appalachian coal could be used in both existing and new power plants in the near future. The EPA has been conducting research for a number of years, with the Bureau of Mines, to improve the performance of coal washing plants. Further, EPA has funded a comprehensive characterization of the washability of Northern Appalachian coal. Coal washing is less expensive from FGD and, where applicable, can be used by utilities and industries.

It should be noted that EPA has been coordinating an interagency environmental-energy program whose charter includes the development of improved SO_x control schemes, such as second-generation regenerable and nonregenerable FGD technology, and chemical coal cleaning techniques. The program also provides for an assessment of the environmental impact of emerging energy-control technologies, such as coal gasification and fluidized bed combustion being developed by the Energy Research and Development Administration and others. Unfortunately, it must be reported that, despite the obvious importance of this R,D & D program, the Congress substantially cut the program in fiscal year 1975 from $191 million to $134 million, and has just reduced the fiscal year 1976 funding from $112 million to $100 million.

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2. Ad hoc panel, Executive Committee Science Advisory Board, Environmental Protection Agency, *Scientific and Technical Issues Relating to Sulfates* (Environmental Protection Agency, Washinton, D.C., 1975).

Particle Discovery at Brookhaven

Many stories have appeared in newspapers and science magazines here and abroad about the discovery of the J or psi particle announced jointly last November at the Stanford Linear Accelerator Center (SLAC) and Brookhaven National Laboratory (Research News, 6 Dec. 1974, p. 909). In the hope of eliminating confusion which has arisen from these articles, I present here a record of the Massachusetts Institute of Technology–Brookhaven experiment taken from our log books and records of 1974.

After the initial shakedown of our electron-pair spectrometer at Brookhaven, we started taking data from April to August on the high-mass region 3.5 to 5.5 Gev of $p + A \rightarrow e^-e^+ + x$. We made a 100-hour run and observed very few counts. During this period we measured the known meson decay region and observed genuine electron pairs, which showed the spectrometer was functioning properly.

At the end of August we changed the setting to cover the mass region from 2.5 to 4.0 Gev. The data exhibited a sharp peak at the mass of 3.1 with little background. The original mass plot of this run was presented in *Physical Review Letters* (*1*, figure 2). The measured width of the peak, $\Gamma < 5$ Mev, was presented in *Nuclear Physics B* (*2*). Before we had time for more studies, the accelerator was turned over to M. Schwartz (Stanford–New York University collaboration).

During the week of 13 October, we informed a few people of our results (for example, T. D. Lee of Columbia University), and in order to make sure that we received a priority over Schwartz on the use of the accelerator in the coming weeks, I informed the management of Brookhaven (in particular R. R. Rau, the director of high energy physics) of the existence of a sharp and narrow peak at a mass of 3.1 Gev.

I was considering announcing the results during the retirement ceremony (17–18 October) of V. F. Weisskopf. We postponed the announcement for two reasons: (i) We realized that old measurements by L. W. Smith (*3*) at Brookhaven had shown the direct μ^-/π^- ratio to be 10^{-4}, a mysterious number that seemed to not change from 2000 Gev to 30 Gev. We found that this ratio could not be easily explained by ρ, ω, ϕ, or J alone, indicating something more exciting might be just around the corner, and we decided to make direct measurement of this number by ourselves (however, even to this day, the mystery of $\mu^{-1}/\pi^{-1} = 10^{-4}$ is still not solved). (ii) There were speculations that high mass electron-pair production from proton-proton collisions came from a two-stage process of $p + p \rightarrow \pi + \ldots$ and $\pi + e^-e^+ \ldots$. This can be checked by a target thickness measurement. The yield from a two-stage process would increase quadratically with target thickness, whereas for a one-stage process the yield increases linearly.

During Weisskopf's fest we had quite a few discussions, in which we disclosed our discovery, with physicists who came for the occasion, for example W. Jentschke of CERN. On 22 October, U. Becker of our group gave an open seminar reporting our results to MIT high energy physics groups.

During the day of Becker's seminar, I was at Brookhaven and received a surprise visit from Schwartz, who had returned to Brookhaven to start after we had finished. He immediately wanted "to see the mass plot of the resonance around 3.0 Gev." Not wanting to spread information further and announce our results in this way, I denied his request and bet him $10 that there was no such resonance. I returned to our counting room and posted a memo which said, "I owe M. Schwartz $10." I paid him after the announcements of the discovery of the J particle. One member of our group, S. L. Wu, and I later talked with Schwartz and others and learned that, at the time of betting, not only Schwartz's group knew about the discovery, but many others knew as well.

In the last week of October, both Y. Y. Lee, of our group at Brookhaven, and I received many inquiries about our results. Members of our group working at the MIT Laboratory for Nuclear Science computer were besieged by people interested in seeing our mass plots. I also received a few phone calls from M. Deutsch at MIT suggesting that we should publish our results quickly, as, by then, many people knew about them.

On 6 November, I paid a visit to G. Trigg, editor of *Physical Review Letters*, to find out if the rules of publication without refereeing had been changed, and I wrote a simple draft.

On 10 November I went to SLAC for a program advisory committee meeting. The moment I checked into the hotel, I received a phone call from Deutsch, who mentioned that he had heard there was great excitement at SLAC but he did not know the nature of their results. I traced Rau to Los Alamos and informed him of my decision to announce our results and placed a call to S. Brodsky at SLAC informing him of our results. He was very excited but did not want to tell me about the SLAC results. He told me that he would arrange for me to give a presentation the next day. The next morning I walked into W. K. H. Panofsky's office at Stanford to inform him of our results. He mentioned that similar results had been obtained at SPEAR (the storage ring at SLAC) over the weekend.

Monday morning, 11 November, Wu called G. Bellettini, director of the Frascati laboratory in Italy, informing him of our results. On very short notice, the ADONE (storage ring) group succeeded in pushing the energy above its normal limit (2×1.5 Gev), set up a special 1 Mev per step searching program, and began their search on 13 November. Since they knew approximately where to look, after only 2 days, on 15 November, Bellettini informed us that a clear J signal had been observed at Frascati.

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I would like to add my recollections—aided by notes made at the time—to Ting's letter.

For several years, considerable resources of the MIT Laboratory for Nuclear Science (LNS) have been devoted to his systematic search for new vector mesons. My first knowledge of the discovery of the J particle came on 22 October 1974, when U. Becker presented a preliminary evaluation of the data to a laboratory seminar. The presentation was so cautious that the full significance of the data did not become clear to most participants. My own understanding was largely based on a private discussion following the seminar. At this point, the Ting group was obviously caught between the contradictory desires of communicating the discovery to other friends and avoiding premature dissemination of specific quantitative results which might still be subject to last-minute corrections. In accordance with their explicit request, I restricted my discussions with outsiders

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