gamma-ray region. Low-lying muon orbits in heavy atoms turn out to have radii comparable to the nuclear radius; so the usual "point-nucleus" approximation will not work and a correction for finite nuclear size is needed.

Further important corrections for deeply bound muon levels are required for vacuum polarization (screening of the nucleus by a cloud of virtual electron–positron pairs), Lamb shift, electron screening and so on. The vacuum-polarization effect is especially significant for 100–500 keV transitions in medium and high-Z atoms. Comparison of theory and observation for these transitions is therefore a sensitive test of QED corrections for vacuum polarization to higher orders.

Measurements reported by the CERN group in 1970 and the Ottawa–Chicago group in 1971 of certain muonic x-ray lines disagreed with each other, by about 120 eV in 430 keV, although the CERN group's data agreed with contemporary calculations (see figure).

The more recent experiments at CERN and SREL are essentially repeats of the earlier ones, but with increased resolution in the detectors, improved determination of the primary calibration standard, and extra care all round. The measured transitions were 5g–4f in lead and 5g–4f and 4f–3d in barium.

Lithium-drifted germanium detectors picked up the radiation from a negative muon beam stopped in appropriate targets. The Ottawa–Chicago group used dual signals, time and amplitude; the time signal provided a time slot for each event, to ensure measurement only of prompt x rays from the transition of interest and not the later radiation from muon capture events, while the amplitude signal provided pulse-height data. Ge(Li) detectors with high resolution (525 eV FWHM at 122 keV and 1050 eV at 468 keV) were calibrated with a gold-198 standard (412 keV) and gamma rays based on this standard. R. D. Deslattes, E. G. Kessler, W. C. Sauder and A. Heiss (National Bureau of Standards) recently redetermined the energy of this standard, finding it 12 eV higher than previously thought.

The figure shows how these two new experimental determinations of the 5g–4f transition in muonic lead are now essentially in agreement with each other and with recent calculations, some of which were reported to the High-Energy Physics and Nuclear Structure meeting by M. K. Sundaresan and P. J. S. Watson (Carleton University) and by G. A. Rinker (Los Alamos) and L. Wilets (University of Washington).

It is now apparent that the QED correction for vacuum polarization has been correctly understood and applied, at least to the 20 parts per million precision of the most recent determinations. But many theoretical speculations concerning possible breakdowns in the theory—put forward when large discrepancies still existed—remain to be explored. They may yet come into their own when high-intensity beams from the new meson factories, and the use of crystal-diffraction spectrometers, bring about another round of improvements in measuring technique. —JTS

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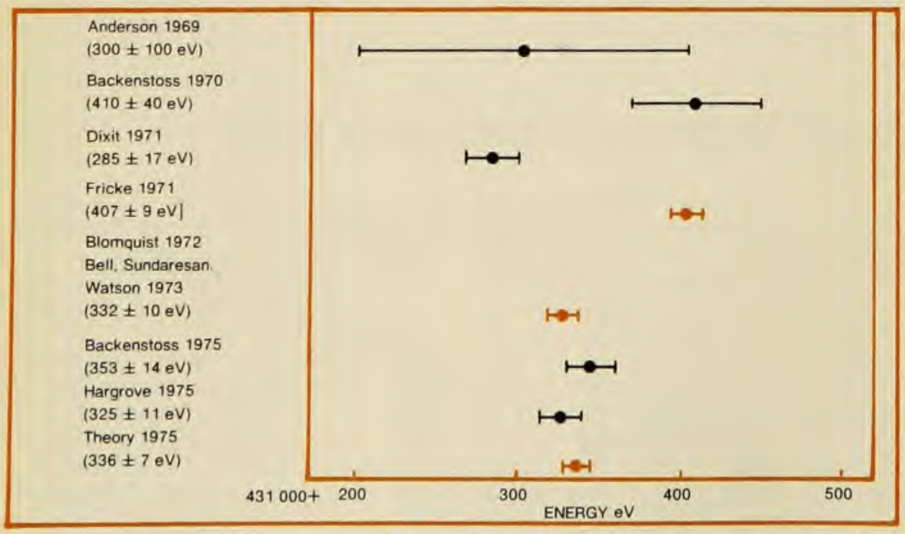

Recent history of experimental (black) and theoretical (color) values of the $5g_{9/2}$–$4f_{7/2}$ transition in muonic lead. The 1975 theory point combines various values presented to the 1975 High-Energy Physics and Nuclear Structure Conference. (Adapted from V. W. Hughes, ref. 1.)

Shuttle studies to include atmospheres and plasmas

The Science Definition Working Group associated with the Atmospheres, Magnetospheres and Plasmas-in-Space Payload has identified scientific objectives for the project. The group has named outstanding problems in each of the three areas and reviewed possible experimental techniques to be used with the Payload, which is to be placed in orbit by means of NASA's Spacelab–Shuttle system.

Those space workers with whom we discussed the Payload program express enthusiasm for the present plans. One significant element cited is NASA's so-called "facility approach" (whereby researchers could supplement their apparatus with some equipment from a group of expensive project-supplied core instruments). Another is the use of active probing techniques; these would be made possible by use of electron accelerators and high-power transmitters capable of interaction with fields and particles in the near-space environment. Some division exists over NASA's manned-mission approach among those we consulted.

The working group, headed by project study scientist Charles Chappell, lists the following categories of atmospheric-science experimentation as feasible within the Payload program: the measurement of minor constituent (chlorine compounds or atomic oxygen, for example) distributions in the atmosphere, studies of atmospheric energetics and dynamics, the study of how changes in solar activity influence climate and the investigation of atomic and molecular processes. Data bearing on theoretical predictions about the depletion of stratospheric ozone could be obtained in experiments that would determine the concentrations and chemistry of atmospheric chlorine compounds and check for a correlation between high-energy proton precipitation and ozone variability. Other possibilities include a look at changes in the D region (65–90 km altitude) at middle latitudes during winter and following geomagnetic storms, and more precise measurement of global horizontal wind fields.

Among the problems in magnetospheric and plasma physics suggested for consideration are probing of convective processes in the ionosphere and magnetosphere, studies of wave injection and wave-particle phenomena, an inspection of magnetospheric particle-interaction processes, wave-propagation studies and investigations of beam-plasma interactions and plasma-flow interactions with material bodies. The measurement of field-aligned (Birkeland) currents, the generation of gravity waves in the upper atmosphere, plasma emf experiments and the creation of artificial comets constitute a sampling of experimental activities urged by the working group for investigations in these areas.

One of the Payload project's departures from previous efforts to investigate the near-space environment of the Earth is its merging of a manned spaceflight program (which would permit on-the-spot modification of experimental setups) with the performance of research in the areas of atmospheric, magnetospheric and plasma physics. Thomas Donahue, head of the Department of Atmospheric and Oceanic Science of the University of Michigan, told us that in his opinion the best place for atmospheric researchers is on the ground.

Another atmospheric scientist, Sidney Bowhill at the University of Illinois Department of Electrical Engineering, disagrees. Noting that he has not been one to criticize the manned Apollo program for absorbing needed research funds, Bowhill asserts that the Payload project should include an on-board expert to supervise experiments while the Spacelab is in orbit. He acknowledges that some research areas—the type that use mostly passive experiments in space—would not need a manned mission, but he sees atmospheric, magnetospheric and plasmas-in-space research profiting from such an approach.

Margaret Kivelson, a magnetospheric and plasma researcher at the UCLA Institute of Geophysics and Planetary Physics, takes a middle position; she views the manned aspect of the program as not of the highest priority, but interesting nonetheless.

Kivelson also warns that other valuable research approaches ought not to be slighted. She feels that such worthwhile missions as the placing of a craft in a trajectory from which it could examine conditions outside of the ecliptic plane might be endangered by excessive concentration on the facility research mode. Donahue, too, is wary of potential limitations on research in his field due to emphasis on laboratory-type experiments to the exclusion of more traditional efforts and due to the constraints on apparatus imposed by the facility approach. However, this approach is intended to open the field of space physics to a much larger body of scientists than have been involved in the past. Previously, the expensive hardware requirements have tended to restrict space research efforts to those from large affluent institutions.

Of particular interest is the "quick-reaction capability" that NASA scientists envision for the project. By using easily retuned equipment, the project would acquire a versatility that would allow it to investigate new areas of research as they become prominent, without undergoing overly long delays.

Overall, the project draws mostly favorable comment. Bowhill refers to it as an "exceptionally well-conceived program," and the Payload facility is viewed as being eminently capable, in terms of the scientific questions to be tackled, of fulfilling its promise and performing important tasks. —FCB

Central peaks

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peak" centered at zero-frequency shift whose intensity became very large as the transition temperature was approached.

At about the same time John Axe and Gen Shirane (Brookhaven) saw similar behavior associated with the phase transition in Nb_3Sn. These results suggested there was trouble with the soft-mode picture.

Subsequently various groups have found central peaks connected with a number of solid-state phase transitions. The most puzzling feature of these observations is the narrow energy width of the central peak—well below the resolution of conventional neutron spectroscopy. As Axe explains, the challenge to the theorist searching for an intrinsic lattice dynamical explanation is to discover some way of coupling phonons together to produce a more complicated mode of excitation with a lifetime three or four orders of magnitude longer than that of the phonons themselves.

Experiments on KDP. In 1974 Nicholas Lagakos and Herman Cummins (City College of the City University of New York) found a central peak in KDP—the first material to fit Cowley's original picture. In a Brillouin-scattering experiment they found a very strong peak centered around zero that was too narrow to resolve.

At the San Juan conference, three groups reported their preliminary indirect observations of a finite width in the KDP central peak. E. M. Alexander (Naval Research Laboratory) and Robert W. Gammon (University of Maryland) discussed an ultrasonic absorption technique. Analyzing attenuation data, they found that the frequency dependence could be explained by postulating that the acoustic phonons are coupled to a central mode. Their estimated width (with large error bars) is 8 MHz.

In a postdeadline paper K. Alex Müller, N. Dalal and W. Berlinger (IBM Research Center, Zurich) reported on electron-spin resonance measurements on KDP and some of its isomorphs, in which a Cr^{5+} paramagnetic impurity had been deliberately imbedded to act as a probe. The spectrum of the spin-resonance line differs dramatically if the dominant motion of the lattice is faster or slower than a characteristic time given by the line splitting. So by changing the temperature of the sample and observing the spectrum, the IBM workers could establish the temperature at which the local lattice frequencies change, from greater to less than the one given by the splitting of the spin-resonance lines. This means Müller and his collaborators find that this characteristic frequency in KDP is smaller than 100 MHz below $T_c + 60$ K.

A third experiment was reported by R. Blinc (Institut Jožef Stefan, Ljubljana, Yugoslavia), who did nuclear-magnetic resonance studies to obtain the width of the central peak in KDP. His results are essentially similar to the esr studies.

A variety of measurements on strontium titanate, made earlier, had also suggested a very narrow central peak (less than or equal to 100 MHz).

Theory. The original picture of Cowley for piezoelectric-ferroelectrics was that a central peak is produced by coupling between the soft mode and the density of acoustic phonons.

In 1971 Feder wrote that a central peak is produced by a nonlinear coupling between a soft mode and temperature fluctuations. The original Cowley model treated only third-order anharmonicity. Then Richard Silberglitt (now at the National Research Council) in 1972 extended this type of calculation to the fourth-order case. The following year Cowley and Geoffrey Coombs (University of Edinburgh) extended their work using the same many-body perturbation theory approach. Cowley told us that the new measurements suggest that this theory does not account for these results.

Another explanation had been offered by Axe, and independently by Franz Schwabl (then at Jülich) who realized that a static defect could produce an infinitely narrow central peak with a divergent intensity if it could properly couple to the soft mode. Axe says the static-defect explanation is out, at least for those materials where experiments show a finite width to the peak.

Although the static-defect explanation does not appear to work, Bertrand Halperin and Chandra Varma (Bell Laboratories) have hopes for a dynamic-impurity explanation. If an impurity is frozen into a site in which it breaks the symmetry of the high-temperature phase, a local domain will be formed with a fixed spin for the distorted phase. Halperin told us that the width of the central peak is determined by the lifetime of domains in the material and that experimentally these are about 10^{-9} sec. He and Varma find that very small impurity concentrations (about 1 part of 10^4) might be sufficient to account for the results if the impurity moves to another site with opposite