cal theory and a fully quantum theory of the atom field interaction is that in neoclassical theory a purely excited state would not radiate at all because the required off-diagonal elements of the atomic density matrix are zero. However, Jaynes told us that no experimenter has ever put an atom in exactly a pure excited state, and if he did it would take only an infinitesimal perturbation to produce conventional spontaneous emission; this is analogous to the state of an inverted pendulum. Thus isolated atoms excited to something near but not quite the fully excited state would exhibit a decay of a spontaneous-emission variety.

The quantum theory of radiation on the other hand predicts a simple exponential decay that some say is brought about by vacuum fluctuations. This was investigated experimentally by Hyatt Gibbs of Bell Labs (*Phys. Rev. Lett.* 29, 459, 1972). He reported observations of incoherent resonance fluorescence from a rubidium atomic beam coherently excited by a 6-nanosec (FWHM) optical pulse. The fluorescence was a maximum for an input pulse area of π and 3π and a minimum for areas of 0, 2π and 4π; this result, Gibbs noted, is in excellent agreement with the quantized field theory of radiation in the Weisskopf–Wigner approximation.

To this, Jaynes told us that the criterion of a 2π pulse was that the fluorescence reach a minimum; thus the correctness of one theory was already assumed in the process of interpreting the data. Gibbs, on the other hand, claims that the input pulse area was calibrated by self-induced transparency —a phenomenon that can be explained by a semiclassical theory.

According to neoclassical theory an atom's radiation is proportional to the square of its dipole moment, which vanishes for an atom in the pure excited state. Even for an equal admixture of ground and excited states the difference between the predicted fluorescences in the two theories is large, Gibbs says. Jaynes disagrees. According to Gibbs, computer simulations of the experiment including actual pulse shape, beam absorption width and radiative damping are in good agreement with quantum electrodynamics but in poor agreement with neoclassical theory, even for all possible values of the "dynamic shift" that occurs in the neoclassical theory. According to Jaynes an analysis of the experiment suggests that one never got far enough from the ground state for differences to show up.

John F. Clauser (Berkeley) pointed out that neoclassical theory is unable to explain previous observations of the polarization correlation of photons emitted successively in a $J = 0 \rightarrow J = 1 \rightarrow J = 0$ atomic cascade. Quantum electrodynamics predicts no coincidences with the analyzing polarizers crossed. Previous semiclassical theories would have predicted a nonvanishing of the coincidence rate with analyzers crossed.

In his lecture Lamb pointed out that in the 1947 experiments on the electromagnetic level shift, Retherford and he separated a beam of hydrogen atoms by a molecular-beam technique such that the atoms in the 1s ground state went on one side of a divider while atoms in the 2s metastable excited state were deflected by recoil to the other side of the barrier. The atoms in the 2s state were then transferred by incident microwaves to the 2p state and were then observed to decay radiatively to the 1s ground state. Lamb points out that, since the excited atoms were rather carefully separated from the ground-state ones, the atoms that pass from the 2s state to the 2p state under the influence of the microwaves would have a wave function that contains only (appreciable) probability amplitudes for being in the 2s or 2p state. Hence there would be no significant matrix element of the transition current corresponding to the transition between the 2p and 1s ground state. Therefore neoclassical theory would seem to imply greatly diminished decay of the 2p state; this is in direct contrast to experimental evidence, Lamb said.

Jaynes points out that the statement that the 2s and 1s atoms were separated is not an experimental fact but an inference based on the assumed correctness of conventional theory. In reply, Lamb told us that this remark indicates that not only does Jaynes disbelieve quantum electrodynamics but he also does not believe in the conventional interpretation of quantum mechanics either.

At the meeting Jaynes and Franken agreed it was premature to decide who had won their bet.

X-ray laser formed from a gelatine–$CuSO_4$ sandwich?

A group at the University of Utah believes they have made a hard x-ray laser. The strikingly simple experiment was reported by John G. Kepros in a post-deadline paper at the Third Rochester Conference on Coherence and Quantum Optics at the end of June. Subsequently Edward M. Eyring, F. William Cagle Jr and Kepros published a paper in the *Proceedings of the National Academy of Sciences* (**69**,1744, 1972).

The report has met with considerable skepticism but also much interest. Many workers have been trying to produce an x-ray laser. It could be used, for example, to make an x-ray microscope, either scanning-spot or holographic, which could have a 1.5-Å resolution, allowing one to observe the electronic structure of matter, particularly the structure of biochemical molecules. Many other applications are envisioned, such as producing coherent excitation of nuclear states and for heating and diagnostics in plasma physics.

The Utah group takes some ordinary Knox gelatine, mixed according to manufacturer's directions in a 10^{-3} molar copper-sulfate solution, and puts the gelatine between two microscope cover glasses. Then they pump the gelatine sandwich with a neodymium-glass laser that produces 30–40 joules in 20 nanosec. They focus the light to a rectangle (1.0 cm × 0.1 mm) on the sandwich. To detect the x rays the experimenters use x-ray film wrapped in four layers of black paper and four layers of aluminum foil. They found spots between 0.1 and 0.2 mm in diameter; the spot size did not alter when they changed the distance between film and sandwich from 30 to 110 cm, thus suggesting that the x rays are collimated.

No precise wavelength determination has been made, but Kepros says he believes they are seeing copper K radiation around 1.5 Å (8 keV); in particular he feels they are probably seeing the $K\alpha_1$ line. The group has done absorption measurements with various metals, finding that the radiation is absorbed and attenuated the appropriate amount by iron and nickel and not attenuated significantly by the aluminum. Also the fact that the radiation passes through about 110 cm of air indicates that the energy is greater than about 5 keV.

The Utah group would like to do a powder diffraction experiment but the alignment problems are enormous, because every time they fire the laser the gel sandwich is shattered. The lab is strewn with pieces of cover glass.

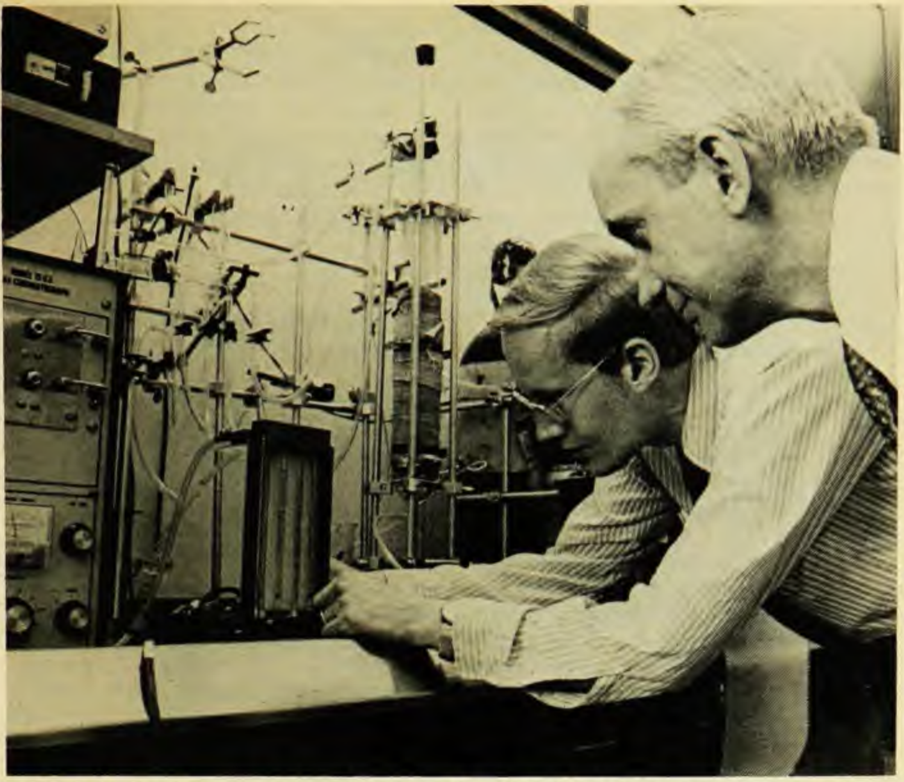
X-ray laser? Light from neodymium-glass laser passes through condensing lens and cylindrical lens (next to hands of John Kepros). Light then strikes copper gel sandwich. At right and rear are x-ray cameras; at left and above are ionization chambers.

If the copper sulfate gel becomes exposed to the air it dries out and the experimenters see no x rays. Even with the proper amount of water the experiment is not reproducible; they see x rays only about 10% of the time.

Assuming the Utah group is seeing the copper $K\alpha$ line at 8 keV, Michel Duguay of Bell Labs (who has been devoting considerable time to x-ray laser considerations) calculates that they would need a pumping power of about 10^{15} watts; the Utah group has only 1.5 gigawatts of pumping power, a million times too small. Kepros told us this is inaccurate. On focusing the intensity is 300 gigawatts/cm^2, he said.

Kepros, at the Rochester conference, proposed that a population inversion may be produced by a shielding effect whereby the oxygen K-absorption edge protects the copper L shell from depopulation by slow electrons. Fast electrons would not be significantly affected by the oxygen and could then remove the copper K-shell electrons, thus allowing the necessary population inversion.

Another possible explanation has been offered by Ray Elton of the Naval Research Laboratory, who has been observing intense x-ray emission coming from a small region in a plasma pinch. Elton and the Utah group feel that the mechanism for the anomalous, highly localized heating observed in the pinch may be similar to the mechanism that produces the high electron energy needed to cause the Utah laser to lase. Elton qualifies this suggestion, however, by noting that the Utah system is a liquid and it is not known whether or not the same kind of plasma conditions are formed when the gelatine system blows up as Elton gets in a two-electrode discharge. —GBL



Rudie J. H. Voorhoeve and Joseph P. Remeika of Bell Telephone Laboratories test new catalytic materials that may be useful for removing pollutants from automobile exhaust.

Bell Labs experiments with auto-exhaust catalysts

In an attempt to understand the mechanism of catalysis a Bell Labs group has found a class of compounds that are good catalysts for the oxidation of carbon monoxide. The Bell group, Rudie Voorhoeve, Joseph Remeika, Paul Freeland and Bernd Matthias (also at the University of California, La Jolla), says[1] that the compounds are promising substitutes for platinum in devices for catalytically treating automobile exhaust.

Matthias became interested in catalysis after talking with Willard Libby (UCLA), who suggested[2] that lanthanum cobaltite might be a good auto-exhaust catalyst. This is the same compound that D. B. Meadowcraft (Central Electricity Research Laboratory, Leatherhead, England) had earlier proposed[3] could be used in batteries.

The Bell group studied the perovskite-like compounds of $RE_{1-x}Pb_xMnO_3$ and $RECoO_3$, where RE stands for the rare earths lanthanum, praseodymium or neodymium. These inorganic oxides are made at high temperatures of about 1200 deg C so that chemically they are quite stable. The experimenters measured the oxidation of carbon monoxide (to produce carbon dioxide) by applying a continuous flow of CO and O_2 to a sample of about 2 cm^2 of catalyst, measuring the oxidation as a function of temperature and a function of time.

They compared their catalysts with a commercial one, the PTX catalyst marketed by Engelhard Minerals and Chemicals Corp; this is a fixed-bed catalyst that consists of about 0.5% (by weight) platinum on silicon dioxide–aluminum oxide arranged in a ceramic honeycomb structure. The experimenters crushed the Engelhard catalyst into pellets and compared it with crushed single crystals of their catalysts. They reported that neodymium lead manganite and praseodymium cobaltite were more active than the PTX catalyst and that the other manganites and cobaltites were about as active as the PTX catalyst. Furthermore they said that the activities of the manganites deteriorate much more slowly than that of the platinum.

Shortly after the Bell announcement, Engelhard's president, Milton F. Rosenthal, issued a statement criticizing the Bell report. He said the Bell results "were based solely on laboratory experiments and that no experiments have taken place with automobiles or with a simulated automobile exhaust-gas environment." He complained that the Bell method was to crush the PTX catalyst, but that Engelhard catalysts were specially fabricated with carefully controlled and dispersed quantities of platinum. In addition he said the flow rates that the Bell group used were too low for practical automotive use.

An Engelhard spokesman told us that the firm has a commitment from the Ford Motor Company to supply 60% of Ford requirements for exhaust catalysts in 1975 and to license them for the other 40% of their needs in that year. —GBL

References

1. R. J. H. Voorhoeve, J. P. Remeika, P. E. Freeland, B. T. Matthias, Science **177**, 353 (1972).
2. W. F. Libby, Science 171, 499 (1971).
3. D. B. Meadowcraft, Nature **226**, 847 (1970).

Shape isomers

continued from page 17

tute), James Nix (Los Alamos) and Wladyslaw Swiatecki (Berkeley) to reason that instead of having a smooth barrier there were actually two parabolas—a deep one where the nucleus normally sits and just to the right of it, on the falling outside wall of the potential, a second, shallower potential. Because the second minimum is higher in energy than the first, the spacing of its energy levels is much larger than the spacing at the same energy in the inside well. When you put neutrons in at different