FWHM VERSUS ABSORBER THICKNESS

The widths of the peaks due to the transmitted electrons (976-keV electrons) were estimated at half of their maximum height and they are plotted as a function of absorber thickness divided by the square of the average velocity in Fig. 4. This plot agrees with the theoretically predicted linear behavior. Comparison of Figs. 3 and 4 shows that the FWHM (subtracting the width due to the instrument resolution) is about 33% of the most probable energy loss for each absorber thickness. Theoretically estimated range of FWHM is 20–25 % of the most probable energy loss.[4]

The presence of an edge around 400 channel and another edge around 800 channel, which is clear for higher absorber thickness in Fig. 1, needs explanation. The edge appears similar to the Compton edge found in gamma spectra.[8] The energy corresponding to these edges were calculated to be 392.3 and 857.0 keV. These energies are the energies one expects for Compton electrons produced by back scattered gamma rays of energy 569.8 and 1063.6 keV. Since ^{207}Bi emit gamma rays of energies 569.7 and 1063.3 keV, it appears the edges in the electron spectra are due to Compton electrons produced in the aluminum absorber by the Bi gamma rays.

This experiment thus enables the students to understand several aspects of interaction of monoenergetic electrons with matter.

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Graveyard shift, Hanford, 28 September 1944—Henry W. Newson

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An account is given of the surprise effects of ^{135}Xe fission product poisoning in the initial operation of the first Hanford plutonium production reactor.

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The late Henry W. Newson had wide experience in the Manhattan Project. In 1941, at the University of Chicago

cyclotron, he took a leading part in a pre-project measurement of the diffusion length of thermal neutrons in a brand of graphite labeled AGX—one of several kinds that were then being tested at two or three laboraborties as possible moderators for the proposed nuclear chain reaction. When the project's Metallurgical Laboratory was organized in Chicago in early 1942, he became increasingly involved in various aspects of the work on the chain reaction. He was a pioneer at Oak Ridge, Tennessee, where on 4 November 1943, he and George Weil were physicists in charge of the first loading of the Graphite Reactor, when it became critical sooner than expected, and he had to rush from the plotting room to tell the loaders to stop. He describes this (another eventful graveyard shift!) in an article in the 1976 autumn issue of the *Oak Ridge National Laboratory Review*. From Oak Ridge he went to Hanford for a year, then spent 1945–46 at Los Alamos, and returned to Oak Ridge. In 1948 he went to Duke University, where he became Professor and Director of the Triangle Universities Nuclear Structure Laboratory. Following Dr. Newson's death in 1978, Mrs. Newson sent some of his papers to Dr. Arthur

H. Snell at the Oak Ridge National Laboratory. Dr. Snell was intrigued by a handwritten account of Newson's experiences at Oak Ridge, and particularly at Hanford. Dr. Snell has added introductory and concluding remarks (italicized), but has purposely edited Newson's penciled first draft very little in order to preserve the freshness and excitement that Newson's writing shows.

One of the greatest of the several great technological gambles of the wartime Manhattan Project was the group of plutonium-producing nuclear reactors that were rushed into construction at Hanford, Washington. Design for them was started almost immediately after the start of the first nuclear chain reaction in December 1942. A series of "exponential experiments" was made at Chicago to test the fuel element design, but from that point it was an enormous extrapolation to the large, complex, multihundred million dollar Hanford reactors. As Dr. Newson explains in the text that follows, the air-cooled 1-MW graphite reactor (the "Clinton Pile") that came into operation in the meantime at Oak Ridge, Tennessee was of little use as a pilot plant for water-cooled Hanford. The CP-2 reactor near Chicago had no cooling at all, and the first reactor, CP-1, was limited in power to only a few watts because it had no shield. In the words of Weinberg and Wigner, "The first full-scale reactors, Hanford, were designed with desk calculators and slide rules."[1] Yet the whole Project hung upon their success. Little wonder that tensions were running high!

The responsibility for design and operation of the Hanford reactors (and also the Clinton Pile) was assigned to the du Pont Chemical Company, but the inventors on the staff of the Metallurgical Laboratory in Chicago introduced the du Pont engineers into the task. In particular, J. A. Wheeler spent an extended period working directly with the du Ponters. Naturally much thought was given to anticipating troubles and difficulties that might arise, and among such difficulties was the possibility that some new nuclide might develop that would have a large slow-neutron capture cross section, thereby robbing the chain reaction of its propagating neutrons and bringing it to a stop. Since most of the new nuclides would be among the products of the nuclear fission, most of the physicists thought that fission-product poisoning could indeed be expected, but that its development would probably be a slow process, and that it might be handled by changing fuel elements.

In spite of this forethought, the appearance of the poisoning due to the radioactive fission product ^{135}Xe sent a shock though the scientific staff of the Plutonium Project. There were several reason for this, the first being that the poison developed in a period of only a few hours, thereby shattering the hope that it might be handled by changing the fuel elements. Would some awkward scheme have to be developed such as having the fuel slugs mounted on slowly moving conveyor belts, spending several hours in the pile, emerging when the ^{135}Xe had accumulated, passing outside of the pile for a day or two, and re-entering after the ^{135}Xe had decayed? But a deeper astonishment also followed from the quick appearance of the poison because the short half-life (9.3 h) of ^{135}Xe implied that the nuclei could not build up to a high numerical level, and for relatively few atoms to have such a large poisoning effect meant in turn that the neutron capture cross section had to be enormous. In fact, at 3×10^{6} barns, it was 10 times larger than the single largest cross section that had previously been seen (that of ^{157}Gd), 100 times larger than that of the familiar ^{113}Cd, a million times larger than the general run of capture cross sections encountered throughout the chart of isotopes, and nine-tenths as large as the largest cross section that is theoretically possible for a resonance at that energy and with a plausible spin.[2]

A third reason is perhaps more speculative and more human. The relationship between the Chicago inventors of the water-cooled concept and the du Pont engineers had been beset with tensions. Most the physicists thought that the Hanford reactors were overbuilt; that is, that the reactivity safety factor was excessive to the point of overexpense and delay in construction. Then they abruptly found that the du Pont people, under the guidance of Wheeler, were completely vindicated in their deliberate caution, and the emotions of the physicsts had to switch from impatience and mild criticism to thankfulness and relief. To use the then-current term, all suddenly found that they were joint "baby sitters" for the pile. But after all, who would have expected a three-million-barn cross section?

The fourth reason was that only the merest hint of fission-product poisoning had been seen in either of Hanford B's predecessors, the graphite pile at Oak Ridge, and the brand-new heavy-water reactor CP-3 at Argonne. That hint came from a test performed at Oak Ridge during a visit by Wheeler; the results were unclear, but were in a direction that put Wheeler on guard.

Thus we come to Henry Newson's remarks, starting with observations on his experience at Oak Ridge, then going on to his main story at Hanford.

OAK RIDGE, 1943–44: HINDSIGHT

The fact that the Hanford design was well on the way before the Oak Ridge pile was operating limited the value of the Oak Ridge pile as a pilot plant for Hanford. Furthermore, while the two piles did have geometric similarities, the heat was removed by pumping air through the pile at Oak Ridge, while water was to be used at Hanford. (In those days the energy, like the heat in a car radiator, was simply a nuisance that had to be removed to prevent vital parts from overheating or melting.) For these reasons the conventional wisdom considered that only a few experiments could be done at Oak Ridge that would be of any help at Hanford. In addition to Oak Ridge's principal objective of producing plutonium (in all about 110 g of plutonium oxide were produced) the two main physical experiments were a test of the Hanford shield and a water activation measurement.

As chief of the experimental physics section, I had asked everyone in it to suggest interesting experiments that could be performed at Oak Ridge without regard to their practical usefulness. A long list resulted, which was not particularly well received by the Hanford designers, the new Los Alamos Laboratory, or the operating group at Oak Ridge. The two former were interested in the experiments mentioned above, although the Los Alamos group asked also to be kept informed on the results of the more exotic (and for the time being, impossible) experiments.

However, the operating group introduced an interesting and fateful criterion. Production of plutonium was the prime purpose of the pile according to the operating point of view. Byproducts such as the experiments on my list were secondary but respectable. Furthermore, other experiments were tolerable *if they did not interfere with production*. Nevertheless, this criterion left room for a greatly varied experimental program. The radiochemists could insert any solid they wished into the pile and study the radioactivity produced by exposure to the neutrons. Others could insert identical samples to study spatial variations in neutron intensity. The biologists could insert small animals in specially designed cages for exposure to the neutrons that escaped from the surface of the pile. The physicists could effectively drill a 1- or 2-in. hole through the shield and into the center of the pile to release a neutron beam almost as well defined as a light beam, to study those neutron properties that were similar to those of ordinary light. But most importantly, all of these measurements could be carried out simultaneously if the pile operated at a reasonably steady power—a condition that the operating group was pleased to supply.

However, the conditions for maximum plutonium production and minimum chance of damage to the pile were not so simple. The aluminum cans that sealed off the uranium metal slugs from the air would melt if the power were too great and would spring leaks and allow the uranium to burn. Since the slugs had been tested before loading and the cans shown not to leak at about 200°C, the operators were told to vary the power so as to keep the temperature of the hottest slugs (those near the center of the graphite cube) at that temperature. Since the cooling air was much warmer in the daytime than at night the permissible power was appreciably greater than average at night, but this did not affect the experiments.

In spite of the advantages of this method of operation one thing was left out, i.e., relatively little could be learned about the operating characteristics of the pile itself if most of the experience gained were to be over a restricted power range. A much better understanding would result, for instance, from idling the pile at low power and observing the effects of changing the temperature or of measuring its response to the "accelerator" under different conditions. However, such measurements were not encouraged. I should add that I was the only one at the Laboratory who argued for a more careful study of those properties, and they would have been done much more thoroughly if I had had the courage of my convictions.

HANFORD, 27–28 SEPTEMBER 1944: MYSTERY, PUZZLEMENT, AND HOPE

We now move a year later from November of 1943, when the Oak Ridge Pile went critical, to the following autumn when a correspondingly rapid shakedown was underway at Hanford, where the first of the three plutonium production piles went critical in September. It differed from the Oak Ridge Pile mainly in designed power, which was increased from 1 to 300 MW. The daily production of plutonium went up proportionally, but the increased wattage created, at that time, scrap heat that had to be dissipated to the water of the Columbia River rather than by the much less efficient air-cooling system at Oak Ridge. However, the shakedown proceeded in much the same way: the effectiveness of the shielding, the safety rods (analogous to the brakes) and the sensitivity of the control rods (analogous to the accelerator of a car) were measured and the uranium loading necessary for ten times the wattage at Oak Ridge was estimated and installed. There were, of course, meticulous tests of such conventional devices as pumps, valves, gauges, and water filters that would have been carried out at a more conventional chemical plant. In a week or so everything was ready for operation at the planned 10 MW. The "acceleration" to this power proceeded without apparent hitch and the accelerator rods were adjusted to hold the power apparently constant. Actually a little had to be added to keep the power from sagging, just as more gas is required when one starts to climb a hill. The operating crew (headed by the late Kent Wyatt) were very familiar with Oak Ridge pile operation and were not surprised because this behavior had occurred there for nearly a day after start-up of the cold pile. They knew that the hundreds of tons of graphite in the pile, which heated slowly, had this "braking" action and required more "gas" for steady operation. Thus the first few hours of operation appeared normal and the "captains and the kings" departed, as they had at Oak Ridge the year before on the night before critical. Shortly afterward, things became "interesting" from the point of view of the pile's chief "baby sitter," the late Donald J. Hughes. He knew that at Oak Ridge the graphite heating effect apparently disappeared in about 27 hours and that at night the accelerator rods required less and less gas for steady operation because the cooling air dropped in temperature as the night wore on. Now the level of operation that he was baby sitting was ten times that at Oak Ridge and so the graphite effect on the accelerator rod should have apparently stopped in one tenth the time, i.e., a few hours. Hughes then carried out his duties and informed Wyatt that something was wrong. If the trouble had been an accelerating rather than a braking action, he could have ruled that the trouble might be disastrous and ordered Wyatt to shut down. However, his function was limited to viewing with alarm and the trouble was tending to safely *stop* the pile. Wyatt could have reduced power to try to slow the deterioration, but it would have done little good; although it would have been worth knowing that the braking action would have continued without much change. Besides, Wyatt clung to the false analogy with Oak Ridge and did not take Hughes's warnings very seriously. Thus the power was held to 10 MW until the accelerator rods sank as it were to the floor boards and everybody watched the power level peter out. By morning the pile was "as cold as the spray on the rock-beaten surf." If the trouble had been graphite heating, icy Columbia would have provided enough of the proverbial cold water in the face to have revived the pile. Hughes had only the satisfaction of saying "I told you so."

I don't know how long Hughes stayed after his shift was over at 8:00 a.m., but the baby sitters' car ordinarily started back to Richland at the end of a shift, and I presume he went back on it. At any rate he came to my house in Richland in mid afternoon; shortly afterward we were joined by John Miles, the old-line du Ponter who was essentially the boss of the baby sitters. He reported that the trouble at the pile was still mysterious, and requested that both Hughes and I double up on the next graveyard shift. This was Hughes's shift and he jumped to the conclusion that he was being superceded. He left the house abruptly, deeply disturbed. I was only a few years Hughes's senior, and our experience had been entirely different—mine at the Oak Ridge Pile and his at the Argonne piles (CP-2 and CP-3). Also, Hughes had more experience at pile controls (which were monopolized within the operating group at Oak Ridge) while I had much more experience in pile design and diagnostics. Thus Miles's request made good sense, but in spite of my arguments Hughes chose not to work the graveyard shift.

The forty-mile midnight drive from Richland to the B pile was gloomy but not solitary in spite of Hughes's absence. There were two or three junior baby sitters who accompanied me. I was worried for fear Hughes (with whom I had been very friendly) was headed for serious trouble, and also that the plutonium project (in which I had invested nearly four years) would turn out to be a half-billion-dollar fiasco.

The gloom got worse after our arrival. There had been no real progress during the day. The possibility that cooling water was leaking into the graphite (which would have acted as a pile poison) had been considered and the water flow had been reduced to a trickle by reducing the pressure, but to no avail. However, the always-reliable George Weil had installed a neutron counter in the center of the pile just before his shift ended and he turned over the baby sitting shift to me. The half dozen of us who remained stood about as at a wake, which it could well have turned out to be. I felt morally if not formally responsible for the morale of the junior baby sitters, but I was at a loss until it occurred to me that when there was no real work for the crew of a ship, busywork had to be invented. I spied the flashing lights of the counter that Weil had just installed, and asked the junior baby sitters to measure the time it took to register 1000 counts, and to plot the length of this time interval against the time of day (or rather night). All the stops were pulled out and we were all waiting for something to happen. I forgot all about the counter measurements and talked to Kent Wyatt who with the help of the logbook and verbal reports was the best informed about the previous shifts. I could find no clues in what he could tell me, and finally wandered back to the table were the results of the neutron counting were being plotted. I expected to find no difference in the recorded times, but—*mirabile dictu*, the measured time intervals had shortened a little more than could be expected from chance! Life was returning! We were now no longer bored mourners, but the wake had taken on a more Gaelic air, and the counter fascinated us. At this point I should have looked for data from the previous afternoon, but the permanent control room instrumentation was not likely to have been sensitive enough to show anything if I had.[3]

However, the measured time intervals kept decreasing, meaning that neutron intensity continued to increase until the interval became too small to measure. We had reached critical!

The rest was easy. We first shut down the pile as far as we could, pulled out all the stops and measured the doubling time again and again. Each measured time interval again became shorter than the last, meaning that we were going more and more over critical. It finally became impossible to measure doubling times, and the pile was brought to a steady power much below 1 MW and the position of the accelerating rods was recorded as time went on. It was now apparent that we were approaching the critical rod position that had obtained when the pile was operated at Oak Ridge power (1 MW).

As it happened, poison had gradually killed off the pile during its first day of operation at ten times Oak Ridge power, and the poison had disappeared during the following day.

I was told that a rumor got around that I had come in that night with a clear idea of what to do. I wish I could claim as much. I was just as bewildered as everyone else.

And so Newson's story ends with the conclusion of his shift. The day shift took over, and with them came Fermi, Wheeler, presumably Hughes, and the senior du Ponters. What happened has been best described by Babcock.[4,5] *They followed for most of the day the increase of the reactivity of the reactor as Newson had done, using the rate at which the control rods had to be pushed in to keep the reactor at the low power level of 0.2 MW. It was becoming increasingly clear that a fission-product poison was involved and it was probable that the half-life of the radioactive poison was in the range of several hours, although this would not be definite because the rate of decay of the poison could conceivably be controlled by that of a radioactive precursor. Then, during the evening, they performed an experiment that turned out to be beautifully revealing.*

The experiment was to boost the power level back to 9 MW, where it had been before the first growth of the poison, and to follow the onset of the poisoning effect by the rate at which the control rods had to be withdrawn in order to hold the power steadily at that level. If the poison was a short-lived descendent of a several-hour precursor, the precursor would follow a growth-to-saturation curve $(1 - e^{-\lambda t})$, *where* λ *is the decay constant associated with the several-hour half-life, and the reactor poison would follow that same curve. Similarly, if the poison itself had the several-hour half-life, its precursors being short-lived, approximately the same curve would be seen. But the observations showed a* faster *growth rate of the poison. This was the tip-off, because it could result only if both the poison and precursor had half-lives of several hours, for then the growth of the poison would not initially be at a constant rate because the population of its parent would itself be increasing with time. The only such combination in the table of fission products was the* ^{135}I *(6.6 hr)*$\rightarrow$$^{135}Xe$ *(9.2 hr) chain, and a combined group made the identification on Friday, 28 September.*[6] *Corroboration came when the power level was again dropped to 0.2 MW, and the turnaround of the reactivity (this time an* increase, *as the poison decayed) could be followed because, unlike the first poisoning, it was still within the range of the control rods. There were then ample data for a numerical fit.*

The news from Hanford quickly sped through the project and stimulated immediate activity at other laboratories. The first experiment was a quick confirmation of the poisoning and its identification at the CP-3 reactor at Argonne.[7] *The second was a similar check at the Oak Ridge reactor, which I shall discuss in a moment. Then there were experiments aimed at the evaluation of the* ^{135}Xe *neutron capture cross section. The radiochemists remeasured the fission yield of the mass 135 chain—a datum needed for the derivation of the cross section from the pile poisoning itself. Three direct cross section measurements followed, using the ingenious method of "negative activation" and performed, respectively, by Oak Ridge radiochemists Elliott, Knight, Novey, and Shapiro,*[8] *by Argonne radiochemists Friedman, Adams, Turkevich, and Sugarman,*[9] *and by Oak Ridge physicists Pardue, Moak, Levy, Wollan, and Meiners.*[10] *In these experiments fission-product* ^{135}I *was separated from irradiated uranium and allowed to decay partially into* ^{135}Xe, *after which the* ^{135}Xe *was extracted and divided into two parts. One part was kept as a control, and the other part was inserted into a reactor for some hours, where some of the* ^{135}Xe *would capture neutrons and turn into stable* ^{136}Xe, *making the irradiated sample* lose *radioactivity relative to the control. Culminating experiments came several years later* *when Oak Ridge teams made detailed measurements of the shape of the ^{135}Xe capture resonance, using neutron spectroscopy and up to 500 Ci of ^{135}Xe.*[11]

So after all of this activity (and more not mentioned) one might ask: Could the trouble have been more clearly anticipated? In particular, what about Newson's sense of frustration in that he was not given a chance to investigate the behavior of the Oak Ridge reactor—the first reactor to be operated at temperature and power? Is there irony in that the poisoning was missed at the Argonne Laboratory because, contrary to orders from General Groves, CP-3 (conceived as a research reactor) had not been operated full time at full power, whereas it was missed at Oak Ridge because in obedience *to General Groves's orders, the reactor there* had *been operated as much as possible at full power? Recognizing that speculation is idle in the practical sense because, as Newson remarks, the Hanford construction was so far advanced, nevertheless it is of some interest in "hindsight" to raise the question: What would Newson have seen if he had indeed been given generous time to study the performance of the Oak Ridge reactor under varying conditions?*

First, let us look at what actually happened at Oak Ridge. Immediately after the news arrived from Hanford, a skilled team under L. W. Nordheim sought the xenon effect.[12] *They obtained early indications of it, but it took them nearly a month to pin it down—and this was when they knew what they were looking for. The difficulty was that at the comparatively low neutron flux of the Oak Ridge reactor the poisoning amounted to only about half of the temperature effect, both going in the same direction with the power level, and both taking place with about the same time scale of hours, so the two were entangled. The team had to re-evaluate the reactor's temperature coefficient of reactivity to see the xenon effect clearly, and find by repeated numerical trials a proper combination of the temperature and the poisoning coefficients, but then indeed "a pronounced effect was seen."*

This was done when the reactor was operating at 3 MW. In most of the period available to Newson the reactor, with smaller fans, was operating at about the same temperature but at only 1 MW, so the xenon effect would have amounted to less than 20% of the thermal effects; furthermore, allowance would have to be made for the nonuniformity of both the temperature and the poison in the reactor. Under these circumstances Newson would have had real difficulty in separating the effects, and it is hard to believe that he could have identified the xenon as was so elegantly done at Hanford. Most probably he would have obtained confusing, nonreproducing results from his measurements, but that itself might well have been sufficient to make him suspect the presence of short-lived poisoning. He surely would then have communicated his suspicions to the people at Argonne, where Zinn and his colleagues were building the CP-3 heavy-water reactor. This reactor came into operation in August 1944, about six weeks before Hanford B. Its neutron flux was much higher than that at Oak Ridge, and because of its smallness the temperature equilibration would have been more in the range of minutes than of hours, so if the Argonne group had looked for it they would quickly have found and identified the xenon—as indeed they later did.

Thus if one follows that line of reasoning, Newson's sense of frustration would have to be accepted as well founded. But then he would have missed the excitement and the drama of that graveyard shift!

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[1]A. M. Weinberg and E. P. Wigner, *The Physical Theory of Neutron Chain Reactors* (University of Chicago, Chicago, 1958), p. 44.

[2]Reference 1, p. vi.

[3]Actually, Newson may not have been the first to see the start of the recovery, because Wilcox Overbeck (a member of George Weil's shift that preceded Newson's) gives his recollection of events in a private communication as follows: (Late on Wednesday evening) "... my telephone rang. Vic Hanson told me 'The reactor seems to have died. You had better go back out there and try to find out what is wrong.' "

"I got William Elmendorf to go with me. When we reached the control room, it was practically empty. Only the operator was there and his instruments all read zero. I got Elmendorf to go out to the reactor and restore our original sensitive instrument, a neutron counter tube that was placed in one of the empty fuel tubes. We had a telephone system so I could tell him to adjust the position of the counter tube until we reached a good counting rate. When he came back to the control room we again checked the counting rate and found that it had increased. I asked Elmendorf if he had changed the position of the counter tube. He said 'No', so I realized that the reactor was coming back to life."

[4]Dale F. Babcock, Nucl. News 7, 38 (Sept. 1964).

[5]Unfortunately, the account of the day's events as given in the official history [R. G. Hewlett and O. E. Anderson, *The New World* (Pennsylvania State University, University Park, PA, 1962), p. 305] is marred by a confusion of the reactor's reactivity with its power level.

[6]According to Babcock (private communication) the identification was the work of several people. Overnight Wheeler had selected three fission-product parent–daughter pairs as possible carriers of the poison, and these were given to different pairs of people for numerical fits. One fission-product pair was quickly discarded, so Wheeler and Wende joined Gast and Babcock in looking at the ^{135}I–^{135}Xe chain. It gave agreement with the pile data, so they rushed to Fermi's office, where he and Leona Marshall had found that their fission-product pair was not fitting the data. Thus all joined in agreement on ^{135}Xe as the poison, and Fermi and Marshall went on to make more detailed calculations.

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[9]M. S. Friedman, R. M. Adams, A. Turkevitch, and N. Sugarman, Metallurgical Laboratory Report CP-2782, 16 April 1945 (unpublished).

[10]L. A. Pardue, C. D. Moak, P. W. Levy, E. O. Wollan, and E. P. Meiners, Metallurgical Laboratory Report CP-2600, 28 March 1945 (unpublished).

[11]S. Bernstein, M. M. Shapiro, C. P. Stanford, T. E. Stephenson, J. B. Dial, S. Freed, G. W. Parker, A. R. Brosi, G. M. Hebert, and T. W. DeWitt, Phys. Rev. **102**, 823 (1956); E. C. Smith, G. S. Pawlicki, P. E. F. Thurlow, G. W. Parker, W. J. Martin, G. E. Creek, P. M. Lantz, and S. Bernstein, Phys. Rev. **115**, 1693 (1959).

[12]L. B. Borst, H. Jones, L. W. Nordheim, L. Slotin, and H. Soodak, Metal-

lurgical Laboratory Report CP-2192, 9 November 1944 (unpublished). Later an extended series of measurements was made by L. B. Borst and A. J. Uhlrich with the reactor at 4 MW; cf. Oak Ridge Nat. Lab. report Mon P-60, may 1946 (unpublished).

[13]Leona Marshall Libby, *The Uranium People* (Crane-Russak, New York, 1979), pp. 180–183.

[14]A. H. Compton, *Atomic Quest* (Oxford University, New York, 1956), p. 192.

Fundamental resonant frequency of a loudspeaker

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The fundamental resonant frequency of a loudspeaker marks its useful low-frequency limit. Its values may be determined by plotting the electrical impedance of the loudspeaker against frequency. This was done for a loudspeaker under various loading conditons. The graphs obtained yield useful information about the loudspeaker's mass, compliance, and other parameters.

Loudspeaker theory is usually one of the more interesting topics in an undergraduate acoustics course, because of the students' interest in high fidelity and sound reproduction. Many students will themselves have had practical experience in designing and constructing their own loudspeaker enclosures, and the use of loudspeaker theory to highlight general acoustics topics such as acoustical impedance greatly enhances the attractiveness of these topics for the students.

Laboratory experiments on loudspeakers in conjunction with such topics would be of interest for the same reasons. However, the usual means of measuring loudspeaker parameters such as frequency response has required the use of expensive and often unobtainable anechoic chambers. Impulse techniques utilizing fast Fourier transforms have made loudspeaker testing without anechoic chambers possible. On the other hand, these newer techniques also require expensive instrumentation as well as the application of mathematical techniques that may obscure the acoustical principles for the student.

There is one useful loudspeaker parameter that can be easily and inexpensively measured by undergraduate students. The fundamental resonant frequency f_r of a loudspeaker can be determined using simple instrumentation, yet it can give important information on loudspeaker behavior. In fact, f_r marks the effective low-frequency performance limit of a loudspeaker system, and hence is often quoted in manufacturer's loudspeaker specifications.

IMPEDANCE OF A DYNAMIC LOUDSPEAKER

A dynamic or direct-radiator loudspeaker consists of a stiff and light cone suspended from a rigid metal frame. The cone is driven by a voice coil of length l attached to the end of the cone. The current i passing through the coil is perpendicular to the radial magnetic field B of a permanent magnet. The resultant driving force F on the cone is equal to Bli.

The cone and voice coil have a combined mass M and the cone suspension has a compliance C that together form a mechanical system. The system can be represented by an analogous electrical circuit in which mechanical quantities are represented by electrical quantities; force by emf, velocity by current, mass by inductance, compliance by capacitance, and mechanical resistance by electrical resistance. The resultant velocity v due to a force F is given by

$$v = F/Z_M,$$

where Z_M is defined as the mechanical impedance of the system. Z_M may be calculated from M and C from the analog circuit (Fig. 1).

FUNDAMENTAL RESONANT FREQUENCY

If the driving force F is oscillating, the magnitude of Z_M is at a minimum when the frequency f is equal to the fundamental resonant frequency f_r of the loudspeaker where

$$f_r = 1/2\pi(MC)^{1/2}.$$

The resultant velocity is at a maximum when this condition holds.

The above assumes that the loudspeaker is oscillating in

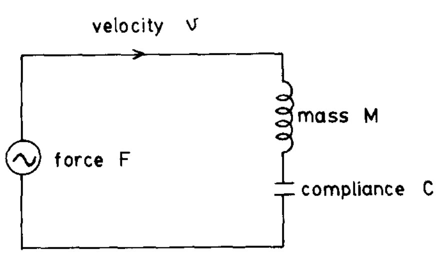

Fig. 1. Electrical analog circuit of a loudspeaker in vacuum.

