ematics, seems to be but one branch. But it is a vigorous and growing branch, and there is reason to hope that it may in time provide an element of unity to oppose the fragmentation which seems to beset contemporary mathematics—and indeed every branch of scholarship.

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Some of the early work of Boole, Frege, and Cantor has been made more available by relatively recent translations into English. Examples are the works cited. The *Principia Mathematica* of Russell and Whitehead, while largely devoted to a formidable formalism, contains many very readable sections of an introductory or summary character. An account of the fundamental theorems of Gödel and Church may be found in Kleene. In describing logical research since 1936 my aim has been not to give a complete history but only to indicate the range of activity by mentioning some of the most actively cultivated areas. Accordingly, I have referred to only a very small sample of the literature, selecting (where possible) works which are largely self-contained. With reference to the following list, for an example of an axiomatic theory of sets and an important contribution toward its metatheory, see Gödel. For an account of the decision problem, see Tarski (1951) for positive solutions and Tarski *et al.* (1953) for negative. The theory of recursive functions and their uses is well described in Kleene. Some historical remarks on algebraic logic, as well as detailed results, may be found in Henkin and Tarski. A very recent and important contribution to the theory of models is that of Keisler, where reference to earlier works may be found. For an account of recent work on infinitely long formulas, see Henkin; an application of such work to a problem on the existence of certain measures appears in Tarski (in press). Applications of logic to algebra are described in Robinson.

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Arthur Holly Compton, Research Physicist

I first met Arthur Compton in 1924, in William Duane's x-ray research laboratory at Harvard University. Compton had come on a visit to attempt to discover why Duane and his associates could not confirm his discovery of the change of wavelength of x-rays on scattering, now known as the Compton effect. I do not know what Compton had been doing just before he arrived, but his appearance late that afternoon was completely nontypical. He was disheveled, unshaven, and obviously overtired. He returned to the laboratory the following morning looking like himself—a well-groomed, energetic, and clear-thinking physicist.

The situation was rather tense, with peculiar overtones. Compton was not the first to perform experiments which indicated that scattered x-rays and gamma rays were more absorbable—that is, of longer wavelength—than their primaries. As far back as 1912 Sadler and Mesham had observed such an effect in x-rays scattered from carbon, and Compton himself, in 1921, had followed others in experiments showing the softening of gamma rays on scattering. But, as has several times happened in physics, the experiment, the interpretation, and the audience were not simultaneously ready, and these prior results attracted little attention. Compton, however, had never completely laid aside those gamma-ray experiments he performed in Rutherford's laboratory, and he turned them over and over in his mind, finally reaching an interpretation based on the transfer of momentum from light quanta to free electrons. Again the "interpretation" was not new; the idea of photons or light quanta had long been in the minds of many physicists. Some had even worked out Compton's equations for the conservation of energy and momentum in the photon-electron collision, ending with the wry remark that this would be a beautifully simple theory of scattering but was of course untenable because everyone knew that scattered light and x-rays were unchanged in wavelength and coherent with the primary radiation. Compton solved the equations independently, however, and was the first to publish the results.

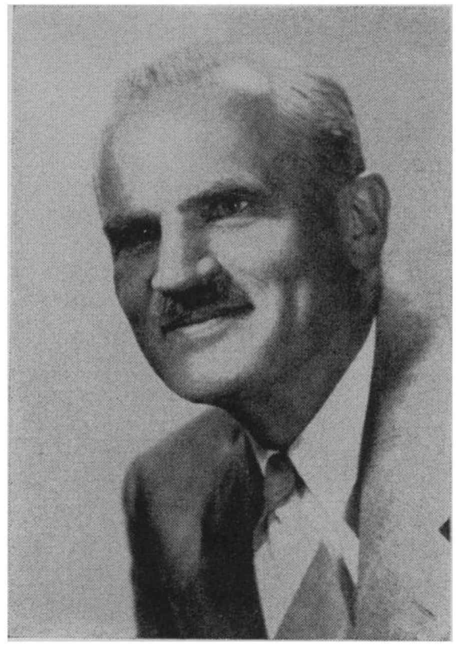
It took Compton to correlate theory and experiment and finally to clinch the matter with a demonstration in which the change in wavelength was precisely measured with a crystal spectrometer and shown to be h/mc or 0.024 angstrom units at 90 degrees, as the calculation had predicted. The audience was ready, because the apparent conflict between the corpuscular and the wave theories of light was in every physicist's mind. A Nobel-prize discovery had been made.

But here at Harvard in 1924, in the laboratory of a highly respected investigator of x-rays, the crystal spectrometer measurements seemed to give different results. The scattered radiation showed, as Compton had found, part of the radiation to be shifted to longer wavelengths, but Duane interpreted this as "tertiary radiation," of the bremsstrahlung type, caused by the deceleration of photoelectrons ejected from the scatterer by the primary radiation. Actually, the shift at 90 degrees, from carbon, of the K x-rays of molybdenum could be quantitatively accounted for by the energy loss in the ejection of carbon K-electrons. The crucial tests of the angular dependence of the shift, and of its independence of the atomic number of the scatterer, had not been decisively performed at Harvard.

A peculiar overtone to the situation was Duane's great resistance to accepting a photon theory of scattering. It was Duane and Hunt who, a few years previously, had quantitatively established the relation between the electron kinetic energy and the maximum frequency of the bremsstrahlung, which, in those pre-wavemechanical days, was considered one of the strongest evidences of a photon theory of light. And Duane himself was at the time working on a thought-provoking attempt to explain the crystalline diffraction of x-rays without recourse to wave theory, using photons only. The essential feature was the quantized transfer of momentum from photon to crystal lattice, in the amount h/d, d being the crystal lattice spacing (*1*). Nevertheless, Duane had resisted Compton's idea from its first pronouncement and had written Sommerfeld, who was here in the United States at that time, of his doubts and his alternative explanation. Sommerfeld's reply, which Duane duly reported to us, was that after a visit to Compton in his new laboratory at Chicago he remained convinced of the fundamental importance of Compton's discovery.

At the time of Compton's visit I was not working on the scattering problem but was working on some problems Duane had suggested, involving fluorescence radiation. All the excitement, however, was in the next room, and I often wandered in to hear the latest scattering news. Compton's visit did not resolve the difficulty, but his incisive questions and earnestness greatly impressed Duane and his scattering group. The Harvard experiments were continued, with more self-criticism, and Duane, who had been spending most of his time directing the Roentgenology Laboratory at Harvard Medical School, neglected those duties to take readings himself on scattered x-rays. Soon some spurious effects were discarded and the results showed clearly that Compton was correct in all respects. Duane quickly withdrew his objections, at a memorable meeting of the American Physical Society. There were some who injected notes of recrimination and personalities into the situation, but the principals involved, Compton and Duane, conducted themselves at the highest levels of scientific controversy.

At this time Compton had just moved from Washington University to a professorship of physics at the University of Chicago and was chairman of the National Research Council's committee on x-rays and radioactivity. He had used the *Bulletin* of the Council for his first announcement of a spectroscopic measurement of the shift in wavelength, and in the same year (1922) the *Bulletin* carried his announcement of the discovery of the total reflection of x-rays. This work alone, if it had not been overshadowed by the work of scattering, would have established him in the first rank of experimental physicists. The earlier work of Stenström in Sweden had indicated that the index of refraction for x-rays was less than unity; Compton realized that this meant there was total reflection from denser to lighter media if the interfacial glancing angle were small enough. He quickly selected monochromatic radiation with his crystal spectrometer and demonstrated that the beam was totally reflected from glass and silver mirrors, and that the effect disappeared if the glancing angle was more than a few minutes of arc. In the hands of subsequent experimenters this became an important method for measuring the refractive index.



Arthur H. Compton

In an outline autobiography written in 1935 Compton has listed what he considers to have been his principal contributions to physics up to that time. The total reflection of x-rays is not on the list, but in addition to the Compton effect he mentions the study of the distribution of electrons in atoms by diffraction methods. In writing his first book on x-rays (*2*), Compton had worked out a method of calculating, from the intensities of diffraction in various orders, the linear density of diffracting material in a direction perpendicular to the set of crystal planes under investigation. The method was applied by Compton's associates and produced elegant electron distribution curves easily identified, in the case of rock salt, as the electron clusters around sodium and chlorine nuclei. Similar and independent investigations were under way in England, in W. L. Bragg's laboratory. Later, in an even more impressive manner, Compton extended the method to the study of scattering from gases, where the sharp crystalline diffraction maxima do not exist and the coherent and incoherent components of the scattering can only be separated by an experimenter with a basic understanding of the complexities of the scattering process. Compton and his associates measured electron distributions in helium, neon, argon, and mercury atoms by this method, which is now being revived in the study of electric charge distribution in nuclei, with ultra-high-energy x-rays.

In his modest résumé of his contributions to physics Compton fails to mention a fundamental contribution to the theory of ferromagnetism. By a very careful investigation of the intensity of x-rays diffracted by magnetized and unmagnetized magnetite and silicon steel, Compton and his associate, the late J. C. Stearns, showed conclusively that the magnetization of these substances could not be explained by a tilting of the planes of electronic orbits in their atoms. As he correctly surmised, this left orientation of the electron spins as the ultimate source of the ferromagnetic behavior.

In the early 1930's Compton began to shift his attention from x-rays to cosmic rays. He began building high-pressure ionization chambers and encouraged his associates to construct simple and rugged electrometers, such as had been developed by Millikan and his co-workers at Pasadena. During this period his fame as a physicist and lecturer was spreading rapidly, and he was eagerly sought as a guest professor by universities throughout the world. He enjoyed traveling (always accompanied by his wife), and he was now able to carry out experimental work on cosmic rays by taking readings on their intensity as he journeyed. Fortunately, his travels often took him to the Southern Hemisphere, and by intelligent evaluation of his intensity readings he discovered a latitude effect—namely, that cosmic ray intensities are less near the equator than at the poles. Again, such an effect had previously been discovered, by a Dutch physicist, Clay, who had measured the intensity as a function of latitude on his travels from Holland to Java, but the effect remained buried in Clay's notebooks and in obscure publications until Compton rediscovered it and at once saw its implication.

The other great name in cosmic rays at that time was R. A. Millikan, whose extensive observations had convinced him that the primary cosmic radiation, incident on the earth's outer atmosphere, was electromagnetic in nature. Compton realized that the probable explanation of his latitude effect was that a significant part, at least, of the primaries was made up of charged particles, kept away from the earth's equatorial regions by the shielding effect of the earth's magnetic field. Millikan was not the type of physicist who gracefully accepts correction of his results (witness the controversy over the quantitative value of the electronic charge, when the value derived in Millikan's beautiful oil-drop experiment was corrected upward by about 0.6 percent in later x-ray work, again initiated in Compton's laboratory), and a lively discussion over the nature of cosmic ray primaries arose. But the evidence of the latitude effect could not be refuted, and the explanation that charged particles play a predominant role in the influx from outer space is now accepted.

In the latter part of the 1930's Compton spent less and less time working with his own hands in the laboratory. It seemed difficult for him to refuse any of the multitudinous requests he received for lecturing, on both scientific and humanitarian topics. There was an intensely religious and idealistic side to his nature, coexisting in a truly remarkable way with his ability to reason in the rigorous and objective manner of physics. His early religious training, received from his mother and father and reinforced by association with his missionary relatives, had made a permanent impression. He was one of the few scientists of stature who could and would address religious groups, and Compton was in constant demand as an outstanding exponent of the compatibility of science and religion.

In the early 1940's he realized, and often mentioned to me, that he was turning over his experimental work to "younger men who could do it better" (!), and I remember a day in 1942 when he solemnly informed me that he was 50 years old. He obviously had been taking stock of his life and was on the point of making decisions on what to accomplish in his remaining years. But before he could retire from an active interest in physics, a great ordeal was in store for him.

When World War II began in Europe, and even after the fall of France, Compton seemed less affected by it than were many other physicists in the United States, who dropped their research projects and reported to Washington, or congregated at the radar laboratory at the Massachusetts Institute of Technology. Actually Compton, as a physicist, was less a product of continental European universities than were many physicists of his generation in this country. He did not join the pilgrimages of the 1920's to Göttingen or Copenhagen, merely stopping off incidentally on a European tour. He seemed less aware than others of the frightful danger to civilization represented by the psychopathic Hitler and his congerie of fanatics. He had led a comfortable, protected life and had, from youth, been shielded from evil incarnate by his optimistic religious faith, which taught good will toward all men and the expectation of good will in return. His cosmic ray research group lost some men to the defense effort but continued under his leadership until the winter of 1941, when we were at war and the extreme gravity of the situation became evident to everyone. He then turned over the project entirely to Marcel Schein, who continued it with a reduced staff.

In October 1940 I had been called away from Chicago to help with rocket research in Washington; Compton had begun to think about what the University of Chicago group could do to increase the military potential of the country and was becoming more and more interested in the uranium fission program. In December 1940 he wrote me in Washington and urged me to return to Chicago and begin a study of the possibilities of using beryllium as a neutron moderator, a project in which he gave me great support.

The story of Compton's involvement in the atomic bomb effort has been written by himself, in his book *Atomic Quest*. I can only record some glancing observations. On 6 November 1941 Compton, as chairman, presented the report of a National Academy Committee organized to review the military potentialities of atomic energy. This report was a masterpiece of scientific and technological prevision; it, as much as any other one item, precipitated the vast uranium project effort. It was Compton at his best, with his full attention and enthusiasm concentrated on one subject. I had seen this happen once before, when he essentially locked the doors of his office in Chicago and, in about 3 weeks, wrote the long and profound chapter on the scattering of x-rays for the book *X-rays in Theory and Experiment.*

As the effort which eventually became the Manhattan Project developed at an incredible rate, the strain on Compton became terrific. He was buffeted unmercifully by the internal frictions in the project, by the difficulties in splitting off men for the work in Tennessee, by distrust between the pure scientists and the commercial engineers, and by the great decisions regarding the role of heavy water and the relative efficacy of liquid cooling and gaseous cooling, to name but a few. In addition, he felt a gnawing doubt about the morality of the whole effort, which, if successful, could very well mean a horrible death for thousands of civilians in the enemy countries.

He presided at long conferences which seemed never to end and were called at all hours of the day and night, with no regard for meal times. He had the physique to endure this; I did not. After my session in the hospital he somehow learned that it was thought that part of my difficulty had been caused by long periods without food. Thereafter he always had tomato juice and sandwiches available for our longer sessions; this made me feel that too great regard was being paid to one man's digestive tract in the midst of a war.

The end of the war brought Compton's resignation as Charles H. Swift distinguished service professor of physics at Chicago and his acceptance of the chancellorship of Washington University at St. Louis, where his famous experiment on the Compton effect had been performed 24 years previously. His administrative position meant that his career as a research physicist was ended, as was my close association with him. The last time I saw him before his death on 15 March 1962 was in the autumn of 1961, at the dedication of the A. A. Michelson public school in Chicago. He gave the dedicatory address, recalling the achievements of his famous predecessor at Ryerson Physical Laboratory. I had known of his serious illness in the preceding years, from which he never fully recovered. At the little reception following the ceremony, however, he talked clearly and naturally as we reminisced about days gone by.

The honors heaped upon him are literally too numerous to mention. Probably the one he appreciated most was the degree of doctor of science awarded him in 1927 by Wooster College, in the town where he had been born and had lived as a child and young man, and where his father, Elias Compton, had been dean and professor of philosophy.

His place is secure as one of the great American physicists of the 20th century.

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Reference and Note

1. It is interesting to note the completely independent rebirth of this idea in the semiclassical interpretation of the angular distributions resulting from the Butler stripping process in nuclear reactions; the necessary length is supplied by the diameter of the target nucleus on whose surface the reaction is supposed to take place.

2. A. H. Compton, *X-rays and Electrons* (Van Nostrand, New York, 1926).

News and Comment

Who Runs America? An examination of a Theory that Says the Answer Is a "Military-Industrial Complex"

The farther reaches of the political spectrum have been fertile ground for theories on how we got into the Cold War and how we can get out intact.

Grouped under headings of left and right, the theories conflict in substance, but they do have in common the assumption that we have come to our present plight because—whatever the appearances may be—the decision-making process is controlled by unseen people who have usurped our constitutional processes. Thus, on the one hand, we have the theory that the Soviet Union runs the placement service for the American foreign policy apparatus and, on the other, we are offered the hypothesis that this same apparatus is dominated by moneyed people who learned "I hate Russia" before they learned "Momma."

Those who feel at home on the middle ground of the spectrum have not been laggard in producing or accepting theories of the Cold War's origins, but they generally have failed to make use of the "invisible forces" concept. This, however, has changed of late, and the principal credit belongs to no one more radical than former President Eisenhower, who, with a few cryptic words, transformed an otherwise unnoteworthy farewell address into the most quoted of that genre since George Washington advised his countrymen not to get mixed up with foreigners.

Eisenhower began by pointing out that a standing military establishment was unknown in the United States until after World War II, and he warned that its influence—"economic, political, even spiritual—is felt in every city, every State House, every office of the Federal government."

While the state of the world makes this establishment necessary, he said, "in the councils of government, we must guard against the acquisition of unwarranted influence, whether sought or unsought, by the military-industrial complex. The potential for the disastrous rise of misplaced power exists and will persist."

Eisenhower was not the first to offer this view, but he was an unlikely source of such pronouncements, and a common reaction was that if Eisenhower says this is a serious problem it must indeed be a very serious problem. Various writers immediately dug into the subject, producing a great deal of material which clearly demonstrated that military men and the people from whom they buy their equipment had become quite intimate during 15 years of Cold War and had not been confining their energies to the production of hardware.

At present, the most prominent and angry product of this research is *The Warfare State*, by Fred J. Cook [Macmillan, $4.95 (376 pages)], an expansion of Cook's work, *Juggernaut: The Warfare State*, which filled a special 60-page supplement of the *Nation* for 28 October 1961. It is Cook's thesis, says Bertrand Russell in a foreword, "that the 'military industrial complex' has become so powerful in the United States that it dominates the Government and is, at the same time, so insane that it is quite ready to advocate what is called a 'pre-emptive' attack against the Soviet State." Russell can be accused of stretching Cook's thesis to fit his own well-advertised conclusions, but it is only a slight stretch, since Cook himself hedges only occasionally in attributing overwhelming power to the "military-industrial complex." He does conclude with the hope that perhaps the tide is turning, but the hedgings and hope are insignificant in relation to the whole work, which abounds with statements such as, "There is hardly an area in our lives today in which the military influence is anything less than supreme," ". . . the entire economy and self-interest of the nation have been chained to the chariots of war," and "The picture that emerges is the picture of a nation whose entire economic welfare is tied to warfare."

This is the sort of stuff that might easily be expected to arouse skepticism, but the reviews—outside of military, quasi-military, and right-wing journals—have generally ranged from courteous to enthusiastic. The New York *Times* said, for example, that Cook was "perhaps a bit too shrill" and had failed to prove that the military-industrial process exercised any illicit power in government, but the reviewer, who covers the Defense Department for the *Times*, was by no means harsh. In the *Saturday Review*, former Congressman Charles O. Porter warmly embraced Cook's thesis, describing it as "timely and fully documented." He added that "it indicts a number of our leading citizens, principally military and industrial leaders, on charges of selfishly and recklessly changing our nation from a peace-loving democracy into a state bent on a holy war to extend the capitalist system."

Cook has no difficulty demonstrating