Book Reviews

How Did It Happen That It Didn't Happen?

The German Atomic Bomb. The History of Nuclear Research in Nazi Germany. DAVID IRVING. Simon and Schuster, New York, 1968. 329 pp., illus. $6.95.

This fascinating book was published in London in 1967 (by Kimber) as *The Virus House: Germany's Atomic Research and Allied Counter-Measures.* The incredible story it tells is fascinating in the extreme, especially to all who worked for the Manhattan District.

The beginning, of course, was Hahn and Strassman's great discovery of nuclear fission in December of 1938. Rapidly thereafter the scientific world came to realize the fateful meaning of the splitting of the atom. This is the story of what happened about it in Germany. It is well written and arranged with good pictures.

First, of course, the Germans had an enormous head start. Second, and less obvious, was that they did very well indeed with the means at hand, and it takes a lot of analyzing to discover why they didn't get the bomb well ahead of us. They knew about all the characteristics of the fission reaction; there was nothing we knew about it that they didn't. They knew about plutonium and, in fact, decided that it was easier to make a bomb that way than with the isotope separation process to get U^{235}. (We did both; the Hiroshima bomb was uranium, the Nagasaki bomb plutonium.) However, the decision to emphasize reactors and plutonium was a very difficult one, and they never did cease working at separating the uranium isotopes; their best progress was made with the gaseous centrifuge, a device we barely looked at before moving to the mass spectrometer and finally the gaseous-diffusion plant. Apparently they never seriously considered the gaseous-diffusion plant. They must have thought of it, however, for the German Gustav Hertz (who, being Jewish, wasn't allowed on the project) had invented it years before.

Another strange matter was their avoidance of the graphite-moderated reactor (our Hanford, Washington, installation). Thus they voted against both Hanford and Oak Ridge and went directly and essentially totally to the Savannah River route (plutonium-producing reactors with heavy water as moderator).

The reasons against the diffusion plant are very obscure, and the author merely notes in amazement that apparently they never thought of it. Well, it seems clear that they must have. It may be that in excluding Hertz they had made the decision, for everyone else on the isotope-separation side was more or less committed to his own scheme—Clusius to thermal diffusion, Groth to the gas centrifuge, and so forth.

The second decision, not to try to use graphite as a moderator, was based on a very clear fact—Bothe of Heidelberg had measured the absorption cross section of graphite incorrectly! An earlier, correct measurement of this very important quantity had been set aside when this famous experimentalist Bothe turned his hand to the job. Once his result was known, nearly the entire effort was put in the heavy-water project, and Harteck (now at Rensselaer Polytechnic Institute) and Suess (now at the University of California at San Diego), who were experts in this matter here, became very important, with many trips to Norway and much difficulty with the British commando and American bombers, who finally stopped heavy-water production completely and thus stopped the whole project, since all else that was left was the gas centrifuge, which wasn't completed.

In the Germans' eyes (blinded by the Bothe measurement), a graphite-moderated reactor could only operate with uranium enriched in the fissionable isotope U^{235}. Therefore they saw uranium isotope enrichment only as a substitute for the heavy-water effort, never really realizing it could do the whole job itself. Yes, they paid dearly for excluding Hertz.

If any one of a dozen events had happened a little differently, there might have been a German A-bomb. What a difference that would have made! A secretary put the wrong agenda in an invitation to the top Nazi and military brass; Bothe mismeasured; Hitler never really heard Heisenberg's description of what an A-bomb would do; and so on. It gives one an eerie feeling.

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Thermodynamics

Entropy and Low Temperature Physics. J. S. DUGDALE. Hillary House, New York, 1967. 206 pp., illus. $6.

Entropy is a concept which was first introduced by Clausius in 1854 and has since been fruitfully applied to such general physical problems as those of crystallization, magnetization, mixing, radiation, and chemical reactions. Entropy can serve as a measure of disorder, of reversibility, of temperature, and so on. As a subject of such wide interest and application, entropy, like the Exodus, has a story that needs to be frequently retold.

Dugdale's interesting book is an exposition and interpretation of entropy in its natural habitat, problems of heat and temperature change. The book is intended for students, and the writing is so clear and well organized that interested laymen and active researchers can profit from it as well.

The first part of the book is a discussion of entropy in thermodynamics. It starts with a historical introduction rich in quotations, including Count Rumford's observation that the mechanical production of heat might be useful "in a case of necessity . . . in cooking victuals." Dugdale then develops the ideas of temperature, thermodynamic variables, and equations of state and goes on to give careful descriptions of the first and second laws. The approach is fairly rigorous, yet tutorial, so that each abstract concept comes paired with some physical example. In this manner the experimental basis of thermodynamics is illustrated by the measurement of specific heat and the second law is illustrated by a thorough, historical description of the Carnot cycle. Entropy itself is first illustrated by considering reversible processes in an ideal gas, and frequent use is made of *S-T* diagrams so that the reader comes off with a good feel for them. The second law and the internal energy $U(S,V)$ are then applied to discussions of enthalpy, free energies, the Clausius-Clapeyron equation, and the Joule-Thomson effect.

The second part of the book is on the statistical interpretation of entropy, that is, as $S = k \ln W$. This section starts with discussion of particles in discrete energy states, microscopic distributions, and the Boltzmann distribution. As examples, the temperature dependences of occupation numbers, energy, and specific heat of a simple system are carefully developed. One of the strong points of the book is that many worked illustrative examples of this kind are provided throughout, and Dugdale also constantly points up the connections among the statistical mechanical and thermodynamic quantities. This development is applied to solids, including a calculation of the specific heat of an Einstein solid, to classical gases, including a derivation of the Sackur-Tetrode equation and the equation of state, and finally, to properties of Bose-Einstein and Fermi-Dirac gases.

The third part of the book is a unique discussion of the third law and of entropy at low temperatures. It includes much lore which the author acquired as a student of Sir Francis Simon. There are instructive discussions of equilibrium thermodynamic properties and the role of entropy as absolute zero is approached. As examples, He^4 and the lambda point are discussed in terms of the entropy of a Bose gas, and the unusual melting curve of He^3 is explained in terms of the nuclear spin entropy. The roles of entropy and *S-T* diagrams in production of low temperatures are discussed in connection with cooling by evaporation, the cascade processes for cooling, the Linde process, the Simon expansion liquefier, and adiabatic demagnetization.

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Carcinogenic Agents

Carcinogenesis: A Broad Critique. Papers presented at the 20th Annual Symposium on Fundamental Cancer Research, Houston, Texas, 1966. Published for the University of Texas M. D. Anderson Hospital and Tumor Institute by Williams and Wilkins, Baltimore, 1967. xvi + 774 pp., illus. $16.

The organizers of the symposium which is reported in this volume successfully steered the middle course between attempting to cover the whole of the field of carcinogenesis superficially and applying such intense specialization as to limit the general appeal of the book. The result is a well-balanced and thought-provoking survey of some of the more important aspects of the subject. The emphasis is on the *agents* which bring about cancer in man or in experimental animals and how these agents interact one with another, or with the host, in the induction of tumors. Discussion of the epidemiology of human cancer and of the detailed biochemical mechanisms which are involved in carcinogenesis at the molecular level is limited.

One of the most important problems in the study of carcinogenesis is the relevance of experiments in animals to the human disease. The difficulties are clearly brought out by the articles on viruses, which occupy about half of the book. The ability to induce tumors by the inoculation of cell-free extracts of tissues or of the viral particles themselves into animals of defined genetic constitution has led to rapid progress in the experimental situation. In the human these direct techniques are not available, and despite the sophistication of modern immunological and electron microscopical techniques progress has been slow. Rabotti and, to a lesser extent, Huebner attempt to define a set of principles akin to Koch's postulates, which may be applicable to viral oncogenesis in man and animals; nevertheless, the most that Shubik in the concluding paper of the conference is prepared to admit is that "there is little evidence that any cancer in man is of viral origin. That this will eventually be contradicted, again I do not doubt."

The understanding of the development of mammary cancer in the mouse has not advanced conceptually to any significant extent in the 30 years since Bittner suggested that three factors—an inducing agent, a hormonal factor, and genetic constitution—were necessary. Nevertheless, considerable progress has been made in the detailed analysis of the system. Nandi presents evidence for two forms of the Bittner (mammary tumor) virus. The first is present in the milk and mammary tissues and has a relatively low degree of strain specificity, whereas the second, strain-specific, form is carried on the red blood cells and is not visible as a B particle under the electron microscope. There is another, qualitatively different, virus, which is able to induce mammary gland nodules. These nodules do not of themselves normally progress to cancer. Liebelt and Liebelt report a massive experiment in which various combinations of inducing agents, hormones, and different strains of mouse are compared. Their evidence supports previous work and strongly suggests that the chemical inducing agent, 3-methylcholanthrene, does not activate latent mammary tumor viruses in low-cancer strains of mice. In all of the excellent work on murine mammary tumors it is disappointing to discover no hint of how the information may be related to the disease in women.

Some chemical carcinogens appear to interact directly with the tissues, while others require metabolic activation. It is a pleasure to read the succinct account of the 20 or more years' work by the Millers on the mode of action of the aromatic amines and azo compounds. Their clear elucidation of some of the problems which remain is most valuable. The study of the nitrosamines by Magee and his co-workers has to a considerable extent complemented that on the aromatic amines because it has been concerned not so much with metabolic activation as with the combination of the carcinogens with the biologically important macromolecules. It is now necessary to design experiments to show the biochemical and biological significance of these carcinogen-macromolecule interactions.

The discovery of the aflatoxins has focused attention on the possible carcinogenic activity of other mold products. The relationships between chemical structure and carcinogenic activity which are emerging will be of considerable value in attempting to protect the human population against potential carcinogens, but the amount of reliance that can be placed on experiments which result in one or two local sarcomas following the injection of the test substance into six or fewer rats is open to question. The discovery during the past decade that the mold products as well as the nitrosamines are carcinogenic suggests that other equally potent