

the same nuclei, but with different half-lives, were observed, one was compelled to assume that isomeric states were propagated along the chain. It seemed unavoidable to regard this as anything but the formation of the *trans*-uranium elements, because no nuclear process could be imagined by which much lighter nuclei could be produced on interaction of U with neutrons. Hahn and Strassmann, however, made the following observation. If a mixture of barium and mesothorium bromide (radium isotope) were fractionated by crystallization, the mesothorium was found to accumulate in the end fraction, while a similar test made with barium and Eka-barium mixture left the latter evenly distributed among the various fractions (a similar result was obtained by I. Curie and P. Savitch in the case of Eka-lanthanum). Hahn and Strassmann pursued this method with all possible means, taking the greatest care to eliminate any possible errors. In 1939 they published a paper entitled "Nachweis der Entstehung aktiver Bariumisotope aus Uran und Thorium durch Neutronenbestrahlung" with the subtitle "Endgültiger Beweis für das Entstehen von Barium aus dem Uran". On this basis Frisch and Meitner could make their proposal that the uranium nucleus splits. More than four years elapsed before the question of the chemical nature of the irradiation products of uranium was settled. For their persistent and meticulous effort Hahn and Strassmann were awarded the Nobel Prize for chemistry in 1944.

## CORRESPONDENCE

### Who Does What ?

SIR,—Your correspondent (*Nature*, 219, 783; 1968), in pointing out the usefulness of the British publication, *Scientific Research in British Universities and Colleges, 1967-68*, directs attention to certain inconsistencies and irritations, such as deficiencies of the subject index. This publication would lend itself to a computer-based operation and, if certain contributors could be persuaded to write more descriptive entries, a KWIC or KWAC subject index could readily be prepared. Most of the entries are particularly suitable for this type of indexing. The branch of the Department of Education and Science which produces the publication, OSTI, should be in a position to organize such an index and would gain a practical stake in automated index production.

Yours, etc.,

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### On the Science of Science

SIR,—In your editorial "The Magic of Numbers" (*Nature*, 217, 793; 1968) and in the letter of Gennadi Dobrov and Alan Mackay (*Nature*, 219, 662; 1968), important questions were raised that deserve further comment. Taking issue with Professor D. J. de S. Price's essay, *Research on Research (Journeys in Science)*, University of New Mexico Press, 1967), the editorial questions the methods of the "science of science", which seem to be based on the assumption "that the part of the individual in scientific discovery is either so predictable or so negligible that it can be altogether ignored".

No competent student of the science of science would suggest that the individual is unimportant in science. To the editorial's question, "Can it seriously be supposed that if Archimedes, Newton and Einstein had never

existed, the present state of science would no different?" the answer is an emphatic no. The individual in science as in art is paramount. Professor Price writes that "If Beethoven had not existed, other men would have written quite different symphonies; Beethoven's private property is unmistakable". But so is Newton's and Darwin's and Einstein's. "If Planck, however, had not made his particular discovery", Professor Price continues, "somebody else would have to have made it and . . . rather quickly". That may be, but without Planck physics would be different today. Another man may have made the discovery, but the total creation that was uniquely Planck's would have been lost to science. This does not mean that the scientist and his work cannot be studied in the context of his society. Government support of research, the evolution and influence of professional societies, the social origins and education of scientists, are significant elements in the growth of science, and they are being studied by historians and sociologists. Such indices as numbers of scientific journals and papers published in them or the percentage of GNP devoted to research and development do not indicate the quality of the science being done or tell anything about the working of a scientist's imagination, but they do give some measure of scientific activity relative to economic, political and social conditions. It may be, as the editorial contends, that "the science of science has far to go before it attains respectability", but the sooner we understand the factors that influence the development of science the sooner we may use science and technology more intelligently than the evidence indicates we have in the recent past.

Dobrov and Mackay ask "is it possible to characterize live scientists and real scientific institutions by quantitative indices which summarize information and illuminate key features of the system?". The answer is surely yes, with the reservation that these indices cannot capture the illusive elements that make up so much of the personality and the creative imagination of a scientist. The historian-sociologist of science works to define and understand these elements, using whatever methods are available. If statistics will help, they should be used. For those responsible for the planning of scientific research the science of science may eventually be of great value even though, as Dobrov and Mackay note, "a broad theoretical understanding of the phenomena of science may not result. . .".

Yours, etc.,

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CORRIGENDUM. In the article "Rosalind Franklin and the Discovery of the Structure of DNA" by A. Klug (*Nature*, 219, 808; 1968) the following further corrections should be made: page 809, column 1, lines 25-28 should read ". . . structure in which the orientation of the helical molecules in the unit cell of the crystal is analysed and a detailed picture of the arrangement of the phosphate groups is proposed"; page 843, column 1, line 66, "chains are" should read "chains appear to be"; page 843, column 2, lines 17-18, should read ". . . data made it possible to propose an orientation for the helical molecule . . ."; the last line of the legend to Fig. 2 should read "Drawing adapted from ref. 9 which is based on the 1953 models".

CORRIGENDUM. In the article "Formation of Dimer Cations of Aromatic Hydrocarbons" by B. Badger and B. Brocklehurst (*Nature*, 219, 263; 1968), values of  $K$  for excimers (the excimer/monomer fluorescence ratio) were mistaken for  $K_e$  (the equilibrium constant) in quoting from references 5, 6 and 7. The order of magnitude of the values calculated for dimer cations and the qualitative conclusions are not affected.