

## The discovery of nuclear fission – a game changer in the history of scientific research\*

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It was on 17 December 1938 that Otto Hahn and Fritz Strassmann announced to the world the discovery of nuclear fission. It was on this day that humanity learnt how to unleash the immense energy potential of the atom. It is an irony of fate that the first practical application of our new knowledge on the energy potential of the atom was the demonstration of its destructive power. Fortunately, commercial electricity from nuclear power stations also became a reality soon after and today more than 400 nuclear power plants are in operation across the world supplying nearly 13% of the world's electricity demand (2012 estimate).

I do not know how many of you are aware that the discovery of fission also changed irreversibly the way in which scientific research is carried out, technologies are developed and taken to the market place for economic gains and marks the beginning of a new era in the history of scientific research. To appreciate this transformation, one needs to start with a brief look at the scientific scene in the early years of the subatomic and nuclear research, namely the late 19th and the early 20th centuries. I have always been intrigued by the number of 'accidental' discoveries in the early decades of research on the subatomic structure of the material universe. The discovery of X-rays by Wilhelm Roentgen in 1895 is a classic example of an 'accidental' discovery. While studying cathode rays, he noted that some fluorescent papers in his laboratory were illuminated at a distance, although his apparatus had an opaque cover. The discovery of radioactivity by Henri Becquerel in 1896 is yet another example of an 'accidental' discovery. While trying to study phosphorescent materials using photographic plates, he stumbled upon uranium. The discovery of the neutron by Chadwick in 1932 and nuclear fission itself by Hahn and Strassman in 1938 were also 'accidental' discoveries. I have also wondered

how these 'accidental' discoveries are made. Standard school texts will like us to believe that it is all a matter of chance. Stories of Newton getting inspired by a falling apple or Archimedes getting inspired while being immersed in a bath tub are well known and substantiate this belief. This obviously is an oversimplification. Millions of people must have been seeing falling apples before Newton, but it was only Newton who recognized the significance of the falling apple and formulated the laws of motion. It requires a trained mind and perseverance even to make an 'accidental' discovery. Sociologists call these as serendipitous discoveries. Serendipity is the effect by which one accidentally discovers something different, something more important, while looking for something else. Most authors who have studied scientific serendipity agree that a prepared and open mind is required on the part of the scientist or investigator to detect the importance of information revealed accidentally. This is also the reason why most of the 'accidental' discoveries occur in the field of specialization of the scientist. In the words of the famous French scientist, Louis Pasteur, 'in the field of observation, chance favors only the prepared mind'. The long and complex sequence of events leading to these discoveries also brought to the fore some of the weaknesses of the human mind. The discovery of the neutron by Chadwick followed an unfortunate misinterpretation of similar results obtained by Joliot-Curies earlier. Early studies of the bombardment of uranium by neutrons by Fermi and his colleagues followed by similar experiments by Curie and Joliot in Paris and Hahn, Meitner and Strassmann in Berlin proved very interesting and at the same time very puzzling. It took some time for the basic discovery that an isotope of barium was being produced in the experiments to be made by Hahn and Strassman. Clearly, the presumption of neutron capture reaction by all the groups was hindering proper interpretation of their results. The interpretation of the barium appearance as nuclear fission by Meitner and Frisch soon followed and

the liquid drop model of fission was born. The human beings behind a discovery being an important component of the discovery process itself, not only their strengths but also their weaknesses matter. The recorded instances of missed opportunities, delays in important discoveries, etc. arising out of prejudices of pre-formed concepts or reluctance to defy accepted peer knowledge only substantiate this.

While on the subject of 'accidental' scientific discoveries and their vulnerability to human weaknesses, I must share with you an India-centric anecdote on how close we came to a major scientific discovery of the century and how we lost it due to human weakness. Soon after the discovery of nuclear fission in Europe, a number of groups across the world also started studying nuclear fission in greater detail. India was not lagging behind in this global endeavour. A group in Bose Institute, Calcutta, led by Shyamadas Chatterjee, was studying slow neutrons and slow protons produced in lead by highly energetic cosmic rays with the help of a large proportional counter lined with boron and filled with  $\text{CH}_4$ . It was incidentally observed that the background count slightly increased when layers of  $\text{U}_3\text{O}_8$  were kept in the immediate neighbourhood of the counter which was placed within a paraffin enclosure. Of course, the increase in counts was small. All the same, it was the first observation of spontaneous fission and interpreted as such. However, his brief communication to *Science and Culture* was withdrawn as it had been sent without the permission of the director. Within a few months (1940), Georgy Flerov from the Soviet Union announced the discovery of spontaneous fission. The Calcutta group went ahead and made very detailed measurements; but in science only the first is remembered. Were we over-cautious? Did we lack confidence in our own competence? I do not know. But one thing is obvious. Scientific research at cutting edge and path-breaking discoveries are centred around professionally trained individuals, driven by curiosity and perseverance.

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The Manhattan Project was a research and development project that produced the first atomic bombs during the World War II. Begun modestly in 1939, the project grew to employ more than 130,000 people and cost nearly US\$ 2 billion (currently about 26 billion dollars). The demonstrated success of the Manhattan Project, which had a well-defined goal, adequate resources and a scientific workforce led to some major changes in the way scientific research is carried out. I call it a transition from research to research enterprise, a transition from research driven by individual curiosity to research driven by collective goals. Another important development following the Manhattan Project was the bootstrap set in by the new scientific discoveries leading to new technologies, new technologies leading to new instruments with far superior capabilities and new instruments leading to yet more new scientific discoveries. While this pushed the limits of knowledge beyond the known horizons, it also made research more dependent on sophisticated instruments and therefore more expensive. To remain competitive, scientists increasingly became more dependent on funding agencies, government or otherwise, for resources. Another important development of the 20th century is the emergence of high-technology products and services in the market place. While some of these came out of the Manhattan Project itself, the century witnessed an increasing role for technologies to dominate the market place both in terms of new products and services and in terms of market competitiveness. Scientific and technological knowledge emerged more and more as a commodity to be acquired, protected and traded. Rigid enforcement of intellectual property rights for commercial reasons led to restrictions on cooperation among scientific groups and duplication of efforts. I sometimes call this corporatization of research. Scientific curiosity as the driving force for research is the unfortunate casualty in this new game. It is not surprising that R&D funding agencies, in their turn, adopted this model of scientific research – set a goal, set the methodology, set deliverables and milestones.

I was Secretary to Government of India, Department of Science and Technology (DST) for more than a decade. DST is a major funding agency for scientific research in the country and I can assure you that no proposal will go through

until one follows the ‘scientific method’ – a method based on well-defined objectives, systematic observations and experiments and analysis. Of course we know that the so-called ‘accidental’ discoveries do not follow this ‘think-straight’ method. But the present scientific system looks at ‘accidental’ discoveries as exceptions rather than the rule. While the new system has delivered by way of new scientific discoveries, new technologies, new products and new services, there are concerns. The system obviously delegates individual scientists driven by their curiosity alone to the background. The ubiquitous peer-review system for research funding is a clear disincentive for out-of-box thinking. The research priorities are likely to be distorted by the funding agencies. The neutrality of science and scientists could also be in question.

When I started my research career in the early sixties, the world was already in the new regime of research in project mode. The Trombay fission group had come into existence under the leadership of Raja Ramanna, with a focus on physics of nuclear fission. I joined the group in 1964. Those were the hard days. Resources were limited. Foreign exchange was scarce. Nuclear instruments were not available off-the-shelf. You had to design and fabricate your own detectors, pre-amplifiers, amplifiers, pulse analysis systems, data recording systems, etc. Travel and communications were expensive and often unaffordable. There was always a lurking fear whether we can do competitive research with all these constraints. We learnt a lot from Ramanna. At the outset, he used to say ‘do your best and have faith in yourself. Think horizontally. Vertical thinking is subject to constraints of all kinds.’ Ramanna was not a ‘safe science’ man. He always dared to differ. Recall that he was the first one to talk of nucleon diffusion between two nascent fragments to understand the well-known asymmetric mass distribution in low-energy fission of actinides. It was almost a decade later that the nuclear physicists accepted that concept and started routinely applying it in describing heavy ion reactions. If you go to Ramanna and say ‘I have carried out this measurement and my results are in good agreement with all previous measurements’, he will say ‘congratulations. You have done a good job, but this is not the problem where you spend more time.’ If on

the other hand, you say ‘I have carried out this measurement and I have a problem reconciling my results with other existing measurements’, he will say ‘very good, double check your measurements. If the discrepancy persists, this is where you should concentrate’. In Ramanna’s view, discrepancies and anomalies are possible precursors of new information. Chasing anomalies was the working principle of Ramanna. This we did with quite a success. The rapid vanishing of shell effects with excitation energy, the postulation of pre-equilibrium fission in heavy ion fusion–fission reactions and the entrance channel effects in heavy ion fusion reactions are all our responses to anomalies seen in the experimental data. All our conjectures have stood the test of time and are widely accepted. The Trombay fission group is perhaps one of the few groups in the world having a sustained programme of research on fission with several important scientific contributions in the area. We have also had our failures – our search for super heavy elements in monazite sands of Kerala; search for cold fusion in deuterium-loaded palladium electrodes, etc. We never let our curiosity down. We owe this to Ramanna.

Ramanna’s message to all of us was the following: (1) Develop your expertise to a level when *you* have full faith in it. (2) Set aside some time for yourself even if you are committed to a well-defined project. Do not let your curiosity die. (3) If you are the boss, give some space to the youngsters to think for themselves. Nurture their curiosities. (4) Respect the peers, but do not be afraid of them. In matters of scientific discoveries, peers are as vulnerable to mistakes as you are. (5) Do not be afraid of anomalies. In fact, chase them. They are more often than not the precursors, the likely signals of some underlying scientific discoveries. Limited resources, facilities and manpower are unavoidable constraints in globally competitive research. Chasing the anomalies offers you an opportunity to optimally use your intellectual resources. (6) R&D is primarily a human-centric activity. Unshackle the human mind.

The message is as relevant today as it was decades ago.

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