

PREFACE

This is the fourth volume of the *Candid Science* series. It is devoted to famous physicists. Volumes I and III contained interviews with chemists and Volume II with biomedical scientists. Magdolna Hargittai (Magdi, for short) has joined me in authorship for this volume whereas she acted only as Editor for the previous volumes. As it happened, she prepared about half of the interviews in the present volume and I prepared the other half. For a few interviews we acted jointly. Both of us are physical chemists, but she has a special interest in cosmology and in the physics of fundamental particles.

The 36 interviews in this volume present a good, however incomplete, cross section of late 20th century physics. Some of the interviewees were already active and successful physicists in the 1930s. Quite a few participated in the Manhattan Project in various capacities. Whether it was a more prominent role, like Edward Teller or Mark Oliphant, or a less conspicuous one, all who participated in it were impacted by the experience. Some of the interviewees represent a link to such early giants of modern physics as J. J. Thomson, Ernest Rutherford, Niels Bohr, and Enrico Fermi. There is in the volume Laszlo Tisza, whose paper in 1944 was one of the forerunners of the Bose-Einstein Condensation and Wolfgang Ketterle, who was one of its discoverers half a century later. There are Nobel laureates, Wolf laureates, and Templeton laureates in this volume. The number of Nobel laureates is conspicuously high, but the non-laureates are no less great physicists than the laureates.

We would like to make our collection of interviews yet more comprehensive. Thus, we have not stopped making these interviews and further interviews with physicists will appear in *Candid Science V*.

Our interviews are, in most cases, by-products of our scientific and family travels. When we go to conferences or other trips related to our work, as well as when we are visiting our children, we use the opportunity to record interviews. We both use the same approach to these interviews. We contact our interviewees in advance, set up a date, and record the conversation on audiotape. Back at home we prepare the transcripts and send them for correction and change to the interviewees. This procedure is repeated as many times as it takes until the interviewee is happy with the material. The taping is very informal, providing only a framework for the interview. On the other hand, it is important to have this personal encounter, which could not be substituted by exchanging letters. We never engage anybody else in transcribing our tapes. Listening to them brings back memories of the personal meeting, helps us to capture what was said, and sometimes even gives us clues for a few additional questions that may then be posed in correspondence or at a next meeting. This approach gives a rather tight control of the interviewee over what appears as the text of the interview. However, our experience shows that acting in this way, rather than looking to reveal some “dark secrets”, has been helpful in getting closer to our interviewees in a human sense than it might be possible by a more aggressive approach. By the same token, during our actual conversations, some of our questions remain unanswered but then we drop the questions as well from the printed version. In this way the reader may sometimes feel that we should have asked a certain question, and, maybe, we did, but there is no trace of it in what is in this book. We are happy with what we have received without any embarrassment.

The interviews are very different in length and depth. Some interviewees were happy to open up more than others; some were willing to talk in greater detail about their science and about their personal lives than others. Again, we were happy to have what we could get. The circumstances of the recordings were also vastly different. Some interviews were made leisurely in a quiet office or home. Others were squeezed into a crowded program of a scientific meeting or, as it happened, into the rich program of the centennial celebrations of the Nobel Prize in Stockholm.

There is one exception among the 36 interviews communicated in this volume and that is the “interview” with Eugene Wigner. It is not a bona

fide interview, rather, a summary of a series of conversations I had during Eugene Wigner's visit to the University of Texas at Austin in 1969, while I was a Visiting Research Associate there in the Physics Department. Those conversations stayed deeply ingrained in my mind and I felt there should be an entry from them in this volume augmented by quotations from others.

We note with sadness that three of our interviewees are no longer among us. Eugene Wigner, Mark Oliphant, and Edward Teller have passed away since our interviews with them. I had corresponded with Edward Teller up to a few weeks before his death on September 9, 2003. I am excerpting two letters here as I believe they augment our Teller interview. The original letters were in Hungarian communicated through his assistant, Mrs. Margit Grigory to whom I am grateful for her help in making it possible to keep in touch with the ailing Teller. Here the excerpts from his letters appear in my translation.

From Teller's letter of August 13, 2003:

...

In my life's work, I loved science a thousand times more than its applications. I agreed to the latter because I took the dangers of war very much to my heart. I hope you know that I was always against our becoming the first to deploy the hydrogen bomb. I only wanted to have the possibility of the H-bombs as a deterrent for wars, and this has worked so far.

I am convinced that the business of scientists is exclusively science itself. The application of science is the business of politicians and consequently that of the voters. I had problems with my fellow scientists, especially with those according to whom we shouldn't have worked on anything like the hydrogen bomb.

Incidentally, in this question we bitterly differed with Oppenheimer. Similarly we had different positions with Enrico Fermi but with him our friendship did not suffer from this difference. The same can be said about Leo Szilard, who was the most gifted in treading on other peoples' corn, but he never bored anybody.

However, let's though speak about science. Very few performed true science and I knew two such people, Einstein and Bohr. I would be curious to know, what Einstein thought when

he received the Nobel Prize definitely not for relativity. There are things that are unavoidable, like benzene [reference to the first part of the letter] and the hydrogen bomb, although it may be important for the moment who thought of them first. Things like relativity and quantum mechanics far surpass all other intellectual activities.

For me it is important that the same four letters describe the DNA of all living creatures. This may bring us closer to the understanding of what life is.

...

From Teller's letter of August 17, 2003 [focusing on the question about success in science]:

Your question is a difficult one, but my answer is easy. I was not an unsuccessful scientist but my scientific research suffered from my work on weapons. Also, in part this happened when I was in the peak of my energies.

In addition, as witnessed by my productive work, I liked to cooperate with others and in this, our disagreement with Oppenheimer caused a lot of damage.

We are grateful to the Hungarian Academy of Sciences and the Budapest University of Technology and Economics as well as the Hungarian National Scientific Research Foundation for their support of our research activities in structural chemistry. Our scientific research brings us to meetings and laboratory visits whose byproducts are often the interviews presented here. A very fruitful period was the three months we spent at the Cold Spring Harbor Laboratory at James Watson's invitation in 2002, which gave a convenient opportunity for several interviews. We appreciate Jim and Liz's hospitality and personal attention during our stay there. Our family vacations provide additional opportunities to expand our interviews project.

We also appreciate the dedicated efforts of the associates of Imperial College Press and World Scientific Publishing Company in bringing out this volume. Senior Editor Ms. Ying Oi Chiew spared no labor and attention in making this book as nearly perfect as possible. Her friendly cooperation and pleasant care enhanced our pleasure in working on this project.

Both Magdi and I are infinitely grateful to all our interviewees for their patience with us and for their fruitful cooperation. We have learned a lot from them in physics, in science history, and in human conduct. Our highest hope is that our readers will similarly benefit from these interviews.

Budapest, October 2003

István Hargittai

Technical Comment

There is an apparent inconsistency in the way some of the names appear in this volume. However, they were given careful consideration. For example, Peter Kapitsa appears also as Kapitza and his first name as Petr and Pyotr as well. We did not want to arbitrarily change from their original appearances that is the result of difference in transliteration and also in the way he used his name in Cambridge and in Russia. Martinus Veltman's first name also appears as Martin, and Gerardus 't Hooft's first name as Gerard at places. We have consulted about this problem with Drs. Veltman and 't Hooft and we tried our best to follow their preferences. Dr. Tisza's first name appears as László when he is being referred to his time in Hungary. Later, adapting to American usage, his name became Laszlo.