

Springer Biographies



Going for Cold

**A Biography of a Great Physicist,
Kurt Mendelssohn**

J. G. WEISEND II
G. TERENCE MEADEN



Springer

Springer Biographies

The books published in the Springer Biographies tell of the life and work of scholars, innovators, and pioneers in all fields of learning and throughout the ages. Prominent scientists and philosophers will feature, but so too will lesser known personalities whose significant contributions deserve greater recognition and whose remarkable life stories will stir and motivate readers. Authored by historians and other academic writers, the volumes describe and analyse the main achievements of their subjects in manner accessible to nonspecialists, interweaving these with salient aspects of the protagonists' personal lives. Autobiographies and memoirs also fall into the scope of the series.

More information about this series at <http://www.springer.com/series/13617>

J. G. Weisend II • G. Terence Meaden

Going for Cold

A Biography of a Great Physicist, Kurt
Mendelssohn

J. G. Weisend II
Accelerator Division
European Spallation Source
Lund, Sweden

G. Terence Meaden
St Peter's College
University of Oxford, UK

ISSN 2365-0613

Springer Biographies

ISBN 978-3-030-61198-9

<https://doi.org/10.1007/978-3-030-61199-6>

ISSN 2365-0621 (electronic)

ISBN 978-3-030-61199-6 (eBook)

© The Editor(s) (if applicable) and The Author(s), under exclusive license to Springer Nature Switzerland AG 2021, Corrected Publication 2021

This work is subject to copyright. All rights are reserved by the Publisher, whether the whole or part of the material is concerned, specifically the rights of translation, reprinting, reuse of illustrations, recitation, broadcasting, reproduction on microfilms or in any other physical way, and transmission or information storage and retrieval, electronic adaptation, computer software, or by similar or dissimilar methodology now known or hereafter developed.

The use of general descriptive names, registered names, trademarks, service marks, etc. in this publication does not imply, even in the absence of a specific statement, that such names are exempt from the relevant protective laws and regulations and therefore free for general use.

The publisher, the authors, and the editors are safe to assume that the advice and information in this book are believed to be true and accurate at the date of publication. Neither the publisher nor the authors or the editors give a warranty, expressed or implied, with respect to the material contained herein or for any errors or omissions that may have been made. The publisher remains neutral with regard to jurisdictional claims in published maps and institutional affiliations.

This Springer imprint is published by the registered company Springer Nature Switzerland AG
The registered company address is: Gewerbestrasse 11, 6330 Cham, Switzerland

Preface by G. Terence Meaden

It was a pleasure to be invited by Dr. John Weisend II to participate in this biography of Dr. Kurt Mendelssohn—a great Oxford physicist who died 40 years ago aged 74—because I am one of few surviving students or colleagues who knew him well. For me the friendship began from the point of view of 4 years as a doctoral student followed by 2 years as a post-doctoral fellow.

Before my first meeting with Dr. Mendelssohn, I had been an Oxford undergraduate at St. Peter's from 1954 to 1957 in the Department of Physics, beginning when the head of the Clarendon Laboratory was Professor Frederick A. Lindemann, also known as Lord Cherwell. He was another great man in the history of the Clarendon and the history of Britain by being Winston Churchill's close scientific advisor during the years of the Second World War.

Professor Lindemann's lecturing colleagues in the undergraduate teaching of low temperature physics included Kurt Mendelssohn and Nicholas Kurti. I much valued the physics lectures of all three. In this way, I became aware of the importance and excitement of the discoveries being made at Oxford about the properties of liquid helium and superconducting metals, and I soon viewed it as an ambitious aim to join one of the teams myself. Thus, it was that in my final year, I registered my name, as did many others, with Mr. T. C. Keeley—the managing director of the Clarendon—as a student who would be interested in undertaking post-graduate research in low temperature physics if the opportunity arose.

I took finals in June 1957, and next month was gratified to receive from Dr. Mendelssohn an invitation for interview regarding the possibility of embarking on a doctoral research project with him. His letter began: "I intend to start in the coming academic year two research projects in one of which you may be interested. The first concerns the investigation of the electric and thermal properties of the transuranic elements in metallic form. Plutonium and neptunium are now available and there is a hope that sufficient quantities of americium will be available in the not too distant future. This work will be coupled with the investigation of other heavy elements, in particular thorium and uranium".

A few days later, I was in Oxford for an interview, got accepted and was at once being shown the cryostat that the workshop had built to Dr. Mendelssohn's design for use in Oxford with thorium and uranium. He added that there would be another cryostat at the Atomic Energy Research Establishment at Harwell, where the work on the highly toxic radioactive metals plutonium and neptunium would be carried out using the protection of a glass-sided glove box. Senior scientific officer Dr. James A. Lee would be my supervisor at Harwell—another convivial lovely man and commendable first-rate scientist.

Thus, it was that in October 1957 I began getting to know Dr. Mendelssohn through frequent meetings with him and the staff of the technical and glass-blowing workshops. Working at first in Oxford, I took the cryostat and its supporting equipment through to the trial and functioning stages when studying the electrical properties of uranium down to liquid helium temperatures. At the same time, I began the first of many hundreds of car journeys to Harwell where I would test and operate a cryostat similar to what I had been using in Oxford but with the extra protection of an enclosed glove-box system. In this period while spending most of my time at Oxford, I came to know what an amiable likeable man Kurt Mendelssohn was, and progressively learnt what I needed to know cryogenically for the forthcoming research at Harwell using the new experimental systems.

The first visit to Harwell came at the start of October. Travelling in Dr. Mendelssohn's car allowed us to talk for nearly an hour during the journey, and it was much the same on the few subsequent trips that we made together because normally I would drive alone. I soon learnt what a brilliant all-rounder Kurt Mendelssohn was as a general conversationalist. KM (this being the usual way when speaking of him in the laboratory) was, like me, willing to discuss almost anything, having a kindly mindset and cheerful worldview. Born in 1906, he had followed an ambitious path in Germany from when he was young, and pursued it in his Oxford years to immense scientific advantage. His investigations had been pioneering and his timely changes of research direction in the 1930s proved highly profitable academically. He assured me that many scientists reach a peak of excellence in their late 20s and 30s, and I should bear that in mind. "Grasp the nettle", he said, means tackling the inevitable difficulties boldly, directly—much as he had done. This was an inspiring start to years of mutual respect and cooperation.

Weeks passed and experimental test runs interspersed with meaningful and successful runs came and went, but let us not forget the failures too. Often the runs that go wrong do not get into the thesis which can read as if every experiment had worked out well. The apparatus would spring a leak or vacuum pumps would break down or spot-welded wires would drop away in the face of the intense changes of temperature to which specimens were subjected. Dr. Mendelssohn was always an optimist. Although he took no part in any experimental runs with me, he followed my progress closely and cheerfully.

This was a world without personal computers and printed circuits. Calculations were made by 4-figure and 7-figure logarithm tables or the cranking by handle of a stepped-gear desktop Marchant or Monroe mechanical calculator the size of a typewriter. The laboratory seemed to be all Wheatstone Bridges, galvanometers,

oscilloscopes, valves or electron tubes and other what-are-now-vintage electronic components, etc.

In Dr. Mendelssohn's group, the situation was that from some time before 1957 experimental research had been gathering a momentum of its own—as also in Dr. Kurti's group. Although the D.Phil. students each worked alone, we were colleagues too—forever obliging by helping, lending, repairing and so on. Dr. Mendelssohn forever sought progress reports and always accepted disappointments cheerily. I much valued how I had been selected for this project by a great physicist and busy man—and one who was a kindly father of five children with a caring, supportive wife Jutta.

Every year we would all be invited to a cocktail party at his splendid home at 235 Iffley Road which he had purchased new in 1937. There were usually some 30 of us crowded into the biggest room in his house, so well hosted by Jutta. There were other occasions too—dinner for four at his house—after I had married in France in 1965, when working at the Le Centre de Recherches sur les Basses Températures in the Faculty of Sciences at the University of Grenoble and brought my wife to visit him and my laboratory friends. Jutta was a charming hostess, perfect for an Oxford professor. Since 1955, KM had been an Oxford University Reader in Physics which is a position fully equivalent to full professor at any university elsewhere.

On one occasion, in my second year as I recall, I drove Dr. Mendelssohn to Harwell in my own car which was a white Triumph TR2 open-topped sports car. He loved it. It was only once though. Later, I learnt that he told members of the Harwell plutonium research team, “Terry brought me here in his sports car, and I kept saying to him ‘seeing that this is a sports car why don’t you drive much faster?’”

By the summer of 1960, KM judged that I had done enough research on plutonium and the other available actinide metals at Harwell and Oxford for my thesis although we had failed to find superconductivity in plutonium and neptunium despite reaching temperatures of 0.7 Kelvin by which temperature both thorium (at 1.37 K) and uranium (close to 0.75 K) had become superconducting. KM was urging me to write up my work, and he took on a new student, David Wigley, to whom I would teach the basics of applied cryogenics so that he would continue with the Harwell in-house research. Satisfied with my doctoral research, KM began discussing what examiners he might choose when the time would come in a few months. In the end, he selected faculty member Dr. Arthur H. Cooke of the Clarendon and Professor B. S. Chandrasekhar of Case Western University who in 1961 was on sabbatical leave in London.

My D.Phil. defence in July 1961 was satisfactory and uneventful. So KM was then saying that the work deserves to be in the Proceedings of the Royal Society, and no, he did not want to be a co-author either, which is something that few Ph.D. or D. Phil. supervisors anywhere would say. About then, he left for a conference and meetings in Tokyo and came back saying he had met a fine scientist who wanted to work in Oxford for a couple of years and learn English properly. He asked whether I would be interested with my doctorate out of the way. I agreed immediately. His name was Dr. Toyochiro Shigi, Osaka University. Moreover, KM had located a

commercial source that would supply a sufficient quantity of the rare helium-3 gas in order that at the Clarendon (not at Harwell) we may take specimens of plutonium and neptunium in protective sealed containers to lower temperatures than could be realized with helium-4. Temperatures approaching a third of a degree above absolute zero could be attainable. Toyo Shigi was another very agreeable colleague. I will mention an idiosyncrasy that was forever amusing. In our several laboratory rooms, each entrance door on its inside carried a blackboard on which anyone would write with chalk when discussing physics. Toyo took this a stage further. He and I would sit cross-legged on the wooden parquet floor and use the floor for chalking out our ideas as we discussed them. KM chuckled every time he found us thus engaged in this opportunistic expression of the Japanese work method.

Even more, I came to realize how KM was a polymath. Although he had spent so much of his life in pure and applied experimental physics, he was much interested in everything else too. When stopping off at Cairo one week early in 1965 on his way back to England from being a visiting professor at Kumasi University in Ghana, he started thinking in a fashion that only a scientist could about certain puzzles posed by the raising of the pyramids. He sought to understand the problems of the Third Dynasty Bent Pyramid and contemporary and later pyramids too. Pyramids were the study domain of specialist archaeologists known as Egyptologists who had had classical educations and not science-based ones. A scientist could refreshingly review the subject independently and dispassionately—and apply the conscientiousness of the scientific method too (viz. collect data, suggest hypotheses, then test them against sure facts in such an open fashion that anyone else repeating the exercise will obtain the same results). He was telling me about the Bent Pyramid when I was in Oxford on a couple of my return visits to see him in 1965 and 1966. This was the beginning of his 10 years research into problems involving structural factors and the reasons why pyramids were built at all, seeing that although they served as mausolea it was scarcely a sufficient motive to warrant the colossal long-term building efforts. How does one encourage so many thousands of men to labour long in undertakings about which most would never witness the final achievement? Surely there was something else too.

His delving into plausible explanations led to the 1974 publication of *The Riddle of the Pyramids*. But here I was in 1966 sympathizing with his approach partly because already as a physicist I had encountered my own problems when reflecting on the purpose of Stonehenge and seeking meaningful answers as to how priests and gang leaders could maintain willing cooperation over many decades from (in the case of Stonehenge) only farm workers of the local clans. Eventually, KM concluded that although the pyramids were built as cenotaphs “the object of the whole exercise was not the use to which the final product was to be put but its manufacture. What mattered was not the pyramid—it was *building* the pyramid”. Because of my long-term interest in trying to understand Stonehenge I thought much the same and yet a little differently, viz. that the answer was also likely related to whatever was the core symbolism of the manifestly sacred pyramids. This could mean, for example, that, as was likely for the builders of Stonehenge, the heavenly rewards promoted by persuasive charismatic leaders were themselves stimulated by emboldened artful priests as to the high benefits in an afterlife for being compliant participants in the

tough work of their present lives. Such can be the benefits when fostered powerfully by shrewd religious narrative and belief.

In the years from 1957 as I got to know KM better, he was nonetheless often absent attending national and international conferences in addition to board meetings arising from the founding of *Contemporary Physics* in 1959 and *Cryogenics* in 1960. He was also putting to good use visiting professorships combined with travel to Africa, the USA, Tokyo and other places and universities. He was a good speaker, a gifted writer. His *Quest for the Absolute Zero* published in 1966 is a marvel of clarity about an exciting difficult subject. Later, after visiting communist China in 1967 he summarized his findings of the tightly closed society by writing *In China Now*, published 1969.

In the early 1960s, he took on the task with Dr. K. D. Timmerhaus (University of Colorado) initiating a series of volumes called The International Cryogenics Monograph Series and invited me to write a book of my choice in low temperature physics. I chose to prepare *The Electrical Resistance of Metals* which came out in 1965 (Iliffe, London) and 1966 (Plenum, New York) and was reissued in 2013. I here repeat a relevant sentence from the preface to this book: “I am grateful to Dr. K. Mendelssohn, F.R.S., whose unrelenting enthusiasm has stimulated and developed my love for low-temperature physics”. And again, in 1970 (although it did not come from KM) I received an invitation to prepare a physics review article for *Contemporary Physics*, which I gladly accepted, on the suggested subject of “conduction electron scattering and the resistance of the magnetic elements”, published 1971.

In 1963 after 9 years in Oxford, I decided to move on and asked KM whether he would recommend me to Professor Louis Weil for a faculty position in the physics department of the University of Grenoble—and this he did with good grace although not really wanting me to leave the Clarendon right then. So I departed and became an assistant professor at a French university. Because there were useful studies still to do on the transuranic elements, research on the properties of such highly radioactive metals continued in England except that in the next decade everything would be done entirely at Harwell. In 1965 in Grenoble, KM visited me and Professor Weil. I was by then engaged to be married, so my fiancée and I took KM out to dinner at a choice Vietnamese restaurant. After Grenoble, I continued at Dalhousie University in Halifax (Canada) as an associate professor, where I set up new cryostats in the Clarendon mode, as so many others from Oxford had done at universities worldwide. I count myself lucky to have spent formative years with Dr. Mendelssohn because much of the Clarendon was a low temperature physics department raised to greatness, thanks to scientists like F. A. Lindemann, Kurt Mendelssohn, Nicholas Kurti, Sir Francis Simon, Arthur H. Cooke, John Daunt, D.K.C. MacDonald, Guy K. White, R. Bowers, J.L. Olsen, B.S. Chandrasekhar, Harry Rosenberg, Brebis Bleaney and Martin Wood together with many other researchers in pioneering aspects of advanced experimental and theoretical physics.

Bradford-on-Avon, UK

G. Terence Meaden

terencemeaden01@gmail.com

Preface by J. G. Weisend II

You have seen them on the road. Large tanker trucks, frequently white, bearing corporate names like Linde, Air Products or Air Liquide. If you look closely you may notice that they also say “Refrigerated Liquid Nitrogen” or, less frequently, “Liquid Helium”. These trucks are a visible sign of the billion-dollar cryogenics industry.

Cryogenics—the study and use of extremely low temperatures—makes possible every MRI scan, the separation, collection and transport of gases like oxygen, nitrogen and argon for industrial and medical use, the flash freezing of food and the creation of immensely strong magnetic fields needed for the study of materials and of the very nature of matter itself.

Two of the largest scientific projects in the world today—the Large Hadron Collider (LHC) in Geneva, Switzerland, and the ITER fusion reactor currently under construction in France—are possible only due to superconducting magnets cooled by liquid helium. Spacecraft, cooled by superfluid helium, have tested Einstein’s Theory of General Relativity and searched the cosmos in the infrared spectrum.

Cryogenics allows the efficient transport of liquefied natural gas from producers such as Algeria to users like Japan and the USA. The production and transport of liquid hydrogen, made possible by cryogenics, will be a vital part of any future hydrogen economy.

The temperatures involved are staggeringly cold. Helium gas becomes a liquid at 452° below zero Fahrenheit, i.e. -269°C . Superfluid helium at less than 456 °F below zero is colder even than interstellar space. Over time, humanity has learned how to create, keep and transport cold.

This is all quite new. Helium was not liquefied until 1908 and the first helium liquefier in the United Kingdom was not built until 1933. For a long time, cryogenics was strictly a small-scale laboratory science. The first helium liquefier in the United Kingdom was custom built, required the use of liquid hydrogen and only produced 20 mL of liquid helium. Today, the delivery of a million times that amount of liquid helium requires not much more than a telephone call and a credit card.

K.A.G. Mendelssohn played a major role in this development. An early refugee from the Nazis, he set up the first helium liquefier in the United Kingdom in 1933 and did fundamental research at Oxford University that increased our understanding of superconductivity and superfluid helium. He produced 188 scientific papers on such topics as the behaviour of superfluid helium films to the low temperature-specific heat of plutonium. He aided the transition of cryogenics from a small-scale science to a major industry by founding the leading scientific journal on cryogenics, setting up an ongoing international conference series, urging industrial development and training many students and visitors who went out and established their own research and industrial programmes.

Fascinated by travel and the world around him, Kurt Mendelssohn developed many collaborations between his group in Oxford and those of foreign countries, including countries such as China, India and Ghana who were just developing their industries. By the time of his death in 1980, Mendelssohn had met both Chairman Mao and Nehru and had personal contacts throughout the world.

Dr. Mendelssohn's efforts in developing collaborations, publications and conferences resulted in institutional structures that we use to this day in cryogenics.

Personally, Mendelssohn was generous, inquisitive, quick-witted with obvious *joie de vivre*. He was an excellent, meticulous, natural experimentalist. Moreover, as Professor Meaden's Preface makes clear, he was dedicated to both training his students and assisting them and his other collaborators with their research and careers whenever possible.

Kurt Mendelssohn had wide ranging interests and wrote books for the general public on topics as diverse as the history of cryogenics, his travels in China, the rise of Western science and the building of the pyramids. He contributed many articles to the popular media on physics, the teaching of science and the state of scientific research in the United Kingdom and other countries. He was frequently sought out by the press for interviews. His leisure time was taken up with photography, travel, gardening and the collecting of antique Chinese porcelain. He was frequently able to combine his work and leisure interests.

Unfortunately, Dr. Mendelssohn and his contributions are not as well-known as they should be, not even among current specialists in cryogenics. This book is an attempt to change this and make him better known to both specialists and to the general public. It is also a description of an inquiring, intellectual life well lived. It is not a complete story, but it emphasizes his contributions to science and his development of connections both within the cryogenic community and to the broader public.

When asked his interests by a journalist during a trip to India, Kurt Mendelssohn always an optimist, replied "anything that goes on in the world". It is in this spirit that his story is told.



Kurt Mendelssohn in 1967

Lund, Sweden

John G. Weisend II

Acknowledgements

Much of the pleasure that came from writing this book resulted from my interactions with the people who have assisted me over the years. Their help has been invaluable and I wish to express my deepest thanks to them. As always, any opinions or errors in the book are mine alone.

Dr. Monica and Dr. James Mendelssohn graciously answered questions about their father, his work and their life together. They have shared numerous family photographs and memories of their father. Two of Kurt Mendelssohn's colleagues at the University of Oxford, Nicholas Kurti and Ralph Scurlock, both provided memories of working with Dr. Mendelssohn and encouraged me in the writing of this book. John Vandore and Stephen Blundell helped connect me to a number of resources on Kurt Mendelssohn and Oxford. B. S. Chandrasekhar and Vinod Kumar Chopra shared their experiences of working in Dr. Mendelssohn's group at Oxford.

Terence Meaden was another of Kurt Mendelssohn's students. He contributed greatly to the volume by preparing the Preface, sharing personal experiences with me and carrying out several close readings of the manuscript. In working closely with Monica and James Mendelssohn, he was given numerous family and academic work-related photographs, and through several long interviews gained considerable personal family information about their father's life. Terence's participation and friendship have significantly improved the book.

B. S. Chandrasekhar and Steve Van Sciver both read drafts of the text and provided valuable comments and suggestions.

The staff at the University of Oxford Bodleian Library were very helpful in providing access to Dr. Mendelssohn's papers and in suggesting other sources of information.

Sam Harrison, Tom Spicer, Cindy Zitter and the entire team at Springer have all been invaluable in the production of this book.

This work has been carried out over a number of years during which time I have been on staff at the SLAC National Accelerator Laboratory, Michigan State

University, the European Spallation Source and Lund University. My colleagues in all these institutions have been very supportive of my efforts on this book.

My family, Shari, Rachel, Alex and Nick, have not only tolerated my absences due to this work but have also patiently listened while I explained the new and exciting item I had just learned. Their continuing support makes this book possible.