Introduction

It is generally agreed that natural science today is an integral part of our politics, our economy, and our culture. It is seen as the major force behind the enormous technological advances of the last few decades, and has become an important part of the budget of any developed society. We are in the era of "big science," in which scientific practice is planned on a large scale nationally and internationally. More than ever, natural science is part of the general cultural debate, posing important economic, social, and ethical questions.[1]

This perception of the importance of natural science is relatively new. It grew rapidly after the Second World War, with its unparalleled massive efforts on projects such as the atomic bomb and radar. For scientists and historians alike, the Second World War was the turning point, when science lost its innocence. But the historical roots of the way science works today must be sought in an earlier time. I would like this book to be a contribution to the search for these roots.[2]

Long before the twentieth century, disciplines such as physics, astronomy, and chemistry had become esoteric enterprises, understood and pursued by a select few. Yet, like other human enterprises, they were part of human history. How does the historian integrate them into the larger historical picture without losing sight of all the complex scientific detail?[3]

Many observers have pointed to scientific change as a way to detect these historical links. During periods of change, scientific preconceptions are themselves in flux, and interact more readily with general historical trends. Even so, the discussion between historians, sociologists, and philosophers of science continues about whether general historical developments are responsible for the rate of growth, the direction, or even the content of science.[4]

Funding provides one obvious means of influencing basic science research. Before the Second World War, science was severely underfunded by today's standards. Yet it had sources of support. The period between the two world wars, which this study will focus on, was the heyday of organized private philanthropy's support of basic science. The Rockefellers, in particular, spent substantial sums to develop institutional and individual science research. Although much has been written about science funding in the interwar period, its effect on actual scientific work has received much less attention.[5]

With hindsight, we see that one particular change in physics research between the wars was of special importance for subsequent historical developments. This was the transition to a concerted effort in the 1930s to theoretical and experimental research on the atomic nucleus, which would become the scientific basis for building the atomic bomb a decade later. Like the general features of science funding during this period, internal and technical aspects of the rise of nuclear physics in the 1930s have received substantial historical attention. Considerably less is known, however, about the relationship between the emergence of nuclear physics and external developments, including its sources of funding.[6]

This book attempts to bridge that gap. It brings together scholarship about the internal origins and development of nuclear physics in the 1930s and concurrent changes in private support for international basic science – in particular, the Rockefeller philanthropies. In doing so, the book places the emergence of nuclear physics into a larger historical context.

I have not attempted to study how all of nuclear physics interacted with all science funding. Instead, I have focused on one carefully chosen scientific institution. The resulting detail is essential to understanding the complex interrelations between basic science and its sources of financial support.

The typical scientific institute between the world wars was large enough to require administrative attention, yet small enough for the director to be personally responsible for both science and policy. In fact, all the activities at such an institute went through the director. Hence, the interconnections between science and funding in an institutional context can be reduced to the scientific and policy endeavors of its director.

Concentration on a single institution has limitations. At any time, the development of physics depends on the joint effort of several individuals at many institutions. This was particularly true of the development of quantum mechanics between the world wars. Indeed, Niels Bohr, the major figure in this account, repeatedly played down his own and his institute's contributions to these developments, emphasizing instead the collective nature of the scientific enterprise.[7]

The institution I have chosen is the Institute for Theoretical Physics at Copenhagen University, directed by Niels Bohr, and in 1965 renamed the Niels Bohr Institute. It is generally agreed that Bohr's institute was a mecca for theoretical physicists between the wars. Under Bohr's direction, the institute was a focal point for the developments that culminated in the formulation of quantum mechanics in the mid-1920s and revolutionized physics. In the following years, Bohr, together with his collaborators, developed the "Copenhagen interpretation" of the new physics, a position that came to be accepted by the majority of physicists.

Then, in the mid-1930s, after the so-called miraculous year of 1932 had created a watershed in experimental discoveries about the atomic nucleus, interest and research at the institute turned to nuclear physics. In 1936, Bohr proposed his highly influential "compound nucleus" model of the atomic nucleus. During the next few years, he oversaw both an increasing theoretical attention to the atomic nucleus and the installation of new and expensive equipment devoted to its investigation. By the outbreak of the Second World War, Otto Robert Frisch, working at the institute, and his aunt Lise Meitner, then in Sweden, explained the process of nuclear fission. At about the same time, the new equipment was put to successful use. The institute had become as central to the new field of nuclear physics as it had been to the development of quantum mechanics in the previous decade. For this reason, it is the natural choice for our study. This book puts the transition to theoretical and experimental nuclear physics at the institute into the broader context of its director's role as policymaker and fund-raiser.[8]

Although little has been written about the relationship between physics and funding at the institute and other places, interesting historical questions arise from even a cursory look at the basic historical documentation. In the next few paragraphs, I will use the publication record at the institute to pose some of these questions. This will provide a general idea of the issues considered in the book.

After the number of publications reached a peak of forty-seven in 1927, publication activity decreased gradually, reaching a low of seventeen papers in 1933. At that time, the publications were yet to reflect concerted research on the atomic nucleus. In 1934 the total number of publications rose to twenty-four as relative emphasis on the nucleus increased. By 1936, when twenty-six papers were published, nuclear physics was the main area of research at the institute. The emphasis on nuclear physics continued, the number of publications reaching a new peak of forty in 1937. Activity then fell slowly, until it almost ceased after the outbreak of the Second World War and the German occupation of Denmark.[9]

If these numbers signify trends, are they to be accounted for by developments outside or inside physics? Specifically, was the decrease after 1927 due to a decline in relevant scientific questions or to a decrease in funding? What was the main research interest at the institute during this period of low activity? What triggered the sudden theoretical and experimental turn to the atomic nucleus after a seeming lack of interest in such questions immediately after the miraculous year? How did Bohr obtain the funds necessary to make the transition? Were his fund-raising efforts motivated by a clearcut policy decision to introduce nuclear physics, or did the turn to a new field emerge as a byproduct of fund-raising efforts motivated by other factors?

From the list of research publications at the institute it is evident that there was substantial activity in biology during this period. This activity was led by the physical chemist George Hevesy and was represented by such articles as "Atomic Dynamics of Plant Growth" and "The Circulation of Phosphorus in the Body." From 1936 until the Second World War, biological work comprised as much as a quarter of the publication output from the institute, the rest being devoted to nuclear problems. Although it is well known that the institute became a major center for nuclear research in the 1930s, historians and reminiscing scientists have paid little attention to the concurrent activity in biology there. How and why did this activity begin, and what was its relationship, if any, to the contemporaneous transition to nuclear physics? Whatever the final answer to these questions, the existence of a biological research program at the institute complicates the picture of the activities there and further motivates a broad approach to the study of the institute's history in the 1930s.[10]

The prologue following this introduction presents a picture of the special atmosphere at the institute as subsequently remembered by the people working there. The prologue has two complementary functions. On the one hand, it provides a general introduction to life at the institute before we turn to the particular case of nuclear physics research. On the other hand, however, the physicists' accounts reflect not established facts, but rather the perception of the research participants. Contrary to the general argument of this book, these physicists remember few or no extrascientific developments affecting their work. Nevertheless, as I will argue, this difference does not refute my own interpretation. In fact, a side effect of the book is to give the reminiscences of those physicists a new perspective. It is therefore appropriate to turn now to their recollections of the atmosphere at the institute.

Prologue: The Copenhagen spirit

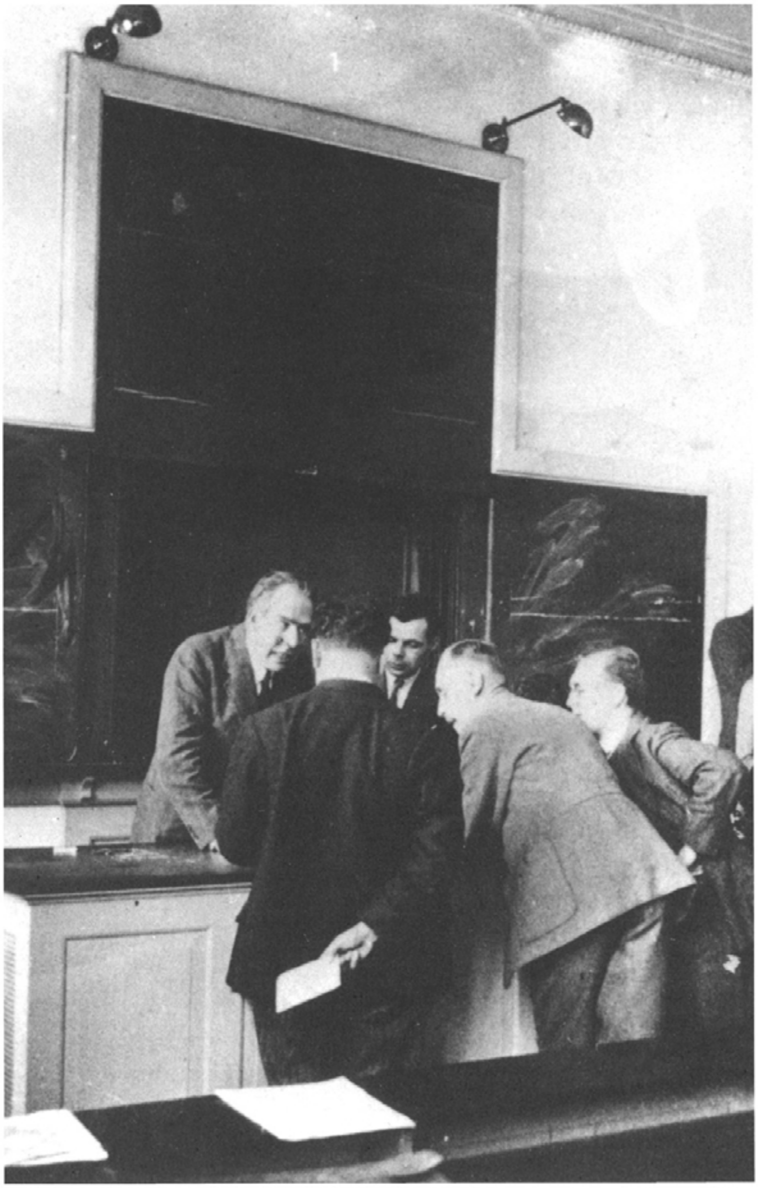
In their accounts of life at the institute between the wars, physicists have often portrayed their experience in terms of what they refer to as the "Copenhagen spirit." This term, or rather its German equivalent *der Kopenhagener Geist,* was first used in print by Werner Heisenberg. In his 1930 textbook on quantum theory, which was based on a lecture series given at the University of Chicago in the spring of 1929, Heisenberg referred to "the Copenhagen spirit of quantum theory," implying a special approach to the conceptual problems of physics. Subsequently, the term has come to refer to the atmosphere and style of work, rather than to specific ideas. It is this later meaning that I will consider here.[1]

By general agreement, the Copenhagen spirit signifies a broad approach to problems and the freedom to pursue one's own research. Victor Weisskopf, an Austrian physicist who spent several years at the institute during the 1930s, points to Bohr's unique ability to develop independent minds rather than assigning specific pieces of work. Weisskopf suggests that Ernest Rutherford, under whom Bohr worked in Manchester after obtaining his PhD in Copenhagen, is Bohr's only possible equal in this respect. Stefan Rozental from Poland – who arrived at the institute in 1938 and, except for the last part of the Second World War, has stayed there ever since – sees the resulting freedom to pursue one's own independent research as the dynamic necessary for the success of any physics research institute. For Rozental, physics research cannot be guided, but develops by circumstance.[2]

According to the physicists who worked at the institute, the cultivation of this intellectual independence included encouraging an informal intellectual and social atmosphere to both work and play. The German experimental physicist Otto Robert Frisch, who spent several years at the institute in the 1930s, tells of his stupefaction when shortly after his arrival he went to a colloquium at which he found Bohr in eager discussion with the Russian physicist Lev Davidovich Landau. Landau was lying on his back on a table, while Bohr seemed to take no notice of this unusual position. Frisch found such unconventional behavior radically different from what he had experienced during his stays in Hamburg and London. Referring to the Landau incident, Frisch wrote later that "it took me a while to get used to the informal habits at the Institute for Theoretical Physics in Copenhagen, where a man was judged purely by his ability to think clearly and straight." The thoroughly informal atmosphere at the institute is recalled by many as a central ingredient in the Copenhagen spirit.[3]

This informal atmosphere extended beyond the discussion of physics. Several physicists report, for example, that Bohr enjoyed American Western movies, and they frequently joined him. The following is a favorite story of at least two visitors. After seeing a Western with a few visiting physicists, Bohr offered a theory as to why the hero always won the gunfight duels provoked by the villain. Bohr reasoned that reaching a decision by free will always takes longer than reacting mechanically; therefore the villain who sought to kill in cold blood acted more slowly than the hero who reacted spontaneously. To test Bohr's theory in a scientific manner, the group bought two toy guns. The theory was duly tested, and verified, with the Russian George Gamow in the role of the villain against the hero Niels Bohr.[4]

However, Bohr did not agree to all the younger physicists' pranks. The Danish physicist Christian Møller reports, for example, that Bohr did not object to a game of Ping-Pong in the institute's library; it was Gamow's insistence on using books as paddles that he objected to. Likewise Frisch, reflecting upon Hendrik Brugt Casimir's swim across a Copenhagen lake fully clothed, considered such behavior inconsistent with Bohr's personality and his running of the institute. Rather apologetically, Frisch suggests that such "childish" behavior by the physicists should be explained as part of their special qualities as human beings: "A scientist *has* to be curious like a child; perhaps one



Animated discussion at the blackboard at the Institute for Theoretical Physics, c. 1930. Left to right: Bohr, Pauli, Lothar Nordheim, and others. [Courtesy of AIP Niels Bohr Library: Landé Collection]

The audience gives rapt attention to a lecture at the institute, c. 1930. Left to right: Jordan, Pauli, Heisenberg, Bohr, and others.

can understand that there are other childish features he hasn't grown out of.'' Bohr's special ability to deal with younger physicists, Frisch seems to imply, was not that he encouraged such childish behavior. Rather, realizing that this behavior was a necessary expression of the physicist's questioning mind, Bohr was wise enough not to discourage it. In all of these accounts Bohr appears as a benevolent father watching his children in amusement from a distance, politely informing his younger colleagues of his disapproval when the play went too far.[5]

The image of Bohr as a father figure is expressed more fully in the many accounts of how he communicated intellectually with his younger colleagues. For many physicists this was the most valued aspect of the Copenhagen spirit. The most common and efficient context for communicating scientific (and nonscientific) ideas in Copenhagen was not formal lectures and seminars. The encounters with Bohr remembered most vividly consisted of personal discussions, which could take place at the institute, at Bohr's summer house, or during sailing trips along the Danish coast.[6]

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Excerpt
More information

Margrethe and Niels Bohr outside the Carlsberg mansion.

