CHEMICAL HERITAGE FOUNDATION

# HALDOR F.A. TOPSØE

Transcript of an Interview
Conducted by

David C. Brock and Leo B. Slater

at

Copenhagen, Denmark

on

19 April 1999

(With Subsequent Corrections and Additions)

Haldor Topsøe

CHEMICAL HERITAGE FOUNDATION
Oral History Program
FINAL RELEASE FORM

This document contains my understanding and agreement with Chemical Heritage Foundation with respect to my participation in a tape-recorded interview conducted by David Brock and Leo Slater on 19 April 1999. I have read the transcript supplied by Chemical Heritage Foundation.

1. The tapes, corrected transcript, photographs, and memorabilia (collectively called the "Work") will be maintained by Chemical Heritage Foundation and made available in accordance with general policies for research and other scholarly purposes.

2. I hereby grant, assign, and transfer to Chemical Heritage Foundation all right, title, and interest in the Work, including the literary rights and the copyright, except that I shall retain the right to copy, use, and publish the Work in part or in full until my death.

3. The manuscript may be read and the tape(s) heard by scholars approved by Chemical Heritage Foundation subject to the restrictions listed below. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of Chemical Heritage Foundation.

4. I wish to place the conditions that I have checked below upon the use of this interview. I understand that Chemical Heritage Foundation will enforce my wishes until the time of my death, when any restrictions will be removed.

**Please check one:**

a. ✓ **No restrictions for access.**
**NOTE:** Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to obtain permission from Chemical Heritage Foundation, Philadelphia, PA.

b. ______ **Semi-restricted access.** (May view the Work. My permission required to quote, cite, or reproduce.)

c. ______ **Restricted access.** (My permission required to view the Work, quote, cite, or reproduce.)

This constitutes my entire and complete understanding.

(Signature) Signed release form is on file at the Science History Institute

(Date) 04-04-2000

Revised 4/5/1999

> Haldor F. A. Topsøe, interview by David C. Brock and Leo B. Slater at Haldor Topsøe A/S, Copenhagen, Denmark, 19 April 1999 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0180).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

# HALDOR F.A. TOPSØE

| | |
|---|---|
| 1913 | Born in Copenhagen, Denmark, on 24 May |

## Education

| | |
|---|---|
| 1936 | degree, chemical engineering, Danish Technical University |

## Professional Experience

| | |
|---|---|
| 1936-1939 | Aarhus Oliefabrik A/S |
| 1939-present | Founder, Haldor Topsøe A/S |

## Honors

| | |
|---|---|
| 1944 | G. A. Hagemann Medal |
| 1968 | Honorary Doctor of Philosophy, Aarhus University |
| 1969 | Honorary Doctor of Technology, Danish Technical University |
| 1982 | C. F. Tietgen Medal |
| 1984 | Queen's Medal for Meritorious Services |
| 1985 | Royal Academy of Sciences Gold Medal |
| 1986 | Honorary Doctor of Technology, Chalmers University |
| 1988 | Odre National de la Légion d'Honneur |
| 1989 | Francis New Memorial Medal |
| 1991 | The Hoover Medal |
| 1996 | "Order of Intellectual Capacity," Morocco |
| 1997 | Eminent Chemical Engineer Award, Delhi, India |

## ABSTRACT

Haldor Topsøe begins the interview with a discussion of his early life and family background. Born in Copenhagen, he grew up in Denmark, and was very involved in his father's Samfundshjælpen, which taught him the importance of collaboration between social classes. Topsøe studied at the Technical University, taking numerous courses in physics, chemistry, and chemical engineering. When he married in 1936, he became involved in his father-in-law's activities in teaching young people to run businesses. As a chemical engineer, and later a businessman, Topsøe gained an interest in the relationship between economics and science. He discusses his firm's involvement in catalysis, how Haldor Topsøe A/S began, and the scientific research that had previously been done on catalysis. Topsøe further discusses the transfer of technology to India and the Third World, the impact of the Green Revolution on chemical industries, and his company's work in refining. He concludes with comments on the future of innovation.


## INTERVIEWERS


Leo Slater is Director of Historical Services at the Chemical Heritage Foundation in Philadelphia. A former research chemist at the Schering-Plough Research Institute, he received his doctorate in History from Princeton University in 1997.

David C. Brock is Associate Historian at the Chemical Heritage Foundation in Philadelphia. He is currently a Ph.D. candidate in the History Department, Program in the History of Science at Princeton University. In 1995, Mr. Brock received his M.A. in the History of Science from Princeton University and in 1992, he earned a M.Sc. in the Sociology of Scientific Knowledge from the University of Edinburgh.

TABLE OF CONTENTS


1 Family Background and Education

Growing up in Copenhagen. Father's involvement in Smafundshjælpen. Political beliefs. Denmark's political atmosphere. Importance of collaboration. Studying at Technical University. Interest in physics, chemistry, and chemical engineering. Marriage. Sharing father-in-law's involvement in training young people in business.

3 Early Career

Interrelation of economics and science. Input/output analysis. Economists as advisors. Interest in catalysis.

8 Forming Haldor Topsøe A/S

Financial help from family and friends. Importance of optimism. Relationship between physics and chemistry. Niels Bohr and the Copenhagen School. Previous research on catalysis. New instrumental techniques.

16 Technology Transfer

Interest in the Third World. Connection to agricultural research. Influence of the Green Revolution. Giving technology to India.

19 Petrochemicals

Work in refining. Ralph Landau. Interest in hydrogenation. Constructing a refinery in Kuwait. Downsizing of large petrochemical firms. Fischer-Tropsch process.

22 Conclusion

Importance of research. Trouble with niche products. Corporate difficulties with shareholders. Emphasis on profits rather than research. Importance of producing good scientists and engineers.

29 Notes

30 Index

INTERVIEWEE: Haldor Frederik Axel Topsøe

INTERVIEWERS: David C. Brock and Leo B. Slater

LOCATION: Haldor Topsøe A/S
Lyngby (Copenhagen), Denmark

DATE: 19 April 1999

BROCK: Perhaps we could begin by talking about your childhood and your family, and then move on quickly to your pre-college education. You were born in Copenhagen?

TOPSØE: Yes. My father [Flemming Topsøe] was a military engineer and the most interesting thing about him is—maybe—that in the early years of this century, he led an effort to create an organization that could, as you put it, keep the wheels running during general lock-outs or general strikes. That organization developed into maybe the largest private organization in our country [Denmark], and it had a large number of members in all parts of our small country. It actually served a role after the First World War, where, for many political reasons on the continent and here, there was substantial effort to redo society, to dislodge our monarchy, also, and convert it into a radical leftist society. By the way, this effort took the form of big fights everywhere in Europe—in the labor market, general strikes, lock-outs, local disturbances, et cetera, where, in a way, one took society as a hostage to the interests, particularly, of the leftists. We had a strong leftist movement. We had a crisis, or several crises, just after the war that caused changes in government and movements to change this country to maybe a republic.

This [Flemming Topsøe's] society, called Samfundshjælpen—it means "Assistance to Society" or something like that—kept the wheels running and it was, shall we say, created in some other countries on the model created in this country. The important thing was that: a) it had to be very broad; b) it had to be so organized it could really do things on short notice; and c) it had to be privately funded without any monies coming from one or the other sector of society. But of course, basically, it had a conservative role in the manner that it wanted to maintain, shall we say, the good things in the structure we had, and to, by evolution, modernize. This Samfundshjælpen was such an organization—such organizations were also created in other Scandinavian countries, and in Germany and England. In Germany, it grew to be very, very big, and unfortunately it was used much later by [Adolf] Hitler as a vehicle to create his SA [Sturmabteilung, or Storm Troopers]. In England, it was headed by Lord [John Rushworth] Jellicoe and also had not a negative role. Obviously, therefore, I grew up very much in the atmosphere that this activity created and it was a very substantial part of my father's life.

It was also interesting in another way, because in Denmark—as, by the way, in these other countries—it created a climate, you might say a vehicle, for all non-revolutionary political quarters and parties to gather and say, "Fine. We can fight, for instance, in the labor market; we can fight for working conditions, for wages, and what-have-you. But we cannot take society as a hostage. We must stop ahead of that." Of course, that's a nice idea, but if you really examine it, it isn't completely possible to avoid that. Now, for instance, we have hospitals taken hostage—society is taken hostage by a threatened strike by the personnel in hospitals. But to a certain extent, it [Samfundshjælpen] was successful. One could, across a broad spectrum of political parties and groupings, work together. It was quite amazing for me that the leaders of the Social Democrats—which at that time was considered by established society as quite leftist—they came to be very good friends of the family and my father, of course, and later myself.

You might say that this taught me something: when it came to important matters, to basic fundamental questions, it was amazing how you could gather 80, maybe 90 percent of the population around similar views, although some came from the extremes. Some came from the military establishment, some came from workers' groups, and so on. Of course, for many of us, that meant that it was, shall we say, a goal in itself to create real understanding and collaboration between this 80 percent. It also showed how, if you took the old, pre-First World War establishment run by a fairly small group, you know—in European countries, and by the way, also in the States—then it was no longer possible to base development on the views of this establishment. You had to bring in, shall we call it, the worker class? I hate the word, but, okay? Therefore, it's always meant a lot to me that even in daily life, one has to accept the necessity of collaboration between all groupings—from the man on the floor to the professor in his cathedral. In a way, the whole circle, to which I think my wife and I feel we belonged, was quite a broad circle. They shared views—some even came from communism. You might say they were reformed Marxists or got to be reformulated Marxists. This broadness meant a lot to me, and still does.

We were two brothers, and I had a privileged family.

BROCK: It's easy to see how your interest in economics, science, engineering, and technology would come out of that excitement surrounding your father's activities.

TOPSØE: Yes. To a certain extent, yes.

BROCK: To a certain extent. What I was wondering was, in your early formal education, were those interests that you could follow?

TOPSØE: Well, not during my formal education. I started at university here, Technical University it's called today, and at Copenhagen University, and spent equal time with physics, chemistry, and chemical engineering. But we had many, many study circles around 1930 where you could follow these interests. When my wife [Inger Veng Topsøe, neé Kunst] and I got engaged to be married, then her father's activities came into my life. He [Aage Kunst] was, for many years, the chairman of a private organization that was responsible for education of young people in economy—in practical economy, not so much in university-type macro-economy, but in teaching young people to run businesses, to sell and buy, and to analyze the economics of their business. The activity of this private organization grew to be very large, and before the Second World War indeed was instrumental in creating a university-level school [Handelshøjskolen] dedicated to teaching and to studies about practical applications of economic theory and related disciplines. It is still growing and of great importance in Denmark. I got involved also in helping him, or in discussing with him, and the group around this NGO [non-governmental organization], how best to create education young businessmen. The organization had a peculiar name. It was called Foreningen til unge Handelsmænds Uddannelse, and literally translated that means "The Society for the Education of Young Business People." They have a large number of professors now.

My wife and I got engaged to be married in 1933, you see, and in those years you were engaged to be married, and only—after some years—when you could support the girl, you married. [laughter]

This was the time when this Society grew rapidly and there was a growing understanding—although slowly growing understanding—everywhere in Europe—in the States it was different—that any industrial scientific activity, or anything like that, in a way had no meaning if it wasn't married to business, to doing business, and to what we in this company [Haldor Topsøe A/S] call "business engineering," creating business through analysis and development. So you see, these years when I started the company, I also had opportunity to follow the development of education in business economics, you might call it.

I also took a great interest, through study circles and otherwise, in the different ideas on how far you could convert macroeconomics studies into a science, which of course has not been successful so far, and will never be completely successful, by the way. But I was interested in the possibilities that logical thinking and logical analysis and statistical studies have to create a picture of macro economic development—actual situations and potential for business creation. Already during my study years, I got very much involved in that by participating in circles of like-minded individuals. Some of us later got jobs in applied economy—banking, civil service, or politics—but very few of us at the time went into the business community. One member of these circles joined the civil service, where he worked to be "Mr. Economics" in Denmark, Mr. Erik Ib Schmidt, with whom I had a friendship since early school days. Many of us were particularly interested in studying how the sciences and economics could work together and we asked ourselves questions like: What information about economics should be available to business? What is the validity of economic information? For how long a time can an economic study, for instance of a country, be gainfully applied? What is the interaction between development in the scientific community and in international macroeconomics? How do scientists and economists collaborate? Is the engineering profession a good catalyst for such collaboration? I, myself, even took time to write a book about these matters, *Danmarks Productionsliv omkring 1935* [*Economic Life of Demnark around 1935*] (1), and here I focused on input/output analysis. Later people have been kind enough to say that this was the first modern study of input/output used as tools for describing a region or a country. In that relation, I established relationships in different parts of the world and also with economic faculties in different universities, and indeed people came to study and discuss.

I do think it is natural to undertake such studies if you are interested in the role of the sciences to bring about economic and social development. Indeed, you might say that scientific and economic developments have only scant meaning if it is limited to development of competence or knowledge. Particularly between war years, where you had so many severe social problems and crises encompassing most of Europe, it was difficult to abstract from the role the sciences could have in bringing about developments that could help solve these dreadful problems. I am not saying that it isn't wonderful to know how the ultimate matter's ultimate particles are behaving, but if you look apart from "science for science"—which, of course, I love—then obviously it is a duty of the scientific community to at least participate in forming a basis for social development, which is then made possible through applied science, technology, and industrial developments, et cetera, including trading. All that necessitates, if you can not in a way convert science and technology into economic activities and describe this interplay in economic terms.

Surely, if you develop a new process—after the war, I was much interested in such developments in the States, where a good friend of mine, Ralph Landau, was a successful leader.

Then, obviously, at a certain point, you come to a crossroads, where the possibilities depend on science and technology and the results on applied economy. To put it in another way, you might also say that it has no meaning for private sector purposes to do fundamental work if you don't look at: How do you make your first sale? That's money. How do you finance the first plant? That's money. What impact does it have on the market? That's money and social impact, and so on.

I must, however, admit that for those years after graduating, I very much hoped that I could stick with "science for science" and get going to the university. But because of the crisis (the economic depression) that wasn't possible, so I had to go into business. Then it had no meaning to work with the sciences—study theoretical physics, as I wanted—if you did not have an economic, financial knowledge, competence, and interest that could help you in building a business anchored in scientific activities—R&D. What I have tried to explain—my father's work, my father-in-law's activities, our circles' interest, attitudes, and activities—meant that the company we set up from the beginning until this day has equally emphasized science and business. The people we admire here are the professors and the salesmen, equally.

BROCK: If I could ask another question about the input/output method or technique. I was interested to read something that you wrote comparing your work of 1935, using input/output methods to analyze the economic situation of Denmark, to the energy and material calculations that the chemical engineer does routinely. Is that something that you were consciously thinking about at that time?

TOPSØE: I didn't catch the question.

BROCK: The connection between the type of analysis that you were doing with your input/output method in economics, and what you were studying in physical chemistry, physics, and chemical engineering.

TOPSØE: Yes, well, it sure has a connection. Because if you want to describe the situation of a country or a society or a group of countries, and how you could expect it to develop—how the impact on social developments and economic developments would be for new technologies, for instance, or new energy situations—then, of course, you had to know what would be the interaction between all sorts of input factors upon the output in any sector of industry. My interest was, therefore, to gain an understanding of the interactions—meaning in this respect things you can measure—between agriculture, industry, small-scale businesses, households, what-have-you. Such a picture of an economy, say of a country, could only be done by assembling a very large number of statistical data. The nice thing would, of course, be if you could carve it up into very small sectors. It's just not possible to do that because statistical apparatus does not permit that. You cannot get valid data if you split up in too-small sectors. So I tried to split it up in a number of more or less traditional sectors.

Of course, you have the problem of: what is the validity of such a picture? How long will it last? What will push it to change? Many things besides progress in science and technology would push it to change—it's very difficult to evaluate. Particularly psychological reactions, one of the main reasons why economy will never be a science. Take, for instance, the Green Revolution. That completely changed agriculture with new seeds, enhanced use of fertilizers, insecticides, and what-have-you. That completely changed the macro economy of many regions and could not be foreseen. Take the transistor, for instance, or new developments in automotive technology. Therefore, I thought that it was interesting if you wanted to do work which, based on science, could come up with new processes, new products, to see what would that—for social reasons, or general economic reasons, desired changes in society—demand from technology and science? And vice versa, new scientific discoveries: what would they eventually lead to in production, products that would impact society, and how would they do that? I was very much interested in also seeing how that would work, not only on a national level, but also on international levels.

In other words, I tried to identify that part of the economic activities that could be analyzed and where you could put in numbers you were sure of, and then find out what the limit of validity for such an analysis was and what would change the situation. I thought that was very interesting, and I have still maintained relations with many people around the world who have been involved in such matters. Unfortunately, many economists fight a meaningless battle, trying to make all of us believe that economics really is a science in the way you term physics a science, for instance. The beauty of economic activity is that it is not a science. Economics is a tool to study situations and study possible consequences of new situations. It's a beautiful tool for that. But for many reasons—including the fact that you cannot put numbers on the market's reactions, on people's reactions, in a democracy like ours, the ultimate decision makers' motives—economists' advisory possibilities or forecasts can never be based on solid fundamentals. But nevertheless, we need them.

It's always been an unpleasant thing for me that there's almost a complete watertight separation between the sciences, which can and do deal with facts—very often misinterpreted, very often incomplete facts, very often influenced by the desire people have to see certain results, like, for instance, with all the environmental people, the Green people—and economists. But you simply have to find a way of making use of the economists in collaboration with the people working in science and technology. It's always been a mystery to me that they don't—they cannot talk to each other. As I said, the way the economists try to advise us is dangerous. Governments, all over the world almost, have an economic advisory board. On this board there are only economists. That's ridiculous. We also live with the never-ending discussion between different economic schools—coming to opposite results and advice.

SLATER: Very interesting. From the historian's point of view, we see a lot of economists in the late nineteenth century coming out of Britain and Germany, in particular, where I think they get their ideas that bother you so much. They see the success of thermodynamics and electromagnetic physical research and then they look to those models as a way of making their own science scientific. It's interesting to hear you, as an engineer, talking about these models, but with a very different set of expectations than the economists had in their early, early years.

TOPSØE: But see, of course, it's very interesting. You can, as we all have to, see to what extent economic tools are useful for a country or company.

I've always hoped that, somehow, the vision of "One World" would come true. We are very far from that. Sometimes we are working in the opposite direction. But nevertheless, we see strong attempts to globalize the economy. But if you look at a company, it is, of course, very important today, with the international competition, to at least understand economic development, social development alternatives, to watch: is this market emphasizing employment? In which case, you can expect certain prime-the-pump activities and so on. Is this market looking for strictly [Milton] Friedman-type policy? In which case, you can expect an emphasis on other matters, for instance, preference for business in the private sector instead of seeing governments as businessmen, which I personally dislike. Not because you can't have a government running business, but you cannot find many politicians capable of doing it. [laughter]

BROCK: I would like to ask about how you came to your interest in catalysis, specifically.

TOPSØE: Yes. Well, surely, I did spend time on what you could call social economic employment matters and so on. I should, if you would permit me, go back to one thing. You asked about my younger years, formative years. I don't know where I was formed. [laughter] But formative years, you asked a little bit about that. I can tell you one thing that people my age, if they're anywhere decent, will never forget. That is the social catastrophe of unemployment around 1930, and this caused Nazism and many other things, more than anything else. I had some English in the circle—we had some English friends, family—and some of them were involved in the steel industry. For instance, in Millsborough, practically everybody was out of work. In our rich country—because even in that situation we were rich because we had all we could eat and much more—you could see in our town here [Copenhagen] thousands of people standing, lining up in a queue to get a cup of soup. You'll never forget that.

You will never have the same view as some of these monetarists, who, of course, are not really meaning it when they say that they don't give a hoot about employment; they give a hoot about money availability and budget balance and so on. Of course, they don't mean it, because with a few exceptions—I've known many of them—they're not that sort of cynical people. They just mean that there's this other road to development and progress. But, I mean, a road can take so long that it's simply inhuman to travel that road. Therefore, whenever you think about all these things, if there is anything like a small risk of having a situation like during the depression in Western Europe—like Wales, Copenhagen, to say nothing about Germany (I will say that the refugees from Kosovo, they are no worse off than the suffering unemployed people then)—then you take another view about the economy. I have no doubt that many things can produce better results after ten years, but we have to look out in order to avoid social catastrophes on the way to paradise. I got very much upset when you found here, in recent years in Western Europe, that most of our political leaders didn't stress our unemployment. Then all of a sudden, a few years ago, they started finding that it may be a good idea—in order to get re-elected—to emphasize employment. Years ago, you couldn't even discuss unemployment with European leaders. Very often you had people who placed other ideas—like some neo-Marxist ideas people still have, or emphasis on certain economic indices—ahead of really day-to-day attention to employment. Excuse me for going back to that.

BROCK: No. That's interesting.

TOPSØE: This is a catastrophe. I'm one of the few ones still active who has really lived with this situation. When I started, most of us didn't get a job. My father said if I wanted to study, of course I could study, but I would enter an academic proletariat, which apparently was worse than the general proletariat. [laughter] Okay, you had a question about catalysis.

BROCK: That's right. I was just interested to know how you came to your specific interest in catalysis.

SLATER: Yes, right now you sound like an economist to me.

TOPSØE: I'm sorry?

SLATER: You sound like an economist right now, not like a chemical engineer! [laughter]

TOPSØE: Okay. Well, apart from, shall we say, the outlook I had from family, from friends, and so on, I took an interest, as a teenager, in different things. I didn't know exactly what I wanted to be. I've always had a little bit of a problem because anything you get involved in presented to me an interest *per se*, in itself. I don't buy this business that people say, "Geography. Oh, that's not my cup of tea. I'm not interested in geography." Or economics, or geology, or physics, or what-have-you. I don't buy that because almost anything, even far from my daily life—if you dive into it, it has an interest. So I've always had the problem that I felt that anything I heard about, "Oh, that must be interesting!" [laughter] Architecture! But I did get most interested in physics and chemistry, and the sciences. I saw physics and chemistry as almost one thing—I hope that's something that we may talk about later.

I wasn't terribly interested in engineering, but I was interested in physics and chemistry. But in order to marry, I had to do something where I could get a job. So therefore I took the exam from the engineering university—well, it wasn't called a university at that time—and then I got a job. But when war came, then this job was no longer available, so to speak. So we wanted to go to United States and work in catalysis. Unfortunately, the German occupation stopped our plans to travel, so we—my wife and I—said, "Well, let us see where we can put together something that can be ready when the war ends." We were pessimists at that time because I knew Germany very well and thought it wouldn't end so quickly.

I was considering either going into catalysis or into bio [biology]. It was enzymatic bio that I was interested in. That's also catalysis, you might say. Then we were a few people, all graduated from the university here, and assembled. I could find a very small sum of money, you would say today, but at that time enough to get started, and we were some five to ten people. We could, at that very early time, install ourselves in some laboratories. That's all described in the book (2). We then selected catalysis partly because all of us were, with different emphasis, interested in physics, theoretical physics, and in chemistry. All of us were interested first in the fundamental sciences and in bringing it into something you could earn your money on and survive.

Good. Let me say that we then assembled, and some of them who joined us got to be quite well known internationally. The last of the early friends, he has just turned eighty and retired [Dr. Anders Nielsen]. We did not want to live on support from government or things like that. We did not want to depend on such support. We didn't mind having some start-up help, obviously not, but we could manage with the help we got from family and family friends. Therefore, we had to think about how we could survive economically. Fortunately, we had some very good friends in Sweden and we could do most of our work, even in Denmark, for our outfit in Sweden.

We selected catalysis because of our interest, but also because we thought that catalysis would have a tremendous growth potential. That was correct, indeed, to say the least. Secondly, because we thought that we might have a chance of earning money in two ways: by bringing research results into the market in the form of catalysts we could manufacture; and in the form of catalytic processes; and—later—maybe a third way, also, in the form of specialized reactors. All this has proven to be possible. It would have been impossible to select an area where we didn't think we could earn enough money to continue and develop. That would have been impossible, and fortunately we thought we could select an area where we had our interests. Again, of course, the selection was derived from some sort of economic analysis. Why would catalysis play a larger role? Because of foreseeable developments, particularly in fertilizers and in oil refining. This is where we put our efforts—in these areas. We did not at that time think of ecology, where air cleaning is an important client to catalysis. Did I answer?

BROCK: Yes. If I could follow up on that, I would like to know what gave you confidence that you could come up quickly with developments in the field of catalysis.

[END OF TAPE, SIDE 1]

TOPSØE: Unjustified optimism was our main raw material, as it always has to be for something new. We thought we had the energy and the knowledge that would bring results. We were too optimistic as to the time and effort. Particularly after the war years, we were starting many research projects with too optimistic a view about how quickly they would bring results. But we had some luck and we had logically analyzed where to put our efforts. Then, of course, we were willing to live, to a certain extent, on a shoestring for many years. Well, you can also say something unpleasant. When we looked around the world, and some of us did know a good deal of the world already, we weren't enormously impressed by the efficiency, particularly efficiency, but also intelligence, of those we had to compete with. That was, of course, arrogant. But unjustified optimism and strong emphasis on work and arrogance—they are necessary.

SLATER: Confidence, anyway.

TOPSØE: Sorry?

SLATER: Confidence. I mean, you have to be convinced in yourself—

TOPSØE: I don't think so. As far as I'm concerned, I have great difficulty in being confident about things.

SLATER: Arrogance comes in!

TOPSØE: Well, yes, but I'm always very much pessimistic about things, and in a way, my standard dream ends up by me saying to myself, "How the hell can this go on?" Something like that. [laughter] Optimism and confidence are two completely different things.

SLATER: One is irrational! [laughter]

TOPSØE: Yes!

BROCK: Well, there are two things I'm very interested in hearing more about, one of which is your ideas about the relation between physics and chemistry. I'd like to talk to you about that, and I'd also like to talk about some of the early developments with the firm, the first technologies, that sort of thing. It's entirely up to you where you'd like to go next.

TOPSØE: Okay. Let us start with the relation between physics and chemistry. Now, if we go back to my study years and researches, then the scientific foundation of chemistry was rather scant. The, shall we say, best anchors were to be found in thermodynamics and obviously we had the benefit of the thermodynamics system of [Josiah Willard] Gibbs, and many others who worked with him or after him. In Denmark, we had the specific benefit of the work by S. P. L. Sørensen and later by [Johannes Nicolaus] Brønsted. Thermodynamics, of course, in a way is explicable only if you go into particle physics, and so on. So the main laws were formulated before people had a hunch about quantum physics, particle physics, and all that. Then you might say that, in a way, thermodynamics—classical thermodynamics—has a character of very useful postulation that really cannot be understood from basic principles. We had a very good situation in Copenhagen University, due to the work of a number of very fine theoretical chemists, by the school that Brønsted created, where we also had many foreigners coming, and not the least a large number of American chemists.

If you look upon the use of, the utility of, chemical science to developments at that time—obviously it's nice to know about thermodynamics. It's nice to know what is possible, and it's nice to know that the whole world around is thermodynamically unstable or meta-stable, or so on, and it really shouldn't be there if all reactions that were thermodynamically possible took place. That's very nice to know and you feel very impressed by this business that the whole world around us is unstable. [laughter]

But what, of course, one didn't know too much about at the time was kinetics. Surely some work had been started in different parts of the world to understand kinetics. In heterogeneous catalysis, the interaction between surfaces and continua—on both sides of the surface, maybe—has to be understood, whereas in homogeneous catalysis, fundamental patterns are different. We were interested only in heterogeneous catalysis, so let me stay with that. In the 1930s, quite a few came up with all sorts of ideas, but they were of a very speculative nature, in a way. I mean, if you can catalyze combustion by putting a high-surface compound—platinum maybe—into a gaseous mixture, then it's easy to say that there must be some parts of the area that are responsible for this. If it was a completely homogeneous situation, it would either be bang or nothing. Therefore, researchers came up with the notion about active sites and also thought that in catalysis, acidity would be of importance, and one looked at the different types of acidity. Even today one is not, I would say, on a completely sound footing when talking about acidity. If you look away from simple questions in aqueous surroundings, then it's not that easy to say what one means with [Gilbert N.] Lewis and Brønsted acidity.

But one spoke a lot about that and we thought that was interesting. Then, of course, one was forced to look a little bit more upon what is really a surface. At that time, the interplay between fundamental physics and chemistry was not too efficient. That mainly was because in physics, you had this tremendous development starting around the turn of the century, with the idea of quanta, relativity—[Max Karl Ernst Ludwig] Planck and a few others starting it, and then [Albert] Einstein and [Niels Hendrik David] Bohr coming up. One therefore took great interest in the advent of particle physics, quantum physics, relativity physics, and all this meant for chemistry and catalysis. One hoped to understand, for instance, the energy situation around particles and relate this also to chemistry.

Much later, as we all know, the advent of isotope chemistry with [Harold C.] Urey and [George C. de] Hevesy opened new avenues for progress. Hevesy was a close friend of the family and his ideas were of great importance to us. You then came into the creation of artificial isotopes, radioactive isotopes, and you got the whole business of fission and—well, maybe it comes into practice, maybe it doesn't—fusion physics.

People were so enthusiastic about that, and really it was a fantastic development. One had, here in Denmark, the great advantage of the so-called Copenhagen School around Niels Bohr. Niels Bohr was my teacher. He and his family got to be close family friends. Of course we were all very enthusiastic about Bohr and the Copenhagen School. Indeed, if I had been able to select, I would have liked to join.

Now, this meant that the work one did in physics was of only scant use to chemistry, with the standpoint chemistry then had. I would venture to say that I have known only a couple of chemists from those years who could even understand what the physicists were doing. But Brønsted and his school did not really focus on what theoretical physicists were doing, and did not see it as a tool for understanding chemistry.

Then another thing was of importance: that from the science community and from the university community around the world, physicists had much more support, interest, and money than the chemists. They rarely took interest in solid state physics. You can be a little bit naughty and say that it was maybe difficult enough for them to deal with the atom model and to elucidate the interplay of particles—atoms, electrons, neutrons, et cetera. The much more complicated structures you have in a crystal was beyond their interest, and beyond the capability of anybody at the time. You may also talk about computations: it is not a simple thing to compute the energy situations in the heavier atoms, but beyond the possibility to make computations of energy situations, stabilities and so on for complete solid state structures—and the continuous solid state is again a simpler state of material than the surface. So if you could gain a Nobel Prize by work in theoretical physics, in quantum physics and so on, then why go into areas that are of interest to chemistry and very, very complicated?

Well, if you look upon catalysis, there were quite a few physicists, after all, who laid a foundation for what later has been very useful, and again I would mention [Enrico] Fermi and Hevesy. I don't think it's my job today to go into that. So you ask what I thought was the interplay. In the 1930s, I would say the interplay was not very efficient between theoretical physics and theoretical chemistry. We thought, when we started, that there was far more to be learned by and useful for chemists interested in catalysis than so far had been, shall we say, realized. I'm not talking about just the few Danes we were in this early stage of the company, but also our friends around the world. There were amazingly few people who were interested in that field of—fundamental chemistry may be too strong a word—but that field of chemistry that was called catalysis. For instance, it wasn't until one of the last years before the war that in the United States the first group was set up focusing on catalysis. I think we called it the Philadelphia Club, and if I remember correctly, the first year sixteen people assembled. Now international catalysis societies can bring thousands of people together. It's interesting to note when you look upon such associations that, already in the 1930s, one needed a network where people came with different luggage. You needed mathematicians, for instance, to tell you what you could and couldn't compute, because you couldn't handle mathematics the way that, for instance, Brønsted did, when he said, "Oh, well, the integration of this equation will probably look like this!" [laughter] It's amazing to see that—I think this American grouping was maybe one of the more interesting—in these very early years in catalysis, one did have a network, although very small. Regretfully, they did not have much knowledge or interest in theoretical physics. They were people like Otto Beek, [Adalbert] Farkas, Gustav Egloff and other Russians who had come to the United States like [Vladimir N.] Ipatieff and so on, but you can always look up who they were.

Now, then, we thought around 1950 that maybe the contribution from physics to catalysis was such that an understanding of catalysis, of the mechanism and the state of the catalyst, was around the corner almost. Some people even said that it wouldn't be too far into the future when one could sit and engineer a catalyst, and also sit at the desk and find out how a reactor would look. Okay. It didn't happen. [laughter] It is maybe about to happen in a very limited way. But when we had this, again, unjustified optimism, we spent an awful lot of effort and money on putting together experimental units that could help in this, and looking into all sorts of spectra and what-have-you. I won't delve into the details of that because you can find that easily.

But we were, of course, in a way hoping that you could study the catalysts, with a focus on the surfaces, sufficiently in detail by these new techniques—ultra-vacuum ESCA [Electron Spectroscopy for Chemical Analysis] spectra, Raman and infrared, and a little later, synchrotron studies—and then get an understanding. So again, we had this unjustified optimism. But in spite of the fact that it was too early, things came out of this effort. In our company, a good deal later, and headed by some of my colleagues, including one of our sons who was working here, Henrik [Topsøe], we then thought that we could realize an old desire in different manners. The old idea was to try and use physical measurement of a catalyst *in situ*, meaning that we had used normally a small reactor—it could also be big—where you could study the kinetics of the reaction, side reactions, and then at the same time make physical measurements of the catalyst.

Actually, some of us had thought about this in a very primitive way. We thought one could even take a huge big reactor, for instance a shift reactor, and put it on a support that would allow you to weigh with kilo accuracy and then simply see how during activation the weight of the catalyst changed. I'm just saying that in order to use such, in a way, advanced ideas, you don't necessarily have to have access to big physics or million-dollar equipment. We had studied, during the war already, the *in situ* behavior of vanadium catalyst for oxidation of $SO_2$ and had done that by quite simple spectroscopic means and found a lot about what the catalyst was really composed of during reaction, and the different parts, situations, throughout the reactor. We had some pleasure publishing that. Nobody read it during the war, but after, people found that that was maybe the first *in situ* examination of kinetics and physics—simultaneously. Thereafter, we emphasized *in situ* work all the time. We said, "You know, we are less interested in learning how a catalyst is composed when you have made it or when you evacuate it from the reactor. We're interested to know how the catalyst looks under actual operating conditions—*in situ*." I think maybe I can say that we took some important initiatives here.

Certainly later, when this got more sophisticated, some of us of the group here did some pioneer work. Part of the work is done, as I said, with more primitive tools. One push ahead was made possible through the advent of Mössbauer spectroscopy. I don't know whether you know about that. Well, [Rudolf Ludwig] Mössbauer is a German physicist who got a Nobel Prize for his work to see how you could use the spectra emitted from an atomic transition to measure very small changes in the situation of certain atoms. Iron is an atom well suited for Mössbauer spectroscopy and make an iron compound. The Mössbauer method is elegant because the energy changes one can measure are very, very small. Therefore, if you emit radiation from a radioactive iron, then want to absorb it by iron in the same state as the emitter of iron, you'll just have to keep them at a fixed distance. But there is a little bit of change to the situation of the two iron atoms, then the emitted radiation is absorbed only if you move the two iron particles in relation to each other. You can therefore find the changes in the condition of the iron atom, under any change that it undergoes, by putting up such a Mössbauer experiment, and find the velocity or relative velocity in relation to the two atoms where you have maximum absorption. This is an extremely elegant method and the energies you can measure there are extremely small and it is one of the most exact measures in science to identify, for instance, energies and so on. It would correspond to being able to measure the distance to the moon with an exactitude of millimeters or centimeters.

So that, therefore, was very interesting for catalysis. Because the change in the energy situation for the catalytically active atoms is very small, unfortunately not so many elements can be used for that. But anyhow, the advent of Mössbauer, thirty years ago or something like that, caused some of us to study that. We indeed put up a Mössbauer laboratory here and other tools for *in situ* are available. We have practically our own lab at a cyclotron in Hamburg, and collaborate with other cyclotrons. We are the largest industrial user of the cyclotron installations in Germany, amazingly. You can use x-rays. You can use, simultaneously, x-rays and other rays. You can use Raman spectra, lasers, and many, many other things. We were the first to do certain of these studies where really you've got information. Our friend, Sir John Meuring Thomas, recently got a nice premium for certain combined studies, but we were a little bit ahead, as he also pointed out in literature. We are still most interested in this and maybe a little bit proud of the way in which we have been able to make this *in situ* work with many, many catalyst types, many, many reactions, and also with fractional reactions—because the catalysts at the top of the reactor have one situation, and at the bottom another. Kinetics are also different. Very often you cannot find one set of kinetics valid from inlet to outlet, where you are close to equilibrium. I would think that we are maybe about twenty groups in the world who pursue this. This is really a marriage between experimental physics and chemistry, where you can go out in the lab and see it, look at it. Here it is. Here we have the spectrograph, and here we have the reactor.

Another thing that is interesting is that in the meantime, the physicists who between the wars didn't take much interest in catalysis are taking interest in it; they are interested in the surface physics much more than they used to be, and we are privileged in Denmark because we have four groups working together—we participate in it—to find out to what extent quantum physics allows you to find the energy situation between gases, for instance, and surface. It is one of the sectors in the sciences where this country is leading, and it started years ago when a physicist worked here for some time. Now he is a professor at the Danish Technical University, Jens Nørskov. He showed how you could determine the energy changes when a hydrogen molecule approaches a metal surface. That was, of course, very theoretical, not generally applicable! But in the meantime, he and coworkers developed a number of methods enabling one to see the energy situation when more complicated molecules approach a complex surface of real interest for catalysis. Of course, this is what it is all about: how does a molecule approach a surface? How does a surface have to look to make it stick to retain it in a situation where the transformation required—activation required—for catalytic reactions to take place, and release the products afterwards. This is a situation that we hoped was around the corner thirty years ago, but we have only now come to the corner. It is certainly not a sharp corner, but we are in the corner.

Therefore, it is of great interest to us to, in parallel with our experimental work, participate in and follow theoretical work. So we established intimate daily collaboration with a few groups, in Denmark and outside, active in this field of theoretical physics and chemistry. We also sponsor the financial part of the work outside our laboratories. We are lucky to have established groups in the universities in Copenhagen and Aarhus and certainly have been lucky to create sufficient interest to establish at the Danish Technical University a group [ICAT, Interdisciplinary Research Center for Catalysis] dedicated to research in catalysis. These activities in Denmark fit nicely into the ever-growing international network of friends and colleagues active in catalysis. That is another answer to what you asked.

Now, there's been another development, so to speak, that has created the situation we are in. That is an interest in surfaces coming from other quarters than catalysis, particularly silicon semiconductors. We've been lucky that we also, at an early time, some thirty-five to forty years ago, took an interest and created a small plant making single crystal silicon. We therefore took a big interest in understanding the silicon surface, which to a large extent is where the action takes place. Not the least when the action is to put something on the surface—oxides, or what-have-you. We had to follow that up. We are still probably one of three suppliers, maybe the only open market supplier, of equipment for making very large single crystal through the float zone method, where you have the melt, not in a crucible, but you melt it through high frequency radiation in a moving band, you might say, between two cylinders that you rotate. We developed this technology at an early time and still it's not so bad. The American government, through the Pentagon, found that in the States, you didn't have anybody active in this technology—manufacturing extremely pure single crystals, which are of major importance, for instance, for sensors for military and civil purposes. We did study the nature of silicon surfaces in a way that you won't be able to in catalysis. In the silicon industry, you want to have big single crystals. In catalysis, you want to have very large surfaces, meaning that you want very small crystals. Therefore they're much more difficult to study. So one learned, we learned, in general—other people did more work than we—a lot about dislocations in the surface. Obviously it had always been a problem for our people working in catalysis. Whether this business of active sites that I mentioned—for instance H. [Hugh] S. Taylor, S. Y. Thomson, and J. K. Thompson—whether that had a meaning or was just some sort of a dream. We certainly also expected that different crystal planes would play a different role for any catalytic reactions, maybe very, very different. It has therefore been major endeavors for researchers to elucidate all the underlying phenomena determining how the normally very small, catalytically active particles played their role in the catalytic reactions, and we have had to follow not the least the many interesting developments in experimental tools related to this. We have had the benefit of access to practically all tools of interest. Let me mention just two: scanning tunneling microscopy, where one of the Danish groups at Aarhus University has done outstanding work, and the use of radiation from cyclosynchrotrones, where we have had the privilege of establishing intimate relations with facilities in Europe.

What I want to stress is that in this important field of catalysis, one has finally established a situation where, through combined theoretic studies, we are gaining understanding on the microkinetics level and on the atomic level. Let me end my long answer by pointing to recent developments where, with the use of scanning tunneling microscopy, you can really find out how complex molybdenum catalysts are arranged on the surface and even find evidence of the configurations of catalytically active sites.

Again, the understanding one is gaining can relate to the *in situ* situation.

[END OF TAPE, SIDE 2]

TOPSØE: The use of electron microscopy is also interesting. We do hope that we can develop methods allowing one to approach the *in situ* situation in spite of the very great difference between the high-vacuum pressure needed in the electron and the pressure applied for industrial catalysis. Based on this, we are about to install two modern electron microscopes and plan to set up a center for this work, hopefully in collaboration with the universities in the Øresund region, including Lund University.

BROCK: Other questions that I would like to ask you have to do with the development of your activities in technology transfer to the developing world, and also the evolution of your experiences in very large-scale projects and the relationship between those.

TOPSØE: Well, I don't think you could have grown up in the decades I grew up without taking an interest in the so-called "Third World." What caused our company to be interested in it, I think, is in a peculiar way associated with catalysis. Before the war, I was employed by a company making edible oils and therefore cakes, as fodder—soybean cakes and other cakes and what-have-you. In this connection we were active partly in this company's facilities and partly in other facilities—in developing new and better seeds. One point that was of interest was to what extent we could grow seeds as raw material for fatty oils—the triglycerides—in our countries, and we spent a lot of work on that, and other people spent a lot of work on that, and part of it was successful, partly not. But obviously that made you very much interested in how you could improve agriculture. One couldn't see how you could attain optimum productivity in agriculture—I'm talking about as products: wheat, rice, and specialties besides it—without a really good understanding of the interplay between seed qualities, water, fertilizers, and also micronutrients, and the interplay with insecticides, pesticides, and herbicides. That was a great and growing interest in our country. We had people who were pioneers in much of that work also. Also some people were pioneers in understanding how you should use fertilizers. Therefore, I had as luggage a good deal of connection with the agriculture research institutes in Europe and in the States, and Archer Daniels [Midland Company] in Midland was a collaborator for the company.

When we took this interest in catalysis, obviously the main petrochemical industry was ammonia—it still is—using about one hundred million tons of hydrocarbons a year as feedstock. We thought it was not possible to really get into ammonia without also having an understanding of the agricultural developments and all that. We followed very much all the work that led to the Green Revolution by this wonderful Norwegian Nobel Prize winner [Norman Ernest Borlaug], and of course, realized how the Green Revolution, with seeds necessitating big input of nutrients, a revolution in the use of chemicals in agriculture, and here not the least nitrogen and phosphate fertilizers.

As we were emphasizing ammonia, and still are—I think we are supplying technology to more than 50 percent of new units being built. Then we, of course, got very much interested in the countries where this Green Revolution was of particular importance. You see, I had a lot of connections with the sub-continent—which is, of course, the largest area for the Green Revolution—and had many, many friends there. Some years after the war, we could travel around in India, and of course you cannot see such a country, even today, just as you could not see the queues waiting for soup in the early 1930s, without feeling that it is, you could say, of great interest, even an obligation to try some way of helping. We had the privilege that some of us, in the circle, not just in the company, could join to help create these developments, and my wife and I at that time—it was a private family company; we had later to change it—we strongly felt that if we had a good technology for making ammonia, we should make the technology freely available to the Indians. That's why we transferred. Listen, we had a project to do that, and I think that was maybe a little bit special because we transferred not only state-of-the-art knowledge, but also an ongoing transfer of technology as developed, whereas—I'm sorry to say—a good many people did, in one way or another, transfer obsolete knowledge. Really, there was no great general understanding of the necessity to see such countries in the Third World develop and so there was a general feeling in the wonderful international financial establishment that was more dangerous than beneficial for the western world.

Well, you have to understand that many of these people had never been out there. They are sitting in their ivory towers and obviously, if you visited India in the early years after the liberation and saw the conditions, you felt differently. We are not idealists. We thought that was a good thing to do, an obligation, and the way we transferred it was that we enabled the Indians to build such plants with ever-reduced input from exports to India. On that basis, India has been able to develop—and also because of other things. I mean, we should be very careful not to exaggerate.

India has developed competence in a number of areas. It started in the fertilizer industry and when you first went out there and met them, if you met twenty, you had met them all. Some of them were extremely intelligent, of course. But those who were very intelligent were unfortunately not very practical and they lacked knowledge. I think that in this particular area, we pushed a little bit to the development of Indian knowledge. Now when you have a meeting with the fertilizer association of India, two thousand people come, and you have the largest group in the world of competent technologists in this area, which I think is nice. One cannot observe that this and other similar developments have any derogatory effect on the western world.

To the contrary, I should think that because of the Green Revolution and also because of the small input we—from the West—have given, India has reduced the problem of hunger. They are badly nourished, under-nourished, but they have managed in a way that they would not have been able to manage without the Green Revolution—with fertilizers! So I hope that you'll bring a strong message to all the holy Green people that if the fertilizer industry and chemical industry related to agriculture have done nothing else, they have saved hundreds of millions of people in the sub-continent from hunger. Then you can ask yourself what would have been the consequence of that.

So therefore—obviously, my wife and I, and the whole company now—we share this feeling that this was a very worthwhile effort, a good thing, but it has been paid for several times over by very extended friendships with wonderful people, very good relations. The pleasure has been to see all this develop, and the use in India of our technologies where we, as I said, transferred in certain way. But now we also do have some business. So it's maybe a combination of what we felt as an obligation, but also an opportunity to do something we should all do. It was fairly easy.

Now, therefore, you might say that catalysis at an early time for us meant ammonia, which meant fertilizers, agriculture, Green Revolution, and still will mean that. Obviously in the rich countries like this country, we can allow ourselves to even force people to cut back on use of pesticides and fertilizers and what-not because maybe it disturbs the marine biology and bio-diversity, and you get some more nitrogen, for instance, in the sea. We can allow ourselves all that sort of really poorly founded—scientifically poorly founded—attitude, but in the Third World, all this is a must. Maybe some of these things disturb the drinking water, but then you can purify the drinking water because you have chemical methods for doing that. But you cannot, in the world at large, allow yourself not to use industrial methods and products just because the Greens have found that maybe under certain conditions it can maybe create life for lobsters a little bit more difficult. Actually, that's what happened here.

You see, they (the Greens) forced Denmark to go through a two-billion-dollar program for conserving sea-water purity in the Baltic Sea and in the Kattegat, as we call it, by cutting down on fertilizer and doing all sorts of other rather peculiar things because some years back somebody showed on television five dead lobsters, saying that they had passed over because of the effect of use of some fertilizer in our agriculture. Yes. But that's a general thing. The Green people undoubtedly have a good case in many, many ways, because we were lax in many, many ways of attending to the environment. But lots of what they are saying is nonsense. Lots of it is uncertain, unproven science, although some is proven. But if you go back forty to fifty years, none of us who were interested in that [attending to the environment]—it so happens we were—none of us could even make a journalist interested in writing about the bad effects of diesel exhaust, for instance. None of us. We developed technology for protection of the environment—as quite a few others did—that received no interest whatsoever. Then, all of a sudden [in the late 1980s], Mrs. [Gro Harlem] Brundtland, the Norwegian Prime Minister—she was to be re-elected, she hoped—invented the Green propaganda as a vehicle for re-election. That's what it has been, to a large extent, until this day, and that's no way of handling problems that have a major resource impact, a major social impact, a major impact on whether you can feed people or not. So that's a poor way to deal with what is basically a good case.

BROCK: It seems that along with fertilizers, you have research programs in the energy area—these technical solutions to energy issues. I'm thinking of DME [dimethyl ether] and biomethanol.

TOPSØE: Yes, you see, we have for many, many years been active in things other than ammonia fertilizers. I don't think a company like ours can develop if you focus on a narrow area. But we have always focused on whatever had to do with catalysis, and a good many other things we worked on from the beginning, like sulfuric acid, methanol, hydrogenation, and oxidation. Many of these areas, we could not afford to follow it up ourselves, so in a way, we gave it away—the knowledge and ideas we had, in exchange for some business we otherwise couldn't have. If we had been very rich from the beginning, we probably wouldn't have developed because it's a danger to be very rich, but if we had had a little more money, we could have structured, for instance, the field of synthesis that Ralph Landau went into. For instance, we, at a very early time, were interested in refining. Immediately after the war, we built the first grassroots refinery built in Europe—after the war in Sweden. So obviously we took a big interest in refining as a field for catalysis. Your question was more what we think about the energy field. Refining, of course, is a very important part of the energy field for automotive energy and other areas, of course it is.

We have been interested in following developments of catalytic processes in refining. We have particularly been interested in hydrogenation, hydro-treating. Another case where we took complete responsibility for a refinery was around 1970. We were responsible for the first all-hydrogen refinery built in the world, built in Kuwait. There wasn't a stream that wasn't hydrogenated, so every stream was purified in anticipation of what the market would demand. When that was decided in the 1960s, you had no Green propaganda and all that sort of business, but there was already a growing feeling that one had to remove sulfur and obviously, to use, for instance, diesel oil with 2-3 percent sulfur, which is really not nice. That was, by the way, a surprising innovative initiative on behalf of the present Emir of Kuwait. At that time, he was just a sheikh, Sheikh Jaber [al-Ahmad al-Sabah], a very delightful person whom I know very well. In this situation, we were responsible for the refinery construction and so on, and one of our colleagues took the job of managing the refinery for a number of years after that. But we did not have all the required technology of our own. Therefore, what we did was to introduce hydrogenation technology that we knew about—developed by others like Chevron [Corporation], UOP [Universal Oil Products], and Gulf [Oil Limited Partnership]. That, to a certain extent, was a little bit unfortunate because we got a lot of knowledge there that for some time we thought precluded us from moving ahead ourselves. We therefore put our research programs a little bit on the back burner, until some years after that we finally were started up. But ever since then, we've been very active. We have our own process, our own catalysts for a number of these, and we intend to be one of the main players in refinery technology.

Well, this was unjustified optimism, but I think now the optimism is a little bit more justified and we are in the front line. You said we were involving ourselves in that. There are, of course, a number of reasons why we are. One is that it is catalysis, and we were, immediately after the war, also in many developments in refinery catalysis, our relations with Dobie [Carl D.] Keith and Gustav Egloff, early UOP, and all that sort of thing. The other thing is that now for a number of years—it's been much in the public eye—this reformulated gasoline and diesel business and all that. Again, with an awful lot of poor science as a basis for propaganda, but it's been much in the public eye. The third thing is that the general process technology we've developed for the chemical industry and so on can be introduced to a much larger degree than it had been to refinery engineering. Then, the latest thing—which happens at intervals almost cyclically—is that the big ones are downsizing their research. Many of them are licensing by the thousands university graduates and some are closing some of their research activities and so on. Those who are purely refiners, not in upstream activities, they are in many cases under very poor economic condition. For instance, Idemitsu [Petrochemical Company] is thought to be nearly bankrupt—after all, the largest refinery group in Japan. Therefore, it looks as if all those in refining are putting themselves a little bit in the hands of the UOPs and such people. We are probably the largest single group developing understanding of the fundamentals in hydro-treating catalysts. I think Henrik is considered one of the leaders there, and the group around him. It's also interesting to see that in refining, where, in a way, they have something they also called "Standard Refinery Engineering Practices," that they are more and more realizing that the processing units are not just black boxes that they somehow get. That they have to interest themselves much more for how they really react inside, how things are inside these black boxes and so on. This has benefited us greatly and also justified that we went into it, because I believe that many people think that one of my colleagues was the first to do well-founded reactor design work based on computers, Dr. [Jørgen] Kjær, who got many international recognitions for that. So that was as far as the refineries are concerned.

In other fields, obviously one of the main areas we are interested in is how to make good use of remote, cheap natural gas. It appears that most of the natural gas resources are in remote areas where you either have to build chemical plants or other uses for hydrogen and then export the products; or where you have to use the gas by re-injection, which is doubtful in some cases; or flare it, which of course the Greens don't like. I think it's also very wrong. Or try to tie it into international networks for energy-like pipelines. Now, vast quantities of gas, which have a very low value at the wellhead—where it's interesting to see where you can come from gas to liquids and export liquids. To export gases as LNT of course is a growing concern, ever developing, but it's very expensive. Landed gas at three dollars plus per million BTU [British thermal unit] in Japan, even if you start with a value much below one dollar in Qatar or Batam in Indonesia. Therefore there is, you might say, two dollars or more per million BTU free raw material. You can use the Fischer-Tropsch process—I believe I am the only living professional who has known both [Franz] Fischer and [Hans] Tropsch—the only active one, I think—and, as in South Africa, make synthetic oil. You can make methanol, which is a diminishing possibility unless certain things happens. You can make DME, which is probably the most attractive diesel substitute. So we are very much interested in that. We collaborate with BPAmoco [Company] and we expect that there might be such a project going in the near future for supply to India.

But you have the new normal situation that if you want to introduce something—normally you cannot introduce it by droplets. You have to build a plant—a big investment, in this case maybe a billion—and you introduce at a time when there's no market! So what do you do? One has a logistics problem. I think we are solving it and that it will happen. We are interested in building such plants. We're not interested in running such plants. We don't have a billion dollars. Not quite. We hope it will happen, but in such areas, you have to have a lot of patience. So besides arrogance and undue optimism, patience has been our main raw material.

Then, of course, there are also other things you might be interested in about the energy field. We are very much interested in power stations, where chemistry can play a role, *inter alia* in exhaust cleaning. We are very much interested in gas turbine areas. I've personally taken a great interest in it because I was chairman of SAS [Scandinavian Airlines System] for many years and I looked at some of their technical problems. I already, some thirty-five years ago, followed intimately the development of air engines and followed developments at Rolls-Royce [Plc], Pratt & Whitney, General Electric [Company] and what-have-you. I still see an interest for us in this field. We believe that we have processes that, combined with gas-turbo units, can increase yields. We think that there is a burner technology that is of interest. We think that there's a materials technology that we can introduce, and we think that there's an exhaust problem where you can maybe come with a partial solution through recovery of energy by chemical processes. For instance, if you want to use methanol as turbo fuel—now there would be too much of it because of MTBs reduction—then you can use the energy in the exhaust as input to the energy conversion of natural gas to methanol. There are many other things we're interested in within this field.

Then, of course, if you look upon mega-plants for using natural gas, you have projects visualizing the manufacture of more than 10,000 tons of methanol equivalent per day, and that is an awful lot of methanol. That is five times the largest existing plant. This is equivalent to a several-hundred-megawatt power station.

We're also very much interested in the combination of power and chemical synthesis. If you convert your energy raw material by gasification, one way or another, then you have up-front a synthesis gas. It's very cheap to run this once through a chemical process and then let the unconverted gas—for instance, unconverted from a Fischer-Tropsch unit—let that push a gas turbine, and you have a combined efficiency in excess of 60 percent. There are indeed many, many areas where you can combine chemistry and power. For years and years, we have always been forced to do it because the standard ammonia plant we built includes a 40 MWM power. We're building some bigger ones now.

SLATER: There's definitely an economics and geo-political input, then.

TOPSØE: You see why surely you come back to your early years, because you can't do anything like that without having macro-economic understanding, and a certain standing in it. You can't. You can't do that without having access to presidents, prime ministers, ministers, governments, and—well, I have had. You cannot achieve progress in these "mega-fields" without a combination of catalysis and engineering. Back to engineering! In the field of gas to liquids via Fischer-Tropsch, we have a very close collaboration with Sasol in South Africa, the only big player converting gas to liquids to the tune of converting thirty to forty million tons of coal a year to ten million tons of synthetic oil. We have revamped fourteen units in their big plant. Each of these units is equivalent to 1,000 tons of methanol a day. All right. It's making Fischer-Tropsch oil, but that means that in one place, we have revamped what is the equivalent of 14,000 tons of methanol a day. So we have the privilege of a very good reference for all that. As the front end in all this gas-to-liquids business, today may be based on our process for catalytic oxidation, then, we expect to be very, very rich. Unfortunately, it is always delayed there. It seems to be a constant distance to richness of about five years. [laughter]

BROCK: What do you consider to be most important for the future of the development of new technologies, research and development, innovation? What are the critical factors for the future?

TOPSØE: In the world, or in our company?

BROCK: Both.

TOPSØE: Generally, there are quite a few factors. First, the overall factor is the attitude that owners of companies—whether they are shareowners or private owners—take to research and development: in-house versus research and development in combination between external and internal sources, or research and development completely out-sourced. Actually, the situation is that for progress in technology-based industries, you do not need in-house research. The reason for that is that technology is dirt cheap. If you take any area, the fraction of the payment that goes to the research and development people—those who have been responsible for progress—and the total income from your technology projects is very, very small. Even with the highest license payments available, you are only talking about a very few percent. So in a way, technology, *per se*, is dirt cheap. If you consider patents rights, access to the use of immaterial rights and experience, just information in the form of drawings, specifications, what-have-you, that will allow you to produce something. Then it is really dirt cheap. This is sometimes surprising, but always not really realized in the community. The reason for it, to a certain extent, may be that when it comes to real, new, valuable technology research, research results and so on, then they come, in most cases, basically from efforts in the university community, the institute community. In these communities, really people aren't—when they start something new—immediately thinking about how to maximize profit from it and, in many cases, do not have a possibility, a channel to obtain income to themselves or to universities for new results. Therefore, all it takes for even a high-technology company, who wants to market high-technology products, to be in the front line all the time is that they have ongoing contacts and channels to a network of institutes, universities, and groups where new things happen. Also to a network of people like ourselves who, in a pure business climate, try and do research.

This access is easy but it isn't productive unless the company in question has a group of people able to evaluate research results, to see at an early time whether what is in the pipeline elsewhere can come out as something useful for them. Meaning that although it isn't necessary to do in-house research, it's necessary to have an in-house competence and capability to follow and have connections with developments in the university world. The university world does not want to see people just coming to scout for business. You have people who have made a business of doing that—as go-betweens, and so on. The university world doesn't want to see somebody coming to say, "You are very active in protein synthesis. Do you have something that we could use to earn money on?" Therefore, although in principle it is not necessary to do in-house research, it's necessary to be considered as a valuable, interesting, and ethical interlocutor, a discussion partner for that area. Normally that, of course, translates into somebody who also has to come with something on behalf of his company. What they have to come with maybe sometimes is ideas—very, very rarely—but always money.

So that's one point.

[END OF TAPE, SIDE 3]

TOPSØE: I'm saying that in order to be a high-technology industry, you do not need to do in-house research. But you do have to be capable of appraising whether or not a certain new high technology is useful for you. There's been a big misunderstanding in the industry, and it's almost a cyclic misunderstanding—coming and going—that basic industries are not so interesting, not so necessary. That is, of course, if it is a permanent view, sheer nonsense because how can you focus on non-basic products—if you can't make them without basic chemicals, for instance, or basic machinery. Therefore, I want to take issue with the view where you distinguish between research development and technological progress for what we call high technology today, and for non-high technology. There is no real difference. Whether you make basic commodity products or niche specialty products, research is always necessary to make progress. If you take such a basic field as ammonia, I can assure you that the plant you design today is very different from one you designed ten years ago. The advances in basics are maybe not mainly advances that would whet the appetite of innovation-oriented people—maybe all they are, and that's often the case today, are advances in the cost of investment. But reducing the cost of investment by just a few percent is normally of the same value as introducing new progress in processing, for instance. It's very rarely that you can find any one step in all the areas we have spoken about—and also in manufacture of catalysts which certainly should be considered a high-technology area—to find one single step coming out of one single research project that will have a greater importance than a single step that will reduce investment by a few percent.

So therefore, the whole issue of high technology versus commodity technology, that's a fake issue, nonsense, and has guided many efforts, and particularly support from people like the Brussels group in completely wrong directions. Now you see the herd, all the old-fashioned, huge chemical groups, leaving what they call "commodity" things, cyclic things—they don't want to have to do with that. They all want to emphasize specialty chemicals or niche products, and they will regret that and the cycle will go back.

That was point two.

Now, another bad thing is that in any company that is not family owned or owned by a small group of individuals or a single individual, the management and even the board normally doesn't know what the owners want. Do you think that [Lou] Gerstner knows what the millions of shareholders in IBM [International Business Machines] want? Do you think he cares about what they want? Do you think he cares about finding out what they want in a way that can guide him in his decisions and in the direction he takes? Surely they have analysts, but the analysts are certainly not representing the owners. They are people active in a profession, which at best is not contributing to our societies, and at worst, creating an awful lot of confusion, because an analyst—he should not have access, in the stock market companies we're talking about, that everybody doesn't have. So therefore, he has no knowledge that everybody doesn't have, and normally, therefore he would not be in a specific position to pass comment or advice related to a company. Of course, he may have a lot of general knowledge about this area that could be useful. But unfortunately, in more cases than not, analysts have somehow had access, or make believe that they have information that only they have, and in that case, that profession is illegal and highly unethical. Now, my point is, in this connection, that financial analysts, or any others working as an advisor in financials, are not a channel from what shareholders want to the company's management or board.

Now, the third thing is that in many cases, the shareholders don't care one hoot about the company. They're only interested in the share movements. Only interested in getting in and out. Therefore, you have a situation where in many cases, even the shareholders—very often temporary shareholders—take no interest in the long-range or mid-range welfare of a company. In technological fields—I'm not talking about retailing or anything like that, or fashion industries—but in fields of what I would call bona fide industries, you need long-term programs, but shareholders don't even have an interest in that. Even worse, the shareholders today are, to a very large extent, people who operate or manage big funds, pension funds, life insurance, what-have-you, who invest everywhere and where they are measured only by almost the month-to-month value of their portfolio. If you think that the Western world can survive that short of short term-ism, you are certainly wrong. Even in cases where a company has a reasonably limited group of core major shareholders, it is very, very difficult to observe any situation where such a group tells the management in explicit terms what they want for the company. The sum total of all this is that more and more industries in the Western world are looking to short term results, to the next day's published market values and dividends, and more and more neglecting creation of value. That means that more and more companies are forced to cater for such views. That does not mean research and development, unless you are so lucky that you have geniuses employed in your R&D group. Because normal people, like all of us here, cannot manage to bring out results from idea to first practical use in less than three, five or often ten years. This whole attitude, this whole development in going more and more from family-owned companies to multi-owner companies, to shareholder companies, to fund owners and so on, is certainly with necessity bringing the Western world into a poor competitive situation.

So that's the worst, as I see it. Now, obviously, we were a family company. We are still 50 percent a family company. The family is still the main decision maker. We are here—and therefore, I may be a little bit subjective in my views—but I think it's very, very important for us to create a situation in the business world where one re-emphasizes the value of the long term, and finds ways of having long-term attitudes, irrespective of whether we have to accept that, more and more, the capital is in the hands of pension funds and life insurance companies, and somehow make sure that they understand that they must eventually also develop long-term people. It's ridiculous that this is a difficulty, because if you take, for instance, the Western world, I think we have about ten trillion—I'm not sure about the figure, but I think we have about ten trillion dollars in funds of this nature, all sort of funds. In my country, we have about one trillion Danish kroner, so I think it's a reasonable figure. Now, that's a very, very large share of the capital available for the businesses, and it's a growing fraction because we want to have good pensions and things like that. One should think that those people who are managing pension money should realize that these monies are monies that will be paid out from next year to fifty, sixty, seventy years hence. I should think that they would understand that they have to emphasize the long term, or at least a term commensurate with the payout. They don't so far, although of course there are differences.

Now, another problem, when we come down to the daily life of such companies like ours, is that we will have increasing difficulties recruiting people into science and engineering. A third problem we covered—that is, business of understanding the interplay between economists and technologists, and between the salesman and the professor.

BROCK: Perhaps we could close just by asking if there is anything that you wanted to return to from the topics we've discussed earlier, or if there is anything else you'd like to discuss.

TOPSØE: I could say a few things about the mission of different people. I think that the mission of government is, of course, to create a climate for progress in a very general manner, and this has so many aspects that we cannot even try to touch upon. But one aspect, of course, is education and there—from the narrow angle of ours—we expect the government to give the industry good scientists and good engineers, et cetera, and in all fields. We emphasize the education to gain knowledge and gain competence. The other thing is that we think the mission of the players is for public institutes, like national research laboratories, to again put focus on the production of good scientists, good experimental people, good research people, good people to come from research results to industry—good, shall we say, as implementers. And to also produce economists who would be kind enough to spend a little bit of time to understand the sciences and technology and therefore refrain from expressing their opinions without this knowledge. The sector research institute—we do not believe much in that. We do not believe it is the role of governments to select which areas need a government-sponsored or funded sector research institute. We mean that in industry and business, we are completely capable ourselves of finding where the combination of market forces, scientific developments, and research activities will lead us. Also we think we are capable of helping solve the problem of the Third World. We think we are maybe in reality, in fact, a better channel, a better vehicle for helping the Third World than all the government and multi-government programs. We'd like very much to see the public hands giving where you really need to give and not undertake things the development banks do most. They were very useful at a time, but are much less useful today. I like and liked Jim [James D.] Wolfensohn and Eugene Black, but the World Bank is no longer a preferred vehicle, nor are other development banks of any importance for supplying the capital and the risk capital you need in the Third World. Actually, apart from ethics and idealism, the oil and gas industry has done far more to transfer technology and to make the semi-developed world collaborate with the developed world. This mission is not recognized. This mission—that global international companies are the best vehicles to create collaboration with the Third World for joint progress—is not normally recognized.

Then I do feel that if you look more into the sciences, it is the mission in the universities to focus on fundamentals, but it is the mission of business companies to at least participate in fundamentals sufficiently to be able to follow what is happening. I myself don't believe that it is acceptable for a serious company not to have an activity in fundamental research.

Another thing about missions and so on: unfortunately, a great many companies—whether it is local trading, in sales, in research and technology, production, manufacturing, what-have-you—do not have the critical mass needed today. Yesterday, yesteryear, the critical mass for just a common engineering company making equipment was maybe one thousand people or something like that. That won't do today. It's many times that. The critical mass for, shall we say, a chemical synthesis company was maybe the equivalent of two thousand tons of methanol a day. That won't do today. It's many times that. The critical mass of a pharmaceutical company was maybe a turnover of two, three billion dollars. That won't do today because of globalization. So if you take a company like ours, a research company focusing on catalysis, we're just a little bit above the critical mass. Many of our competitors do not have the critical mass, and therefore you have a mission—that is, a joint mission between government, universities and business—to somehow bring those who don't have a critical mass and who deserve it to grow somehow. To nourish them, to help them. This is very important because without that, there is but little growth. Or rather, growth has to come from the establishment and that is not enough!

SLATER: Undergrowth? Or seedlings? Or saplings?

TOPSØE: Yes. This is a very big problem. All these problems, of course, translate also into economy. Therefore, when it comes to development in a general way, in the Western world we have to realize this new situation concerning critical mass. It's no use whatsoever to try and do something if you don't have critical mass, or can see that it is around the corner. I mean, why do you think that we are buying several microscopes, and Raman, and electron microscopes, et cetera? Because we feel it's a necessity to be on top of this question. That's a very big problem.

Now, some governments are like all Western European governments—they are emphasizing new start-uppers. Fine, that's wonderful. I mean, if I wanted to create growth in Scandinavia, give me two or three billion kroner; I will hand them out in parcels of half a million to ten million to two thousand people out there, of which I know many, who would deserve it. They would bring out something new. They would work fifteen hours a day with a few people. They would come with something new. They will come up to this, and then they are dead, because their lift doesn't go beyond the mezzanine! [laughter] Therefore, it's far more important to give people what is called "mezzanine capital" and then you talk about ten or tens of millions. When something good has to live in the modern international competitive world, and assuming it's of importance, then you have to have an instrument that cannot be only venture capital, because of obvious reasons. An instrument that allows people who deserve it to come and get ten, fifty, one hundred million to use things, and then at least come into a situation where others think it's reasonable to help them gain the critical mass in whatever area. That's very, very important. Otherwise, we are lost. This emphasis on start-uppers—fine, but it's a fraction of what is needed also in the way of capital.

Then I feel that we still have, in the very all-important energy area, to find our feet. People have not understood what it means to all industries that the, shall we say, the "Arabs"—they sit on the energy. Of course, we can move all industry to the "Arabs," whether they live in Indonesia, Malaysia, Qatar, or Chile. That's a major undertaking, and therefore that is a mission where you still have to find a solution by a combination of geo-political, economic, and heavy industry efforts.

Then you have the big problems about patents and whether or not your technology should be made generally available and in what form.

So I think that in a very primitive, farmer's way of reasoning, all those out there, all the players from the prime minister—by the way a nice chap—all those out there, they need to understand what their missions are and how this has changed by globalization.

Another over-riding problem we have is timing. I mean, we wanted to see Russia—old Russia—change to, reform to, a market-oriented policy overnight. That was a great crime to try to push the Russians to go from a centralist economy to a market-oriented economy in months or years, where it would take generations. That has created huge poverty and killed many people. You just go over there and see what's happened. So this question of timing is very difficult now.

We wanted, after the war, to see "one world"; I certainly want to see one, but not too fast. We want to see the WTO [World Trade Organization] having every country included—China, all of them. Now, it cannot be done—gainfully. It's ridiculous to try and pursue ideas that, by any analysis of anybody who just knows a little bit, would take years, decades, or generations to implement, to push them through. It's ridiculous. We're pushing through Euro-land. Everybody thought—with the exception of my circle—that the Euro would be a smash hit. It's a fiasco, and it's a miracle if it doesn't go from bad to worse. How can you force people to work under the same conditions in all respects when they are very different? I mean, it's not reasonable. That's what they are trying to do—now that we elected [Romano] Prodi. He is a rather nice person, by the way—an old-time professor and a questionable industrial leader. Now, they have elected him. The first thing he says, "We also have to have same taxation in all Euro-land countries." So we have one currency. We're not a member, as you know, of the currency. We have one currency, one set of laws for the labor market, one set of standards, one set of patents, one law to make all technology available to everybody, one interest rate, one budget rule—no more than 3 percent deficit—one monetary policy, and one inflation policy. Then on top of that, one tax policy. At the same time, we believe in the "Bible"—being the market economy, competitive market economy. How can you compete if all conditions are the same? Then you have WTO rules on top of all that? How do you compete? By swindle? Bribing? Or by what?

BROCK: Well, thank you very much for all of your time today.

[END OF TAPE, SIDE 4]

[END OF INTERVIEW]

NOTES

1. Haldor Topsøe, *Danmarks Productionsliv omkring 1935*.

2. Haldor Topsøe, *Inspiration: Haldor Topsøe: Mod og meninger* (Lynge, Denmark: Bogans Forlag, 1988).

INDEX

**A**
Aarhus University, 15
Aarhus, Denmark, 15
Al-Sabah, Sheikh Jaber al-Ahmad, 19
Ammonia, 16-19, 21, 23
Archer Daniels Midland Company, 16

**B**
Baltic Sea, 18
Beek, Otto, 12
Biology, 8, 18
Biomethanol, 18
Black, Eugene, 26
Bohr, Niels Hendrik David, 11
  Copenhagen School, 11
Borlaug, Norman Ernest, 16
BPAmoco Company, 20
Brønsted, Johannes Nicolaus, 10-12
Brundtland, Gro Harlem, 18

**C**
Catalysis, 6-9, 11-12, 14-16, 18-19, 21, 26
Chemical engineering, 2, 4-5, 8
Chemistry, 2, 8, 10-12, 14, 21
  isotope chemistry, 11
  physical chemistry, 5
  theoretical chemistry, 12
Chevron Corporation, 19
Communism, 2
Copenhagen University, 2
Copenhagen, Denmark, 1, 7, 10-11, 15
Cyclosynchrotrones, 15
Cyclotron, 14

**D**
Danish Technical University, 2, 14-15
  Interdisciplinary Research Center for Catalysis [ICAT], 15
Dimethyl ether [DME], 18, 20

**E**
Egloff, Gustav, 12, 19
Einstein, Albert, 11

**F**
Farkas, Adalbert, 12
Fermi, Enrico, 12
Fertilizers, 5, 9, 16-19
Fischer, Franz, 20
Fischer-Tropsch process, 20-21
Foreningen til unge Handelsmænds Uddannelse, 3
Friedman, Milton, 6

**G**
General Electric Company, 21
Gerstner, Lou, 24
Gibbs, Josiah Willard, 10
Green Revolution, 5, 16-18
Gulf Oil Limited Partnership, 19

**H**
Haldor Topsøe A/S, 1, 3-4, 10, 12, 16-22, 24, 26
Hamburg, Germany, 14
Handelshøjskolen, 3
de Hevesy, George C., 11-12
Hitler, Adolf, 1
Hydrogenation, 19

**I**
Idemitsu Petrochemical Company, 20
International Business Machines [IBM], 24
Ipatieff, Vladimir N., 12

**J**
Jellicoe, Lord John Rushworth, 1

**K**
Kattegat, 18
Keith, Carl D., 19
Kinetics, 11, 13-14
Kjær, Jørgen, 20

**L**
Landau, Ralph, 4, 19
Lewis, Gilbert N., 11
Lund University, 16

**M**
Marxism, 2, 7
Methanol, 19-22, 26
Midland, Michigan, 16
Millsborough, England, 7
Mössbauer, Rudolf Ludwig, 13

**N**
Nazism, 7
Nielsen, Anders, 8
Nobel Prize, 12-13, 16
Nørskov, Jens, 14

**P**
Philadelphia Club, 12
Physics, 2, 4-5, 8, 10-14
    experimental physics, 14
    fusion physics, 11
    particle physics, 10-11
    quantum physics, 10-12, 14
    relativity physics, 11
    solid state physics, 12
    theoretical physics, 4, 8, 10-12, 14
Planck, Max Karl Ernst Ludwig, 11
Pratt & Whitney, 21
Prodi, Romano, 27

**R**
Refining, 9, 19-20
Rolls-Royce Plc, 21

**S**
Samfundshjælpen, 1-2
Sasol, 21
Scandinavian Airlines System [SAS], 21
Schmidt, Erik Ib, 3
Semiconductors, 15
Social Democrats, 2
Sørensen, S. P. L., 10
Spectroscopy, 13
    infrared spectroscopy, 13
    Mössbauer spectroscopy, 13-14
    Raman spectroscopy, 13-14, 26
Sturmabteilung (SA), 1
Sulfuric acid, 19

**T**
Taylor, Hugh S., 15
Thermodynamics, 6, 10
Thomas, Sir John Meuring, 14
Thompson, J. K., 15
Thomson, S. Y., 15
Topsøe, Haldor
  brother, 2
  father (Flemming Topsøe), 1-2, 4, 7
  father-in-law (Aage Kunst), 2, 4
  son (Henrik), 13, 20
  wife (Inger Veng Topsøe), 2-3, 8, 17-18
Tropsch, Hans, 20

**U**
United States Government
  Pentagon, 15
Universal Oil Products [UOP], 19-20
Urey, Harold C., 11

**W**
Wolfensohn, James D., 26
World Bank, 26
World Trade Organization [WTO], 27-28
World War I, 1-2, 4, 16, 19
World War II, 3, 8-9, 12-13, 17, 19, 27

**X**
X-rays, 14