Peter J. W. Debye: An Interview

On 24 March Peter J. W. Debye, emeritus professor at Cornell University, began his 7th decade of active research in chemical physics and celebrated his 80th birthday. In anticipation of this occasion, three members of the Cornell staff—Dale R. Corson, provost, former dean of the school of engineering, and, before that, professor of physics and chairman of the department; Edwin E. Salpeter, professor of theoretical physics; and S. H. Bauer, professor of chemistry—interviewed Professor Debye on 5 March. This article consists of questions and answers selected from the tape recording of the interview.

Salpeter: You were in the middle, so to speak, of the early developments in quantum mechanics. I am curious what your reaction was at that time. Was it clear from the beginning that the new theory was going to be the final one, or did you and the others have misgivings that the new mechanics was only an interim development? Also, which of the various, and at that time presumably rival sounding, versions appealed to you the most? When did it become clear that these would be unified?

Debye: By early developments you mean, of course, the proposals of Heisenberg and Schroedinger. Well, I think of "early" as the period 25 years before that when the whole thing started with a kind of interpolation formula by Planck. Nobody wanted to accept it then because it did not appear logical. In Planck's radiation formula half of the argument was continuous and the other half was based on the concept of quanta of energy, which he set equal to $h\nu$ in order to get Wien's Radiation Law. There was much trouble at the beginning. The only man who appeared sensible was Einstein. He had the feeling that if there was anything to Planck's idea then it should also appear in other parts of physics. Well, at that time, he talked about the photoeffect, specific heats, and so forth. Then I tried to formulate the theory of specific heats in a more general way. Also about that time Planck introduced the zero point energy, perhaps around 1910 or '11, because he was not content with his original derivation.

Bauer: This was a patchwork operation?

Debye: Yes. it was patchwork the whole time; many were trying to make the formulation a bit more general. Then de Broglie published his paper. At that time Schroedinger was my successor at the University in Zurich, and I was at the Technical University, which is a federal institute, and we had a colloquium together. We were talking about de Broglie's theory and agreed that we didn't understand it, and that we should really think about his formulations and what they mean. So I asked Schroedinger to give us a colloquium. And the preparation of that really got him started.

Corson: What was the date?

Debye: Oh, I don't know—between 1924 and 1927. It was in the same year that he published his paper, because there were only a few months between his talk and his publication. Of course there was also Heisenberg's theory. Personally, I liked Schroedinger's formulation better, because I was more familiar with differential equations than with matrices. Very soon we saw that one followed from the other.

Salpeter: You say that when Schroedinger gave his talk it seemed to culminate directly in a theory?

Debye: It was all prepared really—by all the discussion which had been going on for years—and it was only a question of mathematical formulations of the ideas which were around.

Bauer: Partly as an extension of this question, please tell us about the many discussions which occurred at that time of the philosophic implications of quantum mechanics.

Debye: Well, look at this thing called an electron. Sometimes energy as radiation goes into an atom to excite an electron, and it enters as one piece. And then, at the same time, you have to consider this radiation as it interacts with the radiation field, and so you have to look at it as if it were a wave. You accept this duality as a necessity. That is really the end of it. You have to look at the experiments from different directions and, depending on the direction, you have the corresponding formulation. This was very hard for people to accept; it was also hard for Einstein. He had calculated the fluctuations of energy in a space containing radiation. Now, there are two parts to it; if you say that it is all waves, you get a certain fluctuation. But, if you take the entropy, from this, with Planck's formula, you may calculate a second fluctuation which is independent of the fluctuations due to the waves. But that is not a real fluctuation; it is inherent in the quantum formulation.

Bauer: And this caused the trouble. Well, nowadays, we don't feel troubled by this quality.

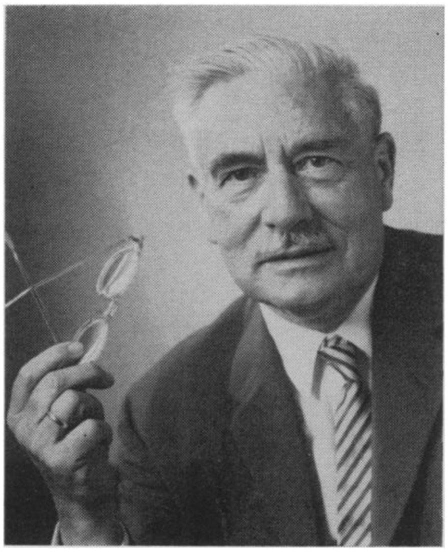
Debye: Well, of course not. People have become accustomed to it. At that time, one had to try to answer whether an electron was a wave or a particle. Of course, it is both.

Physical Models

Corson: Should we change the subject? In reading your papers, Professor Debye, one is struck by the simple ways and the clear physical models that you have developed for explaining how things work. Is the current situation in physics, where we are beset with a tremendous amount of formalism, adaptable to this kind of relatively simple physical picture?

Debye: I have always felt that you cannot do without a picture. If you talk about the hydrogen atom, you have to start with the potential energy between an electron and a nucleus. Then you express it in the form of a Hamiltonian, and so forth. But you have not avoided the picture. The question is whether the model is the main thing, or whether the mathematical handling of this picture is the main thing. Nowadays, there is a lot of emphasis on the mathematical manipulation. I think that is all right, but I cannot do without a picture.

Salpeter: I am also curious, in the



Peter J. W. Debye [W. Mantz]

Peter J. W. Debye has made numerous contributions to our knowledge of molecular structure at the very highest level. He has been among the first to erase the dividing line between chemistry and physics. He has demonstrated and still continues to show us in that clear and penetrating fashion he has trained us to expect of him that the geometry of molecules and the force fields around them (as explored by physical techniques) control their physical and chemical behavior. Professor Debye was awarded the Nobel Prize in Chemistry in 1936. By now he has, in addition, accumulated ten medals and citations, 15 honorary degrees, and has been elected to membership in 20 national academies. He has served as professor in numerous universities. He was director of the Max Planck Institute for the Kaiser Wilhelm Gesellschaft in Berlin from 1934 to 1939. In the decade of the 1940's, he was chairman of the Department of Chemistry at Cornell University. Since 1952 he has been an emeritus professor, and a very active one indeed. He has traveled the world over, and everywhere has been received as a most welcome lecturer. He is renowned for having developed to a fine art the exposition of fundamental concepts in molecular physics and chemistry. We at Cornell are looking forward to his continuing creative contributions to physics and chemistry.—S. H. BAUER

same way as Professor Corson, about the current developments, say in elementary particle theory. Theoretical physicists seem to be groping for some new theories which go beyond quantum mechanics. And there, I think, is really a question of principle, not a question of emphasis. Can one invent a new theory without any physical models?

Debye: Oh, yes. This has been done before. Mathematicians have been saying that something which is simple in mathematics has to have an application in physics. Of course, this is an extreme position. But this is a personal matter: whether one starts with a picture and then tries to make a formulation which represents all the experiments, or if one thinks of a mathematical formulation which looks nice to him, and then looks to see if he can get a physical interpretation. There must then be a confrontation of these two parts.

Corson: Is there an example in your work where, from a simple mathematical formulation, a physical interpretation appeared which came after the mathematics?

Debye: No, not in my case.

Bauer: You have demonstrated that this method of yours is very powerful. Could it be because you emphasize the classical mechanics approach?

Debye: No. As I pointed out, Schroedinger could not have written a Hamiltonian without the picture that Bohr provided for him.

Corson: Let us now go back to the period of the early 1920's, which turned out to be such a productive era for physics. Is there anything that you can point to—were there particularly effective teachers at that time—which led to the productivity of that period?

Debye: Well, you should not forget that there was 25 years of discussion in back of these theories: and it was not merely personal discussions—it was a discussion that went on all over Europe. The atmosphere was full of questions to which every physicist was addressing himself. It was the main topic of conversation between physicists, even between those who were strictly experimentalists.

Corson: Was there a relatively large number of people involved in this?

Debye: There was really a relatively small number of people. When I was in Leipzig in the 1930's, I tried to arrange conferences. When London was discussing molecular forces there were no more than about 35 people present. This was the number who were really interested. Now, if you were concerned with nuclear forces and gathered the people together, you would find about 300. At that time, as now, there were a lot of people who were ready to talk nonsense, but when they were together they were not afraid to say something which they had to take back later, and that was really nicer.

Publication

Corson: One of our current problems is the dissemination of information. We have such a flood of publications it is hard to get significant coverage of a topic in one place.

Debye: Not only that! There is also trouble in that publication takes too much time. It is no good if one has to wait for a year before a new paper appears. The old-time academies in Europe were very good in this respect. For instance, when I was in Goettingen, when we had something new, we presented it to the Goettingen Academy and it was published within 3 or 4 weeks. In principle, this can also be done in our National Academy, but you have to be a member or you must have a member present the paper for you. There is too little in the Academy to cover the important topics. Physicists and chemists do not want to publish there because there are few of them who read the *Proceedings*. What is missing is a journal to which a man can submit two or three pages, in which he describes a new idea, and does not have to wait long before it is tested by other people.

Corson: This is the objective of *Physical Review Letters*.

Debye: You see, things come out of a certain kind of atmosphere. Take, for instance, the process of cooling by adiabatic demagnetization. Well, that was developed in California by Giauque, but I published a little earlier in Leipzig. We had nothing to do with each other, but the concept was in the atmosphere.

Bauer: Now the atmosphere seems to be laden with either solid-state physics or with nuclear particles.

Debye: But the atmosphere is not as penetrating as it was in old times, probably because things are much more complicated now, and there are more people involved.

Salpeter: This brings me to a related question. In your 60 years or so of being in the field of chemical physics, was there any particular period, say a decade or so, which really stand out?

Debye: Oh no. I would not say there is. You see, I can only judge from a personal point of view. I can only talk about my own work and, if I talk about it, well, I have to say I'm interested in these topics and I think they are important at the moment when I am doing them. Later I forget about them. So it's only during the time that I have fun with them that they seem to be important.

Bauer: But, the point is that one can make value judgments. You have a basis for making value judgments because of your much longer experience, and we are looking for these assessments.

Debye: Yes, but you see, a thing which is valuable at the time may not be quite so valuable 20 years later.

Graduate Education

Salpeter: I wish to bring up another point. It seems that physicists and chemists are complaining now that these fields are growing farther and farther apart. We are attempting to stop that—the Science Material Center here at Cornell was set up to accomplish this. But there is a feeling that the separation is irreversible, and it must have been nicer in the good old days. Is that true, or not?

Debye: I think it is just the opposite. Think of the old times. Physicists were doing Maxwell theory and other mathematically involved research. And the important area was what I call theoretical physics, and not mathematical physics. In the old times there were physicists on one side and chemists on the other; and the chemists wrote formulas with bonds between the atomic symbols, in the form of lines. And, if I remember those times, the physicists laughed at the chemists with their lines for bonds. This was nonsense. Now they have come together, and there is an understanding of what these bonds are. I have a feeling that they are now much closer than they were in old times. When one talks about physics and chemistry, he must recognize that there are many, many more facts at present. I say that this does not make science more difficult, because we also have better relationships between the facts. We have found the interconnections, and so there is really not so much of a burden on the students. I always quote Hilbert here. Hilbert said that the best man is he who can forget the most, you see; because he felt that if, on the spur of the moment, you can get the answer by thinking about it, by putting these relationships together you can reconstruct the answer, then you have to remember much less. And this is what one should strive for.

Bauer: Of course, this implies that you have the principles very well formulated.

Corson: Do you think that our methods of teaching and the level of our graduate education in physics and chemistry now promote this unification and broader view?

Debye: Now—partly. But they are still handling these studies more as something to be remembered than something to be understood.

Corson: You still think the emphasis is on the large number of facts rather than on principle?

Debye: Yes. Universities should do that differently.

Corson: You're talking about graduate education?

Debye: Yes. People in industry are complaining about the students they get from universities who know many facts, but if they get new facts they do not know how to interconnect them.

Bauer: Well, it's interesting that we get a variety of feedbacks from industry. Occasionally representatives from industry give the impression that they are dissatisfied with the products of universities because the graduates can only talk in generalities and cannot apply these to specific problems.

Debye: No. They have not learned well enough how to connect one thing with another. I don't think that the way students are handled pushes them to exert themselves in many cases—I see this with my grandsons.

Salpeter: How would you say this could be improved? Do you think lectures, as given in graduate courses, need improvement, or do you think that in American graduate schools the students spend too much time taking courses and don't begin their research early enough?

Debye: This is true too. But in the courses I think they spend too much time on specific problems. They have so many specific problems assigned that they don't have time left to go to the library and browse.

Bauer: The problem, I think, is one which is characteristic of the American educational system in that we insist on keeping track of what the students do. If you insist on that, then you cannot give them free time.

Debye: Yes. There is, of course, a fundamental difference between the European point of view and the American view. In Europe, when a student comes to the university, from that moment on he is supposed to be grown up and responsible for what he does. Here, we have just the opposite attitude. During the time they are in high school they are free, but when they come to the university we begin to treat them like babies. This also hinders the professors, who must devote much of their time to handling students rather than handling scientific problems.

Corson: With respect to the attitude of the faculty toward the students, anytime a faculty member attempts to put a student on his own and make him responsible for his own education or a substantial part of it there are cries of anguish.

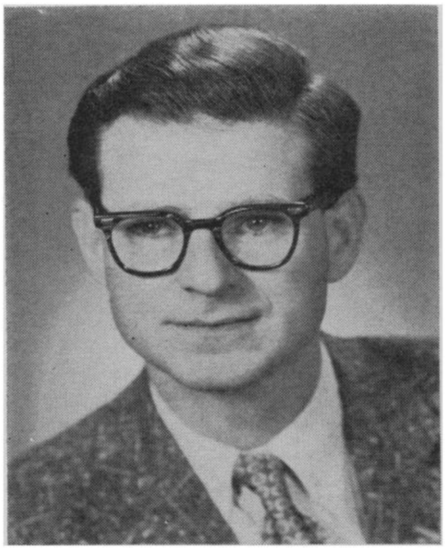
Debye: Yes, I know. But that doesn't mean that it would not be better to do it that way. It is hard to change and I don't know how to do it practically, but could one start, say, with two or three students and handle them differently.

Bauer: This is only part of the problem. A very large number of students are processed in an American university.

Debye: Yes. Look back in history and consider the development of European universities—you will see that the university started as a kind of discussion group of many people who were interested in science, or law, or medicine. Then the teaching part was grafted on to these discussions. But all American universities were founded as teaching institutions, and the research part was grafted on to that.

Corson: In your time at Zurich, how many students did you have working with you?

Debye: Well, working with me is one question, and taking my course is another. In the course, there were 500 to 600 students, perhaps even sometimes as many as 800. These were equivalent to our undergraduate students. And then, those who were more interested had to come to the labora-



Edwin E. Salpeter

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S. H. Bauer

tory where our assistants helped them with their work. Then when a student came to a professor and told him he was interested in working with him, he was given a problem, one which he had to handle in the laboratory. There was no prepared laboratory, and he had about 3 months or so to set it up and to do it. On the basis of this the professor decided whether he wanted to take the student or not. It could also be a theoretical problem. I tried them out with a simple problem first, and then I took only those I wanted.

Corson: How many did you end up with?

Debye: Oh, about 20 or 25 in whom I was personally interested in working with me.

Corson: These were equivalent to Ph.D. theses. That's really a very large number.

Debye: Yes, it is a very large number, but since I selected the students carefully I did not have to put in much time with them, because they were good enough.

Bauer: Do you feel that the older scientists have some obligation to the younger ones, in particular, to create an atmosphere and to indicate a direction for significant research, perhaps to indicate procedures for research?

Debye: Well, all scientists have this obligation. I cannot make a distinction here. If you talk with someone and discuss a problem and get him interested in it, there is really not a question of what is important or unimportant at that stage.

Bauer: Don't you think there is a need for sitting back and taking stock?

Debye: Yes, there is need for that always. If you are asking me what I should do at Cornell, at the moment, in order to help in this, then I don't know because this is a practical question. It depends on the circumstances that are prevailing here. But we're talking about the principle; one must generate enthusiasm and interest. To make a question really alive and exciting it is not necessary to write formulas on the blackboard from the beginning of the hour to the end.

Politicians and Science

Bauer: Does the political atmosphere in the country influence the developments of science?

Debye: Well, this depends on the politicians. If they provide the money and let the scientists do what they want to, then there is no trouble. As you know, the opposite was true in Germany when the Nazis came to power. First they had to decide whether someone would be employed at all by the State, for the State was everything. He had to be a good Nazi, and, after they decided that he was a good enough Nazi, they considered the second question—was he a good physicist?

Bauer: In some areas apparently these politicians attempted to decide what was good for physics.

Debye: Well, yes. Politicians should not interfere in things which they do not understand.

Bauer: Now in the United States a situation exists where we almost insist that the politicians should understand physics, simply because the government is requested to provide so much money. Since the politicians must allocate a great deal of money they must really understand the significance of the work which is being done.

Debye: That is the point, you see. A politician does not have to understand physical theories; he has only to have a feeling for what is important. What we should do, from our point of view, is to give politicians an opportunity to understand what is good and what is really not so good in science. Most of the money is being spent on the development of applications of old principles and not on finding new principles. On our part, we should really make it easy for politicians to understand that the fundamental part of science is more important than the applied, in terms of the long-range program.

Bauer: Yes. In terms of developing new knowledge, it is almost like a natural resource which we fail to exploit, and this requires a very large investment.

Salpeter: Well, where do we fall down? I mean, is it when we teach our undergraduates—say, when the politicians went through college we didn't instill in them enough understanding?

Debye: Oh no. That's not fair. It is the general atmosphere which we create. We suggest to them: "Well, please don't talk about that, because you'll never understand it." That is what we say to many people implicitly. We should not do that. We should say:

"These things are so sensible that a man in the street can understand them, providing I am good enough to make them clear to him."

Bauer: In this respect we have failed somehow, not only with our lawyers and politicians but also, I'm sorry to say, with our non-scientific professors, who should understand the basic aspects of science.

Debye: Yes. They don't have to know science, but they have to understand it. I'm not a man who can talk about languages or history, but I do have a feeling for them. And they should have the same feeling for science and not be antagonistic to it. We should never say: "This is a thing which is far, far above you—you'll never understand it." Well, of course if we do, then they do not even try to understand it.

Research Support

Corson: There are some who suggest that there is perhaps too much research being done in this country, and we're spending too much money on it. Congressional committees are investigating this point. Does this stem from a failure of communication with these people or is there a real danger of spending too much money on research in all areas?

Debye: Well, I would not say that there is danger of spending too much money in general, but there is danger of spending it the wrong way. I think that too much of the money is being spent on applications.

Corson: What about the effectiveness of the way we spend our money for research? We have sponsoring agencies, primarily in Washington, to which various people submit proposals. Committees in Washington evaluate the proposals and decide if they are good or bad. Is there a danger of misdirection by the people who are making these evaluations?

Debye: Well, now I'm going to exaggerate, and an exaggeration is dangerous. Suppose someone submits a proposal, and in this proposal he says that he undertakes to do certain experiments and to get certain results. Since this is very clearly stated and carefully outlined, the proposal is accepted. But I claim that this should not be accepted because, if he already knows what will come out of it, then there's no point in doing it. This, you see, is the trend. What we need are proposals which the sponsors are courageous enough to support, while the investigators do not know what will come out of them.

Corson: The successful proposals tend to be in fields which are popular. Proposals of topics which look strange on the surface do not stand much chance. Isn't there a danger that we really lose significant research in this kind of evaluation? What would have happened to Planck if he had tried to get government support?

Debye: Of course, there would have been trouble. But it was different then. What is now the Max Planck Society was then called the Kaiser Wilhelm Society, and it was supported by the State and by industry. These institutions were for research, and were connected with the university to some extent but were rather free of them. I obtained a certain amount of money from the Society. Now, the philosophy behind these grants was that, if they wanted to establish a new institute, they first looked for a good man with a good idea. And if he had a good idea to develop, they would build around him an institute and give him money. From then on he could do what he liked with it. When I was the director of the Max Planck Institute, which was built with money from the Rockefeller Foundation, I had such a budget and I could use it in the way I liked.

Corson: Do you think we'd be better off in the United States if our funds were awarded according to some such scheme as this, where the sponsors agree that a certain man or a certain department or a certain university has good ideas and give the money without a detailed proposal?

Debye: Yes. That is missing.

Salpeter: Isn't there, however, a danger that in the European universities too much depends on a single man, on the man in charge?

Debye: Yes. I have heard that many times, but in practice I have not seen it. In practice I have only seen that there was much freedom in these institutions, and there was not the director who could act as a czar and dictate what was to be done. He merely indicated the general direction of the field.

Bauer: This permitted departments to grow in a rather different fashion from what we have in the United States. In Europe each department is much more specialized; because the head of the department is working in a certain area, the whole department revolves around that field.

Debye: No. No. For instance, if you take the Max Planck Institute, which I set up, my subdirector was Laue, and he could do absolutely what he wanted and he had money for it. Then there were two others, one in spectroscopy and one in other areas of physics, and they could do what they wanted to do —I did not interfere with them.

Salpeter: But you may be telling us how the system worked when good people were in charge of the crucial positions. What happens, and you can never prevent this, if some incompetent persons are chosen to be heads of laboratories?

Debye: Yes. That happened. I'm only saying that practically it worked well most of the time. There was a small percentage of the time when it did not.

Corson: With reference to the large expenditures for research, do you think the quality of the science that is coming out and will continue to come out is commensurate with the pace at which we're spending money and the rate at which people are committing their efforts to science? In particular, in fundamental physics we now have machines that cost hundreds of millions of dollars and we're thinking of others that will cost many hundreds of millions in the future. Is this going to assure, with any reasonable probability, that we will understand high-energy mesons in some clearer way?

Debye: You can never guarantee that. If you look at the experimental results you will find that these generate much interest. If you are spending much money, can you guarantee in some way that you are going to get a return? You cannot guarantee that—you can only say that there is a probability, and the probability is high, because in the past this has happened.

Bauer: Couldn't you also say the converse? If you don't spend the money, you'll never give yourself a chance to find out. What do you think of the enormous amount of money which is being spent on the so-called space program?

Debye: No. It all depends on your fundamental position.

Salpeter: Another aspect of the large expenditure of money is the hustle and bustle associated with physics, chemistry, and biology these days. People travel a great deal—they go from one conference to another. Is this increased communication apt to be good in the long run, or will it detract from the development of science in that it may keep people from really sitting down and thinking?

Debye: Yes. It is overdone—overdone. There are too many conferences. Those people who go from one conference to another are not contributing very much. This is also true of university professors.

Corson: There is great pressure to get involved in a lot of extraneous activity, on committees in Washington, for example. Is this a good thing?

Debye: Well, if a man does what he really wants to do, and sacrifices his own research effort, then you should let him do it. But from the point of view of the community this is not so good, because he could do some better things.

Bauer: Well, sometimes it is necessary to get involved in this way. In order to make decisions regarding the allocation of funds politicians have recognized that they must have advisers. This takes the best people out of science and converts them to committees who sit and advise.

Debye: Yes, of course. The scientists who respond must have an inclination to do this. We should recognize that they are making a sacrifice, in that this keeps them from finding out something new. It is a loss.

Salpeter: There is a question here. Should one encourage a small number of scientists to take off, say, 5 years to become advisers in Washington?

Debye: Oh. You cannot take 5 years. Really, this is impossible. If a man is really interested in nuclear physics, he cannot just quit for such a long period.

Salpeter: Let me rephrase this question. Should one encourage a new profession of a smaller number of people who are scientists and have become professional government advisers, or should one encourage a much larger number of practicing university professors to spend a few days in a year in the process of advising?

Debye: I think the first is much better. If a scientist spends a few days a year, he cannot put in all his effort, but that's what he should do. It is better to have people who devote 100 percent of their time to this work, and then you will have people who want to do it. There are those who have had a good feeling for physics and chemistry, and also want to work in administration.

Bauer: This last comment brought to mind a question regarding your procedures for doing research. I think you follow the principle that you work on only one problem at a time, and you devote yourself wholly to it.

Debye: Yes. Yes.

Bauer: Do you find that this really pays off?

Debye: Well, I don't know whether it pays off or not. I only know that it gives me fun. You see, I'm not talking about the community—I'm not doing things for the community—I'm doing things for myself. Now, you can say this is bad.

Bauer: I'm thinking more in terms of accomplishment. Do you find that this is a useful way to operate?

Debye: Yes. I think so. The main thing is that you're interested in whatever you are doing 100 percent, and then you regret and resent anything that takes you away from it.

Bauer: Thank you, Professor Debye.

News and Comment

Foundations: Patman Maintains Pressure for Tighter Regulation of Tax-Exempt Organizations

Congressman Wright Patman (D-Tex.), chief advocate of tightening both the law and federal supervision on the financial activities of tax-exempt foundations, late last month presided over 3 days of hearings on the subject before his Small Business subcommittee.

Patman, former chairman of the House Select Committee on Small Business, is now chairman of the House Committee on Banking and Currency, but has retained chairmanship of the Small Business subcommittee on "foundations—their impact on small business."

As a critic of the foundations, Patman clearly sees himself first as a defender of the ordinary taxpayer. In his introductory statement at the hearings Patman referred to three reports produced by the committee staff and said, "More and more the 'cream' is slipping out of our tax system as the great fortunes go into tax-exempt foundations. Thus the 'skim milk' incomes of average, hard working families must shoulder an increasing part of the tax burden, both Federal and state."

In addition, Patman finds fault with activities which put foundations in competition with private business, and he also sees serious implications in the large holdings of common stock by the foundations and in the management of these assets by some foundations.

However, discussion of specific abuses of the present laws and of possible changes in the laws covering tax-exempt organizations dominated the 3 days of hearings, 21 through 23 July, at which the subcommittee heard star witnesses Treasury Secretary Douglas Dillon; former Commissioner of the Internal Revenue Service, Mortimer M. Caplin, who recently resigned; and Commissioner Manuel F. Cohen of the Securities and Exchange Commission, who is reportedly slated for chairmanship of the SEC.

The Patman subcommittee is an investigative group. Legislative powers in this area of foundation affairs are reserved to the tax-writing Ways and Means Committee in the House and the Finance Committee in the Senate. But Patman's harping on the sins of commission and omission of some foundations is likely, in the long run, to affect legislation just as it has, patently, invigorated IRS enforcement action in the foundation sector.

In the 2 years since Patman began close examination of the foundations, his staff has produced three reports. They dealt with more than 500 foundations (including many of the largest), carrying information on such things as receipts, net worth, liabilities, and accumulation of income, and also pro-