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THINKING AHEAD WITH...

LEO SZILARD

One of the fathers of the atom bomb talks about why he shifted from physics into biology, where modern molecular biology is headed, and how the nuclear weapons he helped produce might be controlled

In recent months Leo Szilard has been spending most of his time in Washington, working sometimes in the sunny lobby of the Dupont Plaza Hotel where he lives, sometimes in the cluttered little study next to the service elevators where we interviewed him. He is professor of biophysics at the University of Chicago, but the post involves no teaching (actually, in his long scientific career, he has never taught) and leaves him free to do pretty much as he pleases. Mostly he pleases to divide his time between research into the molecular mechanisms of biology and searching for ideas on how to avoid war.

Twenty-three years ago, Szilard was making some of the first experiments demonstrating the possibility of a chain reaction in uranium and later devised with Fermi the first working atomic reactor.

Through a lifetime of scientific work in Hungary, Germany, England, and this country, Szilard seems to have had a talent for getting into the fields where exciting discoveries were being made. So our first question to him was:

"I like the kind of physics where I can think of something today and do the experiment tomorrow. Today you can't do that in physics."

One thing we're anxious to talk about, Dr. Szilard, is your shift from nuclear physics into biophysics. As background to that, would you tell us about your involvement in the development of the atomic bomb?

In January, 1939, when I learned of the discovery of fission by Hahn and Strassmann, it immediately occurred to me that neutrons might be evaporated in the fission process. This would make a chain reaction possible.

Where were you then?

I was nowhere, really. I lived in New York, thinking what I should think about, when the discovery occurred. Now I had thought about the possibility of a nuclear chain reaction in 1934, but by 1939 I had given it up. When I heard about the discovery of fission, the first thought that came to my mind was that we ought to look immediately whether a chain reaction is possible—and if it is possible, then we ought to keep it secret.

Did you find the idea of secrecy repellent?

I invented secrecy. I had written a number of letters, and we proposed both to the English and the French that if neutrons are emitted in the fission of uranium, this fact should be kept secret.

Did you find much resistance to the idea?

There was considerable resistance, and the resistance became insurmountable after Joliot published his results and showed that neutrons are emitted in fission. I was still in favor of adhering to this policy—because, I said, if we did this, Joliot would have to fall in line; if he didn't, we would know his results and he would not know ours.

During that period was anything published which significantly helped the Germans?

No, except the one paper by Joliot.

Why do you think the Germans did so badly?

I have my views on that. I really think that the German scientists got so little pleasure out of contemplating giving a bomb to Hitler that they failed to make those simple inventions which you had to make before you could say with any degree of assurance that you could make a bomb.

You are saying the German physicists really did not want to find a bomb?

It was not a conscious decision. I think there was a subconscious impediment against putting steam behind this. I can give you an example; for instance, the Germans did one experiment to determine whether the uranium-graphite system would sustain a chain reaction. They concluded wrongly that it would not, and they dropped it. It is one single experiment. Now I tried to find out what happened, and I talked to Professor Heisenberg who made the theory for this experiment. Heisenberg told me that even today he doesn't know what was wrong. You see, with real interest, it would not have been conceivable to do just one experiment.

As to your own role in the bomb work, after the Manhattan Project began. . .

The Manhattan Project didn't take over until the middle of 1942, and by that time we knew that we knew how to make a chain reaction. Essentially, we knew the answer to all the major problems, and this is fortunate because if we had been organized too early we would have been hindered, I think, to the point where we may not have found the answers. It is important to note that we were unimpeded by the Manhattan Project during the formative phases of this work—when the ideas had to germinate, you know. After ideas are all down, then you can organize it; but if you organize it too early you have to go to so many meetings you have no time to produce an idea.

Perhaps we should jump now to the period after the war. You left the Chicago Metallurgical Project after 1945. Was it then that your interest began to shift from nuclear science into biophysics?

No, I was always interested in biology, and in 1933 when I went from Germany to England I thought of shifting to biology. But then I had the thought about the possibility of a chain reaction—and the discovery of artificial radioactivity made physics too exciting, and I couldn't leave. But during the war I made up my mind that when the war was over this would be a good time to make the shift.

Was this entirely a return to an old interest?

No. When somebody does something, there are usually many motivations involved. It is not a single motive. Physics has had a change of character. The interesting portions of physics have moved to higher energies where you have to have a Committee and Planning and getting

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the Machine and getting the Money for the Machine and the Committee deciding which Experiment should be done first—you know. This is not the kind of physics I enjoy.

Not the kind you used to do.

I like the physics where I can think up an experiment today and do the experiment tomorrow. This I could do in the beginning of neutron physics in '34, '35. My neutron source was a little beryllium mixed with radium at the end of a long glass rod. I came into the room and held the source away from my body as well as I could and said: "Well boys, what experiment do we do next." Today you can't do that in physics.

And the basic charm of biology in 1945 was that you could still do this.

Yes, and others. I really find the mysteries of biology more intriguing than the mysteries of physics.

Was there a specific biology problem that appealed to you?

There was no one single problem. It was rather a feeling that I can come to grips with interesting problems very fast if I go into microbiology, which is a very young science. If you go back to Pasteur, in the early days of biology, the microbes were not studied for

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their own sake; they were studied because they cause disease, and it was more or less an applied field. Just before the war people began to study microbes for their own sake, and this was what I wanted to do.

How did you get started?

I teamed up with a young colleague, a physical chemist, Dr. Aaron Novick, who is now director of the Institute for Molecular Biology at the University of Oregon. I knew nothing about his interests, and he was at the time employed by the Argonne National Laboratory. I called him up and said, "Novick, I decided I want to go into biology and want to start out by learning microbiology. Would you like to come along with me?" And he said, "Yes." We had never discussed this before. I just called him up on a hunch, and it worked.

Did you feel you wanted this particular man for his character, for his knowledge of physical chemistry, or. . .

Well, I don't like to work alone. I'm not too good working with my hands. So working in partnership I thought was better.

Is two the magic number for you?

No, three would be all right, even five. I don't like large programs if I can help it.

Is Novick younger than you?

He is much younger than I am, but I don't know how old. [*Novick is 43 today, Szilard 64.*]

At that time not many people with training in physics were working in biology, were they?

A few, but not many. As a matter of fact, when I visited Moscow a year ago last December, Tamm, the distinguished nuclear physicist, said, "Tell me this, Szilard. Today everybody knows that biology is very interesting and that physicists ought to move into biology. But how did you know that in 1945?" I was probably the first physicist with notable achievements in physics who made this jump —and at an advanced age, you see. I was 47 years old at that time. I could not have known for sure that I could make good.

Well, how did you know?

It was not too difficult to know. Such things depend not so much on your intelligence as your character. If you don't fool yourself, then you'll see a number of things which other people may not see because they prefer to fool themselves.

How might you have fooled yourself on this?

You see, at 47, I took a certain risk here. If I had lacked the courage to make the change, I would not have admitted this to myself; instead I would have underestimated the possibilities of biology. I could not have seen clearly the potentialities of biology if I had been reluctant to make this step through lack of courage.

There's a theory that very young men make the discoveries, because everything is new and fresh to them. By moving into a new field at 47, did you get a freshness of viewpoint equivalent to that of a young man?

Yes, of course, I was young in biology. It's the excitement of novelty, you know.

It's not that we're brighter when we're younger?

I think both are true. I think we are brighter when we are younger; and also we aren't prejudiced when we first come into contact with a problem. Once a man has missed the solution to a problem when he passes by, it is less likely he will find it the next time.

Are you as good a biologist as you were a physicist?

Yes. But I am older and therefore less productive.

You were able to come to it strongly as to physics?

I have not yet come as far as I could have, but I am satisfied mainly as a result of three papers I published the last few years. These I like very much.

How did you learn biology? You didn't tackle it like a graduate student?

I didn't go to classes except in summer. I picked up the techniques in the summer. The Cold Spring Harbor biological laboratory offered summer courses in bacterial viruses, and I took one of these courses. After one such course I started to experiment.

You learned the techniques. How about theory?

Any theory that exists you can learn in two days.

But you weren't working as a physicist, studying these things as physical system?

No. I think what I brought into biology—and this is quite important—was not any skills acquired in physics, but rather an attitude: the conviction, which few biologists had at the time, that mysteries can be solved. If secrets exist, they must be explainable. You see, this is something which modern biologists brought into biology, something which the classical biologists did not have. They often were astonished, but they never felt it was their duty to explain. They lacked the faith that things are explainable—and it is this faith, you know, which leads to major advances in biology. An example is the Watson-Crick model for DNA, a model which immediately explains how the DNA can duplicate. Everyone knew that DNA can duplicate, but nobody asked how it does. The desire to know how led these men to fool around with the structure of the molecule, and when they did that they saw that a particular structure would explain the duplication.

After the war, as Tamm said, you realized biology was entering the sort of exciting period physics had in the twenties and thirties. Does the DNA breakthrough mean that the great days are over and that biology will settle down to exploring that?

No, I don't think the great days are over. We haven't even scratched a problem like differentiation.

I'm not sure what you mean by the problem of differentiation.

Well, we have a single cell, an ovum, which is fertilized, and from this ovum we get an organism which has a great variety of different tissues. This is called differentiation—during embryonic development. Even if you understand the single cell, the microbe, you still don't understand this. Now this is a very big portion of life. So we are far from over.

Can we get a clearer picture of the sort of problems you, yourself, are concerned with in microbiology?

I published a paper on the theory of aging in January, '59. This is really on the theory of aging. It may be a wrong theory, but it is a theory—in the sense that it has made hard and fast predictions.

This is at the macrobiological level?

No, because in this theory of aging, aging is a process which goes on in the individual cells. Not the organism, but the individual cells, are the seat of it, and particularly the chromosomes. So it is really a molecular biological theory. I don't know whether it is right or not. It is very difficult to say; we don't have the right experiments to show. But if I say that this is the only theory of aging in existence, I mean that this is the only theory which is quantitative enough to be proven by the right experiments. So I like this theory because it is a theory which can be disproved—proved or disproved.

You said you had three papers you were fond of.

Yes, in March of '60 I published two more papers. One describes a mechanism that would enable us to understand how a microbial cell regulates the level of different enzymes. In a classical experiment, you take a bacterium, B.Coli, which lives in the gut; if it is grown in a nutrient medium in the absence of the sugar lactose, it will not make the enzyme, lactase, which splits lactose. But if you add lactose to the medium, then this bacterium makes a very large amount of the enzyme which it needs to split this sugar. This is called enzyme induction, and in this paper I gave a model of how the process of enzyme induction occurs in bacteria.

Again, a molecular model?

Yes. Now I had had this model already in my head but I had never felt impelled to publish it, because I was not able to connect it with another phenomenon—antibody formation in rabbits—which I felt must be somehow related to enzyme induction in bacteria. You know if you inject foreign protein into a rabbit, it responds by making an antibody which can specifically combine with this foreign protein. How does the cell make the antibody which fits the particular antigen you inject in the rabbit? I tried to make a theory, but something was missing.

So what happened?

In a plane flying back from Stockholm to London, in the Fall of '59, I suddenly had an idea. Everything fell into place, and I thought, "Now I might be able to understand how the antibodies are formed."

Just what was the problem?

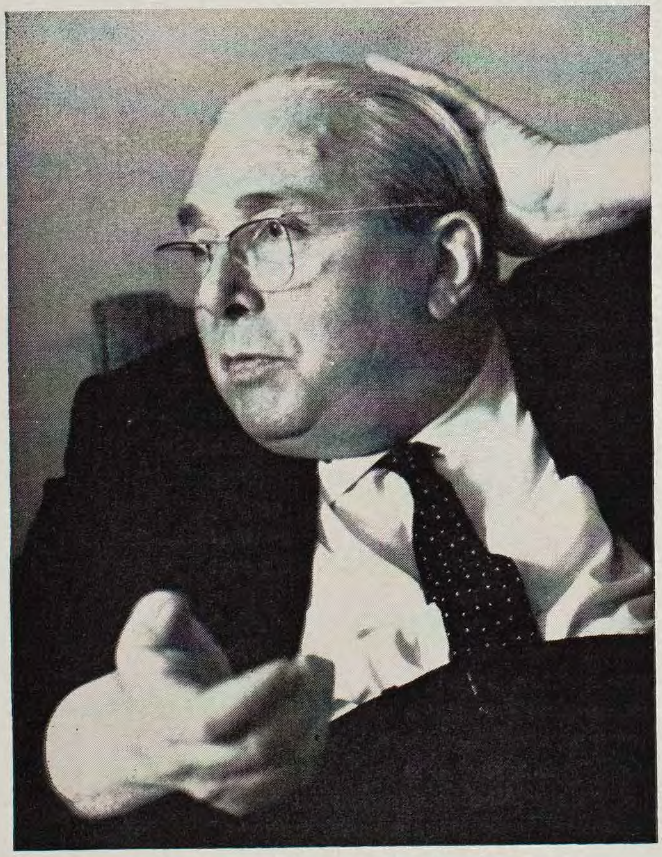
The trouble is that in antibody formation you have to explain two phenomena, simultaneously. One is that if you inject protein into a rabbit and then inject the same protein a month later, the rabbit responds with a much stronger antibody formation. It's called the secondary response.

And the other?

At the same time you have to explain a phenomenon which is called tolerance—namely, that when you inject large quantities of a foreign protein into a newborn rabbit, then later when that rabbit grows up, he cannot make antibodies against this specific protein.

You need to explain both tolerance and secondary response.

Yes, and the idea of how to do this came to me, you see, on this airplane flight. My explanation of the secondary response invokes a mechanism which is endowed with memory; the rabbit "remembers" that it was exposed to this protein before. I had a model for induced enzyme formation, but I had failed to see earlier that if I just changed a constant in it, made it bigger, the model became endowed with "memory." Now at the time when I returned from Stockholm, I knew I was slated for surgery, but they didn't think I was seriously ill, so I said, "Give me six weeks or two months; I have an idea and I want to write this down." And so I wrote these two papers. I told you, on my aging paper, that I just wouldn't know how to bet whether it is right or wrong. But the experiments which have been done since I published this paper on antibody formation have strongly increased my belief that this paper is really right.



"So far I have found that the Russians aren't very interested in arms control. What the Russians are interested in is an economic saving through disarmament."

The work you have been describing seems to be very pure science, entirely motivated by curiosity. Over the next ten or fifteen years would you expect these basic discoveries in microbiology to produce applications?

I will answer this by telling you a story. A year ago last December I was in Moscow to attend the sixth Pugwash conference. . . .

The one on disarmament, the one Wiesner also attended?

Yes. And while I was there I was invited to talk to a writers' club about molecular biology, about my work. One of these writers said, "Now what practical consequences does this have?" So I said, "As far as I can see it has no practical utility whatever—but of course if you had asked me that about nuclear physics in the 1930's I would have told you the same thing." And then the Russian said, "Well in that case, wouldn't it be better if you stopped right now?"

The idea of controlling life processes could be just as disturbing as the idea of controlling nuclear forces. Do you worry much about that?

No, I don't worry about that, but I am aware of it. What's the use of worrying about it? You may have seen my little book, *The Voice of the Dolphins.* There is one story in there, called "The Mark Gable Foundation," about a foundation set up to retard science. You can make a strong case that science is progressing too fast, that we should retard its progress so that social advances can catch up.

How did that work in the story?

The method described is to create a foundation with a very large endowment, and every year you take every creative scientist and put him on a committee to pick out from submitted applications for research grants the most deserving. A system that is very similiar to what is now in operation in America.

Most of the Dolphin stories, I believe, get into the area of disarmament—and generally in a way that strikes most of us as full of paradox. You seem to put your serious thinking, quite often, into the form of satire.

It used to be that if I said something serious, people thought I was joking; but now if I joke people think I'm talking seriously.

Aren't you, usually?

Sometimes. Today you see the world is on the verge of an all-out arms race. We stopped testing for a period, and this somewhat slowed down the arms race. But now we are starting to think in terms of antimissile missiles and decoys to neutralize them; this really is the beginning of a new kind of arms race. I think that people in the administration know that this is not the solution, and they are considering how they could get some sort of arms control.

Are you?

I have attended practically every Pugwash conference, because I wanted to find out what our Russian colleagues are like and how they think. So far I have found that the Russians aren't very interested in what you call arms control. You see arms control means that you slow down the arms race, but it doesn't say what level of expenditure you are stabilizing; many of my American friends talk of stabilizing the arms at a very high level. I couldn't get any spark of enthusiasm for that out of our Russian colleagues. What the Russians are interested in is to achieve a considerable measure of economic saving through disarmament.

You really think that's a major motive?

I'm convinced of it, and you can understand why this should be so. The Russian national income is about 40% of ours, and they spend about as much as we do on arms. This amounts to, say, 10% of our income, 25% of theirs. This is even worse for Russia than it looks, if you consider that their industrial production may be a smaller proportion of national income than ours. I'm quite convinced from all the conversations I had while in Russia that they are very much interested in the kind of disarmament which would enable them to lift this burden of heavy expenditure from their economy.

How much variety of viewpoint have you encountered in your talks with Russians?

There is only one viewpoint—that it's impossible to solve the housing shortage with the arms race as it is, that consumer demands cannot be satisfied, and that there cannot be aid to underdeveloped nations on an adequate scale except if there is disarmament. I have heard no other view in Russia.

But this is not an important motive in this country?

There is no indication so far that any American in a responsible position would accept disarmament, as against arms control, even if a satisfactory inspection were included. I think the basic reason for this is all the uncertainty of how peace would be secured in a disarmed world; obviously, disarmament does not automatically secure peace. An army equipped with machine guns and other conventional weapons can spring up overnight.

So there's a discrepancy in viewpoint?

You see, we are now negotiating in Vienna. You can negotiate successfully only if you have some goods which others want to buy. The only goods I have discovered which the Russians want is an economic saving which disarmament would bring about. This they want to buy, and I think they would pay a commensurate price for it in terms of political accommodations. Obviously, if we have disarmament, we in America cannot retain our military commitments to defend nations which are geographically remote from us and in the proximity of Russia. So to make disarmament politically acceptable to America, it will be necessary for Russia to agree to the kind of political settlement which enables America to withdraw from these commitments without too much loss of face and without sacrificing the security of the nations involved.

You've been spending most of your time in Washington for some months now, thinking and talking about this. What would you like to see done first?

I think the most fruitful thing would be the establishment of a group of scientists and scholars composed of Americans and Russians to talk about how the peace may be secured in a disarmed world. It must be done with the blessing of the governments. Particularly the Russians couldn't do it otherwise. I am often asked if the Russians can talk freely—and the answer is, "Yes, if they are so instructed by the government!" Such a private study could take something like four months. It could end up in a working paper in which Russians and Americans list various solutions, and pointed out in each case what could go wrong—because almost every solution has some weak points—and then they would turn this all over to the governments, which could take over from there.

In these efforts, Dr. Szilard, do you feel you are in an almost hopeless, last-ditch struggle? Or do you feel hopeful?

I think all I need to be fully effective is a 10% margin of hope. It's enough to make you concentrate your efforts. This much hope I think we have, but I won't say we have much more.

Ten percent isn't much, but, as you say, one can live on it.—DC/RC

Readers who wish to learn more of Leo Szilard's ideas, on biology or on world politics, will find some his writings listed in To Dig Deeper, on p. 82.