An Interview With Dewey B. Larson

by Jan Sammer

Taped in August, 1984 at Salt Lake City (in the text version, the order of the paragraphs has in some cases been changed)

JS: I'd like to start out by asking a few biographical questions. Somewhere I've read that your family comes from Norway. Was it your parents or your grandparents who first came to this country?

DBL: My parents were small when they came, infants, I guess. My grandparents came from Norway at the time when my mother was small, and my grandparents lived in North Dakota and Minnesota. And that's where I was born, in North Dakota, and left there when I was about seven.

JS: The whole family moved to Portland?

DBL: Oh no, they came west by stages. My father was a wanderer; he was never satisfied very long in one place. He could see the greener fields over on the other side. So he decided to move on to Idaho, a new country then, just settling the reclamation project Twin Falls, and we lived there for a number of years. But then the grass grew greener over farther west, so we moved up to Boise, and back again, and finally moved out to Oregon.

JS: Your father was a farmer at that time?

DBL: Well, he was never a farmer, he was a merchant. That was his business in North Dakota. He worked for some other merchant for a while and then went into business by himself. Then, when he went to Idaho, the idea was to run a store in town and the farm on the side – so my mother did most of the farming. Then we moved out to Oregon; they bought a property in Wilsonville, which is some twenty-five miles south of Portland and farmed that. Being a farmer did not sit very well with my dad, so before very long we moved into Portland.

JS: You went to high school in Portland?

DBL: I started high school in Idaho; I spent one year there and one year in Buel, which is in Falls Country. So I had two years under my belt when we left Idaho, but when we moved on to Wilsonville, there was no school within ten miles, and there was no particularly good way of getting there, so I didn't go back to school then. I was out for three years, with really no intention of ever going back. But I don't know, some bug hit me at one time I guess – I was working for my father at the store out there, and then I just quit and took two weeks' vacation down at the shore, and went back to school.

I had quite a time in mathematics when I came back. I showed a remarkable development in the few months. When the first report card came out, I got a "D" in mathematics; by the time the Christmas vacations came up I was up to a "B" and still moving. The instructor figured that he had done a remarkably good job in teaching me, a fellow who didn't know anything about the stuff. Well, he happened to mention that progress to my old high school teacher from Boise, and she says to him: "You haven't done anything – he was one of our star pupils in Boise." And she checked up to see what the trouble was, and it seems I was taking third term algebra, and I hadn't taken second term algebra!

JS: Did you like mathematics in school?

DBL: Well, I think I could have turned out to be a pretty good mathematician, if I had gone ahead with that. One of the ways I was able to get through college in three years was that I got credit for all of my freshman mathematics without doing anything. I went in and talked to the mathematics instructor and told him that I didn't see any reason for taking freshman year mathematics. So after pondering it for a while he made me a deal that if I'd make an "A" in calculus he would credit me with the freshman mathematics. That gave me a year's work in maths free of charge, which helped out quite a lot. I didn't pursue mathematics afterwards, because I didn't have time to take a lot of maths courses in college, and there was really no reason for doing it after I got out. The fact that I'm steering the theory away from mathematics now is not because, as some might think, I have a dislike for the subject, but simply because I found out that you can't get to what I was after by that route.

JS: What were your other interests in high school and in college? Was it science from the beginning? Were you interested in physics and chemistry from the start?

DBL: No, I wouldn't say that. Even now, if I had to classify myself, I don't know that I would necessarily classify myself as a scientist.

JS: What was your major?

DBL: I graduated in mining engineering – not because I had any intention of doing mining, but because I only had enough money to get by for three years. Engineering gave me the subject matter that I wanted, and made it possible for me to get through in three years. Besides, I wasn't too well satisfied with any particular course that was being given at the time; all I really wanted was something of the kind they now call general engineering: they didn't have that in my day. It's more or less a mechanical engineering course with more attention to such things as chemistry than you would get in mechanical engineering or civil. So what I did, I set up the kind of a course I wanted and then I checked around to see where I could get it approved. It was the dean of the school of mines who gave me the best proposition: he was willing to take one of these courses and make substitutions on through which would give me what I wanted and still be able to graduate in his school. I was very pleased with the professors I had, generally.

JS: What was the name of the school?

DBL: Oregon State. In my day they didn't emphasize the scholastic achievements in the way that they do now. One time I got some kind of a senior class honor – Linus Pauling and I shared it in the engineering schools – and I don't think it mattered any more to him than it did to me. The B grades I got were not necessary; I only got them when there was some good reason for it other than scholastic. Most of them were in physical education, mainly on the basis of attendance. I disliked the business, so I skipped when I could. I thought one time I found a good way of getting the ten credits without work when I heard that they gave you ten credits just for running around with the cross-country team. Well I ran around with them one time, and finally made it in, I guess, an hour or two late, but decided that that wasn't the way I was looking for.

Maybe I didn't end up with so much respect for authority as I would have in some other place. Well, I don't know, I've always been somewhat of a rebel.

JS: After you graduated came the job with the mining company?

DBL: I was out doing some surveying first; but as soon as it began to rain, well, I wanted a roof over my head. So I decided I'd grab anything that came along temporarily and look for something better later, and got this job that served the purpose at the time. Actually, one didn't quit a job as readily in those days as you do it now.

JS: But the 'twenties were still a good time economically?

DBL: Oh yes; it wasn't until the beginning of the 'thirties that we ran into trouble; as a matter of fact the stock market crash was in the fall of '29 and it took a year and a half to hit our company. But when it did hit it hit with a bang. I wasn't there more than a few months before I got shoved into administrative work, as so often happens with engineers in industry. And that was one of the things that contributed to my going into the research work on the side.

JS: You wanted to continue doing research in some way?

DBL: I didn't want to lose all touch. Later I got back into the engineering department of the company as chief engineer and it was just as well that I hadn't lost touch. But the other stuff just grew.

JS: What was the actual problem that you started to tackle? Was that the problem of inter-atomic distances?

DBL: As I sort of expressed it to myself, I wanted to tackle some particular area – I had been piddling around with little items here and there, and then I decided I wanted to tackle something specific, of the rather first order of magnitude, so that if it did hit something it would be worthwhile. So I thought I would tackle something I knew had an answer, or felt certainly had an answer, and yet nobody had gotten it. That was the idea of a method of calculating physical properties from the chemical composition. I worked on the problem for a great many years, and it was only when that steered me into having to look into the fundamentals to answer the problems I was having, that I got into the idea of changing the fundamental basis of physics.

JS: So you actually started your research in the 'twenties, right after graduating from college?

DBL: Well, all these things start kind of gradually. After all, if the instant money had been available when I went to college, all these grants and everything that you can get now, I undoubtedly would have gone into research work, and I wouldn't have done this, because I would have gone along the conventional lines too far to get away from them. But I had to do something with which I could earn a living right away. When I began to work for the company, I got into a situation there that increased my interest in doing research.

JS: Which company was that?

DBL: It was the gas utility in Portland, that's now known as The Northwest Natural Gas Company. But that company used the services of the faculty at Oregon State University quite extensively for research purposes. We would turn a problem over to them and we'd get back a report.

JS: These were engineering problems?

DBL: Our company was engaged not only in distributing gas; we were also a chemical manufacturer and had quite an extensive chemical business. We sold most of our benzene to New Jersey, shipped it in tank cars clear across the continent. So we had quite a wide range of research interest, and utilized consulting engineers from time to time, including these fellows that were at Oregon State, and for some reason or other I got mixed up with the job of reviewing these reports. When you get a report from a consultant, that doesn't end it; the company then has to decide whether they're going to believe these people and act on the basis of that report. I was one of those who got assigned the job of reviewing them from the company standpoint. Now on a number of occasions I took exception to the report on the grounds of some point that in my opinion was wrong, which they had taken from the textbooks – in other words, the textbooks were wrong. In those cases usually we had them send it back and have them review that point and they found that the textbooks were largely wrong. As an example, one of the big problems in the gas distribution business is leakage. And one of the problems is how far we can go in doing certain kinds of maintenance work or leakage prevention work from an economic standpoint. A question came up one time in connection with that as to the feasibility of certain work, and it depended largely on the amount of change that would occur in the leakage with the change in pressure. So our consultants gave us a report on the economics of this thing and they figured the change in leakage due to pressure by the ordinary orifice flow formula, in which the flow varies as the square root of pressure. So, I got that, and it struck me that that doesn't agree with our experience at all. And so I ran some tests, and tackled the thing mathematically, but by looking at it in a little different way. Instead of regarding this as a flow through an orifice, I recognized the fact that the pipe was buried, so that you actually don't have one orifice, you're flowing through a whole series of orifices before you get up to the ground and get it out into the open air. So then I merely took that same formula and integrated it over a series of concentric orifices, and then it comes out to a direct proportion of the pressure instead of the square root. Once we got back to check it with the faculty, they agreed with that. But that's the kind of thing that led me to believe that we have a lot of errors in textbooks.

JS: It seems like the wrong application of a correct formula.

DBL: Well, it was, in a way, but it's a little more than that. It's a question of a wrong view of a problem. They looked at it as a matter of a flow through an orifice. And actually when you study the thing, it's not a flow through an orifice, it's a flow through a series of orifices. Now that's exactly the same thing that we're doing here with a lot of problems when we're applying the observations to the theory. We are not changing the mathematics or the physical equations, we're changing the view of the problem. So then I finally decided to go at this idea of figuring out that this molecule was structured with such and such atoms in such and such a way in order to have such and such properties.

JS: So you began this a project only after you began to work for this company.

DBL: Oh yes, quite a few years after that.

JS: Would that have been around 1925, say?

DBL: Well, it could have been – I went to work for them in the fall of '22.

JS: What kind of work had been done on this project before? Could you use some of the previous results?

DBL: Oh, there have been all kinds of efforts. In fact, one of the empirical or semi-empirical equations that are being used by the chemists now had to be the result of this kind of an effort. The literature is full of that stuff. The question of equations of state, for instance, the question of how the solid state of matter responds to temperature and pressure has been a subject of enough books to fill a room here. The approach has been to handle it mathematically; they've tried to arrive at certain constants, and then to derive equations whereby they can assign these constants to the individual substances, and then go into their equations to get the properties under different conditions. And the number of adjustable constants has grown to rather absurd proportions in some cases. The Benedict-Webb-Rubin equation of state, for instance, has something like seven or eight of these adjustable constants – which means that when you've got it, you still haven't really answers, because you don't know what these constants mean and what constants to apply to what substances. You've got to go out and check that in the field every time.

JS: You just have to try each one?

DBL: It's a curve-setting process, actually. You try to draw an equation which you can adjust here and there to the point that you can assign these constants and come up with the answers. As a matter of fact, the quantum theory is on the whole very much the same thing. These quantum numbers are the same kind of adjustable constants that they're putting in the equations of state. What I wanted to do is what we have done now, at least part way – that is what I was illustrating in putting my periodic table vertically instead of horizontally, so that each element gets a set of numbers not taken out of thin air or determined from experiment or anything like that, but from pure theory. And to the extent that we have arrived at equations such as that for inter-atomic distance, they are theoretically explained, rather than being, as Feynman said, merely quantitative.

JS: But you also started with a mathematical approach.

DBL: I was trying to do exactly what the constructors of equations of state are doing. I was trying to find mathematical equations in which numbers could be assigned to these different substances, exactly as the rest of them were doing. The only thing is that I came down to the point where I recognized finally that that wasn't going to get me what I wanted, because ultimately I am going back to a number that is arbitrary, or a series of numbers that are arbitrary. So I finally decided what I had to do was to get something that is meaningful to start with and work the other way.

JS: Did you write up these researches?

DBL: There is a handwritten manuscript – something like the Dead Sea scrolls – of what was probably the first version of the first book. It dates back to about 1930.

JS: Really, that far back?

DBL: Yes, somewhere between 'thirty and 'forty – no date on it, but I think the first chapters were probably written around 1930 and the last chapters right around 1940. This was the original manuscript of what eventually turned out to be the book that was published in '59 – but of course I was in the process of writing that over a period of twenty or thirty years. And as I say, that's very much like digging up ancient ruins, but it may be of interest like all ancient ruins, if nothing else. This manuscript that I am talking about was not written for publication; it was written to keep my ideas in a coherent form.

JS: You must have intended to publish it eventually, though.

DBL: I didn't start with it, no, because I had no idea at the time I started it that I would have anything that was complete enough to be worth publishing. You see, I had no intention at the start to go into a new theory of physics; I was tackling a particular problem.

JS: What was the first significant result from this research? I understand that you started to get results sometime in the thirties, especially this equation for inter-atomic distances.

DBL: Well, it's a little hard to say. I was working on a number of things, somewhat intermittently, and this inter-atomic distance business was one of them. But that was the thing that finally shoved me into the need to discard the idea of building mathematical solutions. If I couldn't solve that problem mathematically, that of course carried on into the more general operation too. Here I was working on a problem that should have been purely mathematical as far as I could see, to start with. You get certain elements and you're trying to find why they have certain properties; if you can identify these elements by numbers, then you should be able to get mathematical solutions. But the trouble, as I finally realized, is that there is something wrong with the view of the problem on which you're basing the mathematics. And when that is the case you're not going to get the answer until you revise that view of the problem. So from then on my work was angled toward getting the right view of the problem rather than in getting the right mathematics. And in a very large percentage of the cases, when I got the right view, I didn't need any mathematics. Take the vibration of the photon. In order to have a vibration you've got to have two forces – you've got to have something that throws you off equilibrium, and then a restoring force that brings it back. Working from this view, it's a mathematical problem, and that's what I was trying to calculate, just as other people were. And none of us got anywhere. Finally I concluded that I had to get a better view of the problem. When at last I realized that a simple harmonic motion is just as fundamental as a linear, or translational, motion, I had an answer that doesn't require any mathematics. You start out with motion, and motion as such can occur in different forms. In translational motion you don't require anything to keep it going, it just is. And there's nothing that says it has to go in a straight line. If you have a rotation, in the absence of friction you can rotate forever. Circumferential motion does have two forces, the pull in toward the center, and the angular momentum. But pure rotation doesn't require any interaction of forces. So what I finally had to realize is that at the most fundamental level – that is, only one unit at a time – simple harmonic motion is just as permanent as a rotation or as translational motion. It's really a combination of the two.

JS: Was that the first idea with which you began to diverge from the accepted views?

DBL: Well I can't say exactly where it came in, but that was what I had to realize before I could get started on such things such as the structure of the atom, because before I could do anything else I had to have a vibration, and I had to see how that came directly from the postulates, rather than as a mathematical result of a disturbing and restoring force, which I didn't have at that stage.

JS: You mentioned in one of your lectures that you had these three numbers for the atom, and later you realized that these were the dimensions. Was that part of the mathematical stage of your research?

DBL: That was a gradual development. This was when I was still in the inductive phase, still trying to get the mathematics straightened out. It seemed rather obvious that there was some kind of a mathematical pattern in the periodic table, and the first view of it, one that the chemists have, is that it's a two-component system, that is, they have one series across and one series down.

JS: Did you approach the problem from a different angle right from the start?

DBL: I did start out with a couple of assumptions – not assumptions for the purposes of the theory, but assumptions for the purposes of investigation. I decided that since most of the investigators of this problem had done gases on the theory that gas is the simplest structure, I would make the opposite assumption that the solids are the simplest structure and work on the solids first. And then since they all started with the assumption that the best way to go about it was to get the properties of hydrogen first and then go on with the more complicated substances later, I decided I should tackle the more complicated substances, and work backwards toward hydrogen. Well those working assumptions turned out pretty well. The solid is in fact a good deal simpler than the gas, because the simplicity of the gas is deceiving. It comes about because you really disregard the properties of the gas. The gas laws as we're using them, the properties of gases, are actually the properties of the empty space that we're dealing with, so that in our simple gas phenomenon, we're getting that simplicity by disregarding the phenomenon we're working on. The ideal gas is a gas that has nothing but empty space in it. As soon as you get to the properties of the substance itself, then you have to make a correction. And you have to correct it down to the recognition of these things you already have in the solid to begin with. So in that way the solid is a simpler structure. In the other direction, the value of tackling more complicated substances first is that then you have series relations. The series beginning with sodium, for instance, is quite well defined. So I began with the sodium/potassium series, and I took that as a two-component system as the chemists do, but before I got done I realized that I had to introduce a third number. I got some results with the third number that I couldn't get with the two. But the minute you get into three numbers in anything like this, your immediate reaction is, those must be dimensions. That's an obvious conclusion in a three-dimensional universe. So that was the thing that led into that.

JS: And then from that you deduced the atomic structure?

DBL: Yes. When you got dimensions then the next step was to identify the numbers with the rotations and the speeds. Of course, there were a lot of different angles to work out on that, because even though the structure may not be too complicated, in certain ways it is complicated in that there are alternate ways of fitting into the reference system. We still have more work to do to clarify this point in the theory.

JS: You mean the coupling to the reference system?

DBL: That's part of it, yes.

JS: So you had these three dimensions, and you were working also with the photon. You realized that simple harmonic motion could be permanent. Then somehow you brought these two ideas together and concluded that the photon was actually one of the building blocs of the atom?

DBL: Let's put it this way: The idea of the photon as simple harmonic motion was really the same thing that Galileo had in respect to translational motion. According to the previous ideas based on Aristotle's work it was necessary to have something to cause continual motion, and the problem was to identify what it was that caused the motion to continue – I think that Aristotle himself used angels to push the planets around. Well, Galileo discovered the fact that you don't have to have anything to explain that. That is, motion, because it's motion, is continuous. Now I was merely extending the same thing to this simple harmonic motion, because once started, it is nothing but a motion that continuously changes direction and consequently it requires no more explanation than translation. Now once you have a vibrational motion, you have an object in space. For the purpose of explaining radiation all I had to do was to let this object move in a direction perpendicular to the vibration. There were other questions in connection with radiation of course that I still hadn't clarified, but up to this point that was all I needed. You can't rotate until you have something that is going to rotate.

JS: So basically you took the simplest object that you could conceive of.

DBL: I'm developing from the standpoint of simply a relationship between space and time, and that's the first thing that I was able to derive from that, so that's all I needed. When I got something that would work, that's the most likely thing anyway, so that was the thing then that I decided to rotate. Then the next problem was to see how that fitted with the figures that I had, the results of observation and measurement. And of course, that took a long time, but the thing that kept me at it was that there was continuous progress. You could see the thing developing in a natural and easy-enough manner so that it was clear that you were on the right track. When I say easy manner I don't mean that I saw it easily. It's something I should have seen. I've had that experience so many times – I would come to something as a result of a long and difficult period of work, and then recognized that it was something that if I had been smart enough I should have seen immediately.

JS: You mentioned once that at the beginning you went into a lot of blind alleys, but I imagine that after you got these results you were making pretty steady progress.

DBL: Well the blind alleys were primarily in the inductive phase of the work. You see, there were really two phases to this operation, there was an inductive phase in which I was doing as you always have to do in inductive reasoning, you start from the observed and measured facts, and work back up toward the general principles that govern those. That was what I did until I arrived at this concept of the universe of motion and described it in the postulates. That was back around 1950. So the work up to 1950, the inductive phase of it, was the hard work. Since then it has been the deductive phase. I have simply gone from these postulates and worked down toward the consequences. In the original development, starting in about 1950 and into the deductive part of it, I was working mainly within these general principles. But since I had gone up along a certain route, when I started down I had this information pretty well in hand, in these particular fields, such as inter-atomic distances and the liquid state. Those were the things that I used to get up to the general principles, the postulates. Coming back down again, in the deductive phase was easier because I had already covered them in the other direction. I never had so much trouble with blind alleys after I started on the downhill proposition. In the deductive phase you don't go into blind alleys so much – the problem there is usually a case of not seeing where to go next after reaching a certain point. But if you see it at all you usually see the right direction. That was not true in the inductive phase. It's something like the difference between the two kinds of calculus: In differential calculus you always get an answer; you follow the rules and you go directly from the problem to the answer. Well that's not true in integral calculus. You don't know whether the answer's there or not. That's the same kind of a thing in inductive and deductive research. In the deductive research, if your theory's right, it's just a matter of finding the right avenue of approach and setting it out, but in the inductive phase you don't know whether you've got an answer at the end of the road or not, and you have nothing to tell you if you're on the right road or not, unless you get into trouble. Now when you do get into trouble you're not sure whether it is because you're not smart enough or because you're on the wrong road. For instance, I spent, I would say at least a couple of years on a trigonometric solution to the question of the elements, because I was figuring that maybe it has something to do with the position of the elements in the solid state. I even got some interesting results from that, but finally I was forced to conclude that it didn't work, so I discarded that, threw out two years of work, and started back again on a different basis.

JS: How long did the deductive phase last?

DBL: I would say it must have been about five years before the publication of *The Structure of the Physical Universe* that I finally started downhill.

JS: Until that time you were concerned with formulating the general principles?

DBL: Yes. Up to this time I had been working from the basic information, the basic observations and measurements and trying to get the general principles controlling them. Particularly at that time I was very much interested in finding the reason for these equations I had developed for the inter-atomic distance. You've seen the equations in the book – they're simple enough. But what do they mean? That's the old problem of present-day physics; they have so many of these nice equations, but they don't know what the various quantities are that enter into the equation – they don't know what they signify. In fact there are some scientists dealing with those things that contend that they don't have any meaning, that the mathematical expression is the whole thing. But I was trying to look for the meaning.

JS: Do you remember anything about the circumstances of your discovery?

DBL: I had this mathematical expression on the inter-atomic distance, and I remember this particularly, I was on a trip, driving down to Corvallis, and about half-way down there it struck me that this expression that I had found for the inter-atomic distance was the integral of a reciprocal relation. So I said to myself, well, what does that mean? Well, then I realized that we actually have a reciprocal relation here – what we're doing we're getting the summation of it over an area or a distance. And then I thought, well now, if that's true, if there is a reciprocal relationship here, what if it's a general relation that applies all the way through? So I started thinking what would happen if there were such a general relation. And I could see right away that that answers a lot of questions. By the time I got down to Corvallis I could see that this would open the door to the solution of at least half a dozen problems of long-standing in the physical sciences. It was like rolling up a curtain, and you've got a bunch of answers right in front of you.

JS: Do you remember what some of these half a dozen solutions

were?
DBL: In the first place, it gives you an answer to the speed of light; it has always been a question as to why we should have a certain speed of light and not something else – what this quantity c means. Well, if it's a reciprocal relation, the light has to go at unit speed. That's obvious, don't you think? And once you have that, that leads to the idea of the space-time progression, which in turn leads directly to the explanation of the recession of the galaxies and to gravitation. The nature of gravitation is that it's simply a scalar motion in opposition to the progression. That kind of thing comes out right away. You don't get all the details, but you do see the general picture.

JS: Could you put an approximate date on that trip to Corvallis?

DBL: That was around 1950.

JS: So that was the culmination of the inductive phase?

DBL: That was the point at which we could say I could start putting the postulates together. It didn't take me long, because all I had to do was to postulate what I needed at the time and set up these ideas in a more complete fashion, with the understanding as I started working on it that if I needed something else I would put it in later. As it happened, I actually took out some rather than putting more in. But all this was done in a relatively short period of time, so essentially the postulates followed almost immediately after getting the idea of a reciprocal relation. The only reason that it struck me, and that I still remember where it was, is that when the thing dawns on you it's a kind of a landmark so to speak.
The same thing was true when I first got the idea what electric current was. I can remember that. I was around Tenth street in Portland, crossing the street just by the library. It's a kind of a striking situation, you see, when you get an idea that's immediately clear, a crucial idea. The idea of course in the electricity matter was that electric current is just space moving through matter. It's a silly idea when you take a look at it, but that's the way it works out.

JS: It seems like such a simple concept actually.

DBL: Yes, it's a simple thing. No matter how silly it sounds, it's obviously correct, if you've been working on the thing, and you can see immediately, when you get this idea, that everything falls into place again.

JS: Was that a few years after your realization of the reciprocal relation?

DBL: Well, I can't remember when that was; that must have been a little while after that, because I didn't attempt to get into any work with electric or magnetic matters until quite a way down the line. As far as magnetism is concerned, once you get the idea as to what the electrical situation is, it's obvious that the magnetic phenomena are the two-dimensional analogs of the electrical phenomena.

JS: That was the point, I suppose, at which you decided to publish your findings. Do you remember when that was?

DBL: That was about the middle of the 1950s. When I came to the conclusion that it ought to be published, I sized up the situation and decided that I had about five years of steady work ahead of me before I'd have enough of the loose ends tied up to be able to publish anything. So I was toying around with the idea of getting connected with a university to get access to the university facilities, as well as the time that I didn't have while earning a living doing something else. A friend of mine happened to be the Dean of Engineering at Oregon State then, so I talked to him about it and let him read what I had ready thus far to give him an idea about what I had in mind for completing it. At that time he was on the committee of the National Science Foundation, one of the committees that picks out the projects to be funded. So the idea that he came up with was that he would arrange with the National Science Foundation to get a grant to take care of expenses, and then he would put me on the Engineering Department payroll, along with a proposition of teaching half-time, so that I would have the other half-time to work on my research. It seemed like a very good proposition, but there was one catch in it: It had to be approved by the School of

Science, and they turned thumbs down on us – so that fizzled out. Now as it happened it really didn't make any difference, because while I thought then that I had five years of work ahead of me, in only about six months all these loose ends came together, and I got to the position where I expected to be after five years of steady work. It just turned out there were a couple of items that I hadn't seen clearly enough before, and once I got them, it was just like when a chemical solution turns into a solid at a certain point, all of a sudden. So that then put me in a position where I was able to publish it. I spent a lot of time looking for a publisher.

JS: You did try to find a commercial publisher?

DBL: Oh yes, but I didn't get very far with that. It came back about just as fast as I sent it out. It even seemed to me as if I got it back before I sent it out. Anyway, I tried. So then in the course of trying to get it published, I read all the things I could get a hold of about the publishing business. Of course there are these outfits that will publish on a subsidy basis, but the people who write the books of advice about publishing tell you that that's not a very good deal, that you can do the same thing yourself a lot cheaper and come out just as well. So I decided I would publish it myself, which I did; only I didn't have the finances to publish the whole thing, so I only published about 50 percent in *The Structure of the Physical Universe.*

JS: I know that you list those sections you left out at the end of the book, but I didn't realize it was that much.

DBL: It was at least half, if not more. What I had in mind then was that if we got anywhere with it we would then publish the other half, but by the time we got far enough along to justify doing anything else it was many, many years later and the situation had changed. So I don't want to go back and publish something that's that old, and that doesn't have the benefit of the new light on the subject that has developed in the meantime.

JS: What happened when you published the book in 1959? Was it advertized? Was it reviewed?

DBL: I sent out review copies on a pretty good scale, and all I got in the way of reviews was a couple of foreign notices. I did get a couple of reviews of The Case Against the Nuclear Atom, including the one by Asimov. But aside from that book, which really had nothing to do with the theory, there was no attention in this country.

JS: Were you contacted by scientists in those early years?

DBL: Not by established people. The attention I got was mostly from the same kind of people that we have now in the organization, and from students. I had quite a bit of correspondence with students, and initially most of the lectures I gave were sponsored by some group of students in one place or another, usually over the opposition of the faculty.

JS: Did you try other ways of reaching the scientists?

DBL: The original publication of the work through my first book didn't get very far by itself. Something obviously had to be done to stir up interest, and what I decided to do was to write the series of articles on the liquid state of matter (that was one of the parts that didn't get into the book), and then send out an offer to selected scientists all over the world – I offered not only to send them the papers, but to send them a complimentary copy of the book. About five percent of them accepted that. So, over a period of several years, I sent out these papers as I wrote them, and developed apparently a constituency of people who at least had one ear open. It would be well to get the view of that project from some of those who received them. For example, a group of young scientists at Queen Mary College in England, electrical engineers, most of them, had a good deal to do with the direction which the development of the theory took.

JS: I had no idea that there was a group there.

DBL: That was in the rather early days of my effort to promote the thing; one of the professors there got a hold of the book, and the liquid state papers – something about it impressed him – and he talked to his students about it. Evidently they were a group of more adventurous spirits than the ordinary class, so I had quite a bit of correspondence with them. And they are actually responsible for my undertaking to write more books after the first one.

JS: Really?

DBL: What happened was that in the course of this correspondence they had asked for certain things to be written, and so I wrote something or other along the lines that they wanted, [see "Just How Much Do We Really Know"] and then it occurred to me that I might just as well expand that. That's how *The Case Against the Nuclear Atom* originated.

JS: I see.

DBL: That was the origin of my idea of going on and writing more books about the subject; otherwise I might just have dropped it at the original point.

JS: I can't believe you would have been able to drop it at that point. But why did you choose to send the liquid state papers to the scientists? Was the evidence more convincing there than in other places?

DBL: There is no way you can verify a general proposition specifically, and the early work that I did was almost entirely confined to these basic propositions. And I figured then that there wasn't much use trying to do anything more with the general principles – I covered them in the book. But I wanted my correspondents to know that those general principles could be carried down to specific items that could be tested. So I had to pick out one of the things that I had studied, and it looked to me as if the liquid state had more information than anything else at that time, so that's why I picked that.

JS: Is the liquid state easier to deal with than the solid state? Does it have fewer variables maybe?

DBL: Well, in a way yes, because it's more regular. In the solid you go into so many variations. Take iron for instance. You can't say what is the density of iron, because you've got to indicate the conditions under which you are measuring it; not only that, but you're influenced by the history of the thing.

JS: The stresses, for instance?

DBL: The density of iron depends on what you've done to it in the past. Under the same conditions it will have a different density if you've put it through certain processes. Not that the substance itself has changed any, but the way that the molecules interact changes. You don't have that in the liquid. The density under certain temperature and pressure is always the same.

JS: So you can make predictions more easily?

DBL: You can calculate definitely. As I've mentioned in my books, you can't calculate a specific inter-atomic distance for iron. You can never say the inter-atomic distance of iron in the absence of thermal expansion and pressure is so-and-so. You've got to say, the possible inter-atomic distances are such and such.

JS: You can't be as precise with the solid?

DBL: Oh, you can be precise, but it's now going to be a series of values rather than a single value. It's a multi-valued proposition. But in the liquid state you have a single value for a given property, under given conditions. And then I found some things that I thought would be of interest to the people I was addressing. I am not so sure they were, in retrospect. For instance, I found that there's a fourth state of matter.

JS: You mean the condensed gas state?

DBL: Yes; and I think that's an important point. You can observe it in the stars, it accounts for the surface of the sun.

JS: If it were just gas, it would be diffuse?

DBL: It would be just a cloud that gradually thins out.
JS: One tends to think of the outer layers of the sun as behaving almost as a liquid, because they're under immense gravitational pressures.

DBL: Well, they follow the liquid relationships in some respects, but of course it doesn't meet the usual definition of a liquid.

JS: It seems to me that you've done so much on the liquid state that it merits another volume by itself.

DBL: Well, I don't know if I'll ever get to that or not.

JS: You don't think you could publish it as it is?

DBL: I don't. Too many things now I didn't know thirty years ago, so I wouldn't want to publish anything that I wrote then as of today.

JS: You could just publish it as something that was written thirty years ago.

DBL: Yes, you could tell them that this was written thirty years ago and I don't agree with it all now, but that doesn't go down very well.

JS: I suppose it's a question of priorities, with so much new data coming in all the time from the research laboratories and the space probes.

DBL: So many things come along that I think would be of interest from the standpoint of the way they fit into the theoretical picture. For instance, I've read that they claim to have created in the laboratory the kind of degenerate matter that they have in the White Dwarfs. Their evidence for that was that ordinarily when you collide these particles they bounce off each other in all directions, but they got up to a critical energy level at which some of the particles – as they reported it in this report that I saw – that some of the particles stuck together for a very short period of time, and then separated. And they attributed that to being this degenerate matter during this period of time. That's just exactly what you would expect theoretically from my development. Let's assume they got to the point where they were able to give a greater-than-unit velocity to some of these particles. They would immediately start moving apart not in space, but in time, and to the investigators they would appear to be sticking together.

JS: There are many other items – new discoveries and new theories – like the gravitational lenses, that need to be explained.

DBL: Regarding the gravitational lens I don't think we have enough information to decide what the answer is, but I would be more inclined to think that it is not a gravitational lens, but something analogous to the two images that we see on the radio image of the quasar. I think there should be more effort made to get people to write in questions that they have in mind. The problem is not in getting the answers, but in getting the questions, getting people to write in about things that they're genuinely puzzled about.

JS: Obviously there are a lot of things people don't understand, such as the inter-regional ratio problem that came up at the conference here. Hardly anybody could explain how that's generated.

DBL: I don't think that people who are asking that question realize that they're asking us to do something that the conventional theory doesn't even attempt to do. The inter-regional ratio is not different from any other physical constant. It's just the same as the gravitational constant, or the gas constant, or anything else. Conventional science doesn't try to explain where they come from; it doesn't even tell us that they have any meaning. So actually these fellows ought to forget that for the time being; if they can't see where it comes from, never mind, take that as a given, just as they would have to do if it were anybody else's theory, and wait until they understand the theory before they go into the depths in these things.

JS: They think it's the key to understanding the theory. That's why they're so worried about it.

DBL: But it isn't. It's like saying you've got to understand the gas constant before you can understand chemistry; well, nobody understands where the gas constant comes from.

JS: Did you arrive at the inter-regional ratio as a deduction from the postulates?

DBL: No, I measured it, just like they measure the other constants; and then I tried to find out how it relates to the rest of the theory, just like I did with the gas constant.

JS: So extending the theory basically involves explaining observed phenomena?

DBL: That's only a small part of it; but one case struck me particularly: My findings all along have required that the galaxies would pick up some of these globular clusters while they were still premature, while the stars hadn't formed yet. That of course meant that some of these clusters had to be in the galaxy somewhere, but there wasn't any evidence of it, nothing in the textbooks about it. Now the astronomers find evidence of these big dust clouds, which are just the thing that I had been looking for. And not only that, but they find that there are, as they say, "clumps" inside these clouds that are denser than the rest of them, which obviously are the stars that haven't made it yet; so the whole thing fits in like a glove.

JS: I have a few questions about your unpublished manuscript Beyond Space and Time. It seems to me that you left a lot of questions unanswered in the part that deals with biology.

DBL: Oh yes, certainly.

JS: How cell division takes place, for example, how the inverse sector enters into that – it might be useful in solving some problems in medicine, cancer being uncontrolled cell division.

DBL: Well, in essence it's a handle on something; whether it's a handle on anything useful is another question, but it's a handle. And that of course is the thing that is the most difficult in research

work – to get a handle.
JS: I'd be interested in pursuing that, except my qualifications aren't adequate at this point.

DBL: I suppose I'll have to publish that thing pretty quick, whenever I get around to a way of doing it.

JS: Does it need any more revision?

DBL: No, probably not much.

JS: You say in the preface to that book that it's something very urgent. So I think maybe there's a case for its being published.

DBL: It becomes urgent when I say so. But you see, I will have to publish it now, because I have prepared the way in this astronomy book. I don't spring it out all of a sudden, I lead up to things. Well, what I say here, "The effect of the new information derived from the theory of the universe of motion on our understanding of the relation of the human race to its physical environment has been explored in connection with an extension of the physical investigation into the non-physical fields, the results of which will be reported in a separate publication." That's in answer to item number seven. My item number eight is, "Are we alone, or is there intelligent life elsewhere in the universe?" Remember that question seven was, "Is the human race part of a machine, or does it in some sense have an independent role?" What I read to you is part of my answer. Now in answer to this question "Is there intelligent life elsewhere?" I say this: "The theory opens an avenue of approach to these issues. A preliminary study along these lines has been included in the extension of the physical investigation that was mentioned in the answer to question seven." Then I end the whole thing with the discussion of question ten: "Is there anything outside the process, that is, independent of the universe of motion?" And the last paragraph of the answer to that is the last paragraph of the book: "The findings of the extension of the investigation of the physical universe into the non-physical region are much too voluminous to be included with the physical results, but will be described in a separate publication. But it would not be appropriate to conclude the discussion in this volume without calling attention to the manner in which the clarification of the properties of the physical universe sets the stage for a confirmation of the reality of existence outside that universe. The more complete understanding of physical existence opens the door to an exploration of existence as a whole, including those non-physical areas that have hitherto had to be left to religion and related branches of thought. It is now evident that our familiar material world is not the whole of existence as modern science would have us believe. It is only a part, perhaps a very small part, of a greater whole." Now that ought to be enough of a teaser, no?

JS: How far were you able to go into these non-physical areas?

DBL: My point there is that we're opening up an approach that wasn't there before. As matters stand now, the scientific view is that everything is contained within the reference system, and that everything that exists exists in space and in time. Now if that's true, there is no merit in the claims of philosophy and religion. The people, philosophers particularly, who adhere to that point of view, tell us that such things as ethics are nonsense, they have no basis in reality, because there is nothing that they can be tied to. Well, that has been rather a sore point for a long time, because most scientists feel that they have an intuitive understanding that there are such things and they can't justify that intuitive understanding scientifically. Now, my point is that by eliminating this restriction, since I say that space and time are contents of the universe, instead of the setting of the universe – that means that there is no particular reason why there can't be other contents. So that revolutionizes the whole approach to the thing. That doesn't necessarily mean at that point that there are other contents, but it leaves the door open for producing evidence that there is such a thing, and it's not barred by the present scientific understanding.

JS: How far do you think science will be able to go in exploring that metaphysical region? Is there any limit?

DBL: I can't really say. I opened certain doors in the book that you have read, not particularly following any of them very far. For instance, the question as to the receipt of information. In science you are dealing with information that comes in by way of the senses – physical information, as I call it. And present scientific opinion does not recognize such things as I was just talking about, an intuitive understanding. Intuition has no standing in science. Now I have gone far enough into these open doors that I have been talking about, to establish the reality of these intuitive processes as being just as real as the processes by which we get information through the senses. The problem with both of the cases is the verification. We can't believe everything we see or hear, and we can't believe everything that we think we know intuitively. And, of course, as I have tried to show and, I believe partially succeeded, is that a good many of these other things that we are dealing with, such as religious revelation, and the ESP, and whatnot, are merely forms of the same thing that we're dealing with when we talk about intuition. And naturally, as I said in the book, our scientific insights are no different in essence; if we follow the scientific conclusions to their natural ends, we are only machines. The biological aspect is no different from the physical aspect. Physically, we're just a computer in an odd sort of frame. And that computer, that physical entity, can't get anything that isn't put into it. So if we are going to get something like a new insight into some physical problem – whether it's science or economics, or sociology, or anything else, or religion, we've got to get that insight in some other way than the physical. And I think that reasoning along that line, that is identically the same reasoning you use when arriving at a physical conclusion, you've arrived at the conclusion that there is a reality to this intuitive method of arriving at information. In one of the chapters I show the things parallel in a diagram: We have the information coming in one respect through the senses, and in the other through these intuitive channels, and we have to process them in much the same way. The problem, I think, with our non-physical information is that in most cases it comes in in such a way that it is not processed in the way that we do the physical information. Somebody tells you that he saw a flying saucer, you are kind of skeptical of that, and you ask for verification. If you go to church and the minister lays down a principle that he says has come from on high, you don't question that; you take that on authority. Well, I think what we need to do to get on a better plane of understanding is to realize that the information that comes in that way is no more authentic than the hearsay that we get elsewhere – it may be right and it may be wrong. We need to subject all of it to a reasoning process to verify it.

JS: But you mention somewhere in that book that we are very imperfect receptors of that information. So if the physical universe has no beginning, and if there are so many populated planets…

DBL: I didn't say it didn't have any beginning.

JS: Well, you said that it's possible it didn't have a beginning.

DBL: What I said was that time was created, if a creation took place, at the same time as the rest of the universe. So that there wasn't a beginning, because a beginning implies that there was a time before that when it didn't exist.

JS: Of course, there's always a temptation to project time and space geometrically into infinity, and it's hard to get rid of that misconception.

DBL: Well, it's misconceptions like these that have put us on the wrong track. Those are the basic things we have to correct in order to get the right idea.

JS: What I actually wanted to ask is, if there are civilizations that are far in advance of ours, they would be more capable of receiving this information.

DBL: I would think so. It seems to me that we can argue from the development that has already taken place, particularly in our physical ability to understand, and in our ability to understand non-physical items such as ethical considerations, for instance. I don't think there's any question that what the situation at the moment from an ethical standpoint is far in advance than what it was ten thousand years ago.

JS: Maybe ten thousand years ago, but I am not so sure that there has been much ethical advance in this century, or even over the last two thousand years.

DBL: I think there has; I think the mere fact that you question the ethical standing at the present time is an indication that there has been a big advance. I mentioned that point in the book that there have been periods during modern times when it has seemed that we reverted to savagery, but we have to remember that ten thousand years ago people were savages at all times. So that if we merely show little signs of getting better now, that's an improvement. We have to look at it over a long period of time; I don't think there's any question that we backslid in many respects recently, but that's another thing that I discussed in the book, too, that that is a necessary step that we have to pass through. What we are looking forward to is a time when – just looking at it from the ethical standpoint now, and not saying that that's the basis of all progress we're trying to make, but just looking at that – we have to recognize that what we're looking forward to is a time when individuals will act ethically. Right?

JS: Yes.

DBL: Alright, now then, I want to attach to that something else – "of their own accord."

JS: Oh, I see what you're getting at.

DBL: We started two thousand years ago with a situation where that relatively low percentage of the people that did act ethically did so under a certain amount of compulsion; they were offered the carrot and the stick. That has been one of the primary purposes of religion for thousands of years. And that has continued until relatively recently. Now at some point we have to release these people from their carrot and stick, and get them to do these things of their own accord. I think we're in the process of going through that now. That's why we're questioning religion to a large extent. Well, I think that during this interim period, when the people haven't come up to the level individually, we've got see a kind of backsliding from a moral and ethical standpoint. It's inevitable. That's part of the price of progress.

JS: Well, it's nice to hear that you think that the main direction is in the direction of progress, and not the other way around.

DBL: We've moved up to a point where in order to get any further we've got to cut loose and let the people go by themselves. Even if immediately there's a drop here, then we'll resume the upward trend.

JS: I'm glad you're such an optimist.

DBL: Well, that's not true optimism; that's a deduction from the facts that are before me. A true optimist I think would probably say that we could get along without this temporary drop. But I am not that optimistic. I think it will be quite a while before the standards of the people at large come up to the point at which the religious authorities had them when they lost control.

JS: We seem to be like the optimist who claims that this is the best of all possible worlds; and the pessimist, who agrees!

DBL: That's a point. That reminds me of Bertrand Russell's explanation of the difficulty in producing any scientific basis for ethics. It's been tried so often but you can never get from the fact that it's a desirable thing to the point that you should act that way. As Russell said, the best world for the individual would be the one in which everybody else was honest and he was a crook.

JS: You mentioned somewhere in your book that ethics is something that is peculiar to humans; but aren't there other things peculiar to humans, such as appreciation of music and literature and art?

DBL: I also mention that that may be one of the things in which we have to improve, that I've talked about ethics considerably because the facts in connection with that are somewhat available; I mentioned that there may be a necessity that we improve in esthetic ideas or appreciation in the same manner that we need to improve in ethics; but I have nothing to go on, or I just haven't looked into that. It may be just as important; I don't know, maybe more important. We still haven't come to a conclusion as to the overall objective.

JS: You conclude with the universe being centered on the human being, or the ethical being, whether it is human or in some other location in the universe; that the purpose of the universe is the creation of these ethical beings.

DBL: I'm merely taking the facts as I find them and arriving at the conclusion as to what is being accomplished. And then I'm making the assumption that if it has any purpose, the purpose is what is being accomplished.

JS: In the sense that human beings are the most complex of the organisms?

DBL: Well, I don't think that necessarily enters into the picture. As I see it, there is nothing physical accomplished. It's just going round and round.

JS: I see, it's a cycle; therefore any structure that's created in one sector is eventually destroyed before it can go over into the inverse sector.

DBL: I see nothing being accomplished there; the only one-way process I see is that bringing in unformed individuals, so to speak, and turning out some which presumably are at an advanced stage on the basis of whatever scale is set up for us. That's the only one-way process I see, and therefore I conclude that that must be the purpose if there is a purpose. I have concluded that intuition has a basis, and I intuitively feel that we have a purpose. And the great majority of people agree with me, and that again I have set up as a criterion that anything that the great majority of the human race feel intuitively is probably right.

JS: Of course, this book might be the one you'll be known for, though you probably want to be known first of all for your work in

physics.
DBL: It depends how you're using the term "first." If you mean "mainly," I'd say no. If you mean chronologically, I'd say yes. I'd like to get the physical books known first, but as far as the main event, I think even the economic works are more important than the physical.

JS: Really?

DBL: After all, what good would it do to understand the physical universe better? It does some good, yes, but not immediately, and not probably deeply. What good does it do to understand the economic world better? Well, the effect can be immediate – it could cause an immediate and very drastic change of importance to millions of people. So in that respect it is a more important work than the scientific part. Of course in Beyond Space and Time actually, my contention is that our economic affairs and our scientific affairs are incidental.

JS: How were you able to venture into these different fields, and zero in on the fundamental flaw in each one. Do you follow a general method in your approach?

DBL: What I have seen is that the fundamentals of the different fields of thought are rather closely connected. And it's been my observation that the great deal of trouble that the different branches of thought are having right now is that they're not taking advantage of the advances that have been made in other fields. For instance, in most branches of thought they recognize what they call the law of diminishing returns, which simply says that the ration of the input into a physical process to an output does not remain constant; it decreases, and eventually goes to zero. Well, that's very important in economics, and they recognize it. It's just as important in science, but they don't recognize it. That's the problem we're having now with Einstein saying that nothing can go faster than the speed of light. It wouldn't be bothering us if they were to recognize the law of diminishing returns, because what he is doing, is taking Newton's Second Law, the force is equal to mass times acceleration, and assuming that that carries on into infinity, which it doesn't. It's subject to the law of diminishing returns just the same as anything else. And if he had recognized that he wouldn't have arrived at the conclusion that nothing can move any faster. But then we have a situation on the opposite side, in the physical science they lay a great deal of stress on equilibrium, and the laws of equilibrium are regarded as very important in science, and they're just as important in economics, in fact, they're the key to a lot of economic problems, but they don't recognize it at all, and don't pay any attention to them. That's the kind of thing I am talking about in the matter of general principles – I am interested in the principles themselves, rather than in the narrow line. I'm not talking about equilibria in science; I'm talking about them as a general principle.

JS: Is that the reason you said earlier that you wouldn't classify yourself as a scientist? Is that too narrow a definition?

DBL: Well, if I had to classify myself; on the other hand if somebody says, well, you write this book, are you a scientist, well I say that's a matter of definition. The definition that I would use, the definition of a scientist, I can say that I'm a scientist, but on the basis of the same definition I can say that I am an economist, possibly a philosopher. So I say that if it were possible, I'd like to describe myself as a fundamentalist, but somebody else has preempted that.

JS: One of the main difficulties people tend to have in approaching the theory for the first time is the extra dimensions that cannot be represented in the reference system. How would one begin to visualize these extra dimensions?

DBL: Those things were difficult for me to grasp in the first place; then the question of expressing it to somebody else is even worse. Generally speaking, as far as I'm concerned, the understanding comes kind of gradually. And of course we have a number of such things; in fact the portion of the universe that can be represented in our reference system is smaller than the portion that can't be. We don't have to worry particularly about the cosmic sector, the other half, because we have no access to it anyway, and we have knowledge of it in this way, that whenever we learn something in our own material sector, we know that the same thing holds in reverse over there. But there is this big area in between the two, half of which contains phenomena that are accessible to us, and it is in this area that we have to realize what's going on, and get some kind of a mental picture of something that we can't put into the reference system. Then we've got these two scalar dimensions, or mathematical dimensions that we can't show in the reference system. So it's a little problem each time we come up against something, getting a correct view of it. In the second volume I had to get a grasp on matters such as the induction of charges, and that's one of the things that is very difficult to explain to somebody who wants to put everything in the reference system. We just can't visualize those things in the geometrical framework that we're used to.

JS: But you can represent these things mathematically; it seems to me that there must be some geometry corresponding to that, because there are all sorts of geometries other than the Euclidean. Of course you assume Euclidean geometry in one of the postulates, but if it cannot represent the extra dimensions, then that postulate is not followed through. Could you make certain modifications that would not affect the deductions, but would enable you to represent those extra dimensions geometrically?

DBL: The problem is that the ordinary individual is not any better able to visualize multi-dimensional geometry than he is to visualize the kind of thing that I am talking about, so we go from one thing to another that's just as bad. The problem is that we have three dimensions open to our perception. The person who's trying to understand that is trying to put it in his three dimensions of perception, and if it's something that won't fit into that, he gets the wrong idea.

JS: It just takes some getting used to, I suppose. I'd like to talk about the various advances that you made in the twenty-five years since the first book was published; in my view the most important has been the concept of distributed scalar motions and the

intermediate regions.
DBL: Well, I don't know – after you've cleared up a subject, then the difficulties kind of recede into the background. Of course, it's a continual process. Just on this latest book now *[The Universe of Motion],* I would say that there is no chapter in which something hasn't been cleared up during the writing of the book. It's been a few years now since I wrote Quasars and Pulsars, and of course that only affected a part of the area covered in the new book, so that much of what I'm dealing with now has never been considered by me specifically in astronomical terms. What I've done is to apply the previous physical results to these astronomical problems as I've gone along. And, of course, I've changed my outlook on some things that I perceive more clearly now – the question of the white dwarfs, for instance. I had a rough diagram in *The Structure [of the Physical Universe]* that indicated how those objects would move on the color-magnitude diagram and the same general principle still applies, but the path on the diagram is different now in some respects, because some of the points cleared up. A great many things that showed up in the course of writing this book simply fitted in with the findings that were made in the studies of other physical areas. Take for instance the question of the natural units. I found during the physical study that whenever we come to a critical value of some kind in the physical realm we are almost always, probably always, dealing with either a natural unit, or a small whole number of such units. So here in the astronomical situation I was able to take advantage of that and because I found out in the diagram I first drew for the globular clusters that the three critical points there were related in such a way, that the vertical distance is half of the horizontal distance. So it became rather obvious that this was one unit and that was two units. By recognizing that – even though I still don't know what that unit is, it's a compound unit of some kind – I was able to reverse that over into the white dwarf region and draw the diagram for the other stars. That's purely a result of this finding with respect to the natural units that we verified in the physical world.

JS: I've seen the theory's accomplishments; but the establishment *scientists are apparently looking for something else besides. I talked to a historian of science in Princeton about your work; and the first question he had – what predictions does it make, and have any been confirmed? That's how they evaluate a theory. Well, I mentioned those that you list in* Quasars and Pulsars.

DBL: Well, of course, there is a whole chapter in that book on predictions. Actually, as far as I am concerned, the emphasis on predictions is misapplied. I am not saying this to excuse not having predictions – the theory makes predictions all along the line. You could say that the entire physical universe is a prediction from the postulates. But actually what we are doing is not so much going out into new territory, as consolidating the existing material. While you find somebody like the man you're quoting that hinges the thing wholly on predictions, you will find equally competent scientists who will point out that what we need now is not so much to go out into new territory, but to understand what we already know, or think we know. And that really was the content of that quotation I made from Feynman, in which he said that the next big advance would be the qualitative explanation of these things for which we already have these equations. So I would be inclined very much to downplay the prediction idea. As far as the predictions are concerned, certainly we have done plenty of predicting as far as the quasars and pulsars are concerned; I didn't define the quasars specifically or the pulsars specifically, but I did predict the existence of the class of objects and gave the principal properties of that class of objects. Now, I have also done some predicting that is not recognized at the moment as predicting, but that's just what it is. For instance, I've developed quite a few points in connection with what I call magnetic ionization, and I've applied it to a good many problems. Now, the existence of the magnetic ionization is not recognized at the present time, so that is a prediction. One of these days there will be some method devised of actually measuring it. That, then, will appear as a prediction, whereas now it doesn't; it appears as something I have grasped out of somewhere, that people don't quite understand. This subject we were discussing, the meaning of the interregional ratio: that is a prediction that there exists a physical constant that the scientists so far have not recognized. The existence of what I call the time region, the region inside unit space – that's a prediction. The books are full of predictions, but at the moment they have not recognized those things, and they don't recognize them as predictions. The scientists of a hundred years from now may look at it in a totally different light.

JS: But one has to deal with the way the scientific establishment works today. If we can stress that aspect of the theory, it'll be a lot easier to get the thing accepted.

DBL: I want to mention two things. One of the predictions that we made is that there is a second half of the universe; that's about as big a prediction as you can make. The other point I wanted to mention is that in my opinion the acid test of a theory, aside from the fact that it has to agree with all the observations, is that it has to agree with the new findings, new observations, new measurements; that's where a very large number of theories fall down, and that's one thing on which I think we can show a very good record, because we are agreeing with all these new things coming along. I mentioned this morning about the globular clusters – the immature globular clusters. That is a significant case where something that is very definite now is agreeing with the conclusions that we published years ago, and the same thing is true with this finding about the hydrogen atmospheres in the White Dwarfs; that is an essential consequence of things that I published in 1959. Now they're coming up with something that they have no explanation for that follows directly from what I said in '59. And we can find quite a lot if we actually look for that and want to talk about that. And I think it's quite important; it's a lot more important than the predictions because you might hit a prediction by accident, but you can't agree with a long string of new discoveries unless you've got the right answers.

JS: I think you could formulate them as predictions; you would actually have to have made a statement about what you expected to be found.

DBL: On that White Dwarf business; I stated specifically in 1959 that the interior of the White Dwarf was the region of lowest density. Well then that means that the density gradient is inverse. And that inverse density gradient is the explanation of what they found with the helium atmospheres.

JS: Right. The emphasis on predictions is also based on the idea of Karl Popper that a theory should be falsifiable; that it should be possible to test it, and either qualify it or disqualify it.

DBL: All those things I have brought up as predictions here, they are all falsifiable in Popper's sense. Like many other things, they are not all testable directly; many of them have to be tested indirectly. But that's totally within the scope of what he is talking about.

JS: As long as they can be tested in some way.

DBL: As a matter of fact, it is obvious that all of these are testable, because I've developed them in order to use them, and I've used them – and the use is a test. This question of the interregional ratio, for instance – you can measure it. Anything you can measure is certainly a physical quantity of a real nature. That's one thing I'm going to have to emphasize with our people here, that so many of these things they don't understand, they wouldn't understand in current science either, because there would be no effort to understand them. They would just be measured and left that way. Once in a while somebody complains about that situation; I remember an issue of Science News probably about a year or two ago, where the editor was complaining about this matter of physical constants – that they just had to be taken as coming from nowhere, without explanation.

JS: One attempt at explaining those constants is the Mach hypothesis; Ernst Mach had this theory that the constants somehow depend on the size of the entire universe; but that's kind of a mystical idea.

DBL: He has no mathematical theory there; it's just Mach's principle – it's just an idea. It has no confirmation of any kind. And of course, in our system it has no meaning. He is trying to tell us that gravitation is due to the presence of all of these aggregates of matter out in the distance, but there is no attempt to explain how that can be done. And of course as we explain gravitation as the motion of the atoms themselves, we don't need any such thing. So it's just a useless speculation, as far as we're concerned.

JS: But this apparently absurd hypothesis was treated with great respect, and was in part incorporated by Einstein in his relativity theories. Why are the scientists so ready to suspend their disbelief when it suits them?

DBL: Well, they are basing so much of the present-day theory on the absence of disproof rather than on proof. It's a good deal like the fellow whose wife woke him up one night and says, "John, better get busy, there's burglar downstairs." He assured her that if she heard something down there it certainly wasn't a burglar – they work in absolute quiet. So he had a chance to go back to sleep, and thought that he had really pulled something good, but from then on every time she didn't hear anything, she woke him up. And I think it's very much the same way with the physicists now; every time they don't find anything, why then they invent something.

JS: What is the main obstacle to getting the new theory accepted by the scientists? I sent you some months ago Herbert Dingle's Science at the Crossroads, *which documents his efforts at convincing his colleagues that the Special Theory of Relativity contains a fatal flaw. Even though Dingle had been one of the early proponents of relativity, along with Eddington, and was greatly respected by his peers, that didn't get him very far after he changed his mind and began to oppose it.*

DBL: You see, the problem is that there are a substantial number of errors in present-day physical thought, but in each case an adjustment has been made to agree with the results that they get from the other, and that's one of the things that I wrote in the book there, that you cannot make a change in any one thing as Dingle was trying to do, because if you do you're out of step with all the others. The thing has to be a wholesale revision. The problem is how you get them to accept a wholesale revision. I did think that it might be possible with this new book on astronomy, since we are covering just about the full range of the general aspects of astronomy, disregarding the individual objects, and we have enough new material in their of useful nature, like that use of the globular clusters to measure distances – that should be of use to them regardless of whether anyone believes in the theory or not. We have enough of those things that ought to be of interest to them just on general principles.

JS: Could you clarify a couple of points for me? One of the difficulties I am having is that if the unit of electricity is the electron, there doesn't seem to be any unit of magnetism. Atoms, one could say, are the units of gravitation – they are the smallest units that gravitate, but in magnetic phenomena, there's nothing of the kind.

DBL: Well, the electron is not actually a unit of electricity. It happens to be a physical unit that can take an electric charge.

JS: Of course, atoms can also take electric charges.

DBL: But the electron, as a physical unit, without the electric charge, has no properties except insofar as it is a unit of space – it's essentially a rotating unit of space. And it acts as a unit of space rather than as a unit of something corresponding to charge. It really has no relation to charge. They're really two different electrical phenomena there.

JS: My attempt here has been to find out what are the areas open for future research. So magnetism is one of them.

DBL: Well, I would put it in a little different way. What I have done in this deductive phase of the operation is to start from the postulates and work downward toward more detailed applications, the principles and applications applying to smaller areas. And actually we haven't gone very far in any but a few selected areas. In astronomy, for instance, this book will carry us pretty well along. Most astronomical fields, as I explained in the book, are attempting to study the individual objects, a particular star, for instance. But I was interested in the general principles. The only area I have not gone into in any great detail is spectroscopic work, simply because it's too big a subject and would take too much time. Outside of that we've gone into astronomy fairly thoroughly here. But there are very few areas that we have done that. I would say, a thing like solid compression, there isn't a lot of work that's necessary there, and the inter-atomic distance relations, we did them pretty well, but by and large, almost every physical area is open to a lot more development of the details. If I compare, for instance, the amount of material that is in this new book, the astronomy book, with the astronomical pages I had in *The Structure [of the Physical Universe]* – I probably had about thirty pages there –well, I expanded that up to 450. And all of that is new information – it isn't that I have been any more wordy on the thing. I have probably been as condensed in the 450 pages as I was in the thirty. It's additional material.

JS: So the other areas could be extended in the same way?

DBL: People are continually saying: "Well, what does your theory give us in this area?" Of course, I haven't necessarily gotten to that area yet. My answer to them is, "I've given you the clue, the handle that you can work with, and it's up to you to go ahead and carry it on."