Jerome Lettvin, Neurophysiologist
Interviewed in Hingham, Massachusetts. May 2nd, 2007

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Jerome Lettvin was born in Chicago, Illinois in 1920. His wife Maggie Lettvin was born in Philadelphia in 1927.

LS:
I think young people are tired of listening to adults.

ML:
You know, they are, but it's inspiration — not how much a teacher knows — it's how much they inspire the kids to learn. At the lectures Jerry used to give at MIT he used to tell the kids that the whole point of being there was to learn how to get into more interesting trouble. The only thing you really learn when you go to school is how to find information.

JL:
I didn't have any ambitions at all. The only thing I was interested in was curiosities. I wasn't particularly interested in what you might call standard physics, standard neurology, or standard physiology. I looked for the corners that were unoccupied and which, for some reason or another, had escaped attention.

That's not a way in which you go about doing things, but I was fortunate enough to have had a father who was an anarchist who pointed out to me that the interesting things always lay out of the beaten path. You look for them, and when you find them you play with them.

I started out as a poet and became a physician, then became an electrical engineer, then a neurobiologist. It was never with any sense of searching for what people wanted to know. It was just to understand the thing that I was looking at in a way that made sense. That is a far more difficult job than writing equations.

The interesting things were the problems: were there other ways in which you could express the problem such that analogies and concordances would pop up? It's very much like listening to music and trying to decide what is meant doing it this way rather than that way. That is essentially the way that I've worked all my life. I haven't been after prizes, just curiosity, that's all.

Am I making any sense?

LS:
Can you be more specific? Can you speak to people who have not yet had these experiences?

JL:
Well, I never really had any experience. In a sense I lucked out by being present when a particular problem appeared, and then I spent a year playing with it until it got interesting. That was it. I never went to publish much. What I did publish I did on the lowest possible level that you could do an explanation.

LS:
Tell me about the problem?

ML:
Jerry, tell him about the lecture you gave at Harvard where you showed them so many holes in what they considered the theory of the moment that at the end of it they just sat there stunned.

JL:
Yeah, that is what I do like to do, to tear apart what is already accepted. It's sort of like you're confronted by a jigsaw puzzle in which the pieces fit but the picture doesn't work. What you have to do is take the pieces and rearrange them to get the correct picture.

That's a very different way of pursuing knowledge than what is current, very different. It's not the sort of thing that you teach easily. Namely, you get your fangs into a problem and you…

ML:
… you don't pay attention to what is accepted theory. You say, "Why is that hole there? Why doesn't this fit?" There has to be something wrong with accepted knowledge to explain why it doesn't fit.

JL:
That turns out to be a lot of fun. It doesn't get you anywhere, it doesn't make a great name for you, but it is gratifying to say, "Ah ha! What I smelled turned out to be the case."

ML:
That is because he keeps sifting, and sifting, and sifting.

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JL:

In other words I'm a garbage picker-upper as a mode of science: I focus on the garbage truck. I look at the parts that others choose not to pay attention to. It's interesting the number of things that are not paid attention to… absolutely astounding. Let me give you an example, just as a case in point.

There was a Hungarian by the name of Laszlo Von Meduna, who was one of the psychiatrists who introduced shock therapy. Back in the old days they had shock therapy for schizophrenics.

Now Laszlo was a remarkable guy. He had a background in chemistry and physiology that was… very Hungarian! I had never come across a background like that before.

What he decided, just from regular observation — just occasional observation — was that the best thing you could do for a schizophrenic was to use carbon dioxide. Doesn't that sound weird?

He wrote a book about it which did not get more than one printing and has disappeared from the shelves everywhere. He was a psychiatrist with a fair clientele, and the extraordinary thing about this clientele was that they would arrive in despair and come out perfectly happy, cheerful, able people capable of talking.

Walter Pitts and I sort of observed this and both of us went to Laszlo and said, "Laszlo, why don't you try it on us, just for the hell of it." He said, "I'd be glad to!"

The procedure involved inhaling a gas that was 70% oxygen and 30% CO_2. You pass out after the first two breaths — you take no more than eight breaths — and when you wake up it is an epiphany. Things stand out with such startling clarity that you cannot quite understand how it was that such a thing as this (he points to the window sash – Ed.) and its relation to that (he points to the fastener – Ed.)… how was it not observed that there was a relationship between them? For the next 12 hours Walter and I were walking in a world in which every single thing became completely clear. The clarity was the likes of which you don't experience ordinarily.

I decided that I would try this out on schizophrenics at the state hospital in Manteno, Illinois. At that time I was a psychiatrist. I had a girl who was 17 years old, crawling on the floor like a baby, drooling. She'd been that way for the last 4 or 5 years. She barely talks at all, just mews like a sick cat, and crawls along on the floor because she can't walk. I decided to give her the treatment.

Eight breaths and then she wakes up a perfectly normal 17-year old girl. She talks very clearly and she could remember the condition she had been in. Over the period of the next 10 hours she slowly, slowly declined. It lasted about 10 hours and it raised the question: what the hell was going on? The State of Illinois forbade me ever to use that treatment again. So I was never able to test it again.

You see, nothing like that ever sits in the literature. The most you have is Laszlo's book, and nobody reads that anymore. In fact it's very difficult to find a copy. The point is that something of great importance is being completely neglected because everything in this field bears away from it.

It's at this point that curiosity overwhelms you. On the one hand nobody can make a buck on CO_2. You see [laughs] medication goes where the money can be paid. Here's a treatment that costs practically nothing. Laszlo's using it on a number of extremely depressed executives and it's working like a dream. But there's no way of publishing or talking about it, and you have the state forbidding you from using it. This is a very different way of looking at science, or in this case psychiatry.

ML:

It wasn't just science. Jerry taught History of Science and the kids just loved it. They had to open up a larger lecture hall because so many kids wanted to attend. He'd go in there and talk for 3 or 4 hours and the kids would bring their girl friends, lunches, and just sit there forever. They'd miss classes, they'd miss anything, they didn't care, they loved listening to him. Because he loved what he was doing!

JL:

Relax, relax!

ML:

Most of your job — even as much as you knew — most of your job was inspiration.

JL:

Well, yeah.

The idea was to bring up strange problems. The business of strange problems, the problem that I gave you is not one that you'd think is a major problem. Yet when you think about it…

Ramón y Cajal — the anatomist who got the Nobel prize for his work on neuroanatomy — has a book on what you teach your students, and it is an extraordinary book. I strongly recommend you take a look at it. His advice to a young scientist is one of the nicest, most brilliant books you've ever come across. ("Advice for a Young Investigator", MIT Press, 2004. First published in 1897)

ML:

Jerry, tell him about your autopsies at Manteno. You remember, there would always be people dying up there. But since there were only 7 doctors for 7,000 patients they could barely pay attention. Jerry wanted the doctors to learn what was going on with these patients so when somebody died he'd get all of their records together and he'd *let the doctors know that the next morning he would be doing an autopsy. And the doctors and the staff would read the records make bets on what the person died from.*

JL:
In other words I made it very much like a horse race, in which case everybody was interested and everybody learned. You learned, but you didn't know that you learned. OK? This is very far from the dedicated approach.

ML:
No it isn't! Everything comes into play.

JL:
Well…

When you go through the literature on nerve structure what hits you is what's missing. That sounds a little bit silly but what you realize is that there's a structure in the way people look at things that insists on itself being the only perspective.

It's very strange how much shit there is. Between the direct view of what's going on and what you might call the academic view. This applies all across the board, it's not only in biology and science and psychiatry, it holds practically everywhere. Conservatism is the key word.

What you have to do is find those little crevices, those corners, which are neglected. There is where the gold is. That's no way in which you can go about teaching students.

ML:
Of course it is!

JL:
Well… yes and no. On the one hand, the student has to be able to get out into the world and get a job.

ML:
Not necessarily.

JL:
Look, some of my students have had a lot of trouble.

ML:
Jerry, so have a lot of other people at MIT. The top people at MIT end up driving cabs. The best people end up going in to business, and in between it's variations on a theme.

JL:
This is a very different approach to problems than you get in what's called "proper science." For example, over the last two years I found an engineering problem for which there's no scientific basis whatsoever. Because of this lack of scientific basis I said to myself, "Wait a minute, I'm going to have to smell out what is converging to give me this problem." That became a real joy.

I'm vaguely writing it up now. But it's not the sort of thing that's going to appeal much to my colleagues, nor is it going to attract a large body of acceptance, or followers, or anything else of the sort. It's a curiosity that, over time, might turn out to be interesting, or maybe not.

LS:
You could describe this example a little more.

ML:
I think you should tell him about the Frog's Eye paper and what happened with that.

JL:
[laughs] You know what a nerve fiber is, no? As you know, nerve impulses travel up axons that go from here to there. The serious question is: what is the mechanism, what is the energetic cost of the mechanism, and how sturdy is it?

There is a huge literature, an absolutely enormous literature, on the axon. Enormous. A lot of equations are written, very clear and definite, along with the evidence for the equations. But something smells strange about all of this.

If I were to ask about a computer, for example, "What is the energetic cost of a wire connecting two elements?" That is a question that usually doesn't enter into the problem because there is almost no energetic cost. You have charge flowing from here to there through a wire, and there's no unnecessary heating. You can have various junctions that can be far apart and still operate properly.

When you look at a nerve the first question you ask is, "What is the energetic cost of the signal's travel?" In the case of axons you have nerve fibers traveling in big bundles, traveling from one part of the nervous system to another. What's the cost of this transport, and how good is the transport, how reliable is it, and what makes it reliable? These are not questions that are ordinarily asked.

People say, "Stop giving me the advertising, let's see the equations!" But the problem here is this: nature generally works in strange ways, it sort of goes to the limit of the maximum amount of operation for the least amount of work. This is almost invariably the case.

I know it sounds very bald and brutal but the interesting point about the nervous system is that here you have connections that look like wires in a computer —

extremely much like wires in a computer — yet they're operating in a least five different ways in the transport of the information. And the five different ways are very different from each other. Yet they all reflect exactly the same basic mechanism, which was first discussed in the 17th century and then fell into disrepute and hasn't really appeared again, but the smell is there.

When you talk about axonal physiology, for example, the proper questions to ask are framed in this way: here is an axon, it's got a membrane, it's got an interior — a tri-laminar interior — one of these lamina is called the cytoskeleton and is composed entirely of actin fibers with fixed negative charges on them. The question is how do I get an impulse to go up this fiber without any waste of energy — without any expenditure. Is there a method for it?

Curiously enough, back in 1927 there were a pair of people, Erlanger and Gasser (Joseph Erlanger and Herbert Spencer Gasser – Ed.), who published an experiment that got them the Nobel Prize and was promptly forgotten. You see no references to that work after 1952 or 1953. No references to it at all. Although in that paper they made specifications that should have raised the hair on the head of every physiologist who read it. But the alternative was to say, "That's all shit so let's put it aside." OK?

Now when you go to your colleagues and say, "Look, I would like very much for you to consider…" Your colleagues look at you and say, "Wait a minute, you're recording nerve impulses from the cut ends of axons. That's not the same as an intact fiber. You don't know what is the relation between the cut end to that of the whole fiber. This will not do!" Another one will say, "Look, you have myelin coated fiber. Myelin does not allow anions or cations to go through. Where do you get the impulse for this thing to move…" and so forth.

If you go back to Erlanger and Gasser, they already smelt what was the case, but smell is not enough, in spite of the fact that they got the Nobel Prize the paper disappeared as fast as it appeared.

Here you have a case where, in a certain sense prior to all of the physics and chemistry and physiology — and neurophysiology is now a very big field — nowhere in these fields is there any hint of the explanation for what Erlanger and Gasser observed.

You go back and look at Erlanger and Gasser and you say, "What the shit, this is so gorgeous! Why is everybody ignoring it!" People ignore it because, they say, "Well, you have damaged the fiber!" But cutting the fiber is like cutting a wire: the wire still works up to the cut, so what's the fuss? They say, "No, no, no, we have to be scientific!"

This is the paper that I'm producing now and, like the original Frog's Eye paper, it will excite a few people, but it will disappear. I'm in the game for the fun of it. If someone asks me, “What is science?” or “What is physiology such that you can do these things?” I wouldn’t have the vaguest idea of how to talk about it.

ML:
I think you do. I think it’s sifting. I think that’s true for everything. It’s always sifting, looking for the curiosities.

LS:
Tell me about the Frog’s Eye paper.

JL:
You read it?

LS:
It was boring.

JL:
If it was boring, then you didn’t read it.

ML:
No, no. Not that he didn’t read it, that he didn’t understand it. Most people tell you they didn’t understand it because it was too dense.

LS:
It’s not a criticism. It isn’t my field.

JL:
You see, here’s an interesting thing. Suppose somebody asks me, “How would you record from a wire in a computer that’s connecting two operators?” You know very well that you’re not allowed to drain the current from the wire because that will prevent anything from going through. Is there any way in which you can look at that wire to tell what’s happening, to tell that this impulse is going from here to there? That’s a little difficult because a wire is awful god damned fast. Well, yeah, you can record an impulse, but the shape of it doesn’t have much meaning. It will have a shape, but so what? What does that have to do with the wire?

Nature is a very interesting mother. The question is this, “How do I make a wire that doesn’t act like a wire, but can substitute for a wire in the translation of information?” Is there some process in nature that you can take advantage of so that you’re not using any energy? Oh boy, yeah, there is! And that’s what I’ve been spending the last half year on.

Not that it will make a big “who-hah” when it comes out, but it will explain something that was not explained before except by a batch of equations so tiresome that by the time you finish you say, “Look, let it be! That’s the way it should be. Fuck it! Now we understand!” But no: you don’t understand.

In the Frog’s Eye paper what we showed was that…

ML:
Jerry, let me interrupt for a minute. Did he tell you that he was laughed off the stage when he first presented it? He and his colleagues were practically suicidal, and it got to be one of the most cited papers ever published.

JL:
Suppose I asked you, as a physiologist, how you would go about knowing what it is that is being seen? Not the seeing, but what it is that is being seen.

At this point you say, “Well, I can build computers that will do this.” But in nature you don’t have computers, you have axons carrying impulses without transporting any energy. You say, “What? You’re telling me that you can transport impulses without energy?”

Well, of course you can. A sea wave travels keeping its velocity, and keeping its height. It’s not dissipating energy as it travels. So let’s take a look at wave theory and ask ourselves, “Is there a way in which I can design an organic tube in which I get waves, and in which I can govern sequences of waves?” If I can do this, then the cost of nerve signals is negligible. The processors — the nerve cells, the dendritic axonal formations — those are the computer parts, but I’m just talking about the wires!

So you ask yourself, “How do I build a wire such that there is no dissipation?” That’s been solved in modern computers. You go and take a look at structure of the wires, but then you realize, “Wait a minute, this is sexy, there’s no dissipation by the wires, instead the dissipation is in the processors, not in the wires. Can I get the same thing organically?” And the answer is yeah, but it takes an awful lot of looking.

When we recorded from the optic nerve nobody had ever before recorded the fine fibers, only the large axons. Large axons tell you the light switched on, the light switched off — a big signal is going across the axon — good stuff.

But the frog says, “I want to pick out that fly, that’s the one I’m going to eat. How do I get that fly represented in such a way that I know exactly where to put my tongue?” This is not a trivial question. I know it sounds a little wild and weird, but it ain’t a trivial question.

Most of the axons that people look at in the whole literature of axonology — and that’s a very large literature indeed — these axons are invariably of the large type. But we were looking at axons two orders of magnitude smaller — two orders of magnitude smaller! In other words 1:100 smaller — these carry a lot of information. But there's no access to them in any physiological way using electrodes. On the other hand, if you cut the end of an axon you can record what's coming through from that cut end. That's all I did: to show that the cut ends of axons gave you what information was being carried by the fiber.

This irritated the shit out of everyone because it was a careful repeat of the paper by Erlanger and Gasser. It was a repeat that could be done on a live animal doing no more than cutting the axon and recording from the cut end. And it turned out to be not too difficult.

Nobody in the physiology world will record from the end of an axon on the superstition that if you cut the end you have compromised the operation coming to that end. But what you can show is that, just like a wire, the operation will work up to the cut end. That's what people refused to believe, in spite of the fact that I demonstrated that over 8 hours of repeated stimulation in correct places that I got the same results. People considered this to be an artifact that I had arranged!

Am I making things mildly clear?

LS:
Yeah, but you haven't finished the story of what you did with the result, and how you feel about it today.

JL:
Oh! I'm actually putting out a paper in which I'm reviving the whole story.

It's curious how many problems have a kind of improper approach that holds the proper approach away. That occurs everywhere in biology and in a lot of other places as well. In this particular instance what I'm trying to show is that there are particular cells in the retina, ones which respond to a particular kind of object moving in the visual field in a particular way. There's an axon that comes out of the retina, and if I record from the end of that axon I can tell you exactly what the stimuli are.

That's all it is. Nothing great. Nothing profound.

LS:
Then why does it baffle people?

JL:
Because the dictum is that if you're going to work on a nerve fiber, and it's a living cell of some sort, then breaking it changes its function. This is a given, you

see. But nobody looks to see that you're not dealing with water, you're dealing with gels. Inside the axon you'll have gels, and that means that you're not going to have an effect that will matter that much if you cut it.

If someone says, "Prove it!" It means doing an awful lot of chemistry that is not in the books as of yet. One of the things that you come up against here is that, except for Tasaki (Dr. Ichiji Tasaki at the NIH in Bethesda, MD.), who is a brilliant Japanese physiologist, most people don't really realize that a nerve works by virtue of a phase change in water — a change that is quite violent.

Everybody thinks water is a fluid, it's not. There are 3 to 5 stages of water in which water acts as a switch with microsecond timing. "Microsecond timing in water?!" Yeah, microsecond timing in water.

Examples of this are gotten by looking at Tosaki's work in what he calls anionic gels. What is strange is the fact that you're not looking at the actions of ions in the water, what you're seeing is a rapidly propagating transverse change in the nature of water itself. Water goes from an expanded gel, to a contracted gel, to free water, within a few microseconds. Now that's a very different story! You have to stop looking at textbooks that tell you: "These are the way the ions move…" That's bullshit! It's not that at all!

You see, I'm giving you a few footnotes to what is a seriously expanded problem. I've spent the last 6 months trying to get a 20-page paper out of it. I'm going crazy because it's hard, hard work to lay the whole story out clearly. But once it's laid out clearly it's a different story from anything anyone's ever talked about… except Tasaki.

LS:
What's the paper called?

JL:
I haven't called it anything because I haven't finished it.

ML:
Yeah, you did: "<u>How</u> the Frog's Eye Tells the Frog's Brain."

JL:
Oh, yeah. [laughs] I'm playing with that as a title.

The problem is that there is no current background here. Tasaki is in his late 90's, still going to work every day. He's published at least 6 papers on this particular topic. Each one exceedingly difficult to read, and quite baffling. Some of the analogies are extremely difficult. To imagine that water operates as a switch, not as a medium, sounds like a brand new thing… but it's not brand new, it's a century old. It's just that the literature has been forgotten.

You see, you're asking me how I go about things, I go about things in a way that has nothing to do with what universities teach. It's very different from what universities tell you to do, what teachers tell you to do. You make it up as you go along, and god knows how it comes out; you don't know.

LS:

What's a young person to do?

JL:

Play around. You play around.

ML:

Follow their nose. In our family what we say is, "We make ourselves up." Wherever you want to go, you go, and you'll find a way. Keep looking and you'll find a way. You can't go by what other people tell you. I went totally outside the field of physical therapy to come up with the stuff I did. I figured if Jerry could do it I could. And now the kids are doing the same thing.

JL:

It's actually a lot more fun than fitting in with current beliefs, current teachings. Much more fun.

LS:

How does a person develop a sense of direction?

JL:

They don't develop a sense of direction. The direction will occur to you once you get embedded enough.

ML:

You just start and it happens. You get enough interest and you keep following your nose and it happens.

JL:

Just the way any exploration occurs.

LS:

Most people don't know how to explore. It's not obvious, you know.

JL:
That's right.

ML:
It is true. In fact schools almost teach you not to explore. The minute you have another way to solve a problem they say, "No, no, you have to do it this way." And that shouldn't happen. Kids should be allowed to explore all different ways of solving problems.

JL:
Biology is interesting because when you go back and start looking at structure, the structure of cells and things of this sort, there's an amazing amount of work on it, and awfully good work, but smelling how things fit together is a different story. Now you're talking about strategy, and strategy is a very different kind of thing: the strategy of operation.

I first came up against this with that psychotic girl. There she is drooling, crawling along the floor. Eight breaths of CO_2 later and there she is saying, "Oh! How are you?" Wait a minute!

What ought to be taught is a certain amount of anarchy. That has become a difficult thing now that everything is arranged by managers.

Now look, I'm not saying there are not good physicists who work in astronomy, they're wonderful, and their apparatus's are extraordinary! But, oddly enough, a lot of that extraordinary apparatus was already invented by animals. There are animals that are star-guided. It's surprising how intelligent most animals are.

I had a pet octopus when I was in Naples. He was a big one, a huge one. "Juvenile Delinquent" is what I called him. My son Jonathan would play with him. We'd come into the lab in the morning and the octopus would be waiting for us. Jonathan would climb into the tank, and instantly there would be a tug of war, back and forth.

People talk about animals not having a sense of humor, this octopus knew how to play practical jokes, and it was wonderful to watch. My octopus would play practical jokes: "What do you mean an octopus playing practical jokes?!" you say. But it's true.

I teased JD — he was a big octopus, he had a 5 foot spread of arms — I teased him by holding a fish down for him to grab, then pulling it back [laughs]. JD would start going black-white-black-white indicating a high degree of irritation. Then JD decided to play a joke on me. Let me tell you, anybody who says octopus's don't have a sense of humor… forget it; they're good.

The next morning I walk into the lab and JD is up on the edge of his 20 foot tank. I walk in and smack! … right in my face… he let out a huge squirt of water! He had been waiting for me to come in. Jonathan is in stitches, and then Jonathan says, "Take a look!" and I turn around and there are splotches of water all around the region of where my head would be when I come in. The octopus had planned his revenge and he had been practicing ahead of time. That sounds like a ridiculous story, eh? Well, it isn't.

It's a very different science that I teach.

LS:
What would you say about failure?

JL:
Such as? If something doesn't work, it doesn't work.

ML:
You know what he taught me? He taught me that that's how you find the right route, by doing one failure after another until you find there's only one route left. He used to let his students make one mistake after another, even through he knew that what they were doing was going to lead to failure. He said, 'That's the way they learn." Long and tedious.

JL:
We'll it's isn't tedious. It really is an adventure most of the time.

LS:
What about the dangers of failure?

JL:
So? What about it? Who gives a damn?

ML:
The dangers of failure, Jerry, that's where it gets to be a problem once you have a job, unless you have grant money. That's why everybody knew exactly what results they were going to get before they wrote a grant proposal. They said that they had to, because if they weren't sure of the outcome, then they wouldn't get money next time. They couldn't risk exploring something if they didn't know how it was going to work out beforehand.

JL:
Up to about 1950's there was a kind of freedom in the scientific world — certainly at MIT — that allowed you to play games of all sorts. As two or three decades went by MIT was taken over by managers.

ML:
Ay-yi-yi-yi-yi. There have always been managers. Since the beginning of the world there have been managers. Most of them are called "mother."

JL:
Hmmm.

My father was an anarchist in Russia, a strong follower of Krapotkin (Peter Kropotkin, Russian social philosopher, 1842-1921). It was he who taught me anarchy.

LS:
Are you an ancharist educator?

JL:
Damn right.

ML:
I don't think he is. I think he just teaches because he loves learning himself so much that it just, sort of, boils over and the kids get the benefit. He can't contain himself even at home; he's constantly trying to teach people at the other table. Every good teacher I've ever had was wildly enthusiastic about their subject. They were delighted if you just learned anything.

JL:
You see, there are alternative methods of teaching and research that do not appear in the textbooks. That's all that I'm trying to tell you.

LS:
Let's say you had to give advice to a kid going into college…

JL:
Look for what interests you. Do not pay attention to what people tell you.

Managers have wrecked education immensely. The differences between education and research now and what was done 50 years ago is fantastic. There's a profound difference in the freedom to research and question that you could do then, and what you're not allowed to do now. I wouldn't be able to get a job now under any circumstances.

LS:
An obstacle to any person's development is the illusion of being dependent. Kids encounter this and they don't know what it is. Society used to help kids through this, but our society has taken away the initiations to becoming independent.

One of the objects of my work is demonstrate to young people that they have to navigate this transition themselves. Kids have to find ways to initiate themselves. It's a process, not a goal. It's a self-transformation, not an achievement. It's impossible to describe, and no one but you will know how to do it.

ML:
All the fun is in the process.

Jerry's brother was a concert pianist. When the kids were young we used to go over to his house and they would lie under the piano while he gave master classes. He would go over, and over the same phrase looking for the right way to express it — looking for the way he wanted it to sound. That's exactly what this whole thing is about: it's about the process.

Those master classes were so much more fun than a concert because you were with him while he was finding his way though. It was fascinating. I loved that. The process is always more fun.

JL:
The process is more fun than the ending. The ending is bad but it's the finding that's wonderful.