# WHAT IS SYSTEM THEORY?

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Mr. Inamori, Ladies and Gentlemen:

In this lecture I will not pull on your heartstrings as Dr. Bergström did earlier. I cannot compete with Dr. Shannon who has had some very interesting things to show you. The only concrete object I will show you is this (small blick-like object) thing.

I intend to make this an intellectual lecture. I will try to explain to you what System Theory is about. Why it is needed. Why it is different from other scientific fields (such as physics) and why it is important for the future. And I would like to tell you, in a purely personal way, something about how my own intellectual development proceeded through many years. 〔This will be the content of bracketed expressions below, some of which were added after the lecture.〕

I will mention a lot of famous names. Not to practice name dropping—we all detest that—but to alert you to ideas that you are probably already familiar with so that I can proceed more rapidly to explain my own view of science which, in many ways, is quite different from what American politicians call conventional wisdom.

The first big name, very big name, I want to mention in Bertrand Russell. He wrote:

> "Examine all accepted assumptions."

I agree with Russell. One of the imperatives in scientific research is to worry about things that we are supposed to know. Are these really true? Do we really know them? Science means not only wanting to know, but to know objectively. In any new research the first thing to try to be sure of is that what we inherited from our forefathers is real knowledge and not merely the illusion of knowledge. This is difficult even in mathematics and much more so in other sciences. What do we really know, for example, after we have been told of Newton's inverse square law of gravitation?

A long time before I had heard of Russell I developed an obnoxious trait in school. I was not always thinking about what I was supposed to be thinking about.

When I was taking my maturity examination I wrote an essay that did not confine itself to treating the subject I was examined on. Afterwards, I suppose, there were some discussions as to whether I should be allowed to pass. And it was decided that, although I was surely obnoxious, I was probably not completely stupid. So they let me pass. On some future occasions it was not always quite so easy.

Now I would like to start to explain to you what System Theory is. This is very difficult, even in discussions within the narrowest limits of my own fields of work. Therefore I need help. I shall appeal to Einstein. He was quoted as having said:

> "A scientific explanation must be as simple as possible but not simpler."

I will proceed keeping this requirement in mind.

The first question is this. What *exactly* is a system? This is something I can't even begin to explain, though generally I will call a system something complex, interactive, nonsimplifiable, and that sort of thing. It is more informative to explain certain scientific notions which are closely related to the undefined (but still very concrete) idea of a system.

Perhaps the most basic phenomenon which requires the study of systems is *stability*. Or *instability*. Stability means that a system will continue to exist and function in a normal way in spite of adverse outside effects. (Life is an example of this.) Instability means the system has a tendency to self -destruct. These are phenomena that physicists do not understand—well, let us say more kindly, do not understand within the framework of their usual intuition. The historical development of physics is based on the study of simple, isolated phenomena. These are usually stable since otherwise they could not be studied anyway. Bombs are a counterexample.

Stability and instability are system phenomena. They are the result of interaction between different parts of a system. Stability is an interesting problem mainly in man-made and biological systems. Most physicists are not concerned with such things. They are concerned with atoms, which are also complex systems, but since atoms are usually very stable, physicists tend to worry about other properties of atoms than stability.

Look at my little props. Let us suppose this is a brick. It is very stable in the physicist's sense; it is quite difficult to destroy. But let us suppose that the brick is made to rotate about some axis. Then we have a stability problem: will the brick continue to rotate in a regular, smooth way or will its rotation become more and more irregular and eventually change or stop? This is an embryonic system problem. It is not treatable just by physics. Usually bricks sit still and do not rotate. We need mathematics. There is a famous theorem of Euler which predicts what will happen. If I rotate the body along the axis for which its moment of inertia is maximal (the speaker illustrates this by showing such a rotation to the audience), then the rotation will be stable. If the brick rotates around another axis (the speaker again illustrates this with the same object) then the rotation will be unstable. This is sure knowledge because it is mathematics. Therefore it passes Russell's test.

Given these established and undisputed facts, a physicist would then generalize, in the manner of physics. He would argue as follows. "No matter what object you may take, no matter how complex, rotating it about the axis of its maximal moment of inertia will be stable and rotating it around other axes will be unstable." This has been sometimes called the "MAX-AX Principle."

A friend and colleague of mine, Professor Tom Kane of Stanford University was given the task, about 15 years ago, of researching this principle and, hopefully, proving it. In going from a simple brick, which could hardly be called a system, to a mechanical system of any complexity (for example, satellites with internally moving parts), the physicist simply extrapolates from what he is sure of (not realizing that this is often mathematics and not physics) to other physical situations which to him seem analogous. After all, every satellite has a mass, there is always some axis of rotation corresponding to its maximal moment of inertia, etc. The physicist does not much care what a system is but he firmly believes that the methodology of physics is universal.

My friend Kane (not a system theorist and not a card-carrying physicist either) was rather surprised when, in the course of a summer, he did not succeed in proving the MAX-AX principle by means of mathematics. But he was a good researcher. He quickly established, partly by physical methods, that the MAX-AX principle was false![1)] Indeed, the MAX-AX principle seems to be maximally false; except as it applies to bricks, of course. Kane needed only paper models connected by rubber bands to give a physical demonstration of this fact.

Why is the MAX-AX principle false? Because it rests on primitive physical intuition which has no relation to system phenomena. System results do not work by simple extrapolation. One must understand the role of complexity; we know very little about that but the question is researchable and we are making steady progress. In the case of the unlucky MAX-AX principle, there is a standard method for investigating stability. In applying this method. Kane concluded that, as the number of parts making up the system becomes larger and larger, the “probability” of stability becomes smaller and smaller, for purely mathematical reasons. The MAX-AX principle is not a daring scientific idea; it is simply an example of confused thinking about problems to which the conventional methodology of physics is not applicable.

In Newton’s days, around 1700, it would have been very daring science fiction indeed to talk about satellites, not to speak of satellites with internal moving parts. Yet I don’t think that Newton would have erred as crudely as the unknown inventor of the MAX-AX principle. He understood the difference between basic physics and systems. His investigations about the forces affecting planetary motion, which lead him to his demonstration of his law of gravitation, were the first big success in mathematical system theory. The Sun and a planet constitute a two-body problem, they form a system, and Newton always analyzed them as such.

〔The discovery of moderately high temperature superconductivity in ceramics, which is contradictory to elementary physical intuition and the preexisting theory of superconductivity, is surely an example of a system problem which physicists (but not Newton) have always had difficulty with. This is the work for which Müller and Bednorz shared the Nobel Prize in Physics in 1987.〕

My second example is about control, another important system problem. A newspaper article was published in the American media around 1962, in the early stages of the US effort at space exploration. The article was by a well-known physicist. I have forgotten his name and, unfortunately, also forgot to keep a copy of the article. The rest of my recollection, I hope, is precise.

This nameless physicist argued that it was impossible to go to the Moon. How so? (After all, the Moon seems pretty large in the sky and should not be a difficult target for a space ship.) The argument was technical. The motion of a rocket from the Earth to the Moon is governed by a differential equation. The main forces entering this equation are gravitation and the thrust exerted by the rocket engine used to lift the space craft into orbit. The rocket is a very big piece of equipment. It is difficult to control the thrust of the rocket engine. But if the thrust is not timed very accurately (the article mentioned an accuracy of the order of milliseconds which was far beyond the engineering possibilities for rocket engines available in 1962) then the free-motion trajectory, followed by the spacecraft after the thrust of the rocket is shut down, may be so inaccurate that the space craft may miss the Moon. Landing at a precise geographic spot on the Moon would be therefore clearly too much to hope for.

If you permit me to be also rather technical for a moment, let me indicate the kind of analysis which lead to this conclusion. Using numerical techniques, the differential equations (known since the time of Newton) could be analyzed for perturbations due to the inaccurate functioning of the rocket engine and the inaccurate knowledge of the many physical factors (for example, the distance from the earth to the moon) which enter into these equations. It is then entirely plausible to conclude (but I have not checked the calculation which would have required considerable effort) that even very small uncertainties in the whole system, for example, in the functioning of the rocket, the time when it is shut down, etc. can result in such large effects on the trajectory that the spacecraft might ultimately miss the Moon. Up to this point, the conclusions appear to be correct.

Yet this is also a typical example of competent use of standard scientific and mathematical technical reasoning which results in a grossly incompetent conclusion. Why should I make such a nasty attack on a supposedly objective article? (There were undoubtedly also professional, business, political, and power motives in the background but these are irrelevant for us now.)

The trouble is the underlying assumptions. Let us examine these, implementing Russell's advice. The physicist assumed that:

(i) the system problem is governed by the elementary laws of physics (correct) and these laws are the only important considerations (incorrect);

(ii) the man-made system carrying something of someone to the Moon does not differ, in its important features, from a purely physical "system" like a meteor (wrong).

The physicist had obviously no conception of the broader aspects of the problem. A spacecraft moving from the Earth to the Moon is evidently a man-made system; meteors do not do such things. As a man-made system, the spacecraft is likely to be unstable—in fact, this was the main conclusion of the article, though only unconsciously. An unstable system is of no practical use. unworkable. So a method of control must be found.

There is an enormous difference between man-made systems and purely physical ones because the former depend on the state of technology. The correct analysis of the problem requires asking questions like the following. “Is current (1962) technology sufficiently developed to provide means of controlling the spacecraft? Is the accuracy achievable by control sufficient to allow pinpoint landings on the Moon?” As it turned out, the physicist lost the media debate; the Moon project was not stopped and eventually it was successful.

The physicist’s contribution to the debate was grossly incompetent because for him no considerations existed outside his own narrowly defined field of physics. He has no conception of the fact that many other things were involved, especially human interference with the physical world, which today is called technology. Indeed, in my opinion, the development of technology since Newton is an even greater human achievement than the development of physics, although it is important to remember that modern technology is dependent on prior knowledge in physics. And one of these great achievements concerns the problem and technology of control, which is also one of the most important system problems.

By 1962 the technology of control applicable to spacecraft was adequately developed. Small correction rockets were used to counteract the so-called open-loop inaccuracies of the main rocket engine. These correction rockets are used in the closed-loop mode, that is, actuated on the basis of accurately measured and rapidly processed information about the spacecraft’s trajectory. This requires a formidable array of modern technology, especially electronics and computers.

And, I should add, the U.S. space program culminated in extremely accurate (within roughly 50 meters of the preassigned target spot) and wholly successful landings on the Moon. This fact was the clearest proof of the gross incompetence of the article discussed above. Such technological achievements as manned flight, the transistor, computers, integrated circuits, the laser and many others might well be far more important to humanity as a whole than advances in the basic sciences, and they are largely system problems. The individuals who have contributed to them are not easily identifiable. But that should not diminish our thanks for the contributions involved.

〔Presumably at Nobel's time and perhaps in his will the word "physics" had a meaning quite close to what we now call advanced technology. The Nobel prizes, however, tended to reflect the academic meaning of the word used as self-definition by physics departments at universities. In his Nobel celebration lecture in 1981 Professor Bergström proudly recalled that inventions, that is, advanced technology, had been occasionally honored by a Nobel Prize ("invention" is a word that is explicitly used in Nobel's will); but in fact the year 1912, when the physics prize was awarded to Dalén, marked the last time that this actually happened. Dalén's invention of a regulator for harbor light houses was indeed a clear contribution to system technology, and specifically to control, but turned out to be too small to deserve mention today. Yet the basic ideas of control were undergoing very rapid and permanent development already around 1912. Without these developments, going to the Moon would have been impossible. Perhaps our nameless physicist did not pay attention to control because he knew that Dalén was the last to get a Nobel prize in physics for it.〕

My third example, concerning the relationship between stability and control, was in all the newspapers very recently, under the heading "Japan Airlines Flight 123." The accident that occurred during that flight had nothing to do with control. But one incidental consequence of the accident was that it destroyed the communication links to the control actuators. So the plane could not be controlled. It crashed because of that, and not as a direct consequence of the mechanical accident.

An airplane, as a man-made object, is inherently unstable and must be controlled. It is well to remember that for a long time attempts at powered flight had failed, not because of disregard or ignorance of the laws of physics, but because of ignorance of the necessity of control. An inherently unstable system is not viable until the instability is changed into stability by control.

The development of powered flight and the recognition that the system called airplane required control, contributed greatly to the growth of industrial technology. Control is now ubiquitous. (But no Nobel prizes have been given for anything related to powered flight.) Control was a discovery, an invention. It developed just an slowly as the physical sciences. There seems to be some evidence that irrigation in Mesopotamia was scientifically managed five thousand years ago. In the Hellenistic world, there have been many artifacts, ingenious machines, automata, all of which nearly triggered technological development, but somehow nothing happened. The really rapid development of control technology and then of control theory (which in turn led to system theory) began only in the late nineteenth century.

Why is control possible at all? Is it a physical law? Is it merely a clever trick? These questions were understood in a scientific sense only recently. One of the nicest early achievements in the evolution of system theory was a generalization and strengthening of certain aspects of classical control theory. We now know the following precise result.

*If all parts of a system can be influenced by external physical forces, then the system can also be controlled.*

This means that in practice almost any system can be controlled. If this weren't a scientific fact, we would not have an industrial civilization today. The ancient Greeks tried but did not arrive at an understanding of control (my conjectures) and they remained at a very low level of industrial civilization (a well-known fact).

Control should be viewed as a mathematical thing. It has little to do with any physical circumstance; it applies equally well to the control of an airplane, control of spacecraft, control of a chemical reaction, control of an economy, and biological control processes. It is not a physical law; it neither agrees with nor contradicts any physical law, but it is something more general and certainly more immediately useful. The connection with the real world comes about through the miraculous fact[2)] that the mathematics has much to say about that world.

"Control," as a system concept, has just as much claim to being scientific as any physical concept such as mass or temperature: but "control" is something essentially different. Control theory is scientifically significant because—in my opinion—it is important to know that almost any system can be controlled once we can express its physical characteristics in mathematical language. A counterexample, namely a "system" that cannot be controlled, is given by physics in situations where conservation of momentum is applicable. But these situations are hardly "systems." Conservation laws are very important in physics but they are not very relevant to biology or to man -made systems. Because physicists like conservation laws, they cannot understand system theory. System theorists do not have an aesthetic fixation on these "laws," they are more interested in probing how to get around them. To be blunt, the methodology of research in contemporary physics has become too inflexible to be useful for research on systems. But this was not always so.

As a mathematical result, the fact of "controllability of almost any system" is related to very famous work of Hilbert at the end of the nineteenth century on invariant theory. This is where high mathematics is joined with high technology. The roots of mathematical system theory are much older, however. I shall come bake to this point after explaining to you another system problem, *filtering*.

〔The name, "Kalman filter" has become extremely famous. I was told in Moscow a year ago that there exist over a hundred thousand published papers on the subject. If not an exaggeration, this is a bit surprising since the problem—so it seemed to me—was already solved in my first paper and thoroughly explored in three of four papers following it. Fame is not always kind; there should be many more Wiener filter papers (and there were, before 1960) but there are few today. Moreover, there should have been papers on Kolmogorov filters, but there were almost none. The future may bring new things, for example papers on Kallman filtering.[3)]〕

Filtering is a basic system problem. There is now an enormous variety of applications. The current literature is exclusively concerned with these applications (how to use a computer? What to filter?) and not with basic principles. Those have remained unchanged since 1959.

Well, what exactly is filtering? It is the creation of new kinds of data from physical measurements, by removing the "noise" from the measured data. Filtered data is then used for control, for example. To control a system, its physical characteristics must be very accurately known; in particular, the values of some variables in the system must be known. Filtering may be viewed as manufacturing (by computation) virtual measurements on those system variables which are not accessible to direct measurement. This is done by using the available measurements on those variables that can be directly measured. From this perspective, filtering is a way of extending the power of measuring apparatus.

There is nothing mysterious about a filter; it is simply a computer program. A filter needs a computer to do its job. Filtering theory is needed to tell the computer what to do. Writing a computer program for the filter is relatively easy because filtering theory provides an explicit recipe for doing that. Everything depends on having the right theory.

At this point I should like to quote from the Nobel lecture of December 11, 1954 of Max Born (originally in German[4)]). He is concerned, in one of his examples, with exactly the same problem as filtering; he reaches a conclusion which is very surprising even considering that the lecture was given more than thirty years ago. The conclusion is similar to that of the nameless physicist mentioned above and it is equally nonsensical as science. (Now, however, there are no reservations concerning the physicist's qualifications since I am quoting from his Nobel lecture.)

Born is addressing the question of determinism in physics. He is asking, "Is it possible to have very accurate predictions in physics using the classical equations of motion?" And now I quote:

> It can be easily seen that this is only the case when the possibility of absolutely exact measurement, of position, velocity, etc. is assumed.[5)]

Easily seen, Born says, by simple examples. Then he gives an example.

> Let us think of a particle which bounces back and forth between two complete elastic walls. The particle moves with constant initial velocity, $V_0$, and we can say exactly where it is, provided we know $V_0$ exactly. If we allow, however, a small uncertainty in the value of $Vo_0$ then the uncertainty of prediction of the position of the particle will grow with time so that, if we wait long enough the initial uncertainty will grow to become more than the distance between the walls and after that time, no prediction whatever is possible about the position of the particle.[5)]

Then he summarizes:

> Thus determinism lapses completely into indeterminism as soon as the slightest inaccuracy in data on velocity is permitted.

All this was said and written with rare clarity, in the style of elementary textbooks in physics, exactly four years before my first paper on optimal filtering theory, but also five years after Bode and Shannon's paper[6)] on the same subject and many years after the basic contributions of Kolmogorov and Wiener.

Let us pause.

Is clarity the same as truth?

After the passage of thirty years it is difficult to understand how in 1954 a distinguished physicist could make such a crudely unacceptable *scientific* statement. (I leave aside the puzzle whether physicists who were Born’s contemporaries found his statements acceptable in the light of knowledge of physics at the time.) My point is simple. Born attributes indeterminacy in physics to the difficulty for measurements. But he commits a gross error when he attributes to physics his own assumption that after the initial velocity measurement no further measurements are possible. If we can make some further measurements, and it doesn’t matter much with what sort of accuracy, we can improve the quality of the original measurement by using optimal filtering. (In fact, Born’s example is a pedagogical example of optimal filtering that may be found in many books written after 1954 and perhaps even in Wiener’s “yellow peril,” a wartime, secret report with a yellow cover. It is amazing how far the scientific cultures of physics and system theory must have drifted apart, as early as 1954.)

To repeat: Born’s argument about indeterminacy hinges on the idea that physics can somehow prohibit the observer to record and optimally (mathematically) exploit measurement data. It is not very convincing to use such a prejudiced view of physics as a prop for the argument that “determinism lapses into indeterminism.” Many applications of Kalman filtering owe their origin to the discovery by competent nonphysicists that the conventional physical point of view, as caricaturized by Born’s example, is false or irrelevant. The *real* physics of the situation resides in the problem of explaining an intrinsic noise level that limits the accuracy of measurements. It is widely believed that noise in this sense is quantum-mechanical in origin. We do not have a full theoretical understanding of this at present. Born’s primitive argumentation is not to be found in any good book on system theory; the kindest thing that may be said for it is that filtering theory (and the system point of view) is perhaps not as irrelevant to quantum mechanics as it seemed to Born in 1954.

Born’s opinions are only incidental to this lecture, but the point he raised remains important. How does one do physics when it may have to be accepted that measurements are unavoidably inaccurate? Or, calling inaccuracies in measurement, equations, constants, etc. by the generic name “noise,” what can we do about noise? If we use the word *noise* to remind ourselves of the many unpleasant areas of fuzziness remaining in our present scientific knowledge, then this is certainly a very general and very basic problem, and its importance reaches far beyond physics. Personally, I do not see "determinism versus indeterminism" as a basic issue; but I see it as requiring a better understanding of physical mechanisms that establish an absolute noise level.

To summarize the last few minutes of discussion: By using filtering theory with the help of a computer we can greatly improve the overall effective accuracy of our measurements. We have lost our fear of "lapsing from determinism into complete indeterminism through small measurement errors," because we can use optimal filtering. We need physics to tell us how much noise we have to put up with. Beyond physics, we want to know what differences, if any, there are between science based on exact data and science that must somehow extract knowledge from inexact, often rather noisy data.

This is the dilemma of the social sciences. I would rather call them (and others) soft sciences to indicate that they have not yet been able to resolve the problems involved in soft (noisy) data.

We have now motivated my major remaining topic in system theory. This is what is usually called *realization theory*. My own work goes back to only 1961 although related problems (network synthesis) were explored many years ago by Foster, Cauer, Darlington, and Guillemin (the last my first teacher), always from the point of view of using mathematics as a research tool to be employed in trying to understand systems.

I was and will remain fascinated by the realization problem. It served to educate me about how classical science evolved, to reveal the shallowness of conventional interpretations by historians of science, to understand what Newton had meant by his delphic phrase,

"Hypotheses non fingo"[7)],

and to begin to understand what mathematics has to do with the real world.

I will now explain in nontechnical terms the nature of the realization problem. Suppose I am given data concerning the behavior of a system under some concrete conditions. From this data, I want to know how the system functions. This has two aspects. First, how a system is put together is expressed by a family of equations, or, if you prefer the current jargon, by a model. So I want a model. Second, I do not want to use brain power to find out what the model is, I do not want to guess, I want a computer to find the model. If a computer is to do it, guessing is useless anyway because the computer must be given precise instructions to do its task. The realization problem calls for converting data into a model, and the problem must be described in mathematical terms with such a high precision that even a computer can do it. Realization is, at least abstractly speaking, an absolutely pure mathematical problem.

The end result of the solution of a realization problem, as of any mathematical problem, is a completely precise and true statement, which we mathematicians call a theorem. I cannot explain here how this process works in realization theory, but I want to give you some feel for the essentials.

Any mathematics proceeds by idealization. In realization theory, we regard "data" as something idealized and look for two properties. We first assume that the data are *exact* (in the usual sense of word: absolute accuracy, no noise). We then assume that the data are *complete*, by which we mean that we have as much data as we would ever want, and it is unnecessary (even though it might be possible) to perform any more experiments on our system in the future. "Completeness" in this sense is of course a highly idealized and abstract notion, but it is precisely what is needed to formulate some deep problems.

The main result of realization theory may be expressed quite accurately in ordinary English:

*Data that are exact and complete are realized* (that is, explained)
*by precisely one minimal model.*

The phrase "precisely one" in this statement means that the explanation of the data by a minimal model is *unique*. There is always an explanation and no more than one: any two explanations, even though they may seem to be different, must mean the same. But for the statement to be true, we must require the model to be *minimal*, namely as simple as possible. Einstein's dictum, quoted above, that an explanation *must* be as simple as possible but not simpler, now forms a part of the realization theory. A vague *bon mot* has been transformed into a scientific principle. We shall call the result quoted above the *Uniqueness Principle* of (minimal) modeling.

This result is so important that I will say it once more. Always under the assumption of exact and complete data (this is the best possible situation, a goal), the data must speak for itself, there is only one acceptable explanation, the minimal explanation, which means that I must not add any personal element to interpreting the data. Uniqueness means that the minimal explanation is method-independent, it does not matter how we arrive at it because this minimal explanation is already intrinsic in the data. Under these conditions on the data scientific modeling is a deductive exercise, not a creative act. The work consists of getting, somehow, the information out of the data; the processes for actually achieving this are of secondary importance. It is perfectly all right to use computers; it is not a matter of human pride or ingenuity.

The message of the uniqueness principle is seldom disputed by researchers in the hard sciences. But it is terribly unpleasant to those in the soft sciences, or shall we say presciences, where contriving an explanation, building a model, inventing a theory is still regarded in some wistful way as the supreme creative act. Everyone who practices research, and many do that without a licenese, thinks he has a right to become the Newton of his field.

There are many dissenting voices, however from such a naive dream as science. Leontiff (Nobel Prize in Economics, 1974) diagnosed the current malaise in econometrics (which is statistics applied to economic model building) by observing,

> …the econometricians fit algebraic functions of all possible shapes to essentially the same sets of data without being able to advance, in any perceptible way, a systematic understanding of the structure and the operations of a real economic system.[8)]

Admittedly, Leontiff's language is emotional and intuitive; no matter. By the Uniqueness Principle, there is just no point of fitting more than one (minimal) model to the same data, assuming the data is good (ideally, exact and complete). So there must be something wrong with econometricians. Either their data are bad and then there is little fun or profit in fitting models, or the data are good but econometricians don't know what models or how to fit them.

There are many research groups today that try to develop models for the same thing, for example, for the global economy. For them the uniqueness principle is something very hard to accept, assuming they have heard about it at all. After all, why should there be two groups building two models when there is only one meaningful model? What does each group of modelers contribute? And what's the use of even one group of human modelers when the computer can do their job better?

Leontiff and I agree that the econometric practice today (it should not be dignified by calling it methodology) is very very bad; in fact, it is a dead end. But neither Leontiff nor I can claim this to be an original contribution to science. Scientists and would-be scientists have been warned long ago, by Newton himself, when he wrote "Hypotheses non fingo."

Professor Bentzel, member of the Nobel Committee on Economics, asked after this lecture, but not from me directly, "Why does he (Kalman) say there is something wrong with economics? What's wrong?" The lecture tried to explain that; it cannot be done in a few words. Still, it is surely true that one of the things wrong with economics is that it is not a science—otherwise economics, whose basic problems involve systems would be close to system theory which is not, as yet, the case. Since in science data must (somehow) be related to explanation, models, theories, this must happen also in economics; and it hasn't happened because econometrics, which is supposed to be the tool that relates economic data to theorizing, has been haplessly and hopelessly inadequate for this task up to the present.

Newton had a wicked sense of humor as must be obvious to all those who were taken in by the silly story about the apple, a journalistic canard that he set into circulation at the very end of his life. His contemporaries had no choice but to believe him, but did they understand him? "Hypotheses non fingo"[9)] was mistranslated two years after Newton died as "I do not frame hypotheses." The correct translation ought to be something like "I disdain〔explaining natural phenomena by〕 contrived hypotheses."

By the middle of his life, Newton has educated himself to become the first great system theorist. Of course Newton was also a great physicist and made very great contributions to optics in his youth. But I shall speak about a different side of him, his "proof," after twenty years of work, of the inverse square law of gravitation. It is very much related to what I was saying earlier.

The nature of gravitation was the subject of much speculation in Newton's time. It was easy to guess that the force of gravitation between two bodies is inversely proportional to the square of the distance between them. It is almost as easy to derive this conclusion (as Newton may well have done himself) by combining Kepler's third "law" of planetary motion with the formula expressing the centrifugal force on a body in uniform circular motion. The derivation is not a proof, however. (It is not even convincing heuristics because Kepler's first "law" claims emphatically that a planet moves in an *elliptical*, and not in a circular, path around the sun.)

Newton proceeded to study the problem of gravitation by a method which is essentially the same as realization theory is today. He took all three of Kepler's empirical "laws" of planetary motion as his *data*, and it was a truly remarkable historical accident in the development of Western science that this data automatically satisfies the two crucial requirements of being both exact and complete. Exact, because Kepler's "laws" were stated in mathematical language and therefore automatically exact. Complete, because the most natural assumption for a physicist (and that Newton also was, as I mentioned) is to say that all planets around all suns move in accordance with Kepler's "laws;" if so, then Kepler's "laws," viewed as data about planetary motion, are indeed complete in the sense of the definition I gave earlier.

Newton then worked out the consequences of some difficult mathematical experiments. If the motions are such and such (data), what must the forces (model) be? And conversely, given the forces, what are the motions? These are two sides of the question of realization; what model, "realizes," i.e., reproduces, a given set of data, and how does the data set restrict the possible models that realize it. Newton rigorously established, by means of mathematics, that the gravitational force satisfying the inverse square law makes a (single) planet orbit its sun on an elliptical path so that all three of Kepler's "laws" are satisfied; he also showed, again by rigorous mathematics, that Kepler's three "laws" together imply that the gravitational force must necessarily obey the inverse square law. In this way Newton proved that planetary motion around a Sun, (the two-body problem) and the inverse square law for the gravitational force are in one-to-one correspondence. This is the content of the Uniqueness Principle; Newton rigorously "verified" that the principle was actually a theorem in the special case of planetary motion. Newton did not explicitly state the Uniqueness Principle; that he was well aware of most of its contents will become clear as I complete my lecture.

Newton was absolutely insistent that a physical law is meaningful only if all the data is explained in a *unique* way. Had he himself originated the Uniqueness Principle, he would have surely said that a physical law may arise only through the validity of the Uniqueness Principle; that no one can talk meaningfully about a physical law unless the Uniqueness Principle holds. He regarded as the main accomplishment of his life his "proof" of Kepler's empirical "laws." I prefer to be on Newton's side in not referring to Kepler's "laws" that is, Kepler's rules, as true physical laws, and I am aware of the fact that three hundred years after Newton most physicists still do not appreciate this nuance. Newton categorically did not agree that Kepler established his laws, claiming that "Kepler knew only that the planets moved in noncircular orbits and guessed that these orbits were ellipses"[9] and he would concede only that "Kepler guessed right"[9] Incidentally, Newton's precise mathemataical treatment of the two-body problem by the classical technique of conic sections made it clear to him that there are not only elliptical but also hyperbolic orbits (the latter unknown to Kepler). Newton got a bonus for having "mathematicized" Kepler's "laws": the motion of Halley's comet could be immediately explained by the inverse square law.

By hindsight, Newton might have been more generous toward Kepler; after all Kepler not only gave Newton his data but gave him data which was both exact and complete—the critical requirement of the Uniqueness Principle. In any case, my impression is that Newton used all aspects of the Uniqueness Principle exactly as if he himself had formulated it.

〔The only strictly analogous case I know of in the history of science is Mendel's laws of genetics. Posterity has been at times ungenerous to Mendel. He has been *accused* (by statisticians) of "cooking" his data to be exact and complete, not by the intermediary of a now-forgotten Kepler of plant genetics but by himself.〕

Newton did much more than use the Uniqueness Principle as a kind of philosophical crutch. He knew—whether consciously or subconsciously—that to implement the Uniqueness Principle, that is, to actually find the unique minimal model that explains the data, there is no alternative to mathematics. This was a major part of Newton's intellectual achievement. He created new mathematics which later turned out to be very helpful to him in this task. He found the minimal model (the inverse square law) by mathematics. He was proud of this. He regarded himself as a hard-working mathematician (as he surely was) and resented being accused of plagiarism by someone (Hook) whose intellectual powers were limited to guessing the end result of a tricky process. I think Newton would have been generous in giving credit to computers (and their developers) if computers had existed in 1680 because he understood that realization was a deductive, laborious task, not a creative act.

Newton had a very sophisticated view of science, a view that is much closer to system theory today than to what has become later classical theoretical and mathematical physics. It is a view that is not easy to popularize. Who among us can say that he understood immediately that “proving Kepler’s laws” and “letting the data speak for itself,” are really the same thing? And that this is crucial? In the Scholium Generale (in Latin[10]), Newton tried verbal explanations but (in my view) not quite successfully. We now know that he tried hard because five handwritten drafts of portions of this rather short (two printed pages) scholium have survived in Newton’s handwriting. He expessed himself most clearly (in my opinion) when he wrote, in referring to ad hoc hypotheses which are added to the data in an attempt to produce a more palatable explanation,

> “Prejudicia sunt et scientiam non pariunt.”[11]
> *In English*: 〔Such extraneous hypotheses〕 are 〔merely〕 prejudices which cannot engender 〔good〕 science.

This is unmistakable: Newton warns, *prejudice is no way to do science, you cannot advance science by prejudice.* If you dislike a blunt word like “prejudice” then I could soften Newton for you by saying *you cannot do science by wishful thinking*. (But there is absolute evidence that Newton did write “prejudice,” at least twice.) Unfortunately, in the end Newton replaced several paragraphs of explanations with a single sentence “hypotheses non fingo,” and let it go at that.

By calling *prejudices* those assumptions which are added to data as an attempt to generate a “better” theory Newton was extraordinarily farsighted. For “prejudice” is probably the most accurate word in English or Latin to express the futility of imposing a subjective personal judgment (which in science is seldom more than a guess) to the data, trying to pass it off as part of the scientific process. Newton thunders that you just cannot do science in that way. And he was right; in the very famous concluding paragraph of the Scholium Generale, he made a remarkably good guess at neurophysiology, but added that it is bound to take a long time to get scientific research moving in that area. Indeed, it took two hundred years.

〔I suppose Newton would not be proud of physics today if he were told about superstring theory. He would make fun of it as the most extreme case of “hypotheses fingere” ever. The burden of proof is not on him.〕

Newton would have had no patience with excuses for slow progress in the soft sciences. His data (that is, Kepler's) were very good. On the other hand, the soft scientists never tire of complaining that they cannot do experiments and that, as a consequence, their data are very bad and that they cannot be expected to do better. Consequently, they say, Leontief's criticism (quoted above) is not fair.

I have no love for this argument and I am not lacking in powerful supporters. A qualified critic, von Neumann, reminded us, thirty years ago,[12)] that it is impossible to experiment in astronomy (at least it was then) yet astronomy has not been a soft science. Newton's problem was in astronomy and he had gotten around the impossibility of experiments by inventing realization theory which can be understood intuitively as a very searching way of asking questions of data. A kind of mathematical substitute for experiments. That this is essntial to science was well known not only to the lofty Newton but also to Russell whom I have quoted at the beginning, and to many others. Medawar (Nobel Prize in Medicine, 1961) had said that "experimentation in science is a kind of criticism." I would put it the other way: criticism of the theoretical and intellectual bases of science is a substitute for experiments. Lack of easy experimentation makes the job of the soft sciences harder but does not excuse the lack of performance.

There remains, however, the excuse that data in the soft sciences are always bad. Their data *are* noisy. This is a superficial complaint. The hard scientist should ask instead: What can be done about noise? How much noise does a piece of data contain? How do we know?

Such questions require an answer. The answer ought to come from the methodology of the soft sciences, which at the present time is roughly the same as applied atatistics. Leontiff's criticism, that "mathematical" papers in economics are fruitless, also needs to be answered. These two problems are closely related.

From a scientific view the most obvious failing of theoretical statistics is its reliance on prejudice. The literature of statistics, as far as realization and model building is concerned, consists mainly of the study of various kinds of prejudices. These prejudices are assumptions about noise, invariably made in advance, before any data is available or before the available data has been examined. Statistical procedures (confidence intervals, tests of significance, error estimates) do come up with some educated guesses about the noise, but the validity of such guesses depends critically on the prejudice that has been fixed at the beginning. If the prejudice that leads to the guess about the nature of noise is nearly true the results of data analysis will be acceptable; otherwise the results may be meaningless. The situation with data analysis is similar to experiments is biology; one must be terribly careful not to introduce artifacts into the phenomena one wants to study. Prejudices in statistics correspond to artifacts in other fields; just as bad and more hidden. Contemporary statistics is concerned far too much with the study of its own artifacts. I am aware that "prejudice" is not a polite word, but then neither was "fingere."

I have implied that the field of statistics, the standard way of dealing with noisy data, is not now a science. I should explain this concretely. The trouble began exactly one hundred years ago when Galton, a once-famous British biologist, introduced the hypothesis of regression.[13)] His goal was a theory of heredity (now called genetics). He collected data concerning father-son pairs (for example, the height of each, or their arm lengths, etc.) Using this data, which was not exact but relatively accurate as regards measurement errors, Galton thought he has proved that all living organisms must regresse toward the mean. Since this conclusion presupposed what was delicately called "random mating," Galton enthusiastically promoted eugenics, the attempt to counteract random mating by—well, trying to breed an elite.

The echoes of Galton's nonsense are still everywhere today, especally in the popular press. The sad fact is that Galton, a mediocre scientist, was misled by his own prejudice. He assumed that all the noise in his data was in the sons; if he had assumed the opposite, that all noise is in the fathers (another reasonable prejudice), he would have had to conclude that the population in the distant past was a bunch of clones of the primordial mean and then drifted steadily apart. An examination of Galton's data, which is still available, shows that his conclusion was entirely the result of his prejudice; the data itself does *not* allow any conclusion to be made either in favor of or against the hypothesis of regression. Galton needed different data to study the problem of inheritance.

We should recall that Mendel's hard（low-noise）data *ante*dated Galton's fuzzy thinking by twenty years.

Mendel had been able to find the laws of genetics not only because his data was nearly noise-free but mainly because he allowed the data, and not his own prejudices, to speak. Galton thought he should and could introduce quantitative methods, even mathematics, into biology. He measured the heights of individuals. But he never took the trouble to reflect over what Newton had done and how Newton had succeeded. The relationship between mathematics and science is not nearly as simple as present-day propaganda would have us believe. (As an aside, let us note also that in some cases Newton's insights into pure mathematics are still ahead of what is established knowledge today.[14)])

To be fair, let us not forget that Galton was unlucky. He did not know that the method of regression (or, by its classical name, least-squares), which was inspired by and named after his own regression hypothesis (but which is pure mathematics and thus unrelated to biology), is unusually treacherous. Regression, as a scientific method, is useless because it can only be applied after some basic prejudiced assumptions have been made, as Galton, too, unwittingly had done. Any such prejudice results in creating noise relative to hypothetical exact data. Regression is unusual because it can create noise for any prejudice. Noise will always appear as though it were the true noise in the data. If the right prejudice happens to be applied to the data, all is well, though the lucky guesser may not know it. Otherwise "results" from regression are just an artifact.

Newton would have been unsympathetic, saying: "You must not allow prejudice. If your data is noisy, analyze it in some other way than by just guessing about noise." Galton and his statistical descendants never understood this point. Einstein was much kinder but even less helpful. He had said:

> "God is not malicious but he can be very tricky."

Indeed, Einstein and God must have got a big chuckle out of regression. No matter how you apply it, it seems to give a logically correct result. And so it always gives the wrong answer. Or almost always and one never knows why. This seems somehow unfair. But it is only tricky. As one of my teachers put it, in a published article, "What is Nature's error criterion?"[15)] There is no answer to the question because the question itself is meaningless. Nature doesn't have an error criterion. It doesn't need one. That's how it is. *So deska*.

I hope you like my poetic language. But what I am saying is hard mathematical fact. There is a theorem which states that least squares is compatible with *any* prejudice, irrespective of the data. (Take any prejudice you like, apply it to the data and it will appear as though your prejudice was confirmed by the data. But it is only an illusion because the prejudice was picked before the data.)

A prejudice-free methodology of dealing with noisy data does not exist at present. Conventional ideas of statistics cannot lead us to it. Yet it is surely not impossible to find a way. It may take another Newton to accomplish the leap from exact (noise-free) realization theory to noisy realization theory. I am more modest in my hopes; I predict that someday someone will stand here to receive your very generous prize for it. He will surely have deserved it.

There is reason for this optimism. The uniqueness principle is not directly applicable when the data are noisy, but it is still quite relevant. Using the very old idea of continuity, if the noise is sufficiently small (as in the case of Mendel's data), uniqueness must still hold, approximately. In this situation it does not matter what prejudices are applied in data analysis because the uniqueness principle guarantees that the results are, approximately, method-independent and therefore prejudice-independent, at least as long as the prejudice is not so glaringly false as to destroy continuity near the small-noise limit. There are many textbook examples of data having small noise, but this fact may be camouflaged by an outrageous prejudice of the modeler[16)] who presents the data.

I would like to conclude with a thought quite unlike those that went before. Obviously there are things far beyond high technology, far above what we know about science today, things that are beyond anything our generation can analyze, think about, imagine, feel. I take as my last words the beginning of a medieval hymn which Mahler used to begin his Eighth Symphony,

Veni Creator spiritus.

("Oh come, Creator of the human soul.")

Thank you very much.

Notes

1) T. R. KANE and D. R. TEIXERA FILHO, "Instability of Rotation about a Centroidal Axis of Maximum Moment of Inertia," AIAA (American Institute of Aeronautics and Astronautics) *Journal*, 10 (1972) 1356-1358.
2) E. WIGNER, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," Communications in Pure and Applied Mathematics, 13 (1960) 1-14.

3) R. R. KALLMAN, "Construction of Low-Noise Optical Correlation Filters," Applied Optics, 25 (1986) 1032-1033.
4) Les Prix Nobel, 1954, p. 79-90.
5) ibidem, p. 87.
6) H. W. BODE and C. E. SHANNON, "A Simplified Derivation of Linear Least-Squares smoothing and Prediction Theory," *Proceedings* IRE, 38 (1950) 417-425.
7) In the penultimate paragraph of "Scholium Generale," second and third editions of "*Philosophiae Naturalis Priscipia Matematica*." (This passage appears in print in the middle of page 530 of the third edition of the *Principia* (on page 764 of Volume II of the modern facsimile reedition by A. KOYRE and I. B. Cohen, Harvard University Press, 1972).)
8) W. LEONTIEF, *Science*, 217 (1982) 107.
9) Letter by NEWTON to HALLEY, 20 June 1686. Published as Letter no. 288 in *The correspondence of Sir Isaac Newton,* vol. 2, edited by H. W. TURNBULL. Cambridge University Press, 1960.
10) ibidem.
11) See I. B. COHEN, *Introduction to Newton's 'Principia'* Harvard University Press, 1978, p. 243.
12) J. von NEUMANN, "The Impact of Recent Developments in Science on the Economy and on Economics" (summary of Speech before the National Planning Association). *Looking Ahead* , 4 (1955) 11; reprinted in the author's Collected Works, Vol, VI, pp. 100-1.
13) The usual references are the following: F. GALTON, "Family Likeness in Stature," Proceedings of the Royal Society of London, 40 (1886) 42-63; and "Regression Towards Mediocrity in Hereditary Stature," Journal of the Anthropological Institute, 15 (1886) 246-266.
14) *The Mathematical Intelligencer,* vol.9, no.4, pages 28-32, interview with V. I. ARNOLD.
15) E. A. GUILLEMIN, "What is Nature's Error Criterion?," IRE Transactions, CT-1 (1954) 76.
16) Such a prejudice is the subtle defect inherent in a pedagogical example employed in E. MALINVAUD, *Méthodes Statistiques de l'Econometrie,* Dunod, 3rd edition,

pages 228-237 to illustrate regression calculations.