Modeling and control in physical, life, and social sciences: Some remarks

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Abstract

Mathematical —or mathematico-numerical— models have pervaded all branches of knowledge, and once control theoreticians have become mainly model-builders. Their mathematical skills are called upon to analyze these models, much less to build them. This confers a renewed significance to an epistemological reflection upon what has become the heart of their occupation. We offer some general remarks, and then attempt to reflect on the different epistemological status of (mathematical) models in physics ("the unreasonable effectiveness of mathematics in the natural sciences") and engineering, life sciences, and social sciences, constrained by my limited experience of the second, and very limited experience of the third (except of mathematical finance, hardly a social science).

1 Introduction

1.1 What has happened ?

In the late sixties, the gaullist government created the Institut de Recherche en Informatique et Automatique. The addition of the word "automatique" is due to the advice of Pierre Faure, just back from a PhD with R. E. Kalman at Stanford. He had understood the central role of this type of applications of computer sciences. But today, the modern INRIA (it has acquired the N of National in 1980) pointedly describes its activities as "computer science and applied mathematics". And indeed, most of the researches of the "non computer scientists" at INRIA would

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have hardly been qualified as "automatique" (system theory + control theory + estimation theory and signal processing) in those days.

What has happened ? A major revolution in the status of mathematics: the advent of the cheap powerful computer.

I emphasize here the use of mathematics to describe dynamical processes. (I was educated as a control theoretician.) The same is true more globally. The central tools in that domain are ordinary differential equations (ODE), as partial differential equations (PDE) are in physics. For a long time, the practical use of ODE's was restricted to the very few types that could be integrated in closed form, mainly (but not only) linear ones. Astronomers did integrate numerically nonlinear ODE's "by hand", but this was a feat for astronomers . . . , not engineers. In the early XXth century, came Lyapunov and Poincaré e Bendixon. This extended slightly the domain of practical use of ODE's. But not by much as compared to what happened at the end of the century: the possibility to integrate numerically any ODE.

Needless to say, what I say of ODE's is even more true for PDE's. But it is up to my colleagues in numerical analysis of PDE's to describe it. A whole new branch of mathematics has appeared: numerical analysis. Among his founders, Jacques-Louis Lions was the first head of the new INRIA in 1980.

All of a sudden, the very epistemological status of mathematics changed. In the old days, they could be used to solve "toy problems" that showed that conceptually, they could be used to describe the world. The archetype of such problems is the Brachistochrone problem. This showed the possibility to answer difficult questions with the help of analysis (a new branch of mathematics in this early XVIIIth century), but did not allow one to solve many other applied problems.

Epistemologically, if we accept this distinction, mathematics were typically the tool to derive a priori truths (analytic in the sense of Frege. Kant would have classified them as synthetic a priori). With the advent of the computer, they acquire an experimental character, allowing one to derive a posteriori statements, and, if we accept the classic distinction, synthetic truths, i.e. statements that say something about the physical world.

Traditional system theory was very much restricted to linear systems, and produced, say, the theory of servomechanisms and Wiener filtering, the heart of early "automatique". With the advent of the computer, came new mathematics to harness their power, such as Pontryagin's Maximum Principle and Bellman's Dynamic programming, the heirs of traditional calculus of variations and, respectively, of Caratheodory's theory, made into practical, numerical tools. And then the trend continued. I used to be an "automaticien", (say, control theoretician) but my generation has seen the transformation of that profession into one of constructing mathematical models and analyzing them with these new mathematics where differential equations, but also static equations, many optimization problems, etc. could be solved in the sense, somewhat new for mathematics, of finding numerical solutions for numerical data.

1.2 And computer scientists ?

If this new status of mathematics, and the new form of our job, explains why control theoreticians have written about epistemology, how come that I also quote pure computer scientists ?

The answer is in the most scientific form of artificial intelligence: theorem proving. This attempt has constrained some computer scientists to try and understand what is the heart of mathematics: calculation on the one hand, and theorem proving on the other hand. They have been obliged to reflect on the link with formal logic, and the use of these tools to describe Nature.

Computer scientists have also been compelled to think about languages, and formalize, in various ways, the concepts of syntax, and, more difficult, of semantics of a language. While there are obvious connections with epistemology, I shall not consider here this line of thoughts, more remote, may be, from modeling.

1.3 The rest is only mathematics

These are scattered remarks on this new activity we all perform, i.e. use mathematical models to say something about, and often control, real life systems. I have not become a professional philosopher, as others have done. Just tried to do my job with intelligence. And my aim is to convince younger colleagues that epistemology has become a necessary part of our job.

As a matter of fact, years of practicing modeling and control has led me to the conclusion that the relationship between reality and our mathematical models is far from self evident. And trying to deepen my views on the topic, I discovered that respected authors disagree. At the expense of being pretentious —I am not Einstein, who argued in favor of an epistemological thought among physicists— I therefore decided to talk about epistemology; *my* epistemology, necessarily strongly influenced by my experience as a control theoretician.

When I was a math professor in the engineering department of the university of Nice, I would tell my students : “all the intelligence is in the modeling. The rest is only mathematics.” Of course, it was a bit exaggerated, and coming from a math professor, a bit self deriding. Another way of putting it is that physics, biology, economics, are the real sciences. Mathematics is just an ancillary tool, may be a prominent one.

2 General remarks

2.1 Historical remarks on maths, modeling, and rigor

2.1.1 Rigor and invention

Mathematics started with the attempt to describe physical objects. Geometry was needed to measure fields, to partition them in case of inheritances. Eudoxus of Cnidus developed a rigorous way of calculating areas and volumes, the method of exhaustion. (He proved that the area of the circle is proportional to the square of the radius, that the volume of a cone is one third of the volume of the cylinder with the same basis and same height, etc.)

But the greeks had a problem with numbers, ever since Hipasus (was it him ?), a pythagorean, proved that "there is no number" to describe the length of the diagonal of a square. (The pythagoras school considered the natural numbers to be the founding matter of the Universe, and could only conceive of rational numbers beyond.) Eudoxus almost circumvented this problem with his invention of "magnitudes" represented by lengths, but with which he could calculate. This is how close the ancient greeks came to real numbers, until Hero of Alexandria, more of an engineer than a mathematician in the greek tradition, started calculating with numbers without regard for their rigorous status ... a fruitful approach which was to last until the second part of the 19th century ! (But as I shall say in a moment, I, too, have a problem with real numbers.)

This in itself is something we must not forget. Fourier was criticized in his time for not rigorously proving the convergence of the Fourier series ... Yet his invention was genial. Louis Bachelier was criticized for the lack of mathematical rigor of his concept of noise. He was even blackballed from a position at the university of Paris by Paul Lévy himself. Lévy later recognized his mistake. Dirac used the impulse as a function, which it cannot be. And as we know, Isaacs was criticized for the lack of rigor of his work on differential games. (He did not prove the existence of Value ...) Yet, we also know that it was deep, innovative, and ahead of his time.

All these people were concerned with the modeling of something (heat, finance, particles, combat games), and were not deterred in their *invention* by the difficulty of making it precise. This is not to say that making these things more rigorous later on was not useful. Laurent Schwartz did a grand work in theorizing distributions. But they were invented by Dirac !

I once proposed to a committee gathered by our Prime Minister to rethink the teaching of maths in high schools, that all mathematics majors, the future maths teachers, be obliged to take each semester one course applying maths, but taught outside the maths department, and if at all possible, not by a mathematician. My idea, which was not followed by the committee, was to let them see the use of $\mathrm{d}x$

as a "very small increment", or the relative size of an individual to a population as "essentially zero" because the population is "very large" and "practically infinite" yet "pick an individual at random" in that (countable) population, and all these imprecise statements that go into the intuition of how you build a mathematical model.

I believe that this is where maths are used in science, the very flesh of applied maths. Of course, we, mathematicians, need to revisit these things a posteriori, and make them more rigorous if at all possible. But this "is only mathematics".

2.2 Real numbers, probabilities, mathematical finance

2.2.1 Real numbers

I claim that *real numbers are too rich for modeling physical objects*. The trouble is that we have not been able to find something less rich and as easy to use.

One place where this shows up is in the Banach-Tarsky paradox: it is possible, with real numbers, to describe a way of partitioning a sphere in small pieces, and then rearrange these pieces into two solid spheres with the same diameter as the first one. One immediately objects that it is not possible, because the sum of the volumes of these pieces must be equal to the volume of the first sphere, not twice that volume. You all know the answer: my small pieces have no volume. In technical terms they are not measurable.

If my small pieces have no volume, they have no (defined) mass either. Hence these things cannot exist. The trouble is with non measurable sets which are not models of physical objects.

A similar problem is raised by continuous functions of unbounded variation. The graph of such a function has no length, unless you want to state that it is of infinite length. (If it had a length, it would be larger than any real number.) These un-natural things, such as Bolzano's curve and Weierstrass' snow flake, serve two purposes. One is to show that, again, real numbers are too rich to represent real objects. The second one is that, since we do not know how to do without real numbers, rigor is, nevertheless (!), needed at some point. Before Riemann and Weierstrass, many mathematicians strived to prove the "theorem" that continuous functions are differentiable except at rare points. The great André Marie Ampère even published a "proof". (And after the fact, Hermite wrote to Stieltjes "I turn with terror and horror from this lamentable scourge of continuous functions with no derivatives." [1]

[1] "Je me détourne avec effroi et horreur de cette plaie lamentable des fonctions continues qui n'ont point de dérivées".

2.2.2 Probabilities

This brings me to another construct which I believe is too rich for its purpose: probabilities. Again, the trouble is that in many situations, I do not know how to do without it.

First let me say my admiration for Kolmogorov's construction of probabilities, and what has been done with it: one of the most beautiful achievements of applied mathematics.[2] But yet, as real numbers, somehow too beautiful for its purpose. For one thing, when an engineer asks a question to a mathematician, he gets an answer, but he does not understand it. When he asks a question to a probabilist,[3] he gets an answer as well, but then he does not understand his question !

Typically, diffusions as models of physical noise are, at best, a limit of something physical as some characteristic number goes to zero. But they are not physical things in themselves. The fact that the trajectory of a diffusion be almost surely of unbounded variation is a sufficient proof of my claim, if one accepts the preceding one. The un-natural Ito calculus is, as a matter of fact, directly a consequence of that un-natural fact, as shown by Föllmer's lemma [8].

But diffusions are not the only instance where probabilities look to me as an overshoot. Say conditional expectation. In its elementary form, it is a fairly intuitive concept. But when you need to explain that it is a Radon-Nicodym derivative, isn't it too much of mathematics to model "I do not know, but my best guess is ...".

More fundamentally, a probability law is not a good model of "I do not know". Jean-Pierre Aubin [1] has explained that much better than I shall ever do. In a few words, placing a probability law on the set of possible values of an "unknown" number is giving an extremely rich information about that number, mainly so if several instances are at play. And once you have given this rich information, mathematics will use all of it, far beyond what you intended.

Robust control Part of the literature on robust control has used a tychastic, or viabilist, approach to modeling model's uncertainties. Typically, one lumps the unknown differences between the nominal model and the actual system model in an extra, unknown, system connected in feedback around the nominal model, and assumes a known hard bound on the operator norm of the unknown system. This typically tychastic model was not chosen out of a reflection on the relevance of a stochastic description. It was chosen because of the extreme difficulty of the theory of differential equations with diffusion coefficients, a fact related to my

[2]One should quote at least one attempt at another foundation of probabilities, namely game theory. It did not go very far, but it gave birth to a fascinating book [22].

[3]I am not quite sure that this word exists in English. I will use it to mean a scientist working mainly with stochastic models and probabilities.

claim about the excessive technicality of probabilities. And since one wanted to model uncertainties in the coefficients of the model, they had to do with something else.

Is Nature probabilistic ? I will purposely not discuss that question. Such giants as Einstein, Born, Pauli and others argued over the epistemology of quantum mechanics. Whom am I to try and add anything to their arguments? Concerning macroscopic processes, I like to quote a great probabilist: my late colleague and friend Georges Matheron, who wrote: "*there is no probabilistic phenomenon, there are only probabilistic descriptions of phenomena*".

2.2.3 Mathematical finance

Mathematical finance is an idiosyncratic mix of pure maths and social science, often closer to mathematics than to physics. Yet, Merton and Scholes won the "Nobel prize of economics" (actually the Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel).

Let me caricature the historical status of that respected science. (There is no disregard here: I myself edited, and wrote half of, a book on mathematical finance.) In the late seventies, stochastic control had reached such a sophistication that its clients, engineers, did not understand their questions any more. A sad situation for an applied science. In another part of the technical world, there were financiers. They were in a situation close to that of primitive tribes exposed to uncontrollable dangers: drought, excessive rain, thunder... These tribes have a sorcerer whose task is to ward off these dangers through magical rites. To be credible, these rites need to be unintelligible by the laymen. Then Black and Scholes came along, and you guess the end of the story.

Financiers would say "these are profound mathematics, therefore it is good medicine for us", and the first part of that sentence, at least, is true. And mathematicians would say "these are useful mathematics, the proof is that the financiers are asking for them". And the second part of that sentence, at least, is true.

There was also in that domain a typical instance of the "*great epistemological disease*" of applied mathematics: *play on words*. This is about the traditional model of stock price histories: the geometric diffusion or "Samuelson model" (sometimes called Black and Scholes model). Financiers had a notion of volatility, an imprecise notion, but with a clear meaning. Then came Samuelson's model, with a coefficient σ called "volatility", because it has to do with real life volatility. But then, you prove results about your mathematical model, and state them in plain English, where the word volatility is taken to mean the financiers' volatility.[4]

Notice that no trader has ever used the Black and Scholes formula to price a stock option. Fo one thing, there is no such thing as a geometric diffusion in nature, I already explained that. But worse: if there were such a thing, using the Black and Scholes formula would need that you know the exact Samuelson "volatility" of the future price process. An extremely detailed knowledge of the future, telling you the exact total relative quadratic variation of prices' history. This is why the Black and Scholes formula is used in a reverse fashion, telling from the options' premium what is the banker's estimate of the future volatility, the so called "implied volatility".

Probabilities have been invented to compute mean values. There are at least two families of problems for which we can do without: when an equality is sought for *all trajectories* (except those ... that never show up), as in the Black and Scholes theory, or when an *inequality* should be satisfied in all cases to ensure a floor performance, a natural formulation of hedging. The first case is a bit anecdotal. But it allowed me to give a probability free derivation of the Black and Scholes formula, building on the fact that Ito calculus is not a consequence of probabilities, but of the particular irregularity of the trajectories of Wiener's process.

The second case is not at all anecdotal. It gives rise to profound theories in Finance (see [2]) and elsewhere. The related epistemological thinking has been developed at length by Jean-Pierre Aubin [1], and I also offered my thoughts in our book [2], as did my co-authors. I will not delve into it here.

But the particular case of mathematical finance is instructive of how deep a change of thought it is for practitioners accustomed to probabilistic models. And this is properly an epistemological question. To almost all theoreticians as well as learned practitioners, financial uncertainty of the future *is* probabilistic, when we claim that probabilities are one way of describing it, and that other ways may be as or more relevant in some cases. The following story illustrates this fact.

We were at least four groups, those I gathered in the book, who independently pursued "viabilist" ideas about option pricing, with different mathematical apparatus, at the end of the past century. Around the year 2000, we all had significant results. Yet none appeared in the financial literature until much later, this in spite of our attempts. I remember an article of mines which was rejected by Econometrica within two days of its submission. With the Journal of Mathematical Finance, we ran into a reviewer who thought he understood what we were doing better than we did, and kept explaining to the editor and to us that we were considering the limit

[4] In relation with the misuse of catastrophe theory, Hector Sussman had this quip: *You may decide that you call a matrix an "elephant". You may decide that you call its spectrum a "trump". Then you can prove rigorously that every elephant has a trump. What you may* not *do is claim that you have proved something regarding big gray animals living in Africa.*

of a Samuelson model as σ goes to zero. And since we did not state it clearly, our article was deemed heuristic and imprecise. The article finally found its way in the Siam Journal on Control and Optimization, not in a finance journal.

For these people, it was just impossible to think about an unknown future in other terms than probability theory.[5]

Epistemology is not neutral on ethical grounds. When the ethics of market finance, and hence of mathematical finance, are questioned, most mathematicians shield themselves with a positivist epistemology : "finance obeys laws of nature, that we discover through our experiments and describe with our mathematics, which are just Nature's language (Galileo Galilei). Ethics are the responsibility of those who *use* these laws of nature." But if very different, non probabilistic, models prove efficient, then this proves that the dominant model is not a natural object that we discover, but a description that we invent. This fares better with the performative character of finance models, that the positivist stand ignores, and puts more ethical responsibility with the mathematicians.

2.3 (Big) Data, measurement and models

Data are not measurements. Data are needed to make measurements. But measurements are about the value, say numerical, of a "natural" (physical) *quantity*. One has to have isolated, and named, this "quantity" as being meaningful. Most often a first abstraction of the real world. One must also have an idea of how this natural quantity relates to the data. And most often also, if one wants to measure it, it is because it relates to other interesting things. All this is making a model. There is no measurement without a model. This is an old discussion, as witnessed by [14].

We have, over the years, seen several attempts to construct mathematical models from raw data wit no apriori choice of the type of model sought. "Non parametric statistics", e.g., are a method of description of a data set, not completely apriori free, but sometimes presented as such, with attempts to derive knowledge from it. And more ambitious aims were at times pursued by great system theoreticians. "No a priori" modeling has always appeared to me as epistemologically impossible, and this feeling was the starting point of my interest in epistemology.

The problem arises anew with what is called "Big data" by our computer scientists colleagues. The idea is that facts will be derived from data "without theory", they say, i.e. without any model. Indeed the sizes of the data sets now available are just mind blowing. Google's or Facebook's data are in the petabytes, may be hundreds of petabytes. And "theory free" remarkable results have come down, such as Google Translate, or Google Flu Trends in its first uses, before it goofed up.

[5] An "epistemological rut" in Bouleau's parlance.[3]

I recommend the reading of an article by Tim Harford in the Financial Times magazine [12] for a balanced appreciation of Big Data. He points to classical traps of statistical practice: data biases, false positive, ... that have not disappeared with Big data, contrary to what its proponents claim. This is not to say that "big data" has not achieved or will not achieve impressive things. Harnessing the power of modern computers on modern gigantic data sets is worth investigating for what it can possibly do.

My main point is that without a model, what data show are correlations. Not causal relations. I learned my favorite example from a statistician friend: the perfect correlation he displayed, in yearly data, between the number of divorces in California and the number of elevators installed in Sweden. As a matter of fact, where "theory free" big data has performed well is in recognizing similar patterns: recognizing faces, or a pedestrian in a video, or sentences to be translated. It is bound to fail when it attempts to find causal relationships, as the later failure of Google Flu trend shows, because the very statement of a causal relationship belongs to a model, acknowledged or not.

3 Physical sciences

3.1 The unreasonable effectiveness of mathematics in the natural sciences

3.1.1 Eugene Wigner

The subtitle of course refers to Eugene Wigner's famous article of 1960 [24]. One should remember that Wigner was a prominent architect of modern quantum theory of elementary particles, where purely mathematical symmetry arguments helped construct the model. In this article, he fully embraces Galileo Galilei's stance that "the grand book of the Universe is written in the language of mathematics" (although he does not quote him).

His main claim is what he calls "the empirical law of epistemology":

> The preceding three examples, which could be multiplied almost indefinitely, should illustrate the appropriateness and accuracy of the mathematical formulation of the laws of nature in terms of concepts chosen for their manipulability, the "laws of nature" being of almost fantastic accuracy but of strictly limited scope. I propose to refer to the observation which these examples illustrate as the empirical law of epistemology.

The phrase "chosen for their manipulability" refers to his claim that mathematical concepts are created by mathematicians on the sole merit of their elegance, as shown by the skillful developments they allow them to carry out, with no regard for their role in applications. A somewhat exaggerated statement, if not completely wrong: at the origin of essentially every new mathematical domain, we find an applied problem, and only exceptionally has the opposite be true, that a domain of mathematics develops before any application requires it. What does happen, however, is the appearance of mathematical objects in domains completely unrelated to their original aim, as ellipses in the motion of planets, or the number π in contexts having nothing to do with circles.

In the same vein Lévy-Leblond [18] quotes Langevin: "It is, however, remarkable that none of the mathematicians' abstract constructs, only governed by their desire of logic perfectness and increasing generality, seem to be destined to remain useless for the physical sciences. By a unique harmony, the mind's need, attempting to construct an adequate representation of the physical world, seem to have been predicted and forestalled by the abstract, logical and aesthetic analysis of the mathematician."[6]

Quoting also from the end of Wigner's article :

> The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve.

This echoes the famous quote by Einstein, that *the most incomprehensible thing about the world is that it is comprehensible*.

3.1.2 Gilles Dowek

In his book "Les métamorphoses du calcul" [4, chapter 5], my respected colleague Gilles Dowek, of INRIA, attempts to *prove* that the law of nature are mathematical, thus answering Wigner's above statement that it is "a gift that we neither understand nor deserve". Dowek is a respected computer scientist, working, inter alia, on automatic theorem proving, and thus on the foundations of mathematics. A domain very close to logic.

According to Dowek, Wigner's "gift" is a necessary fact understandable by logic. I will explain why I do not believe in his proof. But beware: on the one hand,

[6] Il est cependant [...] remarquable que, parmi les constructions abstraites réalisées par les mathématiques en prenant pour guide exclusif leur besoin de perfection logique et de généralité croissante, aucune ne semble devoir rester inutile au physicien. Par une harmonie singulière, les besoins de l'esprit, soucieux de construire une représentation adéquate du réel, semblent avoir été prévus et devancés par l'analyse logique et l'esthétique abstraite du mathématicien.

I venture into *his* domain of professional expertise, which is not mine, and on the other hand, his book won the *grand prix de philosophie de l'Académie Française*. I will never win a prize of philosophy. So, he might be right and me wrong. But let me explain my doubts.

Dowek's proof relies heavily on two arguments :

1. Church's thesis that the modern notion of computation, such as described, say, by the Turing machine —equivalent to Church's λ-calculus—, is the ultimate notion of computation. No other, more general concept can be found. May be.

2. Gandy's proof that physical systems perform that type of computations.

From there follows that the working of Nature is amenable to computation, i.e. mathematics.

Gandy's proof relies on the axiom that *a finite system can only be in a finite number of states*, the so called "finiteness of the density of information". Then the first example given by Dowek is the law of falling bodies, stating that the distance d traversed is given as a function of the time elapsed t as $d = (1/2)gt^2$.

I have two reservations about this example, and one deeper disagreement with the whole approach. My two reservations are as follows.

1. The word "*state*" in Gandy's axiom is not defined; at least in the book. An unexpected fact in a literature usually so precise in its statements. We, system theoreticians, have definitions of the "state of a system". Well-posedness for an evolution equation, Nerode equivalence class of inputs for an input-output system, etc. But they apply to the mathematical model, not to the physical object. Here, for instance, only in the model of rational mechanics can we say that the "state" of the system is the position and the velocity of the falling body. It does not say what is its "state" as referred to Gandy's statement.

2. The "*state*" of this system contains at least its position, since it is what we try to "explain". Here it is represented by a real number lying in an interval. How can one use a theory that starts from the dictum that this system can only be in a finite number of states ? (Theoretical computer scientists are as biased toward finite state machines as I am toward control theoretic concepts.)

But my real concern is elsewhere. Computation, as defined in Dowek's book and in modern calculability theory, is what a Turing machine can do: a sequence of replacements of some expressions by others according to a set of rules. My profound belief is that Nature does not *compute* the position of the falling body. It just let the phenomenon happen, with no hidden Turing machine making a computation. And if this phenomenon by itself is to be considered a computation, then obviously a machine can do it: let the machine be a falling body. Said otherwise, analog machines are not computing according to the definition of calculability.

Is this an instance of what I called the "great epistemological disease": play on words, here with the word "compute", or "calculate" ?

In [5], the argument is refined in that Dowek explains that there is an algorithm to compute $(1/2)gt^2$, or in his example of the oscillating pendulum, the solution of the differential equation $m\ddot{x} = -kx$ with adequate initial conditions. My claim is that this algorithm is *our way* of approximating the solution of *our model*. In no way Nature's doing.

3.1.3 Jean-Louis Krivine

Jean-Louis Krivine is a colleague of Dowek, working in the same general domain of proofs and programs, at University Paris VII "Diderot". In [16], referring to [15], he writes "*the belief that the physical world obeys mathematical laws belongs to a medieval anthropomorphism*". In French, he does not write *médiéval*, which is neutral and may apply to wonderful achievements such as medieval art, but *moyenâgeux*, which is depreciative.

His argument starts from the Curry-Howard correspondence, a syntactic analogy between proofs in some formalized logic systems and algorithms in, apparently unrelated, formal calculation systems. Thanks to this correspondence, any logical proof is associated with a *computation*, something a computer can do. And each such algorithm calculating a function corresponds to a logical proof. In that correspondence, the types of the arguments of the function correspond to hypotheses, the type of the result returned by the function corresponds to the theorem proved, and the algorithm to calculate this result to the logical proof.

Now Krivine asserts that the "laws of Nature" exist, but that their mathematical nature are due to the fact that the programs corresponding to our proofs about them have just been written in our minds —these portable computers—, by evolution. Nature does not "obey" our mathematics; our mathematics obey Nature because this coincidence is a selective advantage by which *Homo sapiens* has been selected.

Dowek's and Krivine's positions probably differ rather deeply in their philosophical and epistemological underpinnings; Dowek being, if I understand him well, a rationalist and, to some extent a realist (as opposed to nominalist), believing that the rules of computation, and hence of logic, are per se natural objects that our mind uncovers and that Nature is bound to follow, while Krivine would rather be an empiricist, believing that experience has driven evolution to imprint in our minds whichever logical process that are well adapted to Nature. But yet, both admit that Nature follows exactly mathematical forms.

3.1.4 Jean-Marc Lévy-Leblond

Jean-Marc Lévy-Leblond is a professor of both physics and philosophy in the university of Nice. He was a renown physicist before contributing to epistemology. In [18], he offers an overview of the various epistemological stands to address the effectiveness of mathematics in the natural sciences. (I will shortly borrow citations from that article.) Then he challenges that phrase, explaining that referring to *natural sciences* leads to an overshoot in trying an explanation. Mathematics are in fact much more efficient in *physical* sciences than in any other natural science, such as geology, biology, and even chemistry. So, if one believes in the effectiveness of mathematics in explaining Nature, he must explain both what characterizes physics as compared to the other natural sciences, and why mathematics are more efficient in physics than elsewhere.

His answer is, in short, that we call "physics" this part of natural sciences that have this deep relationship with mathematics, up to the point where mathematical reasoning becomes part of that natural science: "*Its relationship with mathematics is what specifies physics*"[7]". (An do not forget that Lévy-Leblond is a renown physicist.)

Illustrating this viewpoint, he quotes Descartes saluting the introduction of mathematics into mechanics by Galileo Galilei, the founding of physics as a separate science, the ushering into physics of electricity by Coulomb, or of crystallography in the nineteenth century thanks to group theory.

In the process, he denies the status of *language* for mathematics, arguing that if it is a language, it must be universal, and one must again explain why it fits physics better than other natural sciences. As far as I am concerned, I do not understand well that last point (I am not a philosopher, Lévy-Leblond teaches both physics *and* philosophy). I shall come back to that in the next paragraph.

3.1.5 Density

I am a control theoretician, as I will develop in the next subsection, and this influences my understanding of mathematical models, and hence of the relationship between Nature and mathematics.

For me, with less sophisticated needs than true philosophers, it suffices to think of mathematics, not as the language of Nature, but as the language (forgive me Jean-Marc) in which I *describe* Nature. In that respect, I accept Heisenberg's

[7]Son rapport aux mathématiques constitue la détermination spécifique de la physique

stance (quoted by [18]) that mathematical formulas do not represent Nature, but the knowledge we have about it.

Not believing in the proof by Gandy and Dowek, I can see no reason why Nature should exactly follow our mathematical formulations. In that respect, I see my "critique" of real numbers to represent Nature as a strong argument against any pretense that Nature and mathematics coincide in "mathematical laws". But the possibility of mathematics are so rich as to be, in some sense (and with reference to the mathematical sense), *dense* within the set of possible phenomena, allowing us to approach (almost) arbitrarily closely natural phenomena.

Why physics better than other natural sciences ? Here I believe that Lévy-Leblond gives me the answer. I call physics ...

As for the appearance of mathematical objects in contexts completely different from their native context, here I am sort of a realist as Dowek: I believe that while we *invent* mathematical objects which did not exist before (what about natural numbers ?) we *discover* their properties based upon logic, —i.e. mathematics which are a systematization of logical reasoning—, logic which, still in my view, exists as a real (from the latin *res*) object. Hence it may produce these "harmonious" unexpected meetings.

3.2 Man-made objects

Man made objects obey the "laws of physics", of course. They should be, in some sense, the simplest "compound" objects. (Excluding economy, whose status I shall discuss separately). Some, as the Internet, are almost as complex as "natural" objects, or, for that matter, economy, because as economy, their behavior is the cumulative effect of human behaviors. So I rather refer here to the steam engine, ... or an aircraft.

3.2.1 Model and model: polysemy

We, control theoreticians and computer scientists, build mathematical or numerical "models" of these objects. An ancillary remark has to do with the use of natural language in speech and communication. When we explain our use of models to the layman, we should pay attention to the double meaning of the word. Quoting the Webster's New World College Dictionary :

1. A small copy or imitation of an existing object, as a ship, building, etc., made to scale.

2. A person or thing considered as a standard of excellence to be imitated.

Too often, the person we are speaking to understands the second meaning while we mean the first one. This is a cause of misunderstandings, and we shall see that in economics, this is a major debate. I, personally, always add the clarification "as in *small-scale model*" ("modèle réduit" in French, I am not sure of the prevalence of this phrase in English.)

But the ambiguity goes further. Our computer scientist colleagues in CAD say that they have constructed a "mathematical model" of an object when they have found a (mathematical) way of specifying and generating a bunch of numbers that describe the *shape* of the object.

The mathematical models I refer to define "natural quantities", often abstractions themselves, (I come back to that hereafter), quantify them, and try to write equations that relate these quantities and oftentimes, for "dynamical models", their rate of change with time. These usually imply a notion of causes and effects, as the force exerted on a spring is the cause whose effect is the change of length of the spring. Te force "explains" that change of length. Yet, nobody has ever *seen* a force. We see effects that we attribute to an abstraction that we call "force". This abstraction has proved so effective in describing the world that we believe that it is a thing that exists. A form of philosophical realism. But it took time, and Galileo Galilei, to distinguish it from the various concepts of "force" in medieval natural philosophy, a testimony to the fact that it is not so natural.

3.2.2 Model and model: knowledge and control

We may have different aims in building mathematical models, and it may be useful to clarify at least both ends of the spectrum. One end is what I call a "knowledge" model. The aim, here, is to have a computer tool that mimics as well as possible as many aspects of the real thing as possible. Meteorology, of more generally Computational Fluid Dynamics (CFD), is a typical example. The word used is "simulation". Our CFD colleagues are now able to do direct simulation of complex turbulent flows, a feat hardly conceivable 10 years ago. (This is due in part only to the increased power of computers. The increased sophistication of, e.g., multigrid methods, plays an equally important role.)

At the other end of the spectrum lies control models. Here, the aim is to have a model good enough for efficiently controlling a device. I develop below my "paradox of linear control theory".

Of course, many models lie somewhere between these two extremes. In particular, most "knowledge" models have in view later use to control, improve, cure …

The paradox of linear control theory A standard and very successful practice of control theory is to use either analytical or experimental means to develop a linearized model of the system to be controlled in the neighborhood of the nominal trajectory to be followed, and to use this model to effectively control the system. Now ...

- the linear model is good if the system stays close to its nominal trajectory,
- the system stays close to its nominal trajectory if the control is efficient,
- the control is efficient if the model it is based upon is good.

A circular argument, and clearly "unstable". And yet, this has successfully sent men on the moon and brought them back. (The Apollo program made a wide use of the Automatic Synthesis Package developed around Kalman's LQG control theory, which was an extremely young theory at that time: Kalman's articles appeared in 1960 and 1961, and President Kennedy's pledge "to land man on the moon and return him safely to the Earth before the decade is over" was made before a joint session of Congress on May 21, 1961.)

3.2.3 Describing and understanding

Einstein's quote : *the most incomprehensible thing about the world is that it is comprehensible* raises an issue present in essentially all modeling efforts: that of the relationship of *describing* with *understanding*. When pure mathematical symmetries, say, guide the discovery of new particles in physics, I understand that the physicist has the profound feeling of having *understood* something, although Wigner's conclusion about the *wonderful gift which we neither understand nor deserve* somewhat contradicts that statement.

In more mundane applications, I see the sophistication of some models, or their very nature —case of large agent based models— as an obstacle to understanding the system. To me, "understanding" necessitates few mechanisms at work, to see the doings of mathematics with these mechanisms. This requires a very careful choice of which variables and which mechanisms we include in our model. And the good choice depends on what we are looking for. In that respect, I see a model as a mathematical mimic of some device aimed at *answering a question* about the object being modelled. In control models, the question typically is "how should I determine my control to get the desired output ?".

It is where my self deriding quip to my students, that "all the intelligence is in the modeling; the rest is only mathematics" is most to the point. A "good" model should contain just enough variables and equations and algorithms to answer the question addressed to it, but not more.

4 Biological sciences

Here, my limited scientific experience confines me within narrow bounds: I am a newcomer to mathematical biology (roughly, 10 years, six of which as an easy-going retired emeritus scientist), having worked mainly in the related fields of behavioral ecology and evolutionary biology. I will therefore limit myself to some simple remarks concerning these two domains.

4.1 Behavioral ecology

4.1.1 The foundations of mathematical behavioral ecology

How can-we "compute" the expected behavior of living organisms ? The answer lies with the evolutionary pressure exerted by natural selection. We can think of mutations as having performed a random search of the domain of various parameters of the behavior —we say *traits*—, and survival of the fittest as having selected the optimum values of these traits in terms of reproductive efficiency. Therefore, given a mathematical model of the effect of these traits on reproductive efficiency, an optimization computation, —a mathematical operation—, should be capable of determining their actual value.

This program is full of seemingly insurmountable difficulties. Let us underline some. They are not mathematical but logical. First, one must find out the truly relevant traits, among the amazing diversity of traits that Nature offers. How you make this choice clearly depends on what you want to achieve with your model and computations. This raises in a renewed way the issue of knowledge versus control models. Then, one must find a way to quantify these traits, or, if impossible, restrict their possible "values" to a small enough set that it can be searched by enumeration. Finally, one needs a mathematical model of their effect on reproductive efficiency. Given the extraordinary complexity of biological and ecological processes, such a model is necessarily a massive simplification, almost a caricature, of the natural process.

The amazing fact, thus, is that it records some successes, such as achieving testable "predictions". My first experience with that domain, —while learning it under the stewardship of my colleague Éric Wajnberg, of the French National Institute for Agronomical Research (INRA), and with the collaboration of the then intern master student Frédéric Hamelin, now with Agrocampus Ouest in Rennes—, was to predict a behavioral trait of oophagous parasitoids which was later observed by Wajnberg and Guy Boivin, of Agriculture et Agroalimentaire Canada in Québec. Let me point out that the experimental verification is by far the most difficult part of the chain.

4.1.2 Knowledge model and control model

The difficulty of the modeling of these processes, and the massive simplification it implies, is the more problematic that, traditionally, the aim pursued is more like a "knowledge model", according to my above classification, than a "control model". The term "prediction", which I have used, even between quotation marks, bears witness of that fact. The problematic usefulness of such endeavors, when one only believes direct observations, will deserve a section below.

The classic distinction in ecology, proposed by Holling [13], largely accepted since (see [19]) but also disputed lately ([6]), is that of *strategic* models, simple and assumed to be more general, versus *tactical* models, more complex and aimed at describing a particular system. This distinction is reminiscent of the opposition explanatory versus descriptive that we hinted at earlier. A deep question in epistemology, that I do not wish to embrace here. I shall therefore stick with my simple typology, and examine whether it is relevant here.

One old control model in that domain is in pharmacokinetics, where one wants to optimize in some sense the use of drugs. A modern continuation of this track is in enzymatic networks, and, who knows, genetic networks. And new applications are quickly developing, that we should not underestimate, where a control model is sought. Among them, those I know are biological pest control and artificial ecosystems.

A model aiming at improving biological pest control is clearly a control model. Social and … ecological pressure is quickly mounting to develop such agronomical tactics. Some successes, such as the control of *Ostrinia Nubilalis* —the "European corn borer" causing much damage in corn farming all over the world— by the small wasp *trichogramma brassicæ*, suggest that they can, indeed, be efficient. But many failures, either complete or from an economical viewpoint, point to the necessity of improving them, i.e. improve our control.

Artificial ecosystems have been used for a long time. To quote one: sewer stations. The need to improve their efficiency is clear. But many new ones are emerging in so called bio-technological processes, from the growing of vanillin producing mushrooms to industrial chemostats aimed at breeding oil producing micro algae.

There therefore is a growing role for true control models, whose epistemological status is clear, in spite of the massive simplifications entailed. And this is one of the justifications for researches in behavioral ecology modeling, even in seemingly knowledge models: to improve our ability to develop efficient control models.

But I claim that there is more to it.

4.1.3 Knowledge models: what for ?

My discussions with true biologists, including my late sister France Reversat, helped me make my mind about the epistemological status of such researches, and therefore their usefulness. (Epistemology is not a luxury here, it is of the essence.)

Science starts as an induction process with the discovery of regularities in Nature. But the variety of phenomena you might want to investigate is so large that any guide to discovering new regularities is welcome. The more so that checking whether suspected regularities exist is very difficult. As a witness of both the desperate search for regularities by Man, and the difficulty of conclusively observe, or disprove, them, I like to quote the surprising survival of so many superstitions, such as the belief in astrology. (I am being told that astrological web sites attract the largest number of hits, ahead of pornographic sites.) I do not forget that Tycho Brahe's observations, that helped Kepler find his laws and thus Newton discover the "law of gravity", aimed at improving astrological predictions. Measurements are always useful.

Mathematical models are one powerful way to "predict" regularities that nobody would have looked for otherwise. My initial work in mathematical biology had to do with the influence of the age of the parasitoids on the time they spend on a given egg patch… a question raised by an attempt to understand the underpinnings of the famous model by Charnov and his "Mean Value Theorem". But who would go through the pains of verifying that correlation if it were not hinted at by a mathematical model ?

In constructing such models, one must perform a choice of traits, or rather *define* traits. As a matter of fact, as there is no measurement without theory, there is no model without abstracting and naming some properties which, most often, have no obvious "existence" in Nature. *Fitness*, or reproductive efficiency, are such abstract concepts that do have a relationship with the natural phenomena at hand, but that one does not invent before he has evolution theory in his tools to observe Nature. And this remark extends to many other traits we are considering. Once these traits are defined, one has to write mechanisms by which they are related to each other, again choosing among the many mechanisms one can imagine, but are deemed secondary and unimportant. The very fact that so many *choices* are involved is the source of discoveries.

As a matter of fact, if the predictions of the model turn out to be correct, —and direct observations on the real thing are the only referee here—, then we have made an epistemological progress in discovering properties that are efficient in describing the process at hand. If they are *not* verified, then we know that our choice of traits, or of mechanisms linking them, is not correct. Something else dominates the effects we want to investigate.

A more interesting conclusion arises if the predictions of our model are in general satisfied, but when somebody discovers that a given species do not behave that way. Then, we know that there is something particular about that species as compared to comparable ones. Then, a biological fact has been revealed by the mathematical model, or rather by the whole modeling effort.

4.2 Evolutionary biology

4.2.1 Sexual selection as an example

Evolutionary biology attempts to explain the current state of natural species by the evolutionary path followed. One of the main foci of current research in that field is the emergence of collaborative behavior. My own work has been in the so-called sexual selection, the role of sexual success in shaping selection, and more specifically accounting for the "handicap paradox"; a paradox discovered by Darwin himself, later tentatively explained by Fisher, then by Zahavi. It has to do with the fact that in many species, male secondary sexual characters that clearly attract the females are a viability handicap for the males who bear them.

For lack of a mathematical model, Fisher's tentative explanation is not completely clear. The original paper [7] (in the Eugenics Review, 1915) is not easily reachable, and several interpretations of the "Fisher runaway" have been proposed in the modern literature, including a mathematical model by Lande [17]. Zahavi's "handicap principle", as published in the Journal of Theoretical biology in 1975 [25], also a literary model, has also been diversely re-interpreted, although not to the same extent as Fisher's. A mathematical model of it was published in two articles of that same journal in 1990 [11, 10], and yet, we still see recent papers who give a different account of the same original article.

This underlines one benefit of mathematical models: the mathematical language, if properly used —this is usually the case, although not always— is just more precise than natural languages.

The point I want to stress, concerning that ongoing controversy, is that, while Fisher's runaway is typically evolutionary, describing a divergence in the evolution process, Zahavi's handicap principle is basically static: it gives an interpretation of the stability of the current state of affairs in the handicap paradox, it does not say how evolution might have reached this point. And given the sophistication of the conceptual tools needed —arguments of signaling theory, a difficult branch of game theory— the answer is far from obvious. Yet, since there remains a controversy, to argue in favor of an explanation, one has to show that it could, conceptually, have been reached by evolution starting from less evolved states.

To reach that aim, I first cast Grafen's model into the framework of games of signaling, then proposed a variant of it, both more explicit and more satisfactory in its numerical conclusions, and then showed that it was the asymptotic limit of an adaptive dynamics model. I do not claim that the description I reach *is* what happened. I do claim that I show that it is plausible, making the handicap principle of Zahavi and Grafen a more credible answer to the problem of the handicap paradox.

4.2.2 Plausibility model: evolutionary versus teleological explanations

The difficulty is that we are in a domain where direct experiments are not possible, and therefore no "falsification" by experiments either. We are seemingly outside of science as Popper would define it. Yet, I claim that there is a scientific necessity to develop these models.

I have stressed that the basis for mathematical behavioral ecology as I know it is that because of the pressure of natural selection, living species behave *as if* they were maximizing their reproductive efficiency. The obvious risk, when using this paradigm, is to drift into teleological explanations of natural traits, explanations in terms of an "aim" or "purpose". Yet, in the very *definition* of science, one should include that it repudiates any teleological argument. Too often do we here the questions —and worse: answers to them—: "what is the use of this organ ?", "what is the purpose of this trait ?"'. Science knows no purpose nor intention in Nature. The correct question is "what evolutionary advantage did this trait give to that species ?"

This is why exhibiting an evolutionary path under selective pressure that might have driven the living organisms investigated where they are is so important in any "explanation". A typical instance is the role of evolutionary dynamics as it relates to Evolutionarily Stable Strategies (ESS), and the convergence of, say, the replicator dynamics' trajectories toward an ESS.

One does not claim that things actually happened that way. What we do claim is that we exhibit at least one plausible evolutionary path leading to the present situation. Should I describe this not as a "knowledge model", but as a "plausibility model" ? In the absence of any possibility to make a direct verification in vivo, this probably is the natural aim of a mathematical model in evolutionary biology. (May be it has a practical utility in fighting obscurantism in the form of creationism ?)

This provides an answer to the question raised by the impossibility to *falsify* such a model via experiments. Indeed, if the aim is to be plausible, plausibility is judged by the human mind. Therefore we have a form of *verification*: the acceptation by the scientific community.

Control models ? Fisher was a strong advocate of (human) eugenics. This form of "control model" of evolution has, luckily, died. But a closely related topic is that of selection of species, particularly animal species in livestock farming, leading to the so called "breeder's equation", that relates the speed of selection to genetic diversity. Clearly a control model. I do not know whether it is really used.

5 Social sciences

My limited knowledge of social sciences is through my contacts with economy via game theory. It owes much to my former PhD student Marc Deschamps, now with the laboratory Beta in Nancy. But it appears to me that the epistemological status of modeling is far less clear than in other sciences.

Yet, one must first emphasize the great achievements of mathematical economic theory, and particularly of microeconomics, with the help of game theory. As do physics, microeconomics has its "standard model": the "pure and perfect competition" which is well understood and serves as a comparison standard for many more complex, or more realistic, models, and its central concept of Nash equilibrium. And macroeconomics have theirs in the general equilibrium theory.

5.1 The epistemological status of economics

The founders of economic science, say the physiocratic school of the mid eighteenth century with François Quesnay, followed by Anne Robert Jacques Turgot and Adam Smith, were convinced that the economic society is a product of Nature which obeys fundamental laws that they strived to decipher. The same is probably true of the classical school, From Cournot to Walras and Pareto, although we shall come back to a controversy between them. And the aim of John von Neumann and Oskar Morgenstern in their historical book "Theory of Games and Economic Behavior" [23] was explicitly to set Economics on a track similar to that followed centuries before by physics. While they were fully aware of the difficulty of the endeavor, and of the time it would take, they seem to have entertained little doubt that the end product would be an economic theory as efficiently grounded on basic principles as physics is today. I quote from their first chapter:

> *To continue the simile with physics* [. . . here comes a description of the type of developments currently occurring in physics]. *Considering the fact that economics is much more difficult, much less understood, and undoubtedly in a much earlier stage than physics, one should not expect more than a development of the above type in economics either. [. . .] It is not that there exists any fundamental reason why mathematics should not be used in economics. The arguments often heard that [. . .] can all be dismissed as utterly mistaken.*

We may notice the parallel with Auguste Comte's classification of sciences, making mathematics the simplest one before physics —hence my "the rest is only mathematics"—, and sociology, which encompasses economics, the most difficult one.

Of course, all these scholars wanted to understand economics in order to better master it and help define the most efficient public policies to promote wealth and, most often, justice. In that respect, their epistemological stand was very much the same as in physics. Few recognized economy as a man made construction.

The very question of whether Economy is entirely a man made object or, in a large extent, dependent on fundamental "laws of Nature", is a matter of debate. This debate blurs the epistemological categories I tried to distinguish. The distinction between *understanding*, *constructing*, and *controlling* is not at all clear. It is probably why many great economists[8] were very much delved into epistemology.

This confuse picture spills over to the polysemy of the word "model" that I mentioned earlier. Witness of that is a controversy between Walras and Pareto, summarized by the following quotation by Walrass:

> Mr Pareto believes that the aim of science is to draw closer and closer to reality by successive approximations. As for me, I believe that the ultimate aim of science is to draw reality closer to some ideal; and it is why I am trying to define this ideal.[9]

Therefore, such great classical economists do not agree on the nature of economic models: descriptive, as in physics, for Paréto (and von Neumann and Morgenstern), normative for Walras, i.e. outside the categories I considered up to now, since I only used the word "model" with its other meaning: as in "small scale model".

This last controversy itself is compounded by the performative character of economical models, as everybody recognizes. This is a domain of science where the models themselves, once accepted by enough people, or governments for macroeconomics, influence reality. It means that even with a positivist stand à la Pareto, one cannot ignore a normative effect à la Walras.

And of course, a major difficulty is that, because of this character, and because economics are so dependent on the local and momentary contexts, repeating an experiment is all but impossible. Thus, here again, poperrian falsification is bordering the impossible. So economics cumulates two major difficulties: the extreme

[8]if not all. Raymond Aron stressed that Paréto would deride philosophers. "*...Pareto se refuse à entrer dans l'univers intellectuel de la philosophie. Qu'il s'agisse de Platon, de Kant ou de Hegel, "il fait l'idiot", il cite telle ou telle phrase obscure pour le commun des mortels de la Phénoménologie ou de la Logique (il n'a que l'embarras du choix).*"

[9]M. Pareto croit que le but de la science est de se rapprocher de plus en plus de la réalité par des approximations successives. Et moi, je crois que le but final de la science est de rapprocher la réalité d'un certain idéal; et c'est pourquoi je formule cet idéal.

complexity of the process at hand, as in biological sciences, and the quasi impossibility to check correctness through experiments. The economics community has developed, for microeconomics, a path around these difficulties, salvaging, to my opinion, the scientific status of their science.

5.2 From teleological to phenomenological

5.2.1 Microeconomics

A major difference with natural sciences is that the models of microeconomics, modeling the interplay between a small number of agents, are necessarily teleological: economic agents do behave with an aim in sight. The basic tool here is mathematical: game theory, which is is part of decision theory.

The aim of microeconomics is thus to uncover the true determinants of economic agents' choices of decision, and then investigate their logical, mathematical, effects. It is now a widely acknowledged fact that the axioms of rationality of von Neumann and Morgenstern are not a faithful description of this behavior. The famous "department store paradox" of Reinhart Selten emphasizes this point, by showing a game where the Von Neuman "rational" behavior is not credible as the behavior of real agents.[10]

Thus, the major challenge of microeconomics is to find basic principles describing the behavior of economic agents, beyond von Neumann and Morgenstern's "rationality". Indeed, many efforts have been devoted to that aim in this century, from behavioral economy to neural economy. None has been so successful as to win general approval. Tools such as meanfield games might be useful to take fashion into account, and comparable collective effects.

If one is willing to accept classical rationality as a behavioral rule, I said that the economics community has built an original epistemological status of its science, largely tacitly. Construct small models of economic situations, with few variables and few mechanisms linking them —anyhow, computing a Nash equilibrium is usually so difficult as being only possible for small models. Then multiply these models, until many of them, although different, yield qualitatively the same answers. Then, you may be confident that in spite of their being so simple, these models teach you credible qualitative conclusions. Rubinstein [20] has compared this with the role of fables in moral sciences. They picture simple causality effects of some mechanisms that are probably embedded into the real world, and their multiplication lets you build a wisdom which is correct.

This program, however, suffers a further disgrace which is a *mathematical* failure: the nonuniqueness of the Nash equilibrium. Many game problems have a

[10] I like better Hervé Moulin's story of the band of pirates having found a coffer of golden coins.

continuous infinity of Nash equilibria. Which, if any, represents the decision of "rational" players ? Up to now, the search for Nash refinements has failed : none yields existence and uniqueness in sufficiently large classes of games, and none is sufficiently realistic, to be accepted by everybody. *The search for Nash refinements is not building one's home on the sand, but a palace in the clouds !* (Ken Binmore)

5.2.2 Macroeconomics

The aim of macroeconomics, as I understand it, is, or should be, to go from the local teleological model to a global phenomenological one: how do the aims of many players aggregate in the behavior of economy ? Physics show us many examples of such successful change of scale from local, microscopic, descriptions to global behaviors : statistical mechanics, spin glasses, etc. Such concepts as the *market*, the *(inverse) demand function*, and theories such as that of general equilibrium are precisely parts of that endeavor.

Within game theory, this attempt translates into games with an infinite number of players. Typical instances are the market equilibrium, the Wardrop equilibrium, and their modern dynamical counterpart with meanfield games, that all capture the *collective* effects of *individual* selfish behaviors. (A noteworthy fact is that the Wardrop equilibrium is (essentially) the same thing as the biologists' ESS.[11]) Several authors have investigated "local" learning processes that generate a collective dynamics converging toward an equilibrium such as the the Wardrop equilibrium. See, e.g., William Sandholm [21]

If this program, from microeconomic teleological models, to macroeconomic aggregates, was completely successful, then the epistemological status of both ends would be reasonably clear. And we would have models reliable enough to control Economy and, essentially, avoid crises, at least those without major external cause.

We are far from that situation. And we will stay so until fundamental questions such as that of the nature of Economy, a natural object or a man made made object, and a correct model of economic agents, are not solved and taken into account in the scientific approach. I, personally, *believe* that economy is a man made object. But the difference between a *belief* and a *knowledge* is that a knowledge is a belief shared by the scientific community. (And I am not an economist.)

Notice finally that the failure of microeconomics and game theory to give a faithful description of "local" behavior, has been the reason why macroeconomic models were developed directly from phenomenological observations and general

[11] except that the Wardrop equilibrium is written, as a Nash equilibrium, with non-strict inequalities, while Maynard Smith and Price wrote the inequalities defining the ESS as strict inequalities, so that a second order condition pops up in addition to the Wardrop equilibrium.

principles about rational behavior. What I find interesting is that these very different models, supported by whole "schools" of economists more or less fighting each other, have all their domain of relevance, have been useful in some contexts, and then have been abandoned generally without explaining why they had become "wrong". [12]

5.3 Management science

Management science is to economy what engineering is to physics, —and, to some extent only, medical science to biology: a technological development devised to help control relatively simple (not in medical science) objects. As a consequence, it scores many successes in spite of the relatively shaky state of the underlying science.

Two facts contribute to make management science successful. On the one hand, the status of models it uses is clear: control models. This makes the modeling activity much less confuse: abstractions are invented for their operational value, and simplifications are valid in a restricted domain where one wants to operate. As an example, I like to quote the invention of "Good will", denoted G, and the simple equation used by my friends to model it:

$$\dot{G} = -aG + bu$$

where u is the advertisement effort. Who would claim that this is a realistic model of a "natural" quantity ? But no one objects to its use as long as it is efficient. This G is an abstraction whose epistemological status is not very different from that of an electric current's intensity. Even if it seems less "physical".

On the other hand, firms behave much more as "rational" agents than individuals, say consumers. This is even pointed out by David Graeber in his book [9]. He claims that stock companies have been an excuse for a ruthless behavior of managers, seeking maximum profit with no regard for any human feeling. "If it were for me, I would not … but it is my job, my responsibility for my shareholders". Of course, on ethical grounds, this is a problem; but in terms of modeling, a gift !

6 Conclusion

My personal feeling at this point is that there remains epistemological controversies as well in physics as in economy, or for that matter in biology. But the less easily a science is made mathematical, the more important epistemological considerations

[12] I also observe that Keynesian models were considered "outdated" by the late 90's. But when the economic crisis of 2008 came along, all western governments implemented keynesian policies.

become. And following Jean-Marc Lévy-Leblond, we may conclude that we call "physics" those sciences where an epistemological haze is the less damaging.

While I do not claim to have brought much new thoughts to epistemology, I do hope that I have convinced some of you that epistemology is not a luxury, or a hobby for elder scientists, but an important part of our every day job of applied mathematicians.

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