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Editors

The Courant–Friedrichs–Lewy (CFL) Condition

80 Years After Its Discovery

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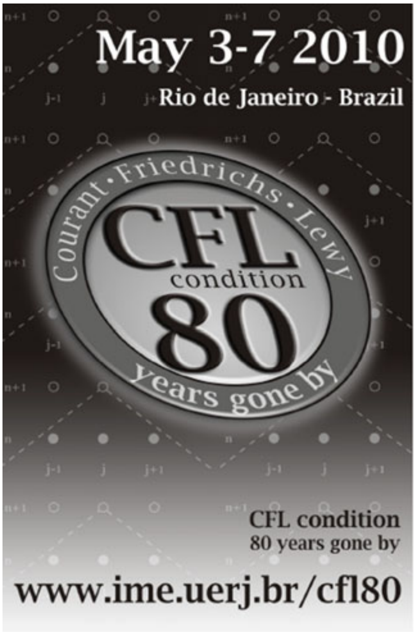

Poster of *CFL condition—80 years gone by* meeting

Having failed to obtain any **CFL** picture, we have switched to a **CLL** one: Richard Courant, Hans Lewy, and Jean Leray at the Arden Conference Center around 1950. It may be thought as a first order approximation… (From Peter Lax files)

Richard Courant, Kurt Friedrichs, and Hans Lewy

Foreword

Despite being largely disseminated nowadays, “impact factors” do not need to be quoted to assure the depth and importance—in so many areas of science and technology—of the article submitted in 1927 by Richard Courant, Kurt Friedrichs, and Hans Lewy to *Mathematische Annalen* and published therein the following year.[1]

The authors’ keen view of finite difference methods applied to approximate solutions of partial differential equations has provided the right hand hold to deal with numerical algorithms within this environment. The idea is to first look for how the studied schemes mimic the main properties of the operators they are intended to approximate—signal propagation speed being the first point to look at—and then to estimate the distance between the continuous model, which lives within the real line, and the discrete one, immersed in real life and, consequently, being tied to treating only numbers we are bound to operate with. They realized how this question is related to the answer to a puzzle posed for a long time to numerical analysts of PDEs: mesh refinements do not always improve the approximations, they can even make approximations worse. They discovered that everything amounts to a desperate need for stability—small changes in input data must never throw output far away from its true habitat. The constraint the discrete schemes must satisfy to guarantee stability became known as the CFL-condition, honoring the three authors.

In March 1967, to celebrate the article’s 40th anniversary, *IBM Journal*[2] published a special issue, Vol. **11**(2), which featured the paper’s translation into English,[3] as well as three articles that report the outcome of numerical methods for PDEs after that historical publication. Each of them has roughly chosen as its focus

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[1] Über die partiellen Differenzengleichungen der mathematischen Physik, Vol. **100**, pp. 32–74. See Appendix B for a reprint of the paper original version.

[2] Now *IBM Journal of Research and Development.*

[3] See Appendix C for a reprint of this translation.

one of the three types of partial differential equations: elliptic,[4] hyperbolic,[5] and parabolic.[6]

Around 80 years had gone by since the CFL paper was printed when a meeting was held in Rio de Janeiro, in May 2010, to once again celebrate its outcome. Hosted by Rio de Janeiro State University (UERJ), it was organized with the participation of Rio's main institutions that deal with computational sciences (see the report in Appendix D). The meeting atmosphere was quite cozy, and it is a pleasure for the organizing committee to thank around 100 attendees that have made *CFL-condition, 80 years gone by* a scientifically rewarding encounter. Our thanks go also to the publishers of these proceedings. We further acknowledge the contributions by Jacqueline Telles (secretarial chores), Jhoab P. de Negreiros (LaTeX expertise), Sandra Moura (website design), and Tania Rodrigues (graphic designer). Additional information about the meeting—in particular some texts from the refereed contributed papers, as well as many pictures taken at the meeting—may be retrieved from its site at

http://www.ime.uerj.br/cfl80

Before summarizing the scientific papers contained in this volume, let us mention one of its special features: the musical piece, recorded especially for these proceedings, authored by Hans Lewy—who was also a composer before turning to mathematics—and played by Leonore (Lori) Lax, one of Richard Courant's daughters. She has also written a text with some recollection of Lewy's visits to Courant's home (see Appendix A, which contains some photos). The recording may be accessed through SpringerExtras at extras.springer.com/978-0-8176-8393-1.

The book opens with an article by Peter D. Lax, the meeting's main speaker. He dwells a little on the CFL paper and after some quick, sharp remarks—quite his writing style—shows some results to corroborate his main assertion: "The theory of difference schemes is much more sophisticated than the theory of differential equations."

Reuben Hersh's contribution deals with a "mysterious" question: Numerical analysts spend their lifetime to reach convergence results that become valid only when the parameters involved turn out to be extremely large. But in everyday life, why are they quite happy getting results drawn from real-life computers, therefore with not so overwhelming numbers?

The article by Rolf Jeltsch and Harish Kumar discusses a model for the different phenomena that occur at current interruption in a circuit breaker. They propose the equations of resistive magneto-hydrodynamics (RMHD), and it turns out that this is the first time a model based on RMHD has been used to simulate plasma arc in three dimensions.

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[4] Seymour V. Parter, *Elliptic equations*, pp. 244–247.

[5] Peter D. Lax, *Hyberbolic Difference Equations: A Review of the Courant–Friedrichs–Lewy Paper in the Light of Recent Developments*, pp. 235–238.

[6] Olof B. Widlund, *On Difference Methods for Parabolic Equations and Alternating Direction Implicit Methods for Elliptic Equations*, pp. 239–243.

Sander Rhebergen and Bernardo Cockburn apply a novel space-time extension of the hybridizable discontinuous Galerkin (HDG) finite element method to the advection–diffusion equation. The resulting method combines the advantages of a space-time DG method with sensible improvement in efficiency and accuracy for the HDG methods.

The paper by J. Teixeira, Cal Neto, and Carlos Tomei indicates how a global Lyapunov–Schmidt decomposition, introduced within a *bona fide* theoretical context, gives rise to a quite effective numerical algorithm (which makes use of the finite element method) for the nonlinear equation $-\Delta u - f(u) = g$ with Dirichlet conditions on a bounded n-dimensional domain.

A filtering technique for the one-dimensional wave equation is proposed and tested in the article by Aurora Marica and Enrique Zuazua. Their concern is the failure of observability from the boundary for the quadratic classical finite element approximation.

Margarete O. Domingues, Sônia M. Gomes, Olivier Roussel, and Kai Schneider have authored an article which studies a wavelet-based multiresolution method. It deals with space-time grid adaptive techniques for a finite volume being the time discretization explicit. Their purpose, both to reduce the memory requirement and to speed-up computing, is reached through an efficient self-adaptive grid refinement and a controlled time-stepping.

Philippe G. LeFloch obtains a parabolic-type system for late-time asymptotics of solutions to nonlinear hyperbolic systems of balance laws with stiff relaxation. For these stiff problems, an approximation based on a finite volume is then introduced which preserves the late-time asymptotic regime. This method carries an important feature; namely, it requires the CFL condition associated with the hyperbolic system under study, rather than the more restrictive parabolic-type stability condition.

Kai Schneider, Dmitry Kolomenskiy, and Erwan Deriaz pose the question: "Is the CFL condition sufficient?". Their numerical results, using a spectral discretization in space, illustrate that the CFL condition is not sufficient for stability and that the time step is limited by non-integer powers (larger than one) of the spatial grid size.

The collection is closed with a paper by Uri Ascher and Kees van den Doel: "Fast Chaotic Artificial Time Integration". The authors claim that some faster gradient-descent methods generate chaotic dynamical systems for the normalized residual vectors. The fastest practical methods of this family in general appear to be the chaotic, two-step ones, but, despite their erratic behavior, these methods may also be used as smoothers, or regularization operators. Besides, their results also highlight the need for a better theory for these methods.

The meeting has also held a special session honoring Peter Lax.

Rio de Janeiro, Brazil

Carlos A. de Moura
Carlos S. Kubrusly

Contents

Stability of Difference Schemes

Peter D. Lax

Abstract The most powerful and most general method for constructing approximate solutions of hyperbolic partial differential equations with prescribed initial values is to discretize the space and time variables and solve the resulting finite system of equations. How to discretize is a subtle matter, as we shall demonstrate.

In this report, some of the proofs are only sketched; details can be found in Chap. 8 of my monograph "Hyperbolic Partial Differential Equations", 2006, AMS.

Keywords Hyperbolic PDE's · Finite difference schemes · Convergence · Stability

One of the seminal observations of the Courant–Friedrichs–Lewy paper of 1928 was that in order for solutions of a difference equation to converge to the solution of the partial differential equation the difference scheme must use all the information contained in the initial data that influence the solution. To satisfy this condition, the ratio of the spatial discretization to the time discretization must be at least as large as the largest velocity with which signals propagate in solutions of the partial differential equation. This inequality is called the CFL condition.

I well remember from the early days of computing, when physicists and engineers first undertook to solve numerically initial value problems, their utter astonishment to see the numerical solution blow up because they have unwittingly violated the CFL condition.

The CFL condition is only a necessary condition for the convergence of difference schemes. Here is an example: discretize the scalar equation

$$u_t + u_x = 0$$

by replacing the time derivative with a forward difference, and the space derivative with a symmetric difference. This scheme diverges, no matter how small the time discretization is compared to the space discretization.

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In this talk, I will report on sufficient conditions for the convergence of various difference schemes. I shall discuss a class of equations studied by K.O. Friedrichs, first order symmetric hyperbolic systems of the form

$$u_t = Au_x + Bu_y, \tag{1}$$

where $u(x, y, t)$ is a vector-valued function, and A and B are real symmetric matrices that may be smooth functions of x and y. The theory of these equations is fairly straightforward: let $u(x, y, t)$ be a solution in the whole (x, y)-space that dies down fast as x and y tend to infinity. Take the scalar product of the equation with u and integrate it over all x and y; we get

$$\int (u \cdot u_t)\, dx\, dy = \int (u \cdot Au_x + u \cdot Bu_y)\, dx\, dy. \tag{2}$$

If A and B are constant matrices, the integrand on the right is

$$\frac{d}{dx}(u, Au)/2 + \frac{d}{dy}(u, Bu)/2, \tag{3}$$

a sum of perfect x and y derivatives; therefore the integral is zero. The integrand on the left side is the t derivative $(1/2)d(u, u)/dt$ and can be regarded as the t derivative of

$$E(t) = \int \frac{1}{2}(u, u)\, dx\, dy.$$

Since the derivative of $E(t)$ is zero, it follows that $E(t)$ is independent of time, the law of conservation of energy.

When A and B are functions of x and y, the integrand on the right equals the sum (3) minus

$$(u, A_x u)/2 + (u, B_y u)/2,$$

a quantity bounded by $cE(t)$, c some constant. Therefore, we conclude that $dE(t)/dt \le cE(t)$, which implies that

$$E(t) \le E(0)e^{ct}. \tag{4}$$

Such an inequality for solutions of hyperbolic equation is called an energy inequality.

We turn now to two-level explicit difference schemes. We look first at the case of one space dimension, that is,

$$u_t = Au_x. \tag{5}$$

The difference schemes are of the form

$$u^{n+1} = C_h u^n, \tag{6}$$

where u^n denotes the values of u at time t and u^{n+1} the values of u at the next time level $t+h$, n an integer multiple of h. We take the space discretization to be h, the same as the time discretization. The operator C_h is a finite sum of the form

$$C_h = \sum C_j T^j, \tag{7}$$

where T is translation by h: $(Tu)(x) = u(x+h)$, and T^j is the j-th power of T. Since we have chosen the space discretization and time discretization to be equal, the CFL condition requires that signals for solutions of the differential equation (5) propagate with speed not greater than 1. Since the signal speeds for solutions of (5) are the eigenvalues of A, this requires that the eigenvalues of A lie between -1 and 1.

In order for the difference scheme (6)–(7) to be consistent with the differential equation (5), the coefficients C_j have to satisfy the conditions

$$\sum C_j = I, \qquad \sum jC_j = A. \tag{8}$$

These conditions are satisfied by

$$C_{-1} = (I-A)/2, \qquad C_1 = (I+A)/2, \quad \text{all other } C_j = 0. \tag{8$'$}$$

Since the CFL condition requires that the eigenvalues of A be not greater than 1, it follows that the coefficients C_j in (8) are non-negative. We appeal now to:

Lemma 1 *Suppose that the coefficients C_j in the operator C_h in* (7) *are non-negative real symmetric matrices that depend smoothly on $x = jh$. Then the L^2-norm of the operator C_h is less than $1 + \mathbf{O}(h)$.*

We sketch the proof: abbreviate u_n as u, $u_{(n+1)}$ as v; then the difference scheme reads

$$v = C_h u = \sum C_j T_j u.$$

Take the scalar product with v:

$$(v, v) = \sum \big(v, C_j T^j u\big).$$

Since the C_j are non-negative, we can apply the Cauchy–Schwarz inequality:

$$\big(v, C_j T^j u\big) \le \sqrt{(v, C_j v)} \sqrt{\big(T^j u, C_j T^j u\big)}.$$

Using the arithmetic–geometric mean inequality on the product on the right, we get

$$(v, v) \le (1/2) \sum (v, C_j v) + (1/2) \sum \big(T^j u, C_j T^j u\big).$$

According to the first consistency condition in (8), $\sum C_j = I$; therefore, the first sum on the right is $(v, v)/2$. If the coefficients C_j are independent of x, the second sum is (u,u), so we get the desired inequality. If the C_j depend on x, it gives rise to a term $\mathbf{O}(h)(u,u)$.

The proof of this lemma is somewhat analogous, though a little trickier than the energy estimate for solutions of the partial differential equation presented above.

It follows from the lemma that the n-th power of C_h is bounded by $(1+\mathbf{O}(h))^n$; these quantities are uniformly bounded if nh is less than some specified number T. This proves the stability of the difference scheme. According to the standard theory of difference approximations, this guarantees the convergence of the difference scheme.

Condition (8) is first order consistency, and it only guarantees that the solution of the difference scheme differs from the solution of the differential equation by $\mathbf{O}(h)$. For higher order accuracy, we need higher order consistency:

$$\sum C_j = I, \qquad \sum jC_j = A, \quad \text{and} \quad \sum j^2 C_j = A^2. \tag{9}$$

These conditions are satisfied by the Lax–Wendroff scheme:

$$C_{-1} = \big(A^2 - A\big)/2, \qquad C_0 = I - A^2, \qquad C_1 = \big(A^2 + A\big)/2, \tag{9'}$$

plus terms of order h. According to the CFL condition, the eigenvalues of A are less than 1; it follows that the eigenvalues of A^2 are less than those of A. If A has a positive eigenvalue, the corresponding eigenvalue of C_{-1} is negative; if A has a negative eigenvalue, the corresponding eigenvalue of C_{-1} is negative.

More generally it is not hard to show that, except for trivial cases, it is not possible to satisfy condition (9) by non-negative matrices C_j. Therefore, for second and higher order schemes, we need a proof of stability different from the proof for schemes with positive coefficients.

Our starting point is a stability criterion due to von Neumann. We associate with the difference scheme (6)–(7) its symbol defined as the matrix-valued trigonometric polynomial

$$C(s) = \sum C_j e^{ijs}. \tag{10}$$

When the coefficients depend on $x = hj$, so does the symbol $C(s,x)$. The stability criterion of von Neumann says that if the scheme (6)–(7) is stable, the absolute value of the eigenvalues of its symbol $C(s,x)$ are not greater than 1.

Sketch of proof In the constant coefficient case, $C(s)$ is independent of x. If we take the Fourier transform of the difference equation (6), we get

$$U^{n+1}(s) = C(s)U^n(s), \tag{11}$$

where $U^n(s) = \sum e^{ijs}$ and similarly for U^{n+1}. We can iterate (11) and obtain the operator linking the initial value U^0 to U^n:

$$U^n(s) = C(s)^n U_0(s).$$

Clearly, if for some value of s, $C(s)$ has an eigenvalue greater than 1, the L^2-norm of $U^n(s)$ blows up exponentially. This shows that if the von Neumann condition is violated, the difference scheme is unstable. This argument can be modified for schemes with variable coefficients.

Von Neumann raised the question whether the stability criterion, possibly somewhat sharpened, is sufficient for stability. For schemes in one space variable, the answer is yes; here is a sketch of the argument.

The consistency conditions for schemes (8) and (9) in one space variable relate combinations of the coefficients C_j to powers of the matrix A in the differential equation (5). It follows that the natural choice for the coefficients C_j will be polynomials in A and its powers. Since A is a symmetric matrix, the symbol $C(s)$ defined in (10) is for each s and x a normal matrix. A normal matrix whose eigenvalues are not greater than 1 in absolute value has norm ≤ 1; therefore, $C^*(s)C(s) \leq I$. From this one can deduce the stability of the scheme.

We turn now to difference approximations of hyperbolic equations (1) in two space variables. We write the difference equation as

$$u_k^{n+1} = \sum c_j u_{k+j}^n, \tag{12}$$

where $k = (k_1, k_2)$ and $j = (j_1, j_2)$ are multi-indices, and u_k^n is the value of the approximate solution at time $t = nh$ and at the lattice point $(k_1, k_2)h$. The analogs of the first order consistency conditions (8) are

$$\sum C_j' = I, \qquad \sum j_1 C_j = A, \qquad \sum j_2 C_j = B. \tag{13}$$

These relations can be satisfied analogously to (9) by setting all C_j equal to zero except at the four neighboring lattice points. We denote these as C_W, C_E, C_S, C_N. We choose, in analogy with (8′),

$$\begin{aligned} C_W &= 1/2(I/2 - A), \qquad & C_E &= 1/2(I/2 + A), \\ C_S &= 1/2(I/2 - B), \qquad & C_N &= 1/2(I/2 + B). \end{aligned} \tag{13′}$$

If the norms of A and B are less than $1/2$, all four matrices above are positive. The argument given in the one dimensional case can be used to prove that this scheme with positive coefficient is stable.

The domain of dependence of the point $(0, 0, 1)$ for the difference scheme (13) is the rectangle $|x + y| \leq 1$ and $|x - u| \leq 1$. The domain of dependence of the point $(0, 0, 1)$ for the differential equation is contained in that rectangle if the norm of $A + B$ and $A - B$ does not exceed 1. This shows that the sufficient conditions $|A| < 1/2$, $|B| < 1/2$ are more stringent than the CFL condition. □

One can modify the scheme (13) so that the new scheme is positive under the CFL conditions

$$|A + B| \leq 1, \qquad |A - B| \leq 1.$$

We turn now to second order schemes for Eq. (1). A straightforward way of constructing such a scheme is to use the Taylor approximation:

$$u(t+h) = u(t) + u_t(t)h + u_{tt}(t)h^2 + \mathbf{O}\big(h^3\big),$$

and then use the differential equation (1) to express u_t and u_{tt} in terms of space derivatives. If we then approximate the first and second space derivatives with symmetric difference quotients, we get the nine point Lax–Wendroff difference scheme. The symbol of this scheme,

$$C(s,r) = \sum C_j\, e^{i(j_1 s + j_2 r)},$$

is

$$\begin{aligned} C(s,r) = I &- A^2(1-\cos s) - B^2(1-\cos r) \\ &- 1/2(AB+BA)\sin s \sin r + i(A\sin s + B\sin r). \end{aligned} \tag{14}$$

The von Neumann stability condition is that the eigenvalues of $C(s,r)$ should be less than 1. But since the matrices A and B do not commute, except in trivial cases, $C(s,r)$ is no longer a normal matrix, so we cannot conclude that the norm of $C(s,r)$ is less than 1; in fact, it is not less than 1.

Instead of looking at the norm of $C(s,r)$, Burt Wendroff and the author have shown that if the matrices A and B have norm less than $1/8$, the numerical range of $C(s,r)$ is contained in the unit circle. We recall that the numerical range of a matrix M is the set of all complex numbers of the form $w \cdot Mw$, w any unit vector with complex entries. Since the spectrum belongs to the numerical range, it follows that the scheme with symbol $C(s,r)$ satisfies the von Neumann criterion. The condition on the numerical range is more than the von Neumann condition and has as a consequence the

Stability Theorem *Suppose a difference scheme of form* (12) *has the following properties*:

(i) *The coefficients $C_j(x)$ are twice differentiable functions of x.*
(ii) *For every s and x, the numerical range of the symbol $C(s,x)$ lies in the unit circle.*

Then the numerical range of the difference operator C_h is less than $1 + \mathbf{O}(h)$.

Such an operator is stable; for, according to the Halmos–Pearcy theorem, the numerical range of C_j^n is less than $(1+\mathbf{O}(h))^n$, and so the norm of C_j^n is less than twice that.

The key tool used in the proof is this result of Louis Nirenberg and the author:

Let P_h be a difference operator of the form

$$P_h = \sum P_j(x) T^j,$$

where the coefficients $P_j(x)$ are twice differentiable functions of x. Suppose that the symbol of P_h,

$$P(s,x) = \sum P_j(x)\, e^{i(j_1 s + j_2 r)},$$

is Hermitian and non-negative for all s and x:

$$P(s,x) \geq 0.$$

Then the Hermitian part of P_h, $\operatorname{Re} P_h = (P_h + P_h^*)/2$, satisfies

$$\operatorname{Re} P_h \geq -\text{constant}\, h.$$

The proof of this estimate is tricky, as indicated by the requirement that the coefficients of the scheme be not once but twice differentiable functions.

The results described above hold for any number of space variables, not just two.

I hope that the discussion presented proves the claim that the theory of difference schemes is much more sophisticated than the theory of differential equations.

Mathematical Intuition: Poincaré, Pólya, Dewey

Reuben Hersh

Abstract Practical calculation of the limit of a sequence often violates the definition of convergence to a limit as taught in calculus. Together with examples from Euler, Pólya and Poincaré, this fact shows that in mathematics, as in science and in everyday life, we are often obligated to use knowledge that is derived, not rigorously or deductively, but simply by making the best use of available information–plausible reasoning. The "philosophy of mathematical practice" fits into the general framework of "warranted assertibility", the pragmatist view of the logic of inquiry developed by John Dewey.

Keywords Intuition · Induction · Pragmatism · Approximation · Convergence · Limits · Knowledge

1 Introduction

In Rio de Janeiro in May 2010, I spoke at a meeting of numerical analysts honoring the 80th anniversary of the famous paper by Courant, Friedrichs, and Lewy. In order to give a philosophical talk appropriate for hard-core computer-oriented mathematicians, I focused on a certain striking paradox that is situated right at the heart of analysis, both pure and applied. (That paradox was presented, with considerable mathematical elaboration, in Phil Davis's excellent article, *The Paradox of the Irrelevant Beginning*, cf. [5].) In order to make this paradox cut as sharply as possible, I performed a little dialogue, with help from Carlos Motta. With the help of Jody Azzouni, I used that dialogue again, to introduce this talk in Rome.

To set the stage, recall the notion of a convergent sequence, which is at the heart of both pure analysis and applied mathematics. In every calculus course, the student learns that whether a sequence converges to a limit and what that limit is depend only on the "end" of the sequence—that is, the part that is "very far out"—in the tail, so to speak, or in the infinite part. Yet, in a specific instance when the limit is

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actually needed, usually all that is considered is the beginning of the sequence—the first few terms—the finite part, so to speak. (Even if the calculation is carried out to a hundred or a thousand iterations, this is still only the first few, compared to the remaining, neglected, infinite tail.)

In this little drama of mine, the hero is a sincere, well-meaning student, who has not yet learned to accept life as it really is. A second character is the *Successful Mathematician*—the Ideal Mathematician's son-in-law. His mathematics is ecumenical: a little pure, a little applied, and a little in-between. He has grants from federal agencies, a corporation here and there, and a private foundation or two. His conversation with the *Stubborn Student* is somewhat reminiscent of a famous conversation between his Dad, the Ideal Mathematician, and a philosophy grad student, who long ago asked, "What is a mathematical proof, really?"

2 A Dialogue

The Successful Mathematician (*SM*) is accosted by the Stubborn Student (*SS*) from his Applied Analysis course.

SS: Sir, do you mind if I ask a stupid question?

SM: Of course not. There is no such thing as a stupid question.

SS: Right. I remember, you said that. So here's my question. What is the real definition of "convergence"? Like, convergence of an infinite sequence, for instance?

SM: Well, I'm sure you already know the answer. The sequence converges to a limit, L, if it gets within a distance epsilon of L, and stays there, for any positive epsilon, no matter how small.

SS: Sure, that's in the book, I know that. But then, what do people mean when they say, keep iterating till the iteration converges? How does that work?

SM: Well, it's obvious, isn't it? If after a hundred terms your sequence stays at 3, correct to four decimal places, then the limit is 3.

SS: Right. But how long is it supposed to stay there? For a hundred terms, for two hundred, for a hundred million terms?

SM: Of course, you wouldn't go on for a hundred million. That really would be stupid. Why would you waste time and money like that?

SS: Yes, I see what you mean. But what then? A hundred and ten? Two hundred? A thousand?

SM: It all depends on how much you care. And how much it is costing, and how much time it is taking.

SS: All right, that's what I would do. But when does it converge?

SM: I told you. It converges if it gets within epsilon …

SS: Never mind about that. I am supposed to go on computing "until it converges", so how am I supposed to recognize that "it has converged"?

SM: When it gets within four decimal points of some particular number and stays there.

SS: Stays there how long? Till when?

SM: Whatever is reasonable. Use your judgment! It's just plain common sense, for Pete's sake!

SS: But what if it keeps bouncing around within four decimal points and never gets any closer? You said any epsilon, no matter how small, not just 0.0001. Or if I keep on long enough, it might finally get bigger than 3, even bigger than 4, way, way out, past the thousandth term.

SM: Maybe this, maybe that. We haven't got time for all these *maybes*. Somebody else is waiting to get on that machine. And your bill from the computing center is getting pretty big.

SS: (mournfully) I guess you're not going to tell me the answer.

SM: You just don't get it, do you? Why don't you go bother that Reuben Hersh over there, he looks like he has nothing better to do.

SS: Excuse me, Professor Hersh. My name is–

RH: That's OK. I overheard your conversation with Professor Successful over there. Have a seat.

SS: Thank you. So, you already know what my question is.

RH: Yes, I do.

SS: So, what is the answer?

RH: He told you the truth. The real definition of convergence is exactly what he said, with the epsilon in it, the epsilon that is arbitrarily small but positive.

SS: So then, what does it mean, "go on until the sequence converges, then stop"?

RH: It's meaningless. It's not a precise mathematical statement. As a precise mathematical statement, it's meaningless.

SS: So, if it's meaningless, what does it mean?

RH: He told you what it means. Quit when you can see, when you can be pretty sure, what the limit must be. That's what it means.

SS: But that has nothing to do with convergence!

RH: Right.

SS: Convergence only depends on the last part, the end, the infinite part of the sequence. It has nothing to do with the front part. You can change the first hundred million terms of the sequence, and that won't affect whether it converges, or what the limit is.

RH: Right! Right! Right! You really are an **A** student.

SS: I know… So it all just doesn't make any sense. You teach us some fancy definition of convergence, but when you want to compute a number, you just forget about it and say it converges when common sense, or whatever you call it, says something must be the answer. Even though it might not be the answer at all!

RH: Excellent. I am impressed.

SS: Stop patronizing me. I'm not a child.

RH: Right. I will stop patronizing you because you are not a child.

SS: You're still doing it.

RH: It's a habit. I can't help it.

SS: Time to break a bad habit.

RH: OK. But seriously, you are absolutely right. I agree with every word you say.

SS: Yes, and you also agree with every word Professor Successful says.

RH: He was telling the truth, but he couldn't make you understand.

SS: All right. You make me understand.

RH: It's like theory and practice. Or the ideal and the actual. Or Heaven and Earth.

SS: How is that?

RH: The definition of convergence lives in a theoretical world. An ideal world. Where things can happen as long as we can clearly imagine them. As long as we can understand and agree on them. Like really being positive and arbitrarily small. No number we can write down is positive and arbitrarily small. It has to have some definite size if it is actually a number. But we can imagine it getting smaller and smaller and smaller while staying positive, and we can even express that idea in a formal sentence, so we accept it and work with it. It seems to convey what we want to mean by converging to a limit. But it's only an ideal, something we can imagine, not something we can ever really do.

SS: So you're saying mathematics is all a big fairy tale, a fiction, it doesn't actually exist?

RH: NO! I never said fairy tale or fiction. I said imaginary. Maybe I should have said consensual. Something we can all agree on and work with because we all understand it the same way.

SS: That's cool. We all. All of you. Does that include me?

RH: Sure. Stay in school a few more years. Learn some more. You'll get into the club. You've got what it takes.

SS: I'm not so sure. I have trouble believing two opposite things at once.

RH: Then how do you get along in daily life? How do you even get out of bed in the morning?

SS: What are you talking about?

RH: How do you know someone hasn't left a bear trap by your bedside that will chop off your foot as soon as you step down?

SS: That's ridiculous.

RH: It is. But how do you know it is?

SS: Never mind how I know. I just know it's ridiculous. And so do you.

RH: Exactly. We know stuff, but we don't always know how we know it. Still, we do know it.

SS: So you're saying, we know that what looks like a limit really is a limit, even though we can't prove it, or explain it, still we know it.

RH: We know it the same way you know nobody has left a bear trap by your bedside. You just know it.

SS: Right.

RH: But it's still possible that you're wrong. It is possible that something ridiculous actually happens. Not likely, not worth worrying about. But not impossible.

SS: Then math is really just like everything else. What a bummer! I like math because it's not like everything else. In math, we know for sure. We prove things. One and one is two. Pi is irrational. A circle is round, not square. For sure.

RH: Then why are you upset? Everything is just fine, isn't it?

SS: Why don't you admit it? If you don't have a proof, you just don't know if L is the limit or not.

RH: That's a fair question. So what is the answer?

SS: Because you really want to think you know L is the limit, even if it's not true.

RH: Not that it's not true, just that it might not be true.

Thanks for your kind attention. What is supposed to be the meaning of this performance? What am I getting at? In this talk, I am NOT attempting to make a contribution to the "problem of induction". Therefore, I may be allowed to omit a review of its 2,500-year literature. I am reporting and discussing what people really do, in practical convergence calculations, and in the process of mathematical discovery. I am going into a discussion of practical knowledge in mathematics, as a kind of real knowledge, even though it is not demonstrative or deductive knowledge. I try to explain why people must do what they do, in order to accomplish what they are trying to accomplish. I will conclude by arguing that the right broader context for the philosophy of mathematical practice is actually the philosophy of pragmatism, as expounded by John Dewey.

But first of all, just this remarkable fact. What we do when we want actual numbers may be totally unjustified, according to our theory and our definition. And even more remarkable-nobody seems to notice, or to worry about it!

Why is that? Well, the definition of convergence taught in calculus classes, as developed by those great men Augustin Cauchy and Karl Weierstrass, seems to actually convey what we want to mean by limit and convergence. It is a great success. Just look at the glorious edifice of mathematical analysis! On the other hand, in specific cases, it often is beyond our powers to give a rigorous error estimate, even when we have an approximation scheme that seems perfectly sound. As in the major problems of three-dimensional continuum mechanics with realistic nonlinearities, such as oceanography, weather prediction, stability of large complex structures like big bridges and airplanes.... And even when we could possibly give a rigorous error estimate, it would often require great expenditure of time and labor. Surely it's OK to just use the result of a calculation when it makes itself evident and there's no particular reason to expect any hidden difficulty.

In brief, we are virtually compelled by the practicalities to accept the number that computation seems to give us, even though, by the standards of rigorous logic, there is still an admitted possibility that we may be mistaken. This computational result is a kind of mathematical knowledge! It is practical knowledge, knowledge sound enough to be the basis of practical decisions about things like designing bridges and airplanes-matters of life and death.

In short, I am proclaiming that in mathematics, apart from and distinct from the so-called deductive or demonstrative knowledge, there is also ordinary, fallible knowledge, of the same sort as our daily knowledge of our physical environment and our own bodies. “Anything new that we learn about the world involves plausible reasoning, which is the only kind of reasoning for which we care in everyday affairs”, cf. [19]. This sentence of Pólya’s makes an implicit separation between mathematics and everyday affairs. But nowadays, in many different ways, for many different kinds of people, mathematics blends into every-day affairs. In these situations, the dominance of plausible over demonstrative reasoning applies even to mathematics itself, as in the daily labors of numerical analysts, applied mathematicians, design engineers.... Controlling a rocket trip to the moon is not an exercise in mathematical rigor. It relies on a lack of malice on the part of that Being referred to by Albert Einstein as *der lieber Gott*.

(For fear of misunderstanding, I explain—this is not a confession of belief in a Supreme Being. It’s just Einstein’s poetic or metaphoric way of saying, Nature is not an opponent consciously trying to trick us.)

But it’s not only that we have no choice in the matter. It’s also that, to tell the truth, it seems perfectly reasonable! Believing what the computation tells us is just what people have been doing all along, and (nearly always) it does seem to be OK. What’s wrong with that?

This kind of reasoning is sometimes called “plausible”, and sometimes called “intuitive”. I will say a little more about those two words pretty soon. But I want to draw your attention very clearly to two glaring facts about this kind of plausible or intuitive reasoning. First of all, it is pretty much the kind of reasoning that we are accustomed to in ordinary empirical science, and in technology, and in fact in everyday thinking, dealing with any kind of practical or realistic problem of human life. Secondly, it makes no claim to be demonstrative, or deductive, or conclusive, as is often said to be the essential characteristic of mathematical thinking. We are face to face with mathematical knowledge that is not different in kind from ordinary everyday commonplace human knowledge. Fallible! But knowledge, nonetheless!

Never mind the pretend doubt of philosophical skepticism. We are adults, not infants. Human adults know a lot! How to find their way from bed to breakfast-and people’s names and faces—and so forth and so on. This is real knowledge. It is not infallible, not eternal, not heavenly, not Platonic, it is just what daily life depends on, that’s all. That’s what I mean by ordinary, practical, everyday knowledge. Based not mainly on rigorous demonstration or deduction, but mainly on experience properly interpreted. And here we see mathematical knowledge that is of the same ordinary, everyday kind, based not on infallible deduction, but on fallible, plausible, intuitive thinking.

Then what justifies it in a logical sense? That is, what fundamental presupposition about the world, about reality, lies behind our willingness to commit this logical offense, of believing what isn’t proved?

I have already quoted the famous saying of Albert Einstein that supplies the key to unlocking this paradox. My friend Peter Lax supplied the original German, I only remembered the English translation.

> *Raffinniert ist der lieber Gott, aber boshaft ist Er nicht.*
>
> The Lord God is subtle, but He is not malicious.

Of course, Einstein was speaking as a physicist struggling to unravel the secrets of Nature. The laws of Nature are not always obvious or simple, they are often subtle. But we can believe, we must believe, that Nature is not set up to trick us, by a malicious opponent. God, or Nature, must be playing fair. How do we know that? We really don't know it, as a matter of certainty! But we must believe it, if we seek to understand Nature with any hope of success. And since we do have some success in that search, our belief that Nature is subtle but not malicious is justified.

This problem of inferring generalizations from specific instances is known in logic as "the problem of induction". My purpose is to point out that such generalizations in fact are made, and must be made, not only in daily life and in empirical science, but also in mathematics.

That is, in the practice of mathematics also we must believe that we are not dealing with a malicious opponent who is seeking to trick us. We experiment, we calculate, we draw diagrams. And eventually, using caution and the experience of the ages, we see the light. Gauss famously said, "I have my theorems. Now I have to find my proofs".

But is it not naïve, for people who have lived through the hideous twentieth century, to still hope that God is not malicious? Consider, for example, people who for thousands of years have lived safely on some atoll in the South Pacific. Today an unforeseen tsunami drowns them all. Might they not curse God in their last breath?

Here is an extensive quote from Leonhard Euler, by way of George Pólya. Euler is speaking of a certain beautiful and surprising regularity in the sum of the divisors of the integers.

> This law, which I shall explain in a moment, is in my opinion, so much more remarkable as it is of such a nature that we can be assured of its truth without giving it a perfect demonstration. Nevertheless, I shall present such evidence for it as might be regarded as almost equivalent to a rigorous demonstration… Anybody can satisfy himself of its truth by as many examples as he may wish to develop. And since I must admit that I am not in a position to give it a rigorous demonstration, I will justify it by a sufficiently large number of examples… I think these examples are sufficient to discourage anyone from imagining that it is by mere chance that my rule is in agreement with the truth… The examples that I have just developed will undoubtedly dispel any qualms which we might have had about the truth of my formula… It seems impossible that the law which has been discovered to hold for 20 terms, for example, would not be observed in the terms that follow.
> (Taken from [19].)