Mathematics to the Rescue

(Retiring Presidential Address)

Cathleen Synge Morawetz

I should like to dedicate this lecture to my teacher, Kurt Otto Friedrichs. While many people helped me on my way professionally, Friedrichs played the central role, first as my thesis advisor and then by feeding me early research problems. He was my friend and colleague for almost forty years. As is true among friends, he was sometimes exasperated with me (I was too disorderly), and sometimes he exasperated me (he was just too orderly). However, I think we appreciated each other's virtues, and I learned a great deal from him.

Sometimes we had quite different points of view. In his old age he agreed to be interviewed, at Jack Schwartz's request, for archival purposes. I was the interviewer. At one point I asked him about the role of modern computation and, I think, the computational significance of his early work on difference schemes. He just would not bite. He had worked on those things to prove existence theorems, and he had not been interested in the business of modeling on computers.

Computing PDEs

I began to think about the origins of finding solutions to physical problems by computational methods. This is not really a case of mathematics coming to the rescue, as I promised in the title, but that will come later. If I could rewrite history, I would have it begin with people solving for the first time some time-dependent hyperbolic partial differential equation by a simple difference scheme and getting nonsense because the scheme is unstable. The answers blow up. The white knights—Courant, Friedrichs, and Lewy (CFL)—would then step in and show them that mathematics could cure the problem. That was, however, not the way. But first, what is the CFL condition? It says [1] that for stable schemes the step size in time is limited by the step size in space. The simplest example is the difference scheme for a solution of the wave equation

$$u_{tt} - u_{xx} = 0$$

on a grid that looks like the one in Figure 1. Second derivatives are replaced by second-order differences with $\Delta t = \Delta x$.

The sum of the values at 1 and 4 is the sum of the values at 2 and 3. It is not hard to show that this is marginally stable in the following sense. If the scheme is stretched so that $\Delta t/\Delta x > 1$ and is adjusted (it will

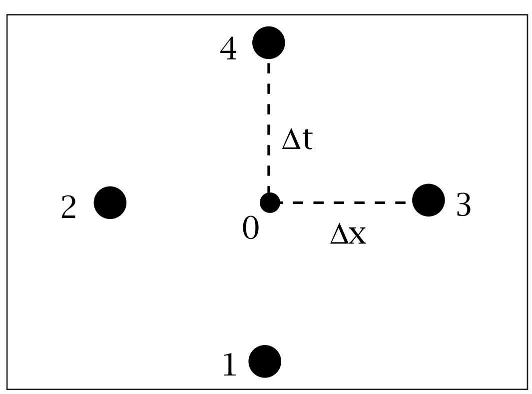
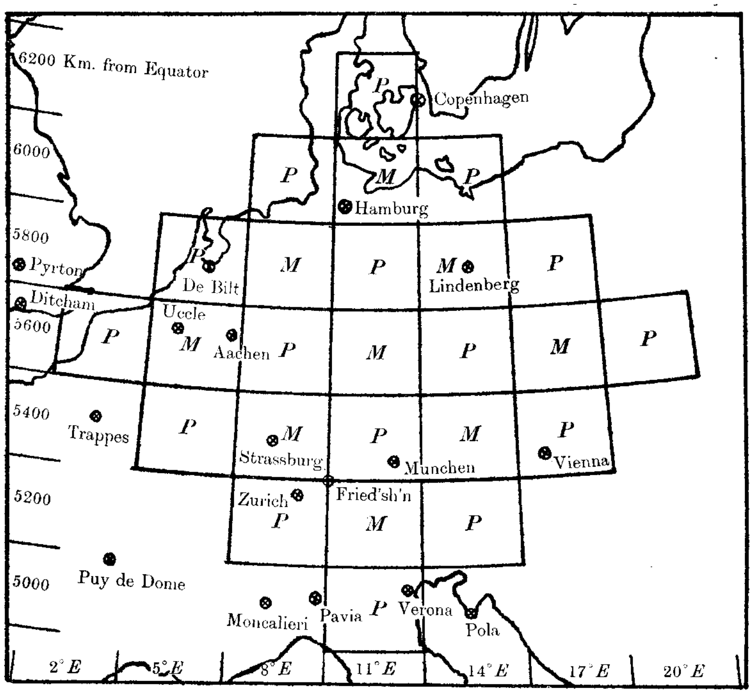


Figure 1. Difference scheme.

Cathleen Synge Morawetz is professor emeritus at the Courant Institute, New York University. Her e-mail address is morawetz@cims.nyu.edu*. This article is based on her AMS Retiring Presidential Address given at the Joint Meetings in San Diego in January 1997.*



From *Weather Prediction by Numerical Process* by Lewis F. Richardson, p. 184.

MAP OF POINTS FOR PRESSURE (*P*) AND MOMENTUM (*M*)

NOTE: These points are placed at the centres of the chequers, and to the centres also the latitude and longitude refer. Each chequer measures 3° from west to east and 200 km from south to north.

Figure 2. Weather grid.

involve 0), then it is unstable. If the scheme is stretched so that $\Delta t/\Delta x < 1$, then it is stable.

The fundamental idea is that the inverse of the grid slope $\frac{\Delta x}{\Delta t}$ is the speed of propagation of signals on the grid and it has to be bigger than the speed of propagation, 1, of the original equation in order to carry forward all the information.

As well as I can gather, the idea of using difference approximations to solve ordinary differential equations approximately goes back to Newton. In essence, the equation

$$\frac{dy}{dx} = f(x, y), \quad 0 \le x \le a, \quad y(0) = y_0,$$

gets replaced by

$$\frac{Y_{n+1} - Y_n}{h} = f(nh, Y_n), \qquad n = 0, 1, \ldots,$$

or some better difference scheme. One solves by stepping from one value of n to the next.

It seems to have been Laplace who brought differences to elliptic partial differential equations, especially through probability and random walks. George Boole, better known to us for Boolean algebra and the logic of computers, did a great deal with the calculus of finite differences and wrote a treatise on it in 1860, including a method for solving the wave equation by differences. He does have some questions about singular solutions, but not about stability.

I did not get back to reading Laplace, because I got hung up reading about Boole's hard life until he got to Cork in Ireland at the age of thirty-four. I also discovered that my father, J. L. Synge, in a foreword to McHale's biography [2] wrote of Boole: "An Englishman, a stranger to Cork (which he found strange), a kindly man, a methodical man, a quarrelsome man when rubbed the wrong way, a victim of his own excessive sense of duty."

Except for the wave equation, the first application of difference schemes in time-dependent problems seems by all accounts to be in the work of the meteorologist Lewis Fry Richardson [3, 4], who set out to predict weather during World War I. He had a complicated time-dependent problem with real data at hand. Not to my surprise (I identify the middle name Fry with good English chocolate and Quakers), Richardson was a pacifist in the British ambulance corps. On a rest leave, while lying in a pile of hay, he wrote out a numerical scheme for his problem. The equations are in two-dimensional space, and then there is time. A shallow water theory has gotten rid of the height variable. The unknown u is a vector, ∇ is a vector of partial derivatives in space, and the equation is

$$\frac{\partial u}{\partial t} = F(u, \nabla u, \nabla^2 u, x).$$

One marches forward in time with a forward difference step. So in the approximation form,

$$\frac{U_{n+1} - U_n}{\Delta t} = F(U, DU, D^2U, x),$$

where D is some space difference.

It is in fact virtually impossible to see, except in a preliminary example, exactly what the difference scheme is. Richardson set up and computed by hand the first time step using a latitude-longitude checkerboard grid based on European weather stations. This was too big to handle along with the edges, and he reduced his grid to that of Figure 2.

He took a time step of six hours and some historical data for the initial values. He had to extrapolate his data to get a Cauchy problem. His answer was, alas, dead wrong. It did not check. First, his model was bad; his grid size impossible for the phenomena he wanted to include. But even if all that had worked, his numerical scheme was probably unstable. Richardson's scheme, in fact, looks not unlike a leapfrog method, such as we showed for the wave equation. He could compute by hand only one time step, and that had to be six hours. His mesh size was about 220 kilometers. If we assumed a speed of propagation for the weather of about 40 kilometers per hour, a very rough guess, then the CFL condition with two space variables would say that the time interval had to be less than four hours. So the scheme would be unstable. Richardson was off, but he was in the right ballpark. What Richardson really did was to break the ice, recognize the problems except for stability, and set up a kind of spreadsheet, a Lotus 1-2-3 for human beings to use in solving large difference problems by hand. But most of all, he drew to the attention of meteorologists the problem of trying to compute and predict the weather by a difference scheme.

CFL were not aware of Richardson's work when they wrote their paper in 1928. During World War II, the CFL condition was rediscovered by von Neumann while computing for the atom bomb. Von Neumann also became deeply interested in weather forecasting by computer after the war and looked at length into Richardson's efforts, noting that Richardson had not studied stability. It was von Neumann who really laid the mathematical basis for weather forecasting by large computers.

Let me add that Richardson was highly literate. He described a huge parallel human computer in graphic terms, applied a well-known poem of Swift's to turbulence, AND acknowledged his wife's help in doing the arithmetic in much of his work.

Scattering

My next subject has to do with scattering. When I asked Friedrichs for a thesis topic in 1948, he suggested perhaps nine. I forget the nine topics, but I think one of them was on scattering, probably in the quantum mechanical framework that Friedrichs was very interested in at the time. I did not like any of the topics, and he was very disappointed at my lack of enthusiasm. So I picked one anyway. Perhaps fortunately, shortly thereafter I got pregnant. So we switched to a problem in fluid dynamics, a subject I was more familiar with, as I had helped to edit the Courant-Friedrichs book on compressible fluid dynamics and shock waves.

However, the problem in scattering I want to speak of reached me from a very different source. My husband had a well-known crystallographer, Isidore Fankuchen, as a colleague. I frequently met him socially. He never failed to launch a great tirade at applied mathematicians for failing to solve the central problem of crystallography: determining the atomic structure of a molecule from scattered data. One needed to do this for molecules, small and large.

In the kind of forward scattering problem Friedrichs looked at, there is a wave coming in from infinitely large negative time, say, a solution of the wave equation. It hits an obstruction, which alters it, and then at infinitely positive time the solution again satisfies the wave equation.

If the obstruction satisfies reasonable conditions, there is a nice map from the incoming wave to the outgoing wave, and the change is called the scattered wave. The study of scattering is the study of the map. The inverse problem is to give the incoming and outgoing wave (or some of it) and find the obstruction: e.g., an obstacle, a change of speed of propagation, a bunch of atoms if it is the Schrödinger instead of the wave equation.

Another well-known inverse scattering problem is for variable frequency, say, ω; consider a solution u of

$$\omega^2 u + u_{xx} + q(x)u = 0, \qquad q(x) \text{ of compact support,}$$

that looks like $u = e^{i\omega x} + R(\omega)e^{-i\omega x}$ at, say, $x = -\infty$ and like $A(\omega)e^{i\omega x}$ at $x = +\infty$.

What is needed to find $q(x)$? Sometimes $R(\omega)$ suffices. But Fankuchen's universal all-important crystallographic problem, although steady, was different.

A crystal is a periodic structure of molecules: for simplicity, one molecule to a box. The incoming wave provided by an X-ray source is a plane wave in three dimensions. It has a fixed frequency, but its direction can be changed.

Let $r = (x, y, z)$. The quantity we want to find is the density of the atoms:

$$\rho(r),$$

which by the periodicity has a Fourier series:

$$\rho(r) = V^{-1} \sum_{\text{all } h} F_h \exp(-2\pi i h \cdot r),$$

where h is a vector with integer components and

$$F_h = |F_h| \exp(i\phi_h).$$

The Fourier coefficient of the scattered wave at high-frequency approximation is proportional to F_h.

Measurements can be made only of $|F_h|$. True, these are made for all h, but this does not look like sufficient data to determine F_h and hence to find $\rho(r)$. What can one do? Assume analyticity of ρ? Are there better asymptotic formulas? Higher order terms? None of these has helped.

The answer was produced by an applied mathematician and a physicist, Jerome Karle and Herbert Hauptman [5], working closely with a chemist, Isabella Karle. The presence of a chemist was very important, because extra information is needed from chemistry about possible shapes, e.g., symmetry or limitations on angles among the atoms.

The first step is to replace the molecule by a collection of points for the atoms with unknown positions.

Thus the density is a finite sum of Dirac delta functions:

$$\rho(r) = \sum_{i=1}^{N} \rho_i \delta(r - r_i).$$

Here ρ_i, the number of atoms at r_i times the weight, is known. Inverting the Fourier series and dropping the constant factors, we find

$$\int \rho \exp(2\pi i h \cdot r)\, dx$$

$$\sim F_h = |F_h| \exp(i\phi_h) = \sum_{j=1}^{N} \rho_j \exp(2\pi i h \cdot r_j).$$

The problem is to find r_j (the position of the atoms) from the absolute value of F_h. Nothing can be found experimentally about the phase ϕ_h. So we have the phases and $3N$ unknown position vectors and "as many as we like" of $|F_h|$. The problem has now a high degree of overdeterminacy.

To the mathematician, this overdeterminacy implies some sort of instability, which shows up in the fact that this inversion is very difficult except for N very small.

One needs some more clues, and Hauptman and Karle supplied many. They won their Nobel Prize [6] in 1985 by executing a computer program that made use of the clues and made it possible to find many molecular structures. Of course, the situation is more complex the bigger the molecule is, but the underlying principles remain the same. Today a bit of chemistry plus a dedicated computer does the job pretty routinely for a large class of molecules. The full story is complicated, and there are still many open problems in this area, especially for very large molecules ([7] and the references there). Not much scattering theory is involved, but what are involved in the clues are the properties of the Fourier coefficients of a positive function first established by Toeplitz in 1911.

Let us first examine the one-dimensional case for the basics and evaluate a certain quadratic form for arbitrary complex vectors ξ_h, ξ_j:

$$\sum_{h,j} \xi_h F_{h-j} \overline{\xi}_j = \int \sum_{h,j} \rho e^{2\pi i (h-j)x} \xi_h \overline{\xi}_j \, dx$$
$$= \int \rho \sum_h e^{2\pi i h x} \xi_h \sum_j e^{-2\pi i j x} \overline{\xi}_j \, dx$$
$$= \int \rho \Big| \sum_h e^{2\pi i h x} \xi_h \Big|^2 dx \geq 0,$$

the inequality holding because $\rho > 0$. Similarly, $\sum\sum \xi_h F_{h-j} \overline{\xi}_j \geq 0$ where h, j are vectors with integral coefficients.

If $\sum\sum \xi_h F_{h-j} \overline{\xi}_j \geq 0$, then rewriting the inequality as

$$\Xi A \Xi^* \geq 0,$$

we see that the eigenvalues of the matrix A are nonnegative. Hence, $\det A$, which is the product of the eigenvalues, is ≥ 0, and every appropriate subdeterminant (obtained by setting appropriate components equal to 0) is ≥ 0. Hence, we find in particular $F_0 \geq 0$,

$$\begin{vmatrix} F_0 & F_h \\ F_{-h} & F_0 \end{vmatrix} \geq 0,$$

and

$$\begin{vmatrix} F_0 & F_h & F_k \\ F_{-h} & F_0 & F_{k-h} \\ F_{-k} & F_{h-k} & F_0 \end{vmatrix} \geq 0,$$

etc. This is Toeplitz's result. The inequality for the 2×2 determinant says

$$|F_h|^2 \leq |F_0|^2 = F_0^2.$$

So $|F_h|$ could be as big as $|F_0|$. If we digest the experimental data and pick out a component F_h that has a large absolute value close to F_0, what does the next inequality tell us? Writing it out, we have

$$F_0^3 - F_0|F_{k-h}|^2 - F_h(F_{-h}F_0 - F_{k-h}F_{-k})$$
$$+F_k(F_{-h}F_{h-k}) - F_0 F_{-k} F_k \geq 0$$

or

$$F_0^3 - F_0(|F_{k-h}|^2 + |F_h|^2 + |F_k|^2)$$
$$+ F_h F_{-k} F_{k-h} + F_k F_{-h} F_{h-k} \geq 0.$$

If we now set $|F_h| = F_0$, we obtain

$$-F_0(|F_{k-h}|^2 + |F_k|^2) + 2\mathrm{Re}\, F_h F_{-k} F_{k-h} \geq 0$$

and

$$-(|F|_{k-h}^2 + |F_k|^2)$$
$$+ 2\cos(\phi_h + \phi_{-k} + \phi_{k-h})|F_k||F_{k-h} \geq 0.$$

This is possible only if $|F_k|$ is close to $|F_{k-h}|$ and

$$\phi_h + \phi_{-k} + \phi_{k-h} \sim 0.$$

Now we are at last getting some phase information.

This was the crucial first approximation relation between the phases. We are assuming F_k and F_{k-h} are not zero. Look for $|F_h|$ close to F_0 and proceed. Now the problem is to use these inequalities to get a first guess at the values of r_i. It is not completely straightforward from there on, but every Toeplitz inequality will give more information on these elusive phases. The inverse statement that if all the Toeplitz inequalities are satisfied we get a positive density was first shown in the one dimensional case by Caratheodory.

Thus, in this case, mathematics comes to the rescue! The true story is much more complicated, but this is the essence. Hauptman, it should be added, got his Ph.D. in the early 1950s from this result.

Transonic Flow

I come now to a subject which I have worked in a great deal and still work in and which I have spoken of before: transonic flow. We begin with an airfoil at rest, here reduced to a two-dimensional body. Past it flows a compressible gas. Sometimes it is a jet engine blade (see Figure 3).

Again, a little history: Already in the 1930s there was contradictory evidence about what happens when the speed (at ∞) of the flow comes close to the speed of sound. In wind tunnels the flow was very disordered, and yet there seemed to mathematicians no reason why the smooth flow (it was governed by elliptic equations with C^∞, even analytic, coefficients) would not go on in the same smooth way as the parameter, namely, the speed at infinity, passed through speeds that produced a small supersonic (hyperbolic) region in the flow.

In the late 1940s, in fact, M. J. Lighthill showed in principle that there could be a smooth object with some subsonic speed at ∞ and a smooth supersonic bubble. Such a flow is called transonic. What would really happen? A smooth transonic flow? A flow with a shock? With many shocks? In the 1950s there were great arguments on the subject. Crucially, Clifford Gardner proposed that the boundary value problem was somehow ill-posed, and therefore in general there would be shocks. I learned at that time a story about von Karman, the mathematical physicist-turned-engineer and one of the fathers of the rocket industry in America. Friedrichs had worked with von Karman in Aachen for two years before the rise of the Nazis. The story is that in contemplating the transonic controversy, he told Friedrichs that applied mathematicians were not much help. They solve problems when engineers already have the answers, but now when they were needed they could not resolve the controversy.

Egged on by Friedrichs and Lipman Bers and some experience with equations that change type, I looked at the case [8] of an airfoil with a symmetric cross-section. One looks only at the upper half (Figure 4). We also have a given speed at ∞ and a symmetric supersonic bubble attached to the profile. The differential equation is for the potential ϕ, with the velocity $\overline{u} = \nabla\phi$; it comes from the conservation of mass:

$$\operatorname{div} \rho \nabla \phi = 0.$$

The density ρ is a function of $q = |\nabla\phi|$ given by Bernoulli's law,

$$\tfrac{1}{2} q^2 + i(\rho) = K,$$

where K is a given constant. Pressure p is a given function of ρ, the speed of sound c^2 satisfies $\frac{dp}{d\rho} = c^2$, and $i(\rho)$ is equal to $\int \frac{c^2}{\rho}\, d\rho$. The equation for ϕ is elliptic or hyperbolic depending on whether $q^2 < c^2$ or $q^2 > c^2$. The boundary condition on the profile is: no normal component of velocity,

$$\frac{\partial \phi}{\partial n} = 0.$$

The speed at ∞ is, say, q_∞.

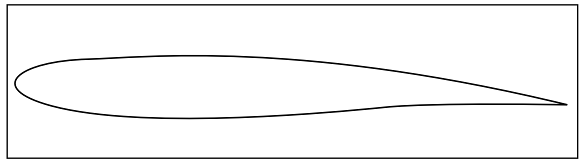


Figure 3. Wing section, a nonsymmetric airfoil.

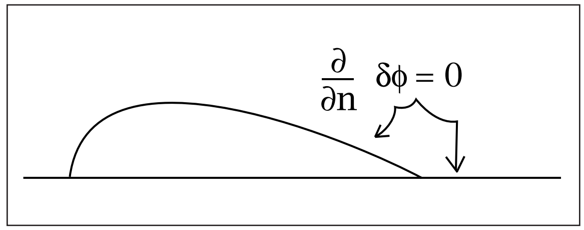


Figure 4. Half of a symmetric airfoil.

To show this boundary value problem is ill-posed, one has to wiggle the data and show that the perturbation problem is overdetermined.

We postpone what we will change. We hit with brute force and linearize about the given solution with δ for perturbation:

$$\operatorname{div}(\rho \nabla \delta\phi + \delta\rho \nabla\phi) = 0$$
$$(\delta q \text{ at infinity}) = \delta q_\infty$$
$$\frac{\partial}{\partial n} \delta\phi + \delta F = 0 \quad \text{on the boundary.}$$

Here δF involves a perturbation of the boundary, and ρ and $\nabla\phi$ are given by the undisturbed flow.

It is plainly a mess to show that there is some perturbation of the data in δq_∞ or δF that will show that this problem has no solution, i.e., that the problem is an ill-posed boundary value problem according to Hadamard.

First consider making just $\delta q_\infty \neq 0$. It is still impossible to prove ill-posedness that way. It is probably not true. So wiggling the boundary is the main possibility. But ρ and $\nabla\phi$ are a mess, too. They are some of the functions that come from the undisturbed flow. What saves the day is the hodograph variables. These are the two components of velocity $\nabla\phi$ of the undisturbed flow or, alternatively, of its speed and flow angle.

I laboriously transformed the differential equation so the independent variables became the hodograph variables of the undisturbed flow and found that $\delta\phi$ satisfied a simple Tricomi-like equation

$$K(\mu) \frac{\partial^2}{\partial \theta^2} \delta\phi + \frac{\partial^2}{\partial \mu^2} \delta\phi = 0.$$

Here $\mu = \mu(q)$ and $K(\mu) \sim \mu$ near $q = c$ in the undisturbed flow.

Such things are no coincidence. This one is due to a simple connection found by Guderley between

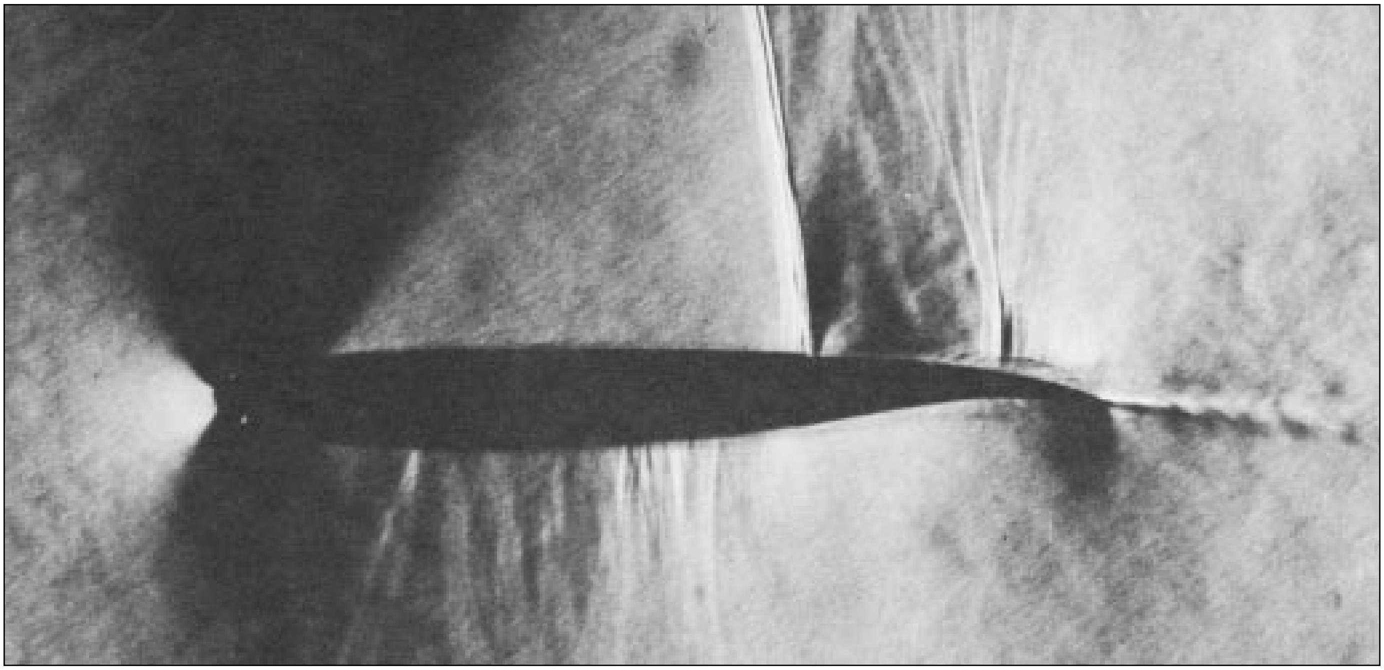


Figure 5. A schlieren photograph of the wing section of Figure 3 in a wind tunnel. The sharp lines are an indication of shocks. There is a wake from the trailing edge.

the disturbance potential $\delta\phi$ and the Legendre potential.

From there on it was almost clear sailing. The ultimate result is that there is a way of smoothly changing the profile so that the boundary value problem is overdetermined. The method is to use quadratic integrals of the derivatives to find a contradiction. The idea of using quadratic integrals goes back to either Noether's conservation law or Friedrich's a-b-c method.

While this result [9] was generally accepted, many engineers had a tough time. I was shown many computations that showed very smooth perturbations. In the long run it turned out that the mesh size in those computations (shades of Richardson!) was too coarse. Another school of thought went back to the idea that the Taylor series for the flow would continue to converge for some sufficiently small supersonic region. Eventually the theorem was accepted.

The next step was the recognition—in particular, by Paul Garabedian—that if a flow with a smooth supersonic bubble *was* perturbed, the nearby flow would have only weak shocks, so that the airfoil could still be useful. (I have tried unsuccessfully to prove this.) Garabedian started a program to compute a profile with a smooth flow with a specified speed at ∞ that could fly or at least be put in a wind tunnel. Lighthill's early work had led Neuwland in Holland to try such a computation. Garabedian's ingenious and extremely mathematical method was successful.

The method is to go from two independent variables, x and y, to complex coordinates $x = x_1 + ix_2$ and $y = y_1 + iy_2$. Make the gas law $p = p(\rho)$ analytic. (Garabedian had already used this method on another problem.) So now the potential ϕ is a complex function that has to be real if x and y are real.

In the late 1960s one could not solve a problem in so many independent variables: x_1, x_2, y_1, y_2. This problem could be reduced to three variables, but then it was just barely possible on the CDC 6600 computer. There was a catch: one was solving an elliptic Cauchy problem with analytic Cauchy data. The solution could develop a singularity. It could also develop a closed streamline (on which $\frac{\partial\phi}{\partial n} = 0$), which would be the airfoil. In case 1, it would be necessary to go back and twiddle the Cauchy data and try again. In case 2, one had a closed wing, but it might not be at all the shape that was wanted. However, Garabedian succeeded. He had his own troubles convincing engineers, although many were very impressed. In the end it was in Canada that Kaprcenski built a wing to Garabedian's specifications, and it tested well in a wind tunnel.

The Garabedian-Korn wing (see Figures 3 and 5) became a standard transonic wing. However, Garabedian's construction method was too mathematical in the long run, and engineers and applied mathematicians found and applied other conceptually simple methods. They also determined flows around fixed airfoils at different speeds at infinity by finite difference methods, which have the effect of inducing a suitable artificial viscosity.

About twelve years ago I turned to looking at existence proofs for weak solutions (i.e., those solutions that admit discontinuities in velocity and that satisfy an entropy condition). I have, of course, never had any doubt that there would be an existence proof if one modified the equations

$$\operatorname{div} \rho \nabla \phi = 0$$

and Bernoulli's law $\rho = \rho_B(|\nabla \phi|)$ by adding an artificial viscosity. Pursuing an analogy to a difference method mainly generated by Anthony Jameson, I tried modifying Bernoulli's law by letting the density satisfy a first-order equation that retards the density

$$\rho - \rho_B(|\nabla \phi|) = \nu \nabla \rho \cdot \nabla \phi.$$

In this work I was joined by Irena Gamba, and eventually we found [10] a simple third-order problem that could be solved for arbitrary ν, the artificial viscosity.

Proving that our solution can be carried to the inviscid limit $\nu = 0$ has proved elusive. (To begin with, I should say one does not use such a simple viscosity equation.) The convergence is very delicate, coming mainly from the methods of Tartar-Murat and DiPerna. So far it has been established only by assuming that the flow angle is bounded and that the flow neither stagnates nor cavitates (cavitation means that $\rho \to 0$, which is unfortunately a possibility). No computations reveal unlimited flow angles, cavitation, or stagnation except where one expects the last at the boundary. But that is not enough.

Now let me describe a last problem from transonic flow that should also have an existence proof. However, it has a peculiar phenomenon known as von Neumann's paradox. A shock is running along with a gas at constant pressure and zero velocity ahead of it, as in Figure 6.

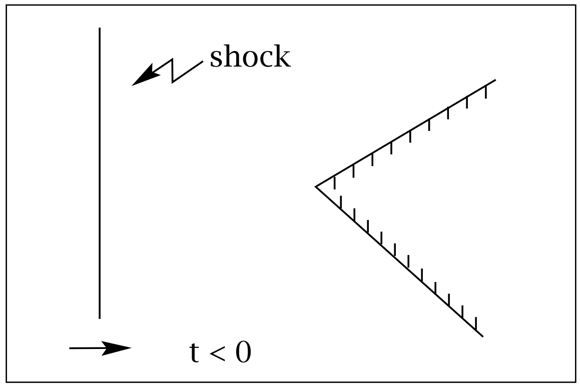


Figure 6. Shock approaching a wedge.

It hits a wedge at time $t = 0$. What happens? First of all, again under modest hypotheses, the flow is self-similar (depends on $\frac{x}{t}, \frac{y}{t}$). If the shock is strong, this flow has been computed, and there have existed for some years very good pictures of what happens. The problem itself goes back to World War II. The wedge is the corner of a building; the shock wave is a bomb blast. What von Neumann was worried about was that the flow patterns predicted mathematically for strong shocks did not check with experiments. Now we are interested in weak shocks (such as we might get in the flow past an airfoil). A transonic or "mixed" flow with a potential ϕ occurs.

Thus, in self-similar variables we find that we have

$$\operatorname{div} \rho \nabla \phi + 2\rho = 0,$$

Bernoulli's law

$$\frac{1}{2}|\nabla \phi|^2 + i(\rho) + \phi = \text{const},$$

and the normal velocity zero, of course, on the wedge.

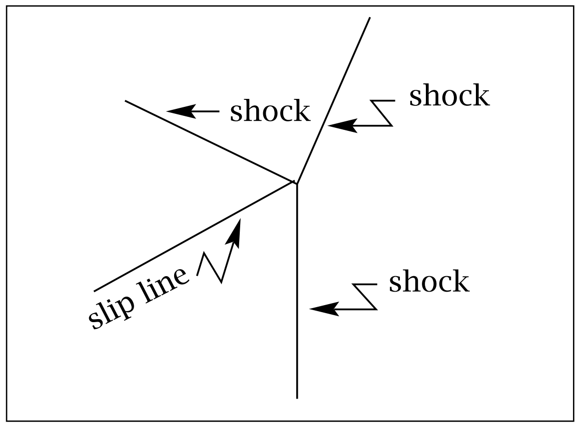


Figure 7. A three-shock or Mach configuration. Across a slip line, pressure is continuous and normal velocity is zero.

This looks so similar to our previous existence problem that everyone feels it is a safe bet that a weak solution exists. What does it look like?

So-called regular reflection is like light wave reflection. It occurs for a certain range of wedge angles (big enough) and certain speeds of the shock. On the other hand, if the wedge angle is very small, computations (and some studies of the linearized problem) show that the incident shock bends and reflects an infinitesimal shock.

However, there is a midrange of flow angle where neither of these possibilities can occur for essentially geometric reasons [11]. It is natural to expect from the strong shock case that the pattern will look like Figure 7.

This now is the paradox: For sufficiently weak incident shocks and an appropriate range of wedge angles, there can be no Mach shock configuration. But computations on very fine meshes with full Euler equations appear to contradict this. An analytic study of shock conditions shows that a configuration like this is impossible even for Euler equations. There is not time here to give all the arguments, but it is the mixed elliptic hyperbolic character that is making trouble. Far out, the equation is hyperbolic; near the wedge corner it is elliptic, and no match has been possible. Perhaps even a perfectly reasonable boundary value problem does not have a solution.

So now we have moved away from "mathematics coming to the rescue" to wondering if we are dealing with difference schemes that perhaps do not converge as expected. This is not a question that engineers are losing any sleep over. But I am.

So now we have looked at three—or rather four—problems. Has mathematics really come to the rescue? Emphatically yes: understanding the stability of what one does when modeling has been done by mathematicians. Using Toeplitz to determine molecules is mathematics. And the study of steady gas dynamics is not possible without answering the mathematical questions.

References

[1] R. Courant, K. Friedrichs, and H. Lewy, Uber die Partiellen Differenzgleichungen der Mathematischen Physik, *Math. Ann.* **100** (1928), 32–74; English translation by P. Fox, On the partial difference equations of mathematical physics, *IBM J.* **11** (1967), 215–234.

[2] D. McHale, *George Boole, His Life and Work*, Boole Press, Dublin, 1985.

[3] L. F. Richardson, *Weather Prediction by Numerical Process*, Cambridge Univ. Press, London, New York, 1922.

[4] J. Todd, Obituary: L. F. Richardson (1881-1953), *Math Tables and Other Aids to Computation* **8** (1954), 242-245.

[5] J. Karle and H. Hauptman, The phases and magnitudes of the structure factors, *Acta Cryst.* **3** (1950), 181–187.

[6] J. Karle, Recovering phase information from intensity data (Nobel lecture, December 9, 1985), *Chem. Scripta* **26** (1986), 261–276.

[7] F. A. Grunbaum and C. A. Moore, The use of higher-order invariants in the determination of generalized Patterson cyclotomic sets, *Acta Cryst.* **A51** (1995), 310–323.

[8] C. S. Morawetz, The mathematical approach to the sonic barrier, *Bull. Amer. Math. Soc. (N.S.)* **6** (1982), 127–145.

[9] ____, On the non-existence of continuous transonic flows past profiles, I, *Comm. Pure Appl. Math.* **9** (1956), 45–68.

[10] I. M. Gamba and C. S. Morawetz, A viscous approximation for a 2-D steady semiconductor or transonic gas dynamic flow: Existence theorem for potential flow, *Comm. Pure Appl. Math.* **49** (1996), 999–1049.

[11] C. S. Morawetz, Potential theory for regular and Mach reflection of a shock at a wedge, *Comm. Pure Appl. Math.* **47** (1994), 593–624.