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Jürgen Moser, 1928–1999

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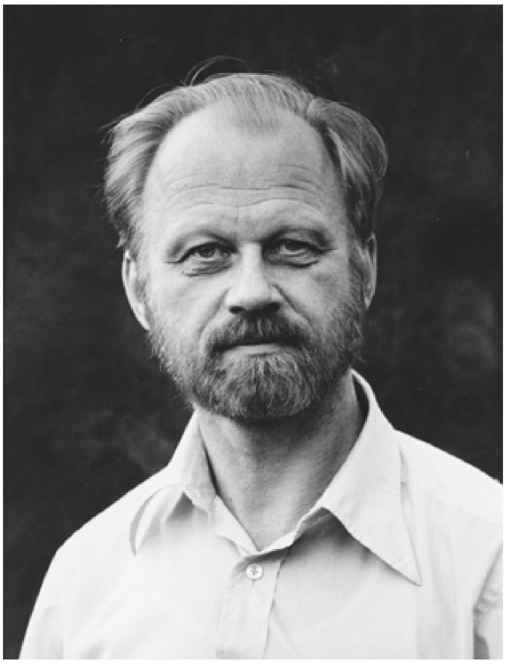

Except for the painful beginning and end, well-deserved good fortune accompanied Moser all his life. Accomplishment and recognition came to him early and lastingly. He was the first recipient of the G. D. Birkhoff Prize of the AMS and SIAM. He was honored by a Wolf Prize, medals were showered on him, academies vied to elect him as their member. He was an outstanding expositor, much in demand. He delivered the Gibbs lecture of the AMS, the Pauli lectures of the ETH, the Hardy lectures of Cambridge University, the Hermann Weyl lectures of the Accademia Lincei, and the von Neumann lecture of SIAM. He was president of the IMU for three years.

He was fortunate in having many good students, with whom he continued close scientific and personal relations. He loved his family fiercely, two daughters and a stepson, six grandchildren and charming and talented sons-in-law.

According to an old Roman proverb, *'what doesn't destroy me makes me stronger'*. This fits Jürgen, for in his early youth he was exposed to deadly danger, but survived the stronger for it. In 1938, the ten year old Jürgen was offered a chance to enroll in an elite boarding school for future leaders, but his parents who saw the Nazi's plan were able to extricate him. Jürgen's father was a neurologist, and as such was sometimes called upon to declare someone as 'unfit to live' according to the Nazi eugenics program. Jürgen's father, courageously, always refused to issue such certifications.

At the age of 15, Jürgen and his gymnasium classmates were pressed by the Nazi authorities into the so-called people's army, and at 16 they were thrown against Russian tanks besieging Königsberg. Only three of his class survived; his older brother was killed. Before the city was overrun, its population was evacuated by boat to the British zone. Many of the boats were sunk, with large loss of life; Jürgen and his parents survived but were separated. By the time they established contact, six months later, Jürgen's parents were back in the Russian zone. In an attempt to join them he was caught at the border and thrown into prison, but escaped. Back in the Russian zone he was not allowed to enter a university because of his bourgeois background, so he crossed the border in the opposite direction. This time he avoided capture, although he was fired upon. He arrived in Göttingen, in 1947, penniless, but already dedicated to mathematics. Fortunately Franz Rellich recognized his exceptional qualities. Under his tutelage Jürgen thrived. He acquired from Rellich his lifelong interest in the spectral theory of differential equations, and probably learned how to lecture on mathematics.

Later Jürgen came under the influence of Carl Ludwig Siegel. Like Rellich, Siegel was a lifelong anti Nazi, but his personality differed altogether from Rellich's outgoing nature. Siegel had a pessimistic view of life. He claimed to have chosen to study astronomy at the university because it was so far removed from the world. After a semester he realized that mathematics was even farther removed from reality, and he switched, but he retained a lifelong interest in astronomy, which he passed on to Jürgen. The book by Siegel and Moser on celestial mechanics grew out of this joint interest. It is a revision of Siegel's 1956 book, which was based on notes Moser took of Siegel's lectures. The 1973 monograph, *'Stable and random motion in dynamical systems, with special emphasis on celestial mechanics'*, based on his Weyl lectures, is written entirely from Moser's point of view.

Jürgen came to the Courant Institute in 1953 on a Fulbright fellowship. This was a part of Richard Courant's policy of bringing talented young German scientists to the US to further their education. After Jürgen had been in our circle for a short time, we realized that he was very special, a prince among men, a knight in shining armor. He had all the German virtues: devotion to hard work, a love of the outdoors, a love of beauty, of music; I don't know where he stood on poetry. He was exceedingly good company to do things with, like hiking in the mountains. He was very good in describing in detail something interesting that happened to him. He loved adventure and to test his powers; he had great self-confidence. 'I thought I could do anything I really wanted to do', he once said in an unguarded moment. He loved hang gliding passionately.

Jürgen was very direct; he said what was on his mind, even when it was not what you had hoped to hear. But that only deepened his friendships, for his judgments were mostly right, and because everything he did was done with great kindness.

Jürgen was a very private person as well. Perhaps this can be traced back to the chaotic years after the war, when he developed the habit of shutting himself off from his surroundings, and concentrating on thinking about a mathematical problem. Sometimes he would put a tea cozy on his head as an indication to his family that he was not to be disturbed.

Richard Courant soon came to admire and love Jürgen. He was delighted to have him as his son-in-law, and as director of the Courant Institute, from 1967 to 1970. Courant had hoped that Jürgen would serve longer, and was disappointed when Jürgen stepped down in 1970. When I pointed out that Jürgen disliked the position, he retorted that 'Jürgen couldn't have disliked it that much, for then he couldn't have done such an excellent job'.

It was Courant who called Jürgen's attention to the problem of the stability of beams in particle accelerations such as the synchrotron, one of whose inventors was Courant's son Ernest. Stability is an essential point in the construction of these accelerators. For instance, in the storage ring of the proton accelerator that operated in the 1970s at CERN, protons proceeding along circular paths were stored for up to 10 hours until they were made to collide with protons circulating in the opposite direction. During this time the protons orbited 10^{10} times around the circular path in a tunnel whose cross section was 16×5 cm^2. If you set the circuit of the protons to a year in the Earth's path around the sun, 10^{10} years surpasses the age of the solar system, so its stability is harder to prove than that of the solar system. No amount of numerical calculation can shed any light on it. Moser found that KAM theory shows that the majority of the protons survive circling around 10^{10} times.

In 1980, Jürgen left the Courant Institute for Zürich. Why is still not clear. He said, somewhat facetiously, that he did not want to watch his friends grow old; fair enough. I think that he found the US too disorderly for his taste. In 1970, while Jürgen was director, a mob claiming to oppose the war in Vietnam occupied the building housing the Courant Institute and tried (but failed) to burn down its computer. This experience took its toll.

Jürgen did not want to return to Germany. Nazism, the war, and the communist rule in East Germany, where his parents still lived, was too much baggage. He came to Zürich when the mathematics department of the ETH was looking for a new appointment. The leading candidate's dossier was presented to the President of the ETH, Dr Ursprung. He demanded assurance about the quality of the person proposed, so he was shown a glowing letter of recommendation for the candidate from Jürgen. 'And who is he?', asked Ursprung. 'The best person in the world working on dynamical systems', he was assured. 'Then why don't we get him?', demanded Ursprung. And get him they did.

Moser's mathematical interests were very broad. They included the theory of partial differential equations, spectral theory, dynamical systems, complete integrability, differential geometry, and complex analysis. He first achieved international recognition in 1960 by his remarkable simplification of the de Giorgi proof of the regularity of solutions of first-order variational problems for a scalar function of n variables. Later Moser related this result to a very general inequality of Harnack type for solutions of second-order equations whose coefficients have very little smoothness.

This work was soon followed by a proof of the existence of invariant curves for area-preserving transformations of an annulus, and more generally the existence of quasiperiodic solutions of Hamiltonian systems that are near completely integrable ones. Moser showed that most solutions, in the sense of measure, are quasiperiodic, provided that some natural conditions are satisfied:

(i) the unperturbed frequencies ω_k have to be rationally independent in the strong sense

$$\left|\sum j_k \omega_k\right| \geq \frac{\text{constant}}{|j|^{\gamma}};$$

(ii) the Hessian of the unperturbed Hamiltonian with respect to the action variables has to be non-zero.

The history of how Moser came to this result bears repeating. Kolmogorov, in 1954, announced a result of this kind for analytic Hamiltonians in a Doklady note, and then to the International Congress of Mathematicians in Amsterdam. *Mathematical Reviews* asked Moser to review these notes; Moser found no complete proof in either of them; an inquiry sent to Kolmogorov remained unanswered. So for the next seven years Moser pondered this problem, and came up with a proof, valid not for analytic Hamiltonians but for the C^k class. Originally k was chosen to be 333, but ultimately it was whittled down by others to $k = 5$; it is known that k has to be greater than 2. The new idea introduced by Moser was to combine Kolmogorov's fast iteration with a kind of smoothing invented by John Nash for the embedding problem and put in a general framework by Jack Schwartz. Moser later found an approach which gave both the analytic and C^k cases.

Moser was intrigued by the correspondence of Weierstrass with Sonya Kowalevsky, published in 1973, about the three-body problem. Poincaré has shown that there are no analytic integrals other than the classical ones. Weierstrass thought that this lack of integrability did not preclude the existence of quasiperiodic solutions. He even wrote down an infinite series for such solutions, but he was unable to prove its convergence. Moser was pleased that his own work had validated the ideas of Weierstrass.

One of the consequences of Moser's theory is that systems near integrable ones fail to be ergodic. Doesn't this shake the foundations of statistical mechanics? Doesn't the inequality of time averages and phase averages depend on ergodicity? Jack Schwartz, in an article titled 'The deleterious influence of mathematics on the physical sciences', has pointed out that ergodicity is indeed needed to show that the time average of *every* continuous function is equal to its phase average on the energy surface. But in statistical mechanics we are not interested in every function, only in those that have thermodynamic significance. These are very special, highly symmetric functions, whose time averages can be calculated without resorting to ergodicity.

At the time of Moser's first contribution to this subject, not many completely integrable systems were known: Jacobi's integration of geodesic flow on ellipsoids, Carl Neumann's study of the motion under gravity of a particle confined to a sphere, Sonya Kowalevsky's top, and a few others. Most people regard them as oddities; but I would like to point out that completely integrable systems have played crucial roles in science. The complete integrability of the two-body problem enabled Newton to show how Kepler's laws are a consequence of his theory of mechanics and gravitation. In the old Bohr–Sommerfeld quantum theory, completely integrable Hamiltonian systems could be quantized. This was sufficient to serve as a stepping stone to the Heisenberg–Schrödinger quantum mechanics and, in the 1940s, Onsager used the complete integrability of the Ising model to show that phase transition takes place at a critical temperature.

A new chapter dawned in the 1960s. Toda introduced his anharmonic lattice with an exponential restoring force, and showed how to represent its solutions explicitly. At about the same time, Kruskal and Zabusky, in their effort to understand the remarkable numerical experiments of Fermi, Pasta, and Ulam, discovered even more remarkable properties of solutions of the Korteweg–de Vries (KdV) equation: the emergence of solitons. Gardner, Green, and Kruskal have subsequently shown that the KdV equation is Hamiltonian, and can be integrated using the direct and inverse scattering mechanism. Faddeev and Zakharov observed that this shows the complete integrability of the KdV equation. Soon it was discovered that the KdV equation is only the first of an infinite sequence of completely integrable equations, called the KdV hierarchy. Thereafter a veritable deluge of completely integrable systems were discovered: the sine–Gordon equations, the cubic Schrödinger equation, the Kaç–van Moerbeke lattice, the Boussinesq equation, the Calogero–Moser system, the Kadomtsev–Petviashvili equation, the Benjamin–Ono equations, and the Davey–Stewartson equations.

Many of these equations could be put in a form where the integrals appear as the eigenvalues of a linear operator associated with the solution. Since Moser had a long-standing interest in the spectral theory of operators, no doubt this added to his fascination with completely integrable systems. In a series of papers in the 1970s he illuminated a number of completely integrable systems, and established connections between them.

Numerical experiments were and are a crucial ingredient in studying dynamical systems and discovering their properties. It was numerical experiments by Fermi, Pasta, and Ulam that first gave the indication of unexpected almost periodicity of solutions of the perturbed wave equation. Likewise, numerical experiments gave the first indication to Kruskal and Zabusky of solitons.

Moser appreciated the necessity of numerical experimentation; he liked to point out that whereas KAM theory proves the existence of quasiperiodic orbits in a small neighborhood, only calculations shed light on the actual size of that neighborhood. Moser sometimes grumbled that some people use computing as a substitute for thinking.

The success of numerical experiments in discovering complete integrability is itself an example of a KAM-type result. For a numerical experiment never calculates an exact solution of equations under investigation, only an approximation to it. However, one can regard the result of the numerical experiment as an exact solution of an approximate system. Here is a precise formulation.

Consider a one-to-one area-preserving map g of, say, the unit square into itself. A numerical approximation to g using m digit accuracy must be regarded as mapping each small square of edge length 10^{-n} into which the unit square is divided onto another such small square. We require the approximate image of each small square to have a non-zero intersection with the exact image of that square under g.

The approximate map is area preserving iff it is a permutation of the little squares. Is there an approximation to g that is a permutation? The answer is yes, and to prove it we appeal to the Marriage Theorem. That theorem says that if every collection of k little squares has collectively at least k eligible targets for every k, then a one-to-one match is possible. The targets eligible to k little squares cover the exact image of these k squares. Since the area of the exact image under g of k little squares is $k \times 10^{-2m}$, at least k squares, each of area 10^{-2m}, are needed to cover it.

Of course the approximate map is not even continuous, much less C^k, so the classical KAM theorem is not applicable. But there may be some result that relates the cycle structure of the approximation to the structure of the orbits of g.

In one of his last papers Moser made an ingenious use of the above approximation theorem to prove that every area-preserving homeomorphism of the square can be approximated in the Koopman topology by C^∞ diffeomorphisms, which furthermore keep the boundary of the square fixed. The Koopman topology is defined in terms of the unitary operators U_g associated with every homeomorphism g, acting on the L^2 functions ω on the unit square by the formula $U_g\omega(x) = \omega(g(x))$. A sequence of maps g_n converge to g if U_{gn} converges strongly in the L^2 sense to $U_g\omega$.

Jürgen retired from the ETH in full possession of his brilliant qualities; he was expected to be a very active elder statesman of mathematics for many years to come. It was not to be. Those of us who knew him and loved him can never come to terms with losing him, never.