THE SOLUTION OF THE N-BODY PROBLEM

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To Philip Holmes,

for his deep mathematics, for his warm and candid poetry, and for the immense intellectual joy he has instilled in me during the time our book took shape.

The wind scrambles and thunders over hills
with a voice far below what we can hear.
Whalesong, birdsongs boom and twitter.
Sea, air, everything's a chaos of signals
and even those we've named veer and fall
in pieces under our neat labels. Waves—
how to speak of the structure of waves
when all disperses and there's nothing fixed to tell?

Philip Holmes, *Background Noise*

Folk-Mathematics

A *folk-tale* is a popular story uttered from one generation to the next. The main source of culture in times of old, oral tradition plays a marginal role in spreading scientific information today. Still, its significance is by no means negligible, and all domains of human activity are more or less influenced by it. Mathematics makes no exception. We all know theorems we have never read in books or papers or learned about at formal presentations. We often don't know a reference, have no idea how, when, and who proved that result. Usually a colleague mentioned it at some conference dinner, during a coffee-break, or in a friendly discussion in our Department. It is striking, it sticks to our mind, and after a while it is part of our mathematical heritage—we just know it. Then we tell it further under similar circumstances, and so the wheel turns on. We will call this component of our knowledge: *folk-mathematics.*

Without denying the positive role folk-mathematics plays in spreading information, we must admit that results gathered through it are sometimes misleading or misunderstood. A typical example is the *Cantor set.* Everybody knows that the middle-third Cantor set has zero Lebesgue measure, and many believe that the middle-fifth analogue has positive measure. Intuitively this sounds plausible: if removing each time a smaller segment, the remaining quantity should be larger. Unfortunately, the intuition leads us astray this time. For any k, the middle-kth Cantor set has zero measure. Though a simple computation would show this, few do it, so the mistake perpetuates from one mathematician to the other. We can indeed obtain a Cantor set of positive measure by assigning a variable removal step. Delete first the middle-third segment, then the middle-ninth, then the middle-twenty-seventh, and so on. This algorithm will lead us to the desired result.

The above example is easy to check, but what are we up to when a more complicated folk-mathematical situation appears? Physicists and mathematicians less familiar with celestial mechanics, have asked me at different occasions to provide details about the "impossibility to solve the n-body problem." Some had heard that Poincaré had proved the result, others recalled only that such a theorem exists somewhere in the literature. After all, this is a natural question. Since Abel and Galois proved the impossibility of solving algebraic equations of degree higher than five through formulae involving only roots, why should there not be an impossibility proof for solving the n-body problem?

The astonishment comes when we respond that the n-body problem has been already solved. Of course, the answer requires explanation, and since this old question of celestial mechanics continues to raise interesting challenges (as it did during the last three centuries), we will present here the intriguing story and the unexpected consequences of all the attempts to obtain an explicit solution.

King Oscar's Prize

Stated for the first time in Newton's *Principia*, the n-body problem of celestial mechanics is an initial value problem for ordinary differential equations: for given initial data $\mathbf{q}_i(0), \dot{\mathbf{q}}_i(0), i = 1, \ldots, n$ (with $\mathbf{q}_i(0) \neq \mathbf{q}_j(0)$ for mutually distinct i and

j), find the solution of the second order system:

$$\ddot{\mathbf{q}}_i = \sum_{i=1}^{n} \frac{m_i m_j (\mathbf{q}_i - \mathbf{q}_j)}{|\mathbf{q}_i - \mathbf{q}_j|^3}, \quad i = 1, \ldots, n, \tag{$*$}$$

where $m_1, m_2, \ldots, m_n$ are constants representing the masses of n point-masses and $\mathbf{q}_1, \mathbf{q}_2, \ldots, \mathbf{q}_n$ are 3-dimensional vector functions of the time variable t, describing the positions of the point-masses. For $n = 2$ the problem was completely solved by Johann Bernoulli in 1710 (see [W], [DH]), but for more than a century and a half after Bernoulli's success, the case $n \geq 3$ eluded the efforts of everyone.

Interest in the problem grew towards the end of the last century, when a special event made the best mathematicians look at celestial mechanics with more concern than ever before. In volume 7, 1885/86, *Acta Mathematica* announced the establishment of a prize in the honour of King Oscar II of Sweden and Norway, to be awarded on the King's 60th birthday: 21 January 1889. The deadline for submission was set for 1 June 1888. Finding a convergent power-series solution of the above initial value problem, was the first—and the most important—among the four questions proposed by the three-member jury: Gösta Mittag-Leffler—the editor-in-chief of *Acta*, Charles Hermite, and Karl Weierstrass. The formulation of the first question, due to Weierstrass, who had shown growing interest in the problem himself, appeared in German and French as follows in our translation (a slightly different translation of the prize announcement was given by Daniel Goroff in [P]):

> "Given a system of arbitrarily many mass points that attract each other according to Newton's laws, under the assumption that no two points ever collide, try to find a representation of the coordinates of each point as a series in a variable that is some known function of time and for all of whose values the series converges uniformly.

"This problem, whose solution would considerably extend our understanding of the solar system, seems capable of solution using analytic methods now at our disposal; we can at least suppose as much, since Lejeune Dirichlet communicated shortly before his death to a geometer of his acquaintance [Leopold Kronecker], that he had discovered a method for integrating the differential equations of Mechanics, and that by applying this method, he had succeeded in demonstrating the stability of our planetary system in an absolutely rigorous manner. Unfortunately, we know nothing about this method, except that the theory of small oscillations would appear to have served as his point of departure for this discovery. We can nevertheless suppose, almost with certainty, that this method was based not on long and complicated calculations, but on the development of a fundamental and simple idea that one could reasonably hope to recover through preserving and penetrating research.

"In the event that this problem remains unsolved at the close of the contest, the prize may also be awarded for a work in which some other problem of Mechanics is treated as indicated and solved completely."

Out of the 12 papers eventually submitted for the competition, 5 treated the n-body problem, none of them, however, succeeded to obtain the required power-series solution. Under these circumstances the jury decided to award the prize to the 35-year-old Henri Poincaré, for his remarkable contribution to the understanding of the equations of the dynamics (called Hamiltonian systems today) and for the many new ideas he brought into mathematics and mechanics. Indeed, Poincaré's memoir, later developed into his monumental 3-volume work *Les Méthodes Nouvelles de la Mécanique Céleste*, layed the foundations of several branches of mathematics and—the most important—opened the way to qualitative methods, as opposed to the quantitative ones that had reigned in analysis since Newton and Leibniz.

Published in volume 12, 1890, of *Acta Mathematica*, Poincaré's memoir offered the first example of *chaotic* behavior in a deterministic system (it involved *homoclinic orbits* in a first return map in the restricted 3-body problem). In fact Poincaré understood the complicated behavior of those orbits only after the prize was awarded to him. The first version of his paper, which was actually the one awarded the prize, incorrectly claimed that such orbits were stable, by missing the important fact that the homoclinic intersection was transversal. Assaulted with questions by Edvard Phragmén, the assistant editor at *Acta* in charge with preparing the manuscript for publication, Poincaré finally discovered and corrected the mistake.

Phragmén had found Poincaré's work very hard to read. The initial version almost doubled in size after Phragmén's repeated requests of clarification. Writing about the subsequent 1895 paper entitled *Analysis Situs*, Jean Dieudonné [Di] characterized Poincaré's style in the following words:

> "As in so many of his papers, he gave free rein to his imaginative powers and his extraordinary intuition, which only very seldom led him astray; in almost every section is an original idea. But we should not look for precise definitions, and it is often necessary to guess what he had in mind by interpreting the context. For many results he simply gave no proof at all, and when he endeavored to write down a proof, hardly a single argument does not raise doubts. The paper is a *blueprint* for future developments of entirely new ideas, each of which demanded the creation of a new technique to put it in a sound basis."

Unfortunately Poincaré's correction came only after the memoir had been printed and some of *Acta*'s issues delivered to subscribers. As editor-in-chief of *Acta*, as a member of the jury, and as a favorite of the King, Mittag-Leffler was put in a delicate position. To defend the honour of the prize and his own credibility and position, he decided to recall the published issues and print the correct version. Poincaré agreed to bear the costs of the first printing: 3585 Swedish crowns and 63 öre, more than the 2500 crowns he had received for the prize (to understand the figures let us mention that Mittag-Leffler's annual salary as a professor at the University of Stockholm had been 7000 crowns in 1882) [A],[BG].

We do not go further into the history and the scandal that followed (the interested reader can find the historical and mathematical details in [DH]—a forthcoming book about the origins and the development of chaos and stability). What matters to us now is the negative result proved by Poincaré in the prized memoir, result that shows the impossibility of solving the n-body problem, but only by use of a certain method.

Is this Problem Unsolvable?

First integrals (or simply *integrals*) for systems of differential equations are functions that remain constant along any given solution of the system, the constant depending on the solution. In other words, integrals provide relations between the variables of the system, so each scalar integral would normally allow the reduction of the system's dimension by one unit. Of course, this reduction can take place only if the integral is an *algebraic*—not very complicated—function with respect to its variables, such that one variable can be expressed as a function of the others. If the integral is *transcendent*, any attempt to obtain such an expression is pointless.

At the time of Poincaré, the method of solving systems of differential equations by finding first integrals was much in use. It had been known for a long time that the n-body problem had 10 independent algebraic first integrals: 3 for the center of mass, 3 for the linear momentum, 3 for the angular momentum, and one for the energy (see e.g. [W], [D1], [D2]). This allowed the reduction of the primitive system from $6n$ (each point mass is represented in space by 3 position and 3 velocity components) to $6n - 10$. Jacobi had shown that using a so-called *reduction of nodes* (some symmetries), the dimension of the system could be further reduced to $6n - 12$, but this was not enough to understand even the 3-body problem—it still left a complicated 6-dimensional first order system unsolved—not to mention higher values of n.

In 1887 the 39-year-old German mathematician Ernst Heinrich Bruns published in *Acta Mathematica* a surprising result [Bru]: *the n-body problem has no other integrals—algebraic with respect to the time, the position, and the velocity coordinates—except the 10 known ones.* Though some gaps were subsequently discovered in Brun's proof, Poincaré had no doubt that the result was true. In his prized paper he proved an even stronger theorem: *there are no other integrals—*

algebraic with respect to the time and the velocities only—other than the 10 known ones. In other words, these negative results showed the impossibility to solve the equations of motion of the n-body problem by reducing the dimension of the system with the help of first integrals.

Of course, this does not mean that the n-body problem is unsolvable, just that a certain method fails to provide the solution. In fact standard results of differential equations theory show that any initial value problem for the equations $(*)$, with initial data not starting from collisions, leads to the existence of a unique solution defined on a maximal interval, which is the whole real line if singularities do not occur. So the problem posed by King Oscar's prize made sense and could be solved, in principle. Unfortunately, the folk-mathematical tradition retained only one aspect of these results and perpetuated the wrong message that the n-body problem was unsolvable. From this derives the whole misunderstanding introduced in the first section.

After a digression into the foundations of mathematics, we will see that the n-body problem was later solved in the spirit of King Oscar's prize.

Brouwer's attack

Every mathematician has opinions about what problems present importance, what branches are difficult, and what directions are promising in mathematics. But unlike other sciences, whatever differences of opinion arise, all mathematicians agree that a result proved two millennia, two centuries, or two years ago, remains true forever. The progress of mathematics has little to do with the foundations. In spite of this, some prominent mathematicians have dedicated time and energy towards understanding the roots of their discipline. And sometimes, their efforts have raised polemics and disputes as poignant as those frequently met in other domains of human activity.

In 1913, the 32 year old Luitzen Brouwer launched an attack against a well established mathematical method of reasoning. As an editor of the prestigious *Mathematische Annalen*, he rejected all submitted papers that used *reductio ad absurdum* as a method of proof. This lead to a scandal. The emergency meeting that gathered the editorial board saved the reputation of the journal in disfavor of the Dutch mathematician. The editorial board resigned as a whole and reelected itself, except Brouwer. Offended by his colleagues' attitude and supported by his government, Brouwer immediately established a rival journal in Holland [G].

That embarrassing incident marked the beginning of a long fight between *intuitionism* and *formalism*, the main schools of mathematical-philosophical thought at the beginning of our century, each claiming to have found—against the other—the only viable way of laying the foundations of mathematics. The necessity of building a ground had occurred as a consequence of the antinomies known already by the Greeks, but which had started now to embarrass the recently established *set theory.*

The main objection of Brouwer's intuitionism, as opposed to Hilbert's formalism, regarded the issue of *existence theorems.* Brouwer considered that a nonconstructive argument merely based on logical steps, cannot be accepted as proof of existence, so *reductio ad absurdum* seemed to him a good point to start the polemic. On the other hand Hilbert, who took Brower's action personally, attempted to show that every theorem can be deduced by logical steps from the postulates of a given axiomatic system. Unfortunately, in this respect the German mathematician was wrong.

In 1931, Hilbert's formalism received a sharp blow when the Austrian logician Kurt Gödel published his incompleteness theorem [Gö]. Gödel proved that *any sufficiently rich, sound, and recursively axiomatizable theory is incomplete.* A recent paper [CJZ] goes even further by showing that, in a quite general topological sense, incompleteness is a common phenomenon: *with respect to any reasonable topology, the set of true and unprovable statements is dense in the set of all statements.* This result has persuaded some mathematicians to believe that the future of mathematics is not with proving theorems but with trying to estimate the probability that a result is true.

On the other hand, Brower's intuitionism—though never fully contradicted by any other theory and still under research in small scientific circles—fell into oblivion because it raised barriers, which the mathematical community refused to acknowledge. Mathematical research has developed almost undisturbed by the fight for its own foundations.

We will further see, however, that the main idea of intuitionism is far from illuminating. In certain cases a constructive proof of existence brings no more information than a nonconstructive one. This is surprising, and the example we offer is the n-body problem.

The Series Solution

In 1913, when he launched the attack that would deprive him of the editorial membership at the *Mathematische Annalen*, Brower was not aware of a paper published one year before in *Acta Mathematica* by a Finn of Sewdish origin: Karl Sundman. If he had known and understood Sundman's work, Brower would have probably never developed his *intuitionism*.

Sundman's paper [Su3] (inspired by a previous work of the Italian mathematician Giulio Bisconcini [Bi]) revisited and republished some of his own results that had appeared in 1907 [Su1] and 1909 [Su2] in a Finnish journal of lesser fame and circulation. One of Sundman's achievements was to find, for almost all admissible initial data, a series solution of the 3-body problem. If he had gotten this result 22 years earlier, he would have probably been awarded King Oscar's prize.

Reading Sundman's paper we see that he obtained a series solution in powers of $t^{\frac{1}{3}}$ for the 3-body problem, a series convergent for all real t, except for a negligible set of initial conditions, namely those for which *the angular momentum* is zero. Indeed, Sundman showed first that there are no difficulties in proving the convergence of the series as long as no collisions take place. He also surmounted the impediment of binary collisions through a process he called *regularization*, which means to analytically extend the solution beyond the collision singularity, and which physically corresponds to an elastic bounce. In this case, his series still proves convergent for all real values of the time variable. Unfortunately he could not apply the same method if a triple collision occurs, but he showed that such a collision takes place only if the angular momentum cancels. In 1941, Carl Ludwig Siegel proved that such a regularization is possible only for a negligible set of masses, so indeed, the analytic continuation of triple collisions is generically impossible.

Sundman's method failed to apply to the n-body problem for $n > 3$. It took almost 7 decades until the general case was solved. In 1991, a Chinese student, Quidong (Don) Wang, published a beautiful paper [Wa], [D1], in which he provided a convergent power series solution of the n-body problem. He omitted only the case of solutions leading to singularities—collisions in particular. (To understand the complications raised by solutions with singularities, see [D2].)

But did this mean the end of the n-body problem? Was this old question—unsuccessfully attacked by the greatest mathematicians of the last 3 centuries—merely solved by a student in a moment of rare inspiration? Though he provided a solution as defined in sophomore textbooks, does this imply that we know everything about gravitating bodies, about the motion of planets and stars in the universe? Paradoxically, we do not, in fact we know nothing more than before having this solution.

In the following section we will explain the conceptual and foundational difficulties arising from this apparent logical and philosophical paradox.

The Foundations of Mathematics

As we earlier mentioned, what Sundman and Wang did is in accord with the way solutions of initial value problems are defined. Everything is apparently all right, but there is a problem, a big one: these series solutions, though convergent on the whole real axis, are practically useless because of their very slow rate of convergence. In applications, one would have to sum up millions of terms to determine the motion of the particles for insignificantly short intervals of time. The round-off errors make these series unusable in numerical endeavors. From the theoretical point of view, these solutions add nothing to what was previously known about the n-body problem.

This unusual situation raises several questions and makes us think once more about the foundations of our discipline. First of all it clarifies that even a constructive solution can be useless from the practical point of view. Then why stick to it, why give *intuitionism* any credibility? Well, this difficulty would still let us have a sound sleep. How many of us really care about *intuitionism* when doing mathematics?

Unfortunately there are other problems. The second would involve the definition of a solution for an initial value problem attached to a differential equation. If our definition is meaningful, then why do we obtain totally useless solutions? It means that in certain cases all our efforts of finding and writing down solutions might be as futile as Sisyphus' work; moreover, we have no way of knowing in advance if this would be the case. What to do then? Eliminate power series solutions from our definition? This would mean to negate two centuries of mathematics and throw many achievements away. Clearly there is no simple answer to this question.

The third problem is connected to what "good" mathematics means. Aware or not, we usually understand by this the mathematics promoted by famous mathematicians. No one would doubt that the mathematics of Weierstarss, for example, was and remains "good." But Weierstrass stated the first problem of King Oscar's prize, a problem tackeld by the sharpest minds of the time. Today we see that the solution of this problem is utterly useless. It was eventually solved exactly as the German mathematician had wished; still, 100 years later, its solution doesn't impress anybody; it presents only mere historical interest. Fortunately, the genius of Poincaré stirred our discipline into the right direction—at least this is what we believe today. But how will mathematicians think hundred years from now on?

The n-body problem—a pillar against the flow of time, a sound landmark on the map of mathematics—has posed and continues to pose new challenges to our restless minds. Almost untouched, mysterious as in the beginning, undisturbed by our keen attempts to unravel its secrets, it has victoriously survived 300 years of siege. It has kindled and witnessed a few revolutions: the beginnings of calculus, of qualitative methods, of relativity, of chaos; tackled numerically, it has contributed to the launch of satellites and to the first human step on the moon. Now it is questioning the bottom slab of differential equations theory, the structure on which a significant part of modern science and technology is based. Do we have an answer to this last challenge?

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