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Resonances and small divisors

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Translated from the French by Kathleen Qechar

During the International Congress of Mathematicians held in Amsterdam in 1954, A.N. Kolmogorov announced an important theorem which was made precise (and proven!) a few years later by V.I. Arnold and J. Moser [Kol54, Arn63a, Mos62]. I would like to present a *very elementary introduction to this Kolmogorov-Arnold-Moser (KAM) theorem* according to which "the solar system is probably almost periodic". My (modest) aim is to show the role of resonances and small divisors in celestial mechanics by focusing on a very simplified example, inspired by the real KAM problem: it is in some sense a "toy model" of the solar system, much easier to understand. Facing a too difficult question, the mathematician has the right to simplify the statement to its maximum, in order to locate the difficulties. I will try to treat this example in detail with the help of Fourier series. The "real" KAM theory is much more difficult: the reader may find more information, along with indications about the proof of the theorem, in J. H. Hubbard's chapter in this volume (Chap. 11).

10.1 A periodic world

We live in a world full of a great number of periodic phenomena. The Sun rises about every 24 hours, the new moon comes back every 29.5 days, the summer about every year... Of course, such examples could be multiplied ad infinitum. This observation is old and the first scientists tried very early to measure these cycles. Sometimes the period is not easy to determine and it is very often only an approximation. Let us think e.g. about the cycle called *saros*: every 6 585 days and 8 hours, the Moon, the Sun and the Earth find themselves in about identical relative positions and there is such a periodicity in the appearance of eclipses. As a matter of fact, due to the 8 hours, the periodicity of eclipses in a given place of the Earth is in fact triple (one day = 3 times 8 hours) so that the period is of 19 756 days (54 years and 32 or 33 days depending on leap years). We can only be fascinated by the precision of the astronomers' observations made during Ancient times which led to the exact determination of this astronomical cycle. Maybe the existence of these cycles in our universe is a preliminary condition for the appearance of life and civilization? Can we imagine the difficulties of living on a planet which would be the satellite of a double star: the rising and setting of the two suns would become entangled in a more or less random way.

Mathematicians have always been fascinated by cycles and one did not have to wait for Fourier to decompose a cyclic phenomenon into a sum of elementary cyclic phenomena. What is more elementary than a point which rotates on a circle with a constant angular velocity? It is of course the model the first observers of the Sun (which rotates "evidently" around the Earth) were thinking about. The situation is a little more complicated in the case of planets, as the paths they follow in the sky seem sometimes complex (see Fig. 10.1).

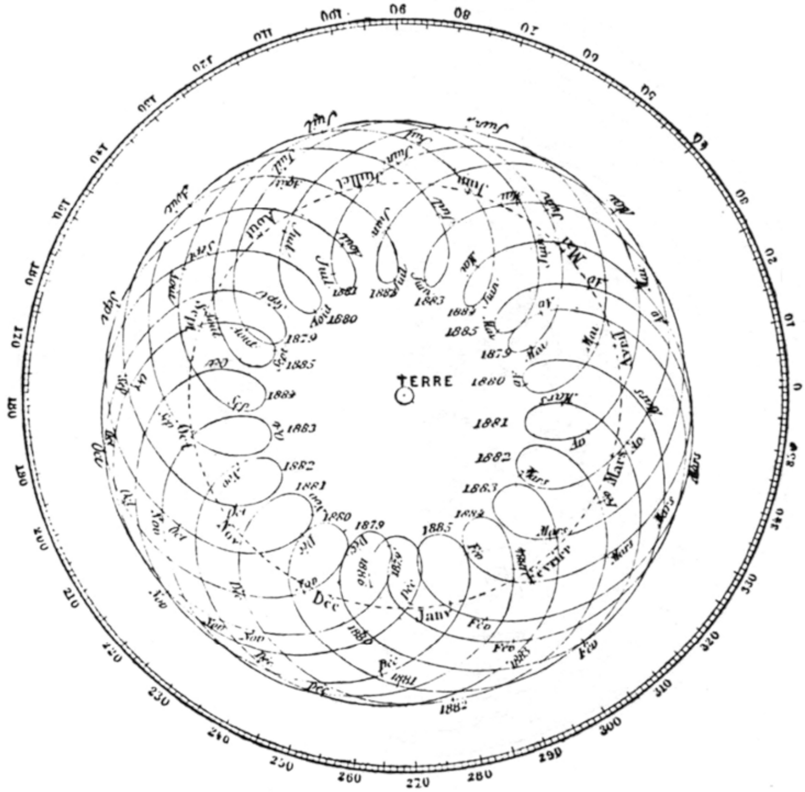

Fig. 10.1. Mercury's orbit seen from the Earth ("Terre"). (From Flammarion's *Astronomie Populaire*.)

In Ancient times, astronomers progressively elaborated a remarkably efficient model giving a precise description, extremely close to the measurements they could perform with their basic instruments. This is the theory of epicycles (see Fig. 10.2) and equants, dating back at least to the time of Hipparchus, which I will not describe in detail and which culminates with the marvelous system of Ptolemy (the *Almageste*, II-nd century). The Earth is in the center and the Sun and the planets turn around the Earth while following finite combinations of uniform circular motions. The reader interested in detailed information concerning Hipparchus and Ptolemy's theories could refer to the article [Gal01].

Ptolemy, one of the greatest geniuses of his time, is only known by contemporary students for his "false" geocentric system theory. And yet! What is a "correct" theory in the fields of physics or astronomy? Isn't the main aim to develop a model which explains experiments of a given era? Isn't any question relative to the "correct" nature of space and time only a metaphysical question which the physicist can ignore?

Copernicus' heliocentric theory superseded Hipparchus/Ptolemy's geocentric theory. Is this new theory more correct than the previous one? One point is clear: Copernicus' theory is nicer and everything seems to fit in a quite harmonious and simple way. This suffices to prefer heliocentrism. But if we take a closer look, Copernicus' theory is not as elementary as it seems. It also uses cycles and epicycles. Ptolemy used 40 cycles and Copernicus still uses 34 of them... The tables established by Copernicus are not more precise than those of Ptolemy. Besides, Copernicus does not present his theory as being "true": he puts at the beginning of his *De Revolutionibus Orbium Coelestium* (1543) a preface, written by Osiander, about which a lot has been written. Did

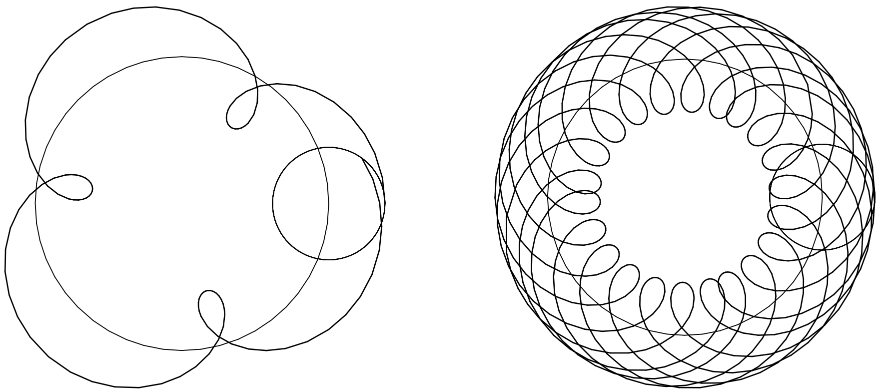

Fig. 10.2. A (simplified) epicycle model. Mercury moves along a small circle ("epicycle") of radius 0.38 (Mercury-Sun distance in Astronomical Units), with period 88 days (Mercurian year), while this epicycle moves along a larger circle ("deferent") of radius 1 AU, with period 1 Earth's year. Left hand side: after one (Earth's) year; right hand side: after seven years

Osiander want to protect Copernicus from the pope's ire? Or on the contrary does this preface reflect Copernicus' opinion? Here is an extract from this preface (see [Cop92]):

> ... it is the duty of an astronomer to compose the history of the celestial motions through careful and expert study. Then he must conceive and devise the causes of these motions or hypotheses about them. Since he cannot in any way attain to the true causes, he will adopt whatever suppositions enable the motions to be computed correctly [...] these hypotheses need not be true nor even probable. On the contrary, if they provide a calculus consistent with the observations, that alone is enough.

Let us return to our cycles. If a phenomenon is periodic with period T, all multiples of T can also be considered as a period. Consequently, if two phenomena have respectively a period T_1 and T_2, the combination of these phenomena will be periodic as soon as a multiple of T_1 coincides with a multiple of T_2, in other words as soon as the ratio T_1/T_2 is a rational number. Since we are talking about astronomy and these periods can only be known approximately, we can consider that these ratios are always (almost) rational. The combinations of the cycles that we observe in our universe define therefore a globally periodic phenomenon. A reader could quite rightly notice that this type of argument may easily lead to gigantic periods and that the physical meaning of a period of e.g. one hundred billion years would be questionable. This reader may be reassured: this question is somehow at the heart of this article and our (pre-pythagorician) "physical hypothesis" that all numbers are rational will be discussed and modified all along this article. Let us therefore start by imagining that all physical functions are periodic...

The idea of combining circles to approach a periodic function may not be due to Hipparchus and Ptolemy but in respect for these geniuses, I would like to attribute them the joint property of the following theorem:

Theorem. [Hipparchus-Ptolemy-Fourier] Let $f : \mathbb{R} \to \mathbb{C}$ denote a continuous periodic curve of period T with values in the complex plane. Then f may be arbitrarily closely approximated by a finite combination of uniform circular motions. In other words, for any $\varepsilon > 0$, there exists a function of the form $f_\varepsilon(t) = \sum_{n=-N}^{N} a_n \exp(2i\pi nt/T)$ (with $a_n \in \mathbb{C}$) such that $|f(t) - f_\varepsilon(t)| < \varepsilon$ for all t.

Clearly, Hipparchus and Ptolemy did not prove this theorem in the modern sense of the term but neither did Fourier[1]. For a "modern proof", the reader can refer to e.g. [Kör89].

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[1] A "theorem" attributed to V.I. Arnold asserts that on one hand no theorem is due to the mathematician which it is named after and on the other hand that this theorem applies to itself.

1. An adelic fantasy.

I would like to allow myself a mathematician's fantasy which is totally useless for the rest of this article and which the reader may skip. The time of contemporary science is modeled by the set $\mathbb{R}$ of real numbers (even if it has been subject to several avatars with the relativity theories). This set does not suggest the idea of successive cycles which we have just mentioned: it flows inexorably from the past to the future. Let us try to formalize time the same way astronomers such as Ptolemy used to think about it, formed by cycles "piled up one on top of the other", in which recurrences are omnipresent.

For any integer $n > 0$, the quotient $\mathbb{R}/n\mathbb{Z}$ formed by real numbers modulo n represents the "cyclic time of period n". If m and n are two integers such that m divides n, there is an obvious projection $\pi_{m,n}$ from the cycle $\mathbb{R}/n\mathbb{Z}$ to the cycle $\mathbb{R}/m\mathbb{Z}$: if we know a real number modulo n, we know it in particular modulo m. Let us define the cyclic time $\mathcal{T}$ as follows: an element t in $\mathcal{T}$ is a map which associates to any integer n an element t_n of $\mathbb{R}/n\mathbb{Z}$ in a way which is compatible with these natural projections, i.e. in such a way that if m divides n, then we have $\pi_{n,m}(t_n) = t_m$. In other words, an element of $\mathcal{T}$ is a way to place oneself in all cycles while respecting the evident compatibilities. Obviously, the "cyclic time" $\mathcal{T}$ contains the "ordinary time" $\mathbb{R}$: to a given real number t, we can associate for every n the point t modulo n in $\mathbb{R}/n\mathbb{Z}$ and these various points are compatible with each other. But $\mathcal{T}$ is much bigger than $\mathbb{R}$ (exercise). We can equip $\mathcal{T}$ with a topological structure which turns it into a compact topological group (exercise). Time as a compact set... a mathematician's (or oriental philosopher's?) dream which illustrates the idea of recurrence. The usual group of real numbers $\mathbb{R}$ is contained as a dense subgroup of $\mathcal{T}$ (exercise). Can one consider $\mathcal{T}$ as a reasonable psychological model for the time we are actually living in? Is this a futile mathematician's exercise? Maybe not. The group $\mathcal{T}$ we have just introduced is the "adelic torus", the study of which is essential in contemporary number theory.

10.2 Kepler, Newton...

I will not describe in detail the marvelous astronomical works of Kepler which are often summarized as Kepler's *three laws*. The first one states that a planet orbits as a conic with the Sun at one focus. The second (law of areas) describes the speed at which this conic is traversed. The third law expresses the period (in the case of an elliptic motion) in terms of the major axis of the ellipse. All of this is far too well-known and can easily be found in many books dealing with rational mechanics. At this point, I would like to insist on two less well-known aspects of Kepler's work.

Kepler is often "blamed" to have only offered a descriptive and nonexplanatory model: what causes the motion of the planets? Newton's law $f = m\gamma$ and the gravitational attraction in $1/r^2$ are wonders but do they explain more than Kepler why objects attract each other? This is similar to the comparison Ptolemy/Copernicus: Newton's laws prevail over those of Kepler by their aesthetic aspect and because they allowed a revolution in physics (and in mathematics). However, they do not explain the cause of the phenomenon (and of course, I could make the same kind of comments on the explanatory character of general relativity).
Kepler's zeroth law : *if the orbit of a planet is bounded, it is periodic, i.e. it is a closed curve.*

If one thinks about this, it is incredible.

Nowadays, one can show the following result (Bertrand's theorem, already known to Newton?). Let us suppose that a material point moves in the plane while being attracted towards the origin of the plane (the Sun) by a force whose modulus $F(r)$ only depends on the distance r to the origin. Let us suppose that all the orbits which are bounded are in fact closed curves. Then, the force $F(r)$ can only be the Newtonian attraction $F(r) = k/r^2$ or the elastic attraction $F(r) = Kr$ (not very reasonable in astronomy!). Why did "mother Nature" "choose" THE law that ensures the periodicity of motion? This is a mystery physics will not explain soon!

How is the motion of a planet if the force of attraction towards the central point is another function $F(r)$? This is a classical question of mechanics and Newton himself studied a great number of cases in his *Principia* (1687). A bounded orbit consists of arcs which join the successive apogees and perigees (see Fig. 10.3). These arcs are obtained from one of them using a symmetry and rotations, the angle of which depends on the considered orbit. Somehow, we can consider that the motion is the result of two periodic phenomena: one relates to the periodic variation of the distance to the Sun and the other

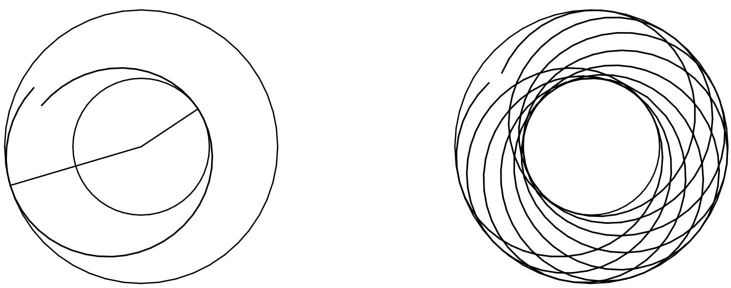

Fig. 10.3. An almost periodic orbit (between the apoapsis and periapsis circles). LHS: an apoapsis and the subsequent periapsis; RHS: after several turns

relates to the periodic variation of the direction of the straight line joining the Sun to the planet. The orbit is periodic if the two periods have a rational ratio and it is almost periodic otherwise. Only the forces in r and $1/r^2$ are such that this ratio is always rational and it happens that it is then equal to 1, so that in these two cases the orbits close themselves in fact after one complete turn. The law $1/r^2$ is a *resonance* of nature since it corresponds to the equality of the radial and angular frequencies.

Kepler must have been filled with wonder when he realized that the orbit of Mars is periodic. This statement is not very obvious when we observe it from the Earth and that does not follow in any way from the epicycle models of Hipparchus-Ptolemy-Copernicus.

I should also mention Kepler's "fourth" law which is rarely cited because it is false, but which Kepler considered as his main discovery. This law was meant to explain the numerical values of the major axes of the orbits of the six planets (which were known at that time). The construction is marvellous, almost philosophical: it is a question of successively encasing the five regular (Platonic) polyhedrons in inscribed and circumscribed spheres (see the beautiful Fig. 10.4 extracted from *Harmonices Mundi* (1619)): the radiuses of the spheres give the radiuses of the orbits (up to similarity of course). Should we make fun of this? Of course not, because it seems that the obtained result is very close to reality and especially because it is an attempt of geometrization of space and motion. Other attempts were very successful later in history. In [Ste69], Sternberg encourages those who make fun of Kepler to also make fun of contemporary theoretical physicists who relate the elementary particles to linear representations of simple Lie groups. The search for groups of symmetries is at the heart of science no matter what the subject is: the icosahedron group, gauge groups, or approximate symmetries in an almost periodic motion, or in a quasicrystal.

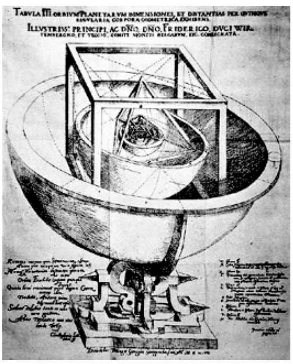

Fig. 10.4. Harmonices Mundi

10.3 An almost periodic world

Thus, the world we inherited from Hipparchus, Ptolemy, Kepler and Newton is a *periodic world*. More precisely, each planet is periodic but the solar system is "almost periodic" in its totality since there is of course no reason that the ratios of the periods of the different planets are rational numbers.

Irrational numbers do exist. The sum of two periodic functions whose periods have an irrational ratio is not periodic. But it almost is... The formalization of this idea is recent. Let us begin with two "reasonable" definitions:

Definition. Let f denote a continuous function from $\mathbb{R}$ to $\mathbb{C}$ and $\varepsilon > 0$ a (small) positive real number. A real number T is an ε-period if for every t in $\mathbb{R}$, one has : $|f(t+T) - f(t)| < \varepsilon$.

Definition. Let f denote a continuous function from $\mathbb{R}$ to $\mathbb{C}$. We say that f is almost periodic if for every $\varepsilon > 0$, there exists a number $M > 0$ such that every interval in $\mathbb{R}$ with length greater than M contains at least one ε-period.

The theory of almost periodic functions is rich. The interested reader may read the book [Ste69], in particular for its link with the history of the celestial mechanics. Here are two theorems. The first one is rather an exercise which is left to the reader:

Theorem. Let $a_1, \dots, a_k$ be complex numbers and $\omega_1, \dots, \omega_k$ real numbers. The function f from $\mathbb{R}$ to $\mathbb{C}$ defined by $f(t) = \sum_{n=1}^{k} a_n \exp(i\omega_n t)$ is almost periodic.

The second theorem is much more complicated. Formally, it is due to Bohr but for the same subjective reasons as those exposed earlier, I also attribute it to Hipparchus and Ptolemy.

Theorem. [Hipparchus-Ptolemy-Bohr] Every almost periodic function may be arbitrarily closely approximated by functions of the preceding type.

Now that these definitions and theorems are presented, I can start to make the content of this article more precise. *Is the universe in which we live almost periodic?*

10.4 Lagrange and Laplace: the almost periodic world

The proof of Kepler's laws using from those of Newton supposes a "simplified" solar system in which a single planet is attracted by a fixed center. One learns in elementary courses of mechanics that the problem is not much more difficult in the case of two masses which attract each other mutually: each one of them describes a conic. But of course, there is not only one planet in the solar system. Even disregarding many "small" objects, we can consider that nine[2]

[2] This paper was written before Pluto was "expelled" from the official list of planets!

2. A remark about the recent history of Physics.

The turbulence of fluids is a quite complex phenomenon which has been puzzling physicists for a long time, at least starting from Leonardo da Vinci, and whose practical applications are more than obvious in aeronautics. How can we understand these eddies of all sizes in turbulent fluids, and the flow of energy from larger eddies towards smaller ones, up to the dissipative scales (Kolmogorov's theory [Kol41])? It is astonishing to note that physicists as eminent and imaginative as Landau and Lifschitz presented for a long time turbulence as an almost periodic phenomenon, of which the number of frequencies depends on the Reynolds number (related in particular to the viscosity of the fluid). It is only with the second edition (1971) of their famous treatise on fluid mechanics that they became aware that the almost periodic functions are too "well behaved" to represent this phenomenon and that it is necessary to call upon much more "chaotic" functions: it is the beginning of the theory of strange attractors, a beautiful example of collaboration between mathematicians and physicists. Old habits are difficult to loose: the epicycles are still present in our scientific subconscious and it is difficult to get rid of them. Should we forget the epicycles and almost periodic functions in the description of our solar system? Are the conservative systems, such as the solar system, also subject to some kind of chaos (and in which sense?), as in the case of the dissipative systems (turbulence)? Somehow, the theorem of Kolmogorov-Arnold-Moser is reassuring: it asserts that under good conditions (explained further on), the almost periodic functions are sufficient to describe the motion of planets.

planets orbit the Sun and attract each other mutually. This N-body problem is mathematically far more complicated and in a sense which I cannot describe precisely here, it has been known since the beginning of the twentieth century that it is impossible to "integrate" it.

For lack of finding "workable" exact solutions for the motion, we are reduced to finding approximate solutions. Lagrange and Laplace are prominent among those who developed best the theory of perturbations. Of course, as a first approximation, the dominant forces in the solar system are the forces of attraction towards the Sun because the mass of the Sun is much bigger than those of the other planets (in a ratio of approximately 10^3). We can thus think that the planets will more or less follow the (periodic) elliptical Keplerian orbits and that those ellipses will change slowly because of the perturbing influence of other planets. How important are these small perturbations? Are they likely to significantly modify the harmony of the Keplerian system? These are difficult questions. We could fear the worst: perhaps a perturbing force of the order of 1/1000 times the principal force could significantly modify the radius of an orbit after a time of about a thousand times the characteristic time of the problem (the year). In other words, we could fear that within a thousand years, the radius of the terrestrial orbit may be divided (or multiplied) by two. This would have important consequences on the history of our civilization! Since we did not notice any catastrophe of this kind in our past, what is the phenomenon that explains why the perturbations perturb less than what we could fear?

The theory of perturbations is complicated and requires many calculations but the basic geometrical idea, such as Gauss explained it, is very simple (like many great ideas). Let us consider a particularly simple case: the Sun, of very large mass, is (almost) fixed; a planet P_1 revolves uniformly on a circular orbit, and another planet P_2 of very small mass compared to P_1 is launched on an orbit around the Sun which is more or less circular and external in comparison to the one of P_1, in the same plane (see Fig. 10.5). Let us imagine that the radius of the orbit of P_2 is really bigger than the one of P_1 so that the angular velocity of P_1 is really bigger than the one of P_2 (according to Kepler's third law). Since the mass of P_2 is very small, one can think that it does not perturb very much P_1 which will therefore stick very closely to its circular trajectory. As for the planet P_2, it is subject to two forces: the main one towards the Sun and a perturbing one towards the planet P_1. The perturbing force is weak but not negligible; its direction oscillates unceasingly because P_1 revolves very quickly. The idea consists of supposing that these oscillations of the direction of the perturbing force can be averaged: in practice, this means that one replaces the revolving planet P_1, by its orbit where one uniformly distributes the mass of P_1. In other words, the planet P_2 is not attracted by a moving planet P_1 but by a circular ring at rest. Is this approximation valid? This is what we will be discussing hereafter. The end of the argument is easy. One knows since Newton that outside the orbit of P_1, the forces of attraction of the Sun and the fixed circular object can be reduced to the force of attraction of a single punctual mass placed in the center. To summarize, everything occurs as if the planet P_2 was subject to the Newtonian force produced by a point whose mass is that of the total mass of the Sun and P_1. Thus, the planet P_2 will almost follow a periodic orbit. In other words, the perturbing forces did not perturb the periodic character of the planet P_2 and this fits with our historical observation: during a few thousand years, the radiuses and the main characteristics of planets did not evolve much.

Many questions are raised by this idea. Is it legitimate to replace a force, whose size and direction vary, by a constant force which is the average of the

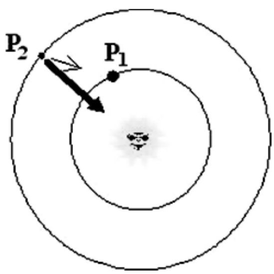

Fig. 10.5. Perturbation of the motion of a small planet P_2 by a planet P_1

varying force? Clearly, there is a situation where this idea cannot work. Let us suppose that the circular orbits which the planets P_1 and P_2 would follow if their masses were infinitely small (and thus unperturbed) are such that the ratio of their periods is rational, 10 for example. This would mean that if the initial positions of P_1 and P_2 are in conjunction e.g. every 10 revolutions of P_1, the two planets are again in exact conjunction. Obviously, to take the average of the perturbation along the orbit of P_1 would not mean much since the angular coordinates of P_1 and P_2 are strongly correlated and the conjunctions are much too regular. On the other hand, if the ratio between the periods is irrational, it seems reasonable to replace the perturbation by its average (see Fig. 10.6). Here is a statement which goes in this direction: it is a particularly simple ergodic theorem (which Lagrange and Laplace did not know, at least explicitly).

Theorem. Let $F(x, y)$ denote a continuous function with real or complex values which depends on two angles x, y considered as elements of $\mathbb{R}/\mathbb{Z}$ (the angle unit is a full turn). Let α and β denote two frequencies whose ratio is irrational. Then, when the time T tends to infinity, the integral $\frac{1}{T}\int_0^T F(x_0 + \alpha t, y_0 + \beta t)\, dt$ converges uniformly to the mean value of F, i.e. to the double integral $\iint F(x, y)\, dxdy$.

Proof. The set of functions F for which the theorem is true is obviously a vector subspace of the space $C^0(\mathbb{R}/\mathbb{Z} \times \mathbb{R}/\mathbb{Z}, \mathbb{C})$ of complex continuous functions on $\mathbb{R}/\mathbb{Z} \times \mathbb{R}/\mathbb{Z}$. This subspace is closed in the uniform topology: a uniform limit of functions which verify the theorem also verifies it. According to Fourier (with two variables), the subspace generated by the functions of the type $\exp(2i\pi(nx+my))$ is dense in $C^0(\mathbb{R}/\mathbb{Z}\times\mathbb{R}/\mathbb{Z}, \mathbb{C})$. It suffices then to verify that each one of these functions $\exp(2i\pi(nx + my))$ satisfies the theorem but this is an explicit and simple calculation which I leave to the reader. QED

Let us come back to Lagrange and Laplace. Given a real number, it is probably irrational and we may think that the method of Lagrange and Laplace is justified. Still, we should be aware that we took a particularly simple case of only one perturbing planet orbiting on an almost circular path. In its principle, the method applies to the other situations. Let us consider an almost Keplerian solar system, with small perturbations and let us average the perturbations

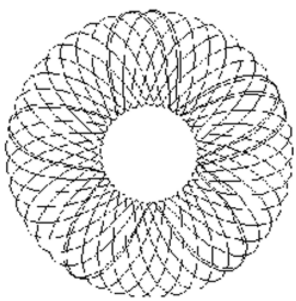

Fig. 10.6. An almost periodic motion

on their configuration spaces. We hope that there are no resonances, i.e. no rational linear relations between the periods which appear. This leads to the *stability theorem* of Laplace which asserts that in the averaged system, the major axes of the orbits remain constant in time, ensuring a certain stability to the system. Finally, this "justifies" the fact that the effects of perturbations are smaller than the ones we could fear *a priori*.

What kind of mathematical credit can we give to this type of "proof"? If we seek "true stability theorems" which are valid for infinitely long times, we will find nothing in Laplace's works which resembles a proof, and the assertions which we sometimes meet according to which "Laplace showed the stability of the solar system" are largely exaggerated. On the other hand, if we seek mathematical statements which are valid for long but finite times, we can hope to transform these methods into theorems, at least in certain particular cases. No matter what, this kind of method lets us think that if the perturbations are of the order of ε (10^{-3} in our system), these perturbations have no global effect at a time $1/\varepsilon$ as we might expect a priori but rather after a time $1/\varepsilon^2$ ("the next term in an asymptotic expansion"). We should have a quiet life for about 10^6 years, which is more reasonable than 10^3. The reader who would like to know more about these perturbation methods may consult some treatises on celestial mechanics if he is brave enough or [Arn89, Arn83, AA68] for a conceptual presentation.

Thus, we inherit from Lagrange and Laplace an almost periodic world, at least for a million years! But they also leave us many questions: what is the role of these resonances between the periods of the planets which put in danger the averaging arguments? Is the stability of the motion perpetual or does it get destroyed after a million years? How can we make this "stability theorem of Laplace" rigorous? It took almost two centuries and the works of mathematicians as powerful as Poincaré, Siegel, Kolmogorov, Arnold and Moser to get to partial answers which themselves raised other questions.

10.5 Poincaré and chaos

At the end of the nineteenth century, Poincaré invented rigorous geometric methods in order to approach a global understanding of the N-body problem. As a matter of fact, he focused on the *restricted three-body problem*: two punctual bodies orbit in a Keplerian way in a plane, around their center of mass, and a third punctual body, with infinitely small mass, is subject to the attraction of the two other masses. Here are some questions studied by Poincaré in his famous article *Sur le problème des trois corps et les équations de la dynamique* (1890) [On the three-body problem and the equations of dynamics]. Is the trajectory of the small mass confined in a bounded domain of the plane if its total energy is sufficiently small? For an initial "generic" condition, is there a risk of collision between the bodies? Is the dynamical behavior of the small body almost periodic? Unfortunately I will not describe this historical article of Poincaré. I will only point out that Poincaré proves the existence of a great number of periodic orbits and that he attempts to understand the dynamics in the vicinity of these periodic orbits. At that point, he makes an error and sins by optimism in a proof (he is used to doing so): his great memoir awarded by king Oscar of Sweden is false. In haste, he has to correct it and this correction will prove to be of considerable scientific richness: Poincaré creates on this occasion the theory of chaos. He highlights trajectories whose behaviors are very far from being almost periodic:

> "Let us try to represent the figure formed by these two curves and their infinite number of intersections each one of which corresponds to a doubly asymptotic solution, these intersections form a kind of web, of fabric, of network with infinitely tight meshes; each one of these curves should never intersect itself, but it must fold up itself in a very complex way to come to cut infinitely often all the meshes of the network. One will be struck by the complexity of this figure, which I do not even attempt to draw. There isn't anything more proper to give us an idea of the complication of the three-body problem and, in general, of all the problems of dynamics where there is no uniform integral and where the Bohlin series are divergent." (Poincaré [Poi90])

The history of this error and the way in which Poincaré transforms it into success is fascinating. I recommend the book [Bar97] which is entirely devoted to this question, and the article [Yoc06].

Thus, even if the initial conditions which lead to these examples of chaotic trajectories are not very close to the physical conditions of our solar system, we know thanks to Poincaré that the orbits of the celestial bodies are not necessarily almost periodic. Will we find such orbits in our solar system? In any case, it is necessary for us to be more modest in our search of stability. Previously, we sought to know whether the orbits of planets are almost periodic and we are now much less ambitious since the question becomes the following one. If we launch the planets of a solar system on almost circular orbits around a Sun with very great mass, will the planets remain forever confined in a bounded domain of space? Might it be possible that a planet be ejected from the system for example?

10.6 A "toy model" of the theory of perturbations

We are going to build up a very simple (and even naive) model. On the cylinder $\mathbb{R}/\mathbb{Z} \times \mathbb{R}$, let us consider the transformation f which associates to the point (x, y) the point $(x + \alpha, y)$ where α is an irrational angle. We are going to iterate this transformation and study its dynamics. This is a first simplification: instead of studying dynamics in continuous time (in $\mathbb{R}$), we are going to use a discrete time (in $\mathbb{Z}$). After n iterations, the point (x, y) is sent to the point $(x+n\alpha, y)$ so that the orbits of f spread on the circles $y = const$. We can thus think about f as the dynamics of an almost periodic system. Now, let us try to perturb the motion by imposing to our point of $\mathbb{R}/\mathbb{Z}\times\mathbb{R}$ a "push" towards the top or the bottom which only depends on the first coordinate. In other words, we are now studying a transformation g which associates to the point (x, y) the point $(x + \alpha, y + u(x))$ where u is a certain very regular function defined on $\mathbb{R}/\mathbb{Z}$ (i.e. a periodic function of period 1) which we can think of as being a small perturbation. What is the new dynamics? The n-th iteration of g maps the point (x, y) to the point $(x + n\alpha, y + u(x) + u(x + \alpha) + \cdots + u(x + n\alpha))$.

Lagrange's averaging principle suggests to replace the impulse u by its average on the circle. Of course, if this average is different from 0, we can easily understand that the successive iterations of g will have a tendency to make the second coordinate tend to infinity so that the perturbed system is not stable. Thus, let us study the situation when the average of u on the circle is equal to 0: on average the second coordinate is not modified. Can we deduce that g is stable, in the sense that its orbits stay bounded? This is the simplified problem we are going to study. In symbols, the question is the following:

Let u denote a periodic function of period 1, which is infinitely differentiable, and whose integral on a period is equal to 0. Let α denote an irrational number and x a real number. Are the (absolute values of the) sums $u(x) + u(x + \alpha) + \cdots + u(x + n\alpha)$ bounded when the "time" n tends to infinity?

Let us begin with a lemma which is a special case of a lemma of Gottschalk and Hedlund:

Lemma. Let us fix x_0 in $\mathbb{R}/\mathbb{Z}$. The absolute values of the sums $u(x_0)+u(x_0+\alpha) + \cdots + u(x_0 + n\alpha)$ are bounded if and only if there exists a continuous function v on $\mathbb{R}/\mathbb{Z}$ such that for all x one has $u(x) = v(x + \alpha) - v(x)$.

Proof. If $u(x)$ is of the form $v(x + \alpha) - v(x)$, the above sum "telescopes" to: $u(x_0) + u(x_0 + \alpha) + \cdots + u(x_0 + n\alpha) = v(x_0 + (n + 1)\alpha) - v(x_0)$. Thus its modulus is bounded by twice the maximum of $|v|$ (which is finite because v is periodic and continuous).

Conversely, let us assume that $|u(x_0) + u(x_0 + \alpha) + \cdots + u(x_0 + n\alpha)|$ is bounded by $M > 0$. This means that the orbit of the point $(x_0, 0)$ in the cylinder $\mathbb{R}/\mathbb{Z}\times\mathbb{R}$ stays confined in the compact cylinder $\mathbb{R}/\mathbb{Z}\times[-M, M]$. Let K denote the closure of this orbit. This is a compact set which is invariant under the transformation g. Among all the non-empty compact sets contained in K and invariant under g, let us choose one which is minimal for inclusion (use the property that the intersection of a family of non-empty compact sets, which is totally ordered for inclusion, is not empty) and let us denote it by $\mathcal{M}$. I claim that $\mathcal{M}$ *is the graph of a continuous function* v *from* $\mathbb{R}/\mathbb{Z}$ *to* $\mathbb{R}$.

To justify this assertion, I first observe that the projection of $\mathcal{M}$ on the first coordinate is a non-empty compact set in the circle, which is invariant under the rotation with irrational angle α. All the orbits of such a rotation are dense in the circle. Consequently, the projection of $\mathcal{M}$ on the first coordinate is necessarily the full circle $\mathbb{R}/\mathbb{Z}$.

Now, let me prove that for each x in $\mathbb{R}/\mathbb{Z}$, the "vertical" line $\{x\} \times \mathbb{R}$ only meets the minimal set $\mathcal{M}$ in one point. In order to prove this statement, I consider the vertical translations $\tau_t(x, y) = (x, y + t)$. Obviously, these translations commute with g so that the image by τ_t of an invariant set under g is also an invariant set under g. Consequently, $\tau_t(\mathcal{M})$ is invariant under g and so are the intersections $\tau_t(\mathcal{M}) \cap \mathcal{M}$. We have chosen $\mathcal{M}$ as a minimal non-empty compact invariant set. It follows that for all t, the intersection $\tau_t(\mathcal{M}) \bigcap \mathcal{M}$ is either empty or equal to $\mathcal{M}$. But if $\tau_t(\mathcal{M})$ would coincide with $\mathcal{M}$ for a t different from 0, then $\mathcal{M}$ would be equal to $\tau_{kt}(\mathcal{M})$ for every integer k and would not be bounded (let k tend to infinity). Therefore $\tau_t(\mathcal{M})$ and $\mathcal{M}$ are disjoint when t is different from 0 and this means that $\mathcal{M}$ meets each vertical $\{x\} \times \mathbb{R}$ at a unique point $(x, v(x))$. Thus, $\mathcal{M}$ is the graph of a function v of $\mathbb{R}/\mathbb{Z}$ to $\mathbb{R}$. As this graph is compact, the function v is continuous (a traditional exercise). The assertion is proven.

We still have to express analytically that the graph of the function v is invariant under the transformation g. The image of $(x, v(x))$ is $(x + \alpha, v(x) + u(x))$ and has to be equal to $(x + \alpha, v(x + \alpha))$. We obtain as expected $u(x) = v(x + \alpha) - v(x)$ and the lemma is proven. QED

Before continuing, let me restate the lemma in a geometric way. As soon as an orbit of the transformation g is bounded, it remains confined in an invariant circle which is the graph of a continuous function. All the other orbits are then bounded. In other words, in this case, the family of circles $y = const$ which is invariant under the non-perturbed transformation f is replaced by the family of perturbed circles $y - v(x) = const$ which is invariant under the perturbed transformation g.

This leads to a question of harmonic analysis. *Given an infinitely differentiable function* u *whose integral on the circle is equal to 0, and given also an irrational number* α*, does there exist a continuous function* v *on the circle such that* $u(x) = v(x + \alpha) - v(x)$ *identically?*

The Fourier series are particularly well adapted to study this problem. As the function u is infinitely differentiable, it can be expanded as a Fourier series:

$$u(x) = \sum_{-\infty}^{+\infty} u_n \exp(2i\pi nx).$$

Let us also seek the function v through its Fourier series expansion (we will discuss the convergence of this series afterwards):

$$v(x) = \sum_{-\infty}^{+\infty} v_n \exp(2i\pi nx).$$

(I use the complex notation for convenience: as the function v is real, the complex numbers v_n and v_{-n} are conjugate). Then we have:

$$v(x+\alpha) - v(x) = \sum_{-\infty}^{+\infty} (\exp(2i\pi n\alpha) - 1) v_n \exp(2i\pi nx).$$

Identifying the Fourier coefficients of $u(x)$ and of $v(x+\alpha) - v(x)$, we thus obtain:

$$v_n = \frac{u_n}{(\exp(2i\pi n\alpha) - 1)}.$$

The assumption according to which α is irrational means that $(\exp(2i\pi n\alpha) - 1)$ is different from 0 for n different from 0. Therefore the v_n's are well defined for n different from 0. For $n = 0$, our hypothesis on the average of u means precisely that $u_0 = 0$ so that we can choose any value for v_0 (which of course corresponds to the fact that if v is a solution to our problem, $v + const$ is also a solution).

To summarize, Lagrange's principle seems to work. We have certainly found a function v which is a solution to our functional equation, or at least its Fourier series expansion. But does this series converge and does it define a continuous function as we expect? This is our new problem.

3. How can we "see" on a Fourier series that it defines a regular function?

Let us consider a periodic function h of period 1 and let us expand it as a Fourier series:

$$h(x) = \sum_{-\infty}^{+\infty} h_n \exp(2i\pi nx).$$

How can we "see" on the sequence of coefficients h_n that the function h is infinitely differentiable for example? If the function h is supposed to be continuous and not more, is the sequence h_n subject to some constraints? These are delicate questions (which Fourier did not seem to have considered) about which we nowadays know a lot. In this interlude, I will simply give some very elementary observations which will suffice for my discussion. The n-th coefficient h_n is given by Fourier's formula:

$$h_n = \int_{\mathbb{R}/\mathbb{Z}} h(x) \exp(-2i\pi nx)\, dx.$$

If h is continuous, then the sequence h_n must be bounded. Caution: the converse is very far from being valid and my bound is rather crude. One can prove e.g. that the sequence h_n tends in fact to 0 and that the series $(nh_1 + (n-1)h_2 + \cdots + h_n)/n$ is convergent.

If h is continuously differentiable, we can calculate the Fourier coefficients h'_n of its derivative by the well-known formula $h'_n = 2i\pi n h_n$. The continuity of the derivative and the previous observation show that there is an estimate for the decay at infinity of h_n of the form $|h_n| < Cst/|n|$. If h is infinitely differentiable, we can repeat this argument for all derivatives. Thus, the Fourier coefficients of an infinitely differentiable function have *a rapid decay.* This means that for every integer k, there exists a constant $C_k > 0$ such that $|h_n| < C_k\,|n|^{-k}$.
Conversely, let us consider a rapidly decreasing sequence h_n and let us form its associated Fourier series. It is easy to prove that this series is indeed convergent and defines an infinitely differentiable function.

These simple remarks will suffice but it is a pity to have to leave such a topic without having really gotten into it. The book [Kör89] is magnificent (but requires more mathematical technique).

4. Numbers which are more or less irrationals?

Every irrational number may be arbitrarily approximated by rational numbers. Let us try to make this assertion quantitative. Let α denote an irrational real number. Let us fix a (small) real number $\varepsilon > 0$ and let us seek a rational number p/q (where $q > 0$) such that $|\alpha - p/q| < \varepsilon$. Such a p/q always exists but if ε is very small, a rational p/q which verifies this inequality has necessarily a very large numerator and denominator. What is the minimal value of q as a function of ε? At what speed does this function tend to infinity when ε tends to 0? All depends on the irrational number being considered. In this interlude, we present the basics of the theory of *diophantine approximation*, which is important in our problem.
Some numbers are exceptionally well approximated by rational numbers. The most famous example is the number defined by Liouville:

$$\lambda = \sum_{n=1}^{+\infty} 10^{-n!} = 0.110001000000000000000001000000000000000000000000\ldots$$

If we truncate the series at order n, we find a rational number whose denominator is $10^{n!}$ and which approximates λ with a difference smaller than $2.10^{-(n+1)!}$, which is extraordinarily small in comparison to the inverse of the denominator $10^{n!}$. For any physicist, this number is rational since it is different from 0.110001000000000000000001 by less than 10^{-120} which is a lot smaller than any physically observable number. Nevertheless, not only does the mathematician know that λ is irrational (its decimal expansion is not periodic) but also that Liouville has proven that λ is in fact a transcendental number. If the reader is not impressed by the approximation speed of λ, he may replace the factorials $n!$ by double factorials $n!!$ or even by any increasing function from $\mathbb{N}$ to $\mathbb{N}$, which may even be non-recursive. Thus, given any function $\varepsilon(q)$ from positive integers to positive numbers, tending to zero when q tends to infinity, we can always find irrational numbers α which are approximated by rationals "better than $\varepsilon(q)$", i.e. for which there exists infinitely many rationals p/q such that $|\alpha - p/q| < \varepsilon(q)$.
Some irrational numbers resist to the approximation as much as they possibly

can. A lemma of Dirichlet shows that every irrational number may be approximated by rationals "up to $1/q^2$":

Lemma. *For any irrational number α, there exists infinitely many rationals p/q $(q > 0)$ such that $|\alpha - p/q| < 1/q^2$.*
Proof. Let us project the first $N+1$ multiples $0, \alpha, \ldots, N\alpha$ in the circle $\mathbb{R}/\mathbb{Z}$. At least two of these projections are at a distance smaller than $1/(N+1)$ in the circle. This means that we can find $0 \leq k_1 < k_2 \leq N$ such that $(k_2 - k_1)\alpha$ is at a distance less than $1/(N+1)$ of an integer p. Writing $q = k_2 - k_1 \leq N$, we obtain $|q\alpha - p| < 1/(N+1) < 1/q$. We observe that $|q\alpha - p| < 1/(N+1)$ implies that q tends to infinity when N tends to infinity. QED

Definition. An irrational number α is diophantine if there exists a constant $C > 0$ and an exponent $r \geq 2$ such that for any rational p/q $(q > 0)$ one has $|\alpha - p/q| > C/q^r$.

5. A diophantine number: the golden mean

The most famous example of a number which is badly approximated by the rationals is the golden mean $\phi = (1 + \sqrt{5})/2$.

Theorem. There exists a constant $C > 0$ such that for every rational p/q, we have $|\phi - p/q| > C/q^2$.

In fact, we could even prove that we may take $C = 1/\sqrt{5}$ and that ϕ is the irrational number which has the worst approximation by rationals (see [Niv56] for a precise statement and for further details on these questions of approximation by rationals).

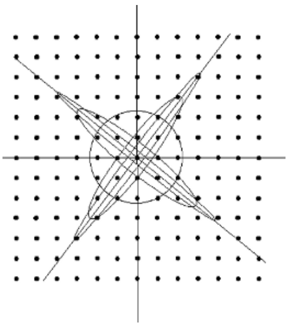

Fig. 10.7. Lattice and eigendirections

Proof. (Outline) Let us consider the matrix $\Phi = \begin{pmatrix} 0 & 1 \\ 1 & 1 \end{pmatrix}$. It has two eigenvalues: ϕ and $-\phi^{-1}$. The slopes of the eigen-directions are also ϕ and $-\phi^{-1}$(see Fig. 10.7). The linear forms $\pi_1(x, y) = y - \phi x$ and $\pi_2(x, y) = y + \phi^{-1}x$ are eigenvectors of the transposed linear map, with eigenvalues $-\phi^{-1}$ and ϕ respectively. The matrix Φ acts linearly in the plane $\mathbb{R}^2$ and preserves the two eigen-lines as well as the lattice of integral points since its coefficients and those of its inverse are integers. Note that Φ dilates the first eigen-line ($\phi > 1$) and contracts the other one. We are seeking to measure the degree of approximation of ϕ by rationals. In other words, we are looking for points on the line of slope ϕ whose coordinates are "as integral as possible". Let us consider a disk D big enough in the plane whose center is the origin. In this disk, there is only a finite number of points with integral coordinates, so that there exists a constant $C_1 > 0$ such that for all integral points in D different from $(0,0)$, we have: $|\pi_1(q,p)\pi_2(q,p)| > C_1$. Let us study the effect of the action of the matrix Φ^n. The disk D is transformed in the interior D_n of an ellipse, laid down along the line of slope ϕ, and the estimate $|\pi_1(q,p)\pi_2(q,p)| > C_1$ for the integral points (q,p) different from 0 and located in D implies the same inequality for all integral points of D_n different from 0. This is clear because the product $|\pi_1\pi_2|$ is invariant under the action of Φ. Thus the inequality $|\pi_1(q,p)\pi_2(q,p)| > C_1$ is valid for all integral points in all the D_n's. When n varies in $\mathbb{Z}$, these D_n's cover a whole "hyperbolic" neighborhood of the eigen-lines, of the form $|\pi_1(x,y)\pi_2(x,y)| < C_2$. To summarize, we have proven that there exists a constant $C_3 = \min(C_1, C_2)$ such that for any integral point (q,p) of the plane (different from $(0,0)$), we have $|\pi_1(q,p)\pi_2(q,p)| > C_3$. Now, let us distinguish two sets of rationals p/q according to whether $|\phi - p/q|$ is greater than or less than a fixed small enough quantity $C_4 > 0$. On the first set, the inequality $|\phi - p/q| \geq C_4$ implies in particular that $|\phi - p/q| \geq C_4/q^2$. On the second set, the inequality $|\phi - p/q| < C_4$ implies an inequality of the form $|\pi_2(q,p)| > C_5|q|$ (in fact $C_5 = \phi + \phi^{-1} - C_4 = \sqrt{5} - C_4$ is appropriate) so that we have $|\pi_1(q,p)| > C_3C_5^{-1}/|q|$ and so $|\phi - p/q| > C_3C_5^{-1}/q^2$. Thus indeed we have $|\phi - p/q| > C_6/q^2$ for all integral points different from 0 with $C_6 = \min(C_4, C_3C_5^{-1})$. QED

10.7 Solution to the stability problem "in the toy model"

Let us take up the problem again. Starting from a function u on the circle, whose integral is 0, and which is infinitely differentiable, we seek to know whether there exists a continuous function v whose Fourier coefficients are given for n different from 0 by

$$v_n = \frac{u_n}{(\exp(2i\pi n\alpha) - 1)}.$$

Since u is infinitely differentiable, the sequence of Fourier coefficients u_n is rapidly decreasing (see Box 3). The terms $(\exp(2i\pi n\alpha) - 1)$ which appear in the denominator are different from 0 but they may be arbitrarily small because α is irrational. This is the *small divisors phenomenon.* These denominators could be so small that the Fourier coefficients v_n may become very big and the Fourier series of v may diverge. Therefore, the difficulty is to know who is winning: is it the numerator which rapidly tends to zero or the denominator which may be very small? The answer, which the reader has already guessed, depends on the quality of the approximation of α by the rationals.

First of all let us assume that α satisfies a diophantine condition $|\alpha - p/q| > C/q^r$ (see Box 4). Let us note that $|(\exp(2i\pi n\alpha) - 1)|$ is nothing else than the euclidian distance between the points 1 and $\exp(2i\pi n\alpha)$ on the unit circle in the complex plane. Since the length of a chord is bigger than $2/\pi$ times the length of the arc which subtends it, we may write that $|\exp(2i\pi n\alpha) - 1|$ is $2/\pi$ times bigger than the length of the circular arc joining 1 to $\exp(2i\pi n\alpha)$, i.e $2/\pi \times 2\pi \times$ the distance between $n\alpha$ and the closest integer p. Thus, we obtain an estimate of the small divisor of the form:

$$|\exp(2i\pi n\alpha) - 1| > 4C/|n|^{r-1}.$$

Since u_n is rapidly decreasing, there exists for every k a constant C_k such that $|u_n| < C_k n^{-k}$. Thus, we obtain an estimate for the Fourier coefficients:

$$|v_n| < (C_k/4C)/|n|^{(k-r+1)}.$$

Since this is valid for every k, the sequence v_n is rapidly decreasing and hence the Fourier series converges to an infinitely differentiable function v. In other words, the continuous function v exists and the perturbed motion g is stable. In this case, we have obtained our justification of the Lagrange-Laplace method, at least under the diophantine condition and in the (naive) framework of our "toy model".

If the rotation angle of the non-perturbed motion is diophantine, the perturbed motion is always stable, whatever the perturbation u (assumed to have 0 integral and to be infinitely differentiable).

What happens if α is not diophantine, e.g. if it is the Liouville number we previously defined? *We may then construct unstable examples i.e. for which the averaging method does not work.* Let $\alpha = \lambda$ denote the Liouville number. We know that there exists a sequence of integers p_k such that $|\alpha - p_k/10^{k!}| < 2.10^{-(k+1)!}$. Thus, for every k, we have $|\exp(2i\pi 10^{k!}\alpha) - 1| < 2\pi.2.10^{k!-(k+1)!} = 4\pi.10^{-k.k!}$ (this time, note that a chord is smaller than the arc which subtends it). Let us construct a sequence u_n as follows. Let $u_0 = 0$ and $u_n = 0$ if $n > 0$ is not an integer of the form $10^{k!}$ and let $u_{10^{k!}} = k.(\exp(2i\pi 10^{k!}\alpha) - 1)$. Finally, let us define u_n for $n < 0$ by $u_n = \overline{u_{-n}}$ for $n < 0$. This sequence is evidently rapidly decreasing because $k.10^{-k.k!} = k.(10^{k!})^{-k}$. This defines the periodic function u (with real values) infinitely differentiable and with 0 integral. When we compute the corresponding coefficients v_n, we find, by their very construction, that $v_n = 0$ if n is not of the form $10^{k!}$ and $v_{10^{k!}} = k$ so that the v_n's are not bounded. Thus, there does not exist any continuous function v whose Fourier coefficients are the v_n's and our problem has no solution: there is no continuous function v such that $u(x) = v(x + \alpha) - v(x)$. We know that this means that the perturbed motion is not stable and that the averaging method does not apply.

The theorem of Kolmogorov-Arnold-Moser is analogous: it asserts that the averaging principle works if the frequencies which come into play are diophantine and if the perturbations are weak enough. A (slightly more) precise statement will be given in the following lines.

10.8 Are the irrational diophantine numbers rare or abundant?

We are all convinced that rational numbers are rare among real numbers, even if it took a lot of work from the mathematicians of the past to be clearly conscious of this fact. For a contemporary mathematician, who is used to the infinite sets *à la* Cantor, the explanation is easy: the rational numbers are countable whereas the real numbers are uncountable. For this reason, to assume that the ratio of the periods of two planets is irrational seems reasonable and the converse has very little chance of happening.

We saw in the previous paragraph that the "rational/irrational" distinction in celestial mechanics should better be replaced by a "non-diophantine/ diophantine" one. I have already explained that the Liouville number, although being mathematically irrational, is "physically rational" and we have just noted that if a frequency is equal to this Liouville number, the averaging method may fail.

Are the diophantine numbers abundant? There are essentially two possible mathematical definitions for abundance and it happens that the answer depends on the choice of the definition:

The first possible approach is that of *Lebesgue's measure*. Let us say that a subset X of $\mathbb{R}$ is *negligible in the sense of Lebesgue* or that it has *0 Lebesgue measure* if for every $\varepsilon > 0$, we may find a countable collection of intervals $I_n \subset \mathbb{R}$ whose sum of lengths is smaller than ε and whose union covers X. Let us say that $X \subset \mathbb{R}$ is of *full Lebesgue measure* if its complement is negligible in the sense of Lebesgue. One of the most interesting aspects of this concept is that the union of a countable collection of negligible sets is negligible. Of course, what is important for this theory to work is that a set cannot be both negligible and of full measure. This is an exercise left to the reader.

The second approach is due to Baire. Let us say that a subset X of $\mathbb{R}$ is *meager in the sense of Baire* if it is contained in a countable union of closed sets of empty interiors. Let us say that X is *residual in the sense of Baire* if its complement is meager. As with the previous definition, the countable union of meager sets is meager (easy) and a set cannot be both meager and residual (this is Baire's theorem).

Which notion of abundance is best adapted to our intuition? The question is delicate and sometimes generates violent polemics among mathematicians. For the case we are interested in, i.e. the abundance of diophantine numbers, the situation is caricatural.

Theorem. The set of irrational diophantine numbers is both meager in the sense of Baire and of full Lebesgue measure.

The proofs are not difficult but they are instructive. Let us write the definition of the set Dioph $\subset \mathbb{R}$ of diophantine numbers by using quantifiers:

$$\mathrm{Dioph} = \{\alpha \in \mathbb{R} \,|\, \exists r \in \mathbb{N}\, \exists n \in \mathbb{N}\, \forall (p,q) \in \mathbb{Z} \times \mathbb{N}^\star \ : |\alpha - p/q| \geq \frac{1}{nq^r}\}.$$

Thus Dioph is a countable union indexed by r and n of closed sets which are clearly of empty interiors: Dioph is meager in the sense of Baire.

In order to prove that Dioph is of full Lebesgue measure, let us fix a real $r > 2$ and let us consider the set

$$\mathrm{Dioph}_r = \{\alpha \in \mathbb{R} \mid \exists C \in \mathbb{R}_+^\star \ \forall (p,q) \in \mathbb{Z} \times \mathbb{N}^\star \ : |\alpha - p/q| \geq C/q^r\}.$$

It suffices to prove that Dioph_r is of full Lebesgue measure because $\mathrm{Dioph}_r \subset$ Dioph. In order to prove this, we show that its complement meets the interval $[0,1]$ on a negligible set in the sense of Lebesgue (note that Dioph is invariant under integral translations). Indeed $[0,1] \setminus \mathrm{Dioph}_r$ is the intersection with $[0,1]$ of the following sets defined for $C > 0$:

$$\mathrm{NonDioph}_{r,C} = \bigcup_{q=1}^{+\infty} \bigcup_{p=0}^{q} \left] \frac{p}{q} - \frac{C}{q^r}, \frac{p}{q} - \frac{C}{q^r} \right[.$$

This is a countable union of intervals whose sum of lengths is smaller than $2C \sum_q \frac{q+1}{q^r}$. This sum converges because $r > 2$ and the sum is arbitrarily small if C is small enough. Thus, by definition, $\mathrm{NonDioph}_{r,C}$ is negligible and this proves that Dioph is of full Lebesgue measure. QED

Of course, the previous statement is not mathematically contradictory but it leaves us in an awkward situation. Which meaning will the physicist rather give to the concept of abundance? My personal experience seems to show that physicists do not either have any miraculous solution to suggest. I will come back to this question in the last section but for now let us do “as if” the good concept was that of Lebesgue.

We can therefore conclude that the set of rotation angles for which the perturbed motion is stable is of full Lebesgue measure and we should therefore be satisfied with this result since it covers most of the cases (but we should not forget that if we had preferred Baire to Lebesgue, we should have had the opposite conclusion).

10.9 A statement of the theorem of Kolmogorov-Arnold-Moser

It is difficult to give a clear-cut statement of the KAM theorem. I will first start by stating a precise theorem which is a special case and I will then try to describe the general theorem, but I will need to be much fuzzier then.

Let us consider a transformation f of the cylinder $\mathbb{R}/\mathbb{Z} \times [-1,1]$ defined this time by $f(x,y) = (x+y, y)$. Again in this case, the circles $y = const$ are invariant and f induces a rotation on each one of them but contrarily to the “toy model”, the angle of this rotation depends on the circle since it is equal to y. This map is often called a “twist” for obvious reasons. Now, let us perturb f, i.e. we consider a map g of the form

$$g(x,y) = (x + y + \varepsilon_1(x,y), y + \varepsilon_2(x,y)).$$

As a matter of fact, we ask that g maps the cylinder to itself, i.e. that $\varepsilon_2(x, \pm 1) = 0$ identically. We also assume that g preserves the area, i.e. that its jacobian is identically equal to 1. Let us fix an irrational number α in the interval $[-1, +1]$ and let us suppose that it is diophantine. *The KAM theorem asserts that if $\varepsilon_1, \varepsilon_2$ are small enough, then there exists a curve which is invariant by g, close to the curve $y = \alpha$, and on which the dynamics of g is conjugate to a rotation of angle α.*

We must first give a meaning to "$\varepsilon_1, \varepsilon_2$ small enough". The initial theorem was formulated in 1954 by Kolmogorov in the space of real analytic functions and it is with respect to this (exotic) topology that we may understand the smallness [Kol54]. Kolmogorov only gave global indications on the proof and it is Arnold who gave the rigorous proof of this theorem in 1961, still in the analytical case [Arn63a]. In 1962, Moser succeeded in accomplishing the feat of proving the theorem in the space of infinitely differentiable functions [Mos62]. In fact, Moser used functions which are 333 times differentiable and the topology of uniform convergence on these 333 derivatives... The mere fact that it is necessary to use as many derivatives shows the difficulty of the proof. Nowadays, it is known that the theorem is true with 4 derivatives and false with 3 [Her86].

I have to give up the idea of giving even a sketch of a proof of the theorem. I would simply like to explain that, contrarily to the toy model case, this is a *nonlinear* problem in the (infinite dimensional) space of curves. The linearization of this problem essentially leads to the problem we have already discussed. To switch from a nonlinear problem to a linear problem, the mathematician uses the implicit function theorem, which is correct in a Banach space but false in the Fréchet spaces which occur here. This is why this theorem requires quite formidable techniques of functional analysis (see about this point in the second part of [Her86]).

Each diophantine number α has a corresponding neighborhood in which the theorem applies. The more diophantine α is, i.e. the more difficulties it encounters to be approximated by rationals and the more the invariant circle of angle α is robust under the effect of the perturbation. Thus, given a perturbation $(\varepsilon_1, \varepsilon_2)$, we cannot apply the theorem to every diophantine number. Typically, given the perturbation, some invariant circles remain and the others "break down". Furthermore, the theorem warrants that for a small enough perturbation, the Lebesgue measure of the set of circles which remain is arbitrarily close to the full measure. Thus, we may say that if we perturb f a little, there is every chance that an orbit remains located on a circle and be almost periodic. The situation in the so-called *instability zone*, outside these invariant circles, is very complicated: a lot of problems remain open and research keeps being very active.

What is the link between this theorem and celestial mechanics? Let us consider the restricted three-body problem: two masses revolve one around the other in a Keplerian way and a third infinitely small mass orbits in the same plane. This third mass is attracted by the two others but does not perturb them. In order to describe the dynamics of the third mass, we introduce the phase space: two position coordinates and two velocity coordinates are needed, which gives a space of dimension 4. The conservation of total energy forces the third object to stay in a 3-dimensional submanifold. So, we have to study the dynamics of a vector field in a certain 3-dimensional manifold. For this purpose one can use the method of Poincaré's sections which consists in studying the successive returns of the orbit on a surface transverse to the vector field. This leads to iterate a transformation in dimension 2 of the type we previously considered. Without any detail, the KAM theorem we have cited allows to prove the stability of the system formed by these three bodies. Many more pages, formulae and pictures would be needed to justify this point.

When we consider a "real" solar system, with many planets, the phase space and Poincaré's sections are of higher dimension, and the invariant circles need to be replaced by invariant tori of higher dimensions. This complicates the statement of the theorem but the spirit remains the same: these invariant tori resist the perturbations if the frequency ratios in the initial system are diophantine enough. The general KAM theorem deals with this case.

Thus, the "physical" consequence of KAM is the following. *If we launch a system of planets of small enough masses around a Sun of big mass in initial conditions which are close to that of a Keplerian system, the dynamics which will result from this will be almost periodic, at least for a set of initial conditions whose Lebesgue measure becomes fuller and fuller as the masses of the planets tend to* 0. Outside this set of initial conditions, the theorem does not say anything, apart from the fact that they are rare (in the sense of Lebesgue measure).

This is the reason why our solar system "stands a good chance of being almost periodic"...

10.10 Is the KAM theorem useful in our solar system?

The KAM theorem and its proof are magnificent. From a certain view point, this may suffice to the mathematician. I have no intention of debating here in a few lines of the complex relationship between mathematics and physics but the KAM example could undoubtedly be used as a starting point.

Originating from Physics, the problem has generated a whole branch of mathematics which perfectly suffices to itself and which also generates some other problems which are often totally without any physical content. But it seems to me that even the "purest" mathematician has the duty to go back to the initial problem: has it been solved? Here are some elements of answer:

The KAM theorem applies in the case of "small enough" masses. If we closely study the proof we realize that it applies to very small masses, smaller by several orders of magnitude than what is observed in our solar system. It would clearly be useful to obtain efficient and effective versions of KAM, let us say for masses 1/1000 times the mass of the Sun. We are still very far away from this and, unfortunately, few colleagues find this mathematical issue to be fascinating.

The forces which act in the solar system are mostly gravitational but other forces are non-hamiltonian (e.g. the solar wind can “slow down” the planets). After several hundred thousands years, the effects are perhaps not negligible and the KAM theorem cannot help us to understand the situation. Indeed, is there an interest other than philosophical or mathematical to prove that the “theoretical” (= hamiltonian) solar system is stable or instable? The physicist wants to understand the situation for the near future (let us say that a few billion years would suffice him).

The union of the invariant tori given by the theorem has a large Lebesgue measure but it has an empty interior. Which is the good abundance concept in physics? As I have already explained earlier, mathematicians cannot answer this question and physicists have to show them the way.

Experience shows that many frequencies encountered in the solar system seem to be very rational. The following example, taken from [Bel86], is really impressive. Let us consider the angular frequencies ω_i^{obs} $(i = 1, ..., 9)$ of the 9 planets (measured in such a unit that the frequency of Jupiter equals 1). It turns out that when we modify *very slightly* these values, we can find “theoretical” frequencies ω_i^t which are *exactly* linked together with integral linear relations: the following table exhibits a 9×9 matrix with small integer entries, with a lot of zeros, which exactly anihilates the vector of theoretical frequencies. Note that the discrepancies $\Delta\omega/\omega = (\omega^{obs} - \omega^t)/\omega$ are extremely small.

	Planet	ω_i^{obs}	ω_i^t	$\Delta\omega/\omega$	n_1	n_2	n_3	n_4	n_5	n_6	n_7	n_8	n_9
1	Mercury	49.22	49.20	0.0004	1	–1	–2	–1	0	0	0	0	0
2	Venus	19.29	19.26	0.0015	0	1	0	–3	0	–1	0	0	0
3	Earth	11.862	11.828	0.0031	0	0	1	–2	1	–1	1	0	0
4	Mars	6.306	6.287	0.0031	0	0	0	1	–6	0	–2	0	0
5	Jupiter	1.000	1.000	0.0000	0	0	0	0	2	–5	0	0	0
6	Saturn	0.4027	0.4000	0.0068	0	0	0	0	1	0	–7	0	0
7	Uranus	0.14119	0.14286	–0.0118	0	0	0	0	0	0	1	–2	0
8	Neptune	0.07197	0.07143	0.0075	0	0	0	0	0	0	1	0	–3
9	Pluto	0.04750	0.04762	–0.0025	0	0	0	0	0	1	0	–5	1

The book [Bel86] contains a very interesting paragraph on these resonances which are observed in our solar system. It contains in particular a discussion on the “hypothesis of Moltchanov” according to which “every oscillatory system having been subject to an extended evolution is necessarily in resonance and is governed by a family of integers”. Thus, for Moltchanov, the small non-hamiltonian forces keep the systems away from the diophantine frequencies and push them in the zone where the KAM theorem does not apply... It seems to me that justifying or invalidating this hypothesis remains a magnificent challenge for today’s mathematicians.

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