Michel Hénon and the Stability of the Solar System

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I. Introduction

My first meeting with Michel Hénon was at the occasion of a conference organised by Claude Froeschlé on dynamical interactions in the Solar System at Aussois in the french Alps. During this conference, one free afternoon was devoted to a hike in the mountain. One should never stress enough the importance of these free times in scientific conferences, when there are no talks, and thus when people can discuss freely about their projects. At the time, I was thinking of starting a study group for the reading of the "Méthodes Nouvelles de la Mécanique Céleste", the masterpiece of Henri Poincaré. As stated by Ivar Ekeland (Ekeland, 1987), it appeared that this fundamental work was often quoted but never read. Discussing of this project to colleagues, I had often some reaction saying that, it was interesting, but too difficult. On our way back from the shelter of the Fond d'Aussois, in this nice autumn afternoon, I had a long discussion with Michel Hénon who strongly encouraged me to continue in this project. Later on, I had an enthusiastic support from Alain Chenciner, and we started this three year project on the reading of Poincaré. This study, which could appear as unusual at present for a young researcher, who is normally pushed to publish as much as possible, instead of spending time reading a 100 years old book, had profound implications as it was at the origin of the creation of our multidisciplinary research group (Astronomie et Systèmes Dynamiques) in the Bureau des Longitudes, gathering astronomers and mathematicians. Moreover, the numerous discussions that we had during these lectures of Poincaré were of fundamental help for me for the understanding of the chaotic behaviour of the Solar System (Laskar, 1989, 1990).

II. Historical background

The question of the stability of the Solar System is a very old question that was already raised by Newton in his Opticks volume (Newton, 1706, 1718). He envisioned the presence of God in the apparent ordering of the planets, but on the opposite, he admitted that the perturbations among the planets may lead to large instabilities in a way that a divine intervention would become necessary. This was highly contested by the rival of Newton, Leibniz, who found that suggesting that the clockwork of God does not achieve indefinite stability and needs to be mended from time to time, is contesting the power of God. The question of the stability of the Solar System was thus a question of limiting or not the divine power, and was one of the main scientific questions of the XVIIIth century. It was even more important as after Kepler already in 1625, Halley had found that it was necessary to add some empirical linear drift in the mean motions of Jupiter and Saturn in order to retrieve the Chaldean observations.
transmitted by Ptolemaea (see Laskar, 2013). Jupiter was going towards the Sun while Saturn was escaping from the Sun. It was thus natural for Newton to question the stability of the Solar System, and it was not until the end of the XVIII century that the question found a satisfying answer. Indeed, Laplace demonstrated that at first order, the mean values of the semi-major axis are constant. He also showed that the apparent variations of the mean motion over the past two millennium were due to an oscillation of the longitude resulting from the proximity of the 5:2 resonance between the motions of Jupiter and Saturn. Laplace thus demonstrated that no instabilities could arise from the varying size of the planetary orbits. Lagrange and Laplace completed the first "proof of the stability of the Solar System" by demonstrating that at first order in the masses, eccentricities and inclinations, the planetary orbits suffer precession motion of their perihelion and node, and oscillations of their eccentricity and inclination, but of small values that do not allow for collision (see (Laskar, 2013) for a detailed account).

As stated by (Poincaré, 1897), many other "proofs" of the stability of the Solar System will follow. It does not mean that the previous ones were insufficient, but that they addressed only to some approximation of the problem, which may be far from reality. Poincaré himself demonstrated that the three body problem is not integrable, and that the series used by the astronomers were divergent, thus not allowing to decide for the stability of the Solar System in infinite time. He also exhibited the very complex behaviour that can have the solutions of the three body problem, describing in great details the intricate imbrication of the stable and unstable varieties in the vicinity of an hyperbolic fixed point. It is in these regions that originate the chaotic solutions that are very sensitive to small changes of their initial conditions (Poincaré, 1899). In fact, Poincaré did not seem to think that his results apply to the physical Solar System, and in (Poincaré, 1897), he assumed that the small tidal dissipative effects were more important than the chaotic diffusion, and thus could stabilise the system which would end in a state of rigid rotation, where all the planets would be synchronised, and synchronised with the rotation of the Sun. Although the tidal effects do exist, it can be estimated that their effect is much smaller than what Poincaré forecasted, and we can for example estimate to the order of $10^{15}$ billion years, the time needed to circularise the orbit of Jupiter (Correia and Laskar, 2011).

In addition, although Poincaré demonstrated that there cannot be a domain of initial conditions with integrable, regular solutions, he did not excluded that there could be some special conditions under which the perturbation series would converge, and thus the solutions would be regular (Poincaré, 1892). Sixty years later, Kolmogorov (1954) actually demonstrated the possibility of regular quasiperiodic solutions in a perturbed Hamiltonian system. The convergence of the series is obtained under the conditions that the perturbation is small and that the frequencies of the system verify some Diophantine condition, which for two degrees of freedom systems means that the frequency ratio is far from the rationals (see Chierchia, 2006; Dumas, 2014).

III. Regularity and chaos in Hamiltonian systems

A first application of Kolmogorov theorem to a planetary problem was later on provided by (Arnold, 1963a,b), in a planar case, for a ratio of the semi-major axis
close to zero. This impressive result was considered as another "demonstration" of
the stability of the Solar System, and is quoted as such by Jurgen Moser in a general
audience paper (Moser, 1978). Moser himself contributed to the theory with an alternate
demonstration of Kolmogorov theorem, valid for a larger class of functions (Moser,
1962). These results are since known as KAM theory (Kolmogorov-Arnold-Moser).
One of the main result of the KAM theory, is to state that regular quasiperiodic solutions
may exist in places where it was previously thought that all solutions will be unstable.
Moreover, as the perturbation goes to zero, the density of the regular solutions goes to
unity. This was at first considered as a mathematical curiosity, but very soon, Michel
Hénon was the first to show numerical evidence of this behaviour of conservative
(Hamiltonian) systems, in a celebrated paper on the dynamics of stars in a galactic
potential (Hénon and Heiles, 1964). It appeared clearly that the general setting described
by the KAM theory, with an intricate mixture of regular and chaotic trajectories was
actually present in realistic physical models.

These numerical simulations were of great interest for Arnold who was at the time
in Paris. It was actually unusual for a mathematician to be interested by numerical
simulations. For most of the mathematicians of the time in France, largely influenced by
the Bourbaki rewriting of all mathematics in a very formal way, numerical simulations
were of little interest. Arnold wrote a long letter to Hénon (in french) that has been
preserved and where he proposes some interesting questions related to the numerical
experiments of Michel Hénon (Fig. 2). In these six pages he discusses various problems
that could be addressed by numerical methods.
IV. HÉNON’S COMMENT ON ARNOLD’S THEOREM

In the archives of M. Hénon, at the end of this long letter from Arnold, was attached a short comment on Arnold’s theorem on the planetary problem (Fig.3). This note can
be transcribed as follows

We have (ARNOLD, 1963, Usp. mat. Nauk., n° 5, p. 16)

\[
\delta^{(3)} \leq e^{2n}(32n^2 + 100n)^{-2n}; \]
\[
\delta^{(5)} \leq \delta^{(3)}
\]

and, p. 23

\[
\delta_1 < \delta^{(5)} ; \]
\[
M = \delta_1^{8n+24}.
\]

from where

\[
M < \left( \frac{e}{32n^2 + 100n} \right)^{2n(8n+24)}
\]

\textit{M is thus extremely small} : for \(n = 2\), the smallest value of interest, we have :

\[
M < \left( \frac{e}{328} \right)^{160} < 10^{-320} !!
\]

Thus the theorem has only a theoretical interest and is absolutely not of practical use, at least in the presented form (cf p.18, §1.5, 23) : The perturbation needs indeed to be extremely small.

As an example, the stability of the Solar System is not demonstrated.

In this note, Hénon makes an estimate of a bound on the maximal size of the perturbation that is allowed for the application of Arnold theorem. The value of this bound, which can be thought as the ratio of the mass of the planet with respect to the Sun, is extremely small. For the most favourable case, with only two degrees of freedom, it provides a constant of \(10^{-320}!!\).

When I started to study the long time behaviour of the Solar System, in the 80s, I was not aware of the manuscript note of Michel Hénon, but I knew the result, as it had been published in (Hénon, 1966), without the remark on the Solar System stability. He just stated there:

\[Ainsi, ces théorèmes, bien que d’un très grand intérêt théorique, ne semblent pas pouvoir en leur état actuel être appliqués à des problèmes pratiques, où les perturbations sont toujours beaucoup plus grandes . . .\]

These statements were important for me as I read them in the late 80s, as they showed that the question of the stability of the Solar System was not solved. At the time, the main idea was that the Solar System is \textit{probably stable (by any reasonable definition of the term) over time scales comparable with its age} (Murray, 1988). After the results
of Laplace and Lagrange on the stability at first order, many other "proofs" had come, the latest one being the demonstration of Arnold. At the time, the mathematicians were continuing their efforts, in order to provide rigorous proofs of this stability for a realistic model. But the estimate of Hénon showed that the path to these rigorous proofs was still long.

V. Arnold’s paper

Although I had read the comment of Michel Hénon, I had no clue on how he was able to derive his estimate from Arnold (1963a) paper. Indeed, this paper is very hard to read, and even after its study made during the thesis work of Philippe Robutel on the prolongation of Arnold’s work (Laskar and Robutel, 1995; Robutel, 1995), the derivation of Hénon was not clear. At the occasion of the present celebration of Michel Hénon, I went back to the paper of Arnold, and specifically searched for the estimate of Hénon. It appears that the derivation is actually relatively simple, once the derivation of Arnold is taken for granted. The bound to search for is the $M$ bound in equation (2) of Fig.4. In fact, following the notes of Hénon, it becomes easy to understand the derivation of the estimate. In page 16, we have
§2. Formulation of the Theorems

2.1. THEOREM 1. Suppose that the Hamiltonian function $H(p, q)$ is analytic in the domain $F$: $p \in G$, $|\Im q| \leq \rho$ and has period $2\pi$ in $q = q_1, \ldots, q_n$. Let $H = H_0(p) + H_1(p, q)$, where $H_0$ is in the domain $F$ and $H_1$ is analytic in $G$. If 

$$\det \frac{\partial H_0}{\partial p_1, \partial p_j} \neq 0,$$  

(1)

Then for any $\kappa > 0$ there exists $M = M(\kappa, \rho, G, H_0) > 0$ such that if in $F$ we have 

$$|H_1| \leq M,$$  

(2)

then the motion defined by the canonical equations 

$$p = -\frac{\partial H}{\partial q}, \quad q = \frac{\partial H}{\partial p},$$  

(3)

has the following properties:

1°. There exists a decomposition $\Re F = F_1 + F_0$, where $F_1$ is invariant (i.e. together with the point $p, q$ contains the trajectory $p(t), q(t)$ of the motion (3) passing through it), and $F_0$ is small: \[ \text{mes } F_0 \leq \kappa \text{ mes } F \].

2°. $F_1$ is composed of invariant $n$-dimensional analytic tori $I_\omega$, defined parametrically by the equations 

$$p = p_0 + f_\omega(q), \quad q = Q + g_\omega(Q),$$  

(4)

where $f_\omega, g_\omega$ are analytic functions of period $2\pi$ in $Q = Q_1, \ldots, Q_n$, and $\omega$ is a parameter determining the torus $I_\omega$.

3°. The invariant tori $I_\omega$ differ little from the tori $p = p_\omega$:

$$|f_\omega(Q)| < \kappa, \quad |g_\omega(Q)| < \kappa.$$  

(5)

4°. The motion (3) on the torus $I_\omega$ is quasi-periodic with $n$ frequencies $\omega_1, \ldots, \omega_n$:

$$Q = \omega, \quad \omega = \frac{\partial H_0}{\partial p} |_{p_\omega}$$  

(6)

\[ \text{FIGURE 4. Arnold's theorem (Arnold, 1963a).} \]

Most of the estimates of $\delta^{(3)}$ are complicated, but it is sufficient to consider the simplest one, which expresses easily in term of $n$, that is 

$$\delta^{(5)} \leq \delta^{(3)} \leq e^{2n}(32n^2 + 100n)^{-2n}.$$  

(5)
Following Hénon, we have then in page 23

2°. If we can find \( M(\kappa, \rho, G^{(i)}, H_0) \) in each of the domains \( G^{(i)} \), then

\[
M(\kappa, \rho, G, H_0) = \min_i M\left( \frac{\kappa}{2m}, \rho, G^{(i)}, H_0 \right)
\]

gives the proof of Theorem 1. We shall therefore assume henceforth that condition 1°. of Theorem 2 is satisfied in the domain \( G \). We shall prove Theorem 2 assuming \( M = \delta_1, T = 8n + 24, \delta_1 \leq \delta_2(n) \) (\( n, \Theta, \rho, \nu, D \)), where the constant \( \delta_2(n) \) is defined in Theorem 2. In view of (2) §1 the conditions of Theorem 2 are satisfied and so its conclusion holds.

which gives \( M < \delta_2^{(5)} 10^{-24} \), that is

\[
(6) \quad M < \left( \frac{e}{32n^2 + 100n} \right)^{2n(8n+24)},
\]

as reported by Hénon. The application for \( n = 2 \) gives

\[
(7) \quad M < 10^{-333.05}
\]

which is close to the value \( 10^{-333} \) reported in (Hénon, 1966). This result was important, as it clearly showed that there was a gap between the rigorously demonstrated results of stability of the mathematicians and the real Solar System. Indeed, if one only takes 3 degrees of freedom, the estimate becomes \( 10^{-672} \). For a spatial planetary system limited to Jupiter and Saturn, after the reduction to heliocentric coordinates, and reduction of the angular momentum, there remain four degrees of freedom (e.g. Robutel, 1995) (\( n = 4 \)), and thus \( M < 10^{-1131} \).

VI. Chaos in the Solar System

In 1981 was held in Les Houches a summer school devoted to the "Chaotic behaviour of deterministic systems". I was not present to this conference, but I understand from the comments of many that this conference was an important landmark in the rising field of application of dynamical system theory to realistic physical systems. Although not present, I benefited from the chapter written by Michel on the numerical exploration of Hamiltonian systems (Hénon, 1983) which I studied thoroughly as everything was there in order to understand the numerical experiments on chaotic systems. Present at Les Houches were in particular Alain Chenciner, and also Jack Wisdom, who later on made a postdoctoral stay in Nice Observatory where he worked with Michel Hénon on the billiard dynamics (Hénon and Wisdom, 1983). Later on, he did the first study of the chaotic behavior of the rotational motion of Hyperion, which was the first example of observable chaotic behaviour in the Solar System (Wisdom, Peale, and Mignard, 1984).

As stated in the beginning of this text, the better understanding of the dynamical behaviour of realistic models of physical systems was of fundamental importance...
for understanding that the motion of the planets themselves is chaotic (Laskar, 1989, 1990). Using dedicated computer algebra, I had been able to average the equations of motion of the planets over their rapid motion, and thus to construct, following the works of Lagrange and Laplace, a "secular" system of equations that represented the slow precessing motion of the planetary orbits. The final system contained 153824 polynomial terms, but as only the slow precessing motions were present, it could be integrated very efficiently with a very large step size of 500 years compared to half of a day that was required by a conventional integration of the equations of motion. For several years, I had on hand the numerical output of the integrated solutions over a few millions years, but had difficulties to understand their complicated behaviour, until I realised that the system was actually chaotic, with an intricate network of secular resonances (Laskar, 1987, 1989, 1990).

In the 80s, there was still a huge gap between the small fraction of pioneer scientists like Michel Hénon and the majority of researchers in astronomy who were still thinking as if everything were regular, as if Henri Poincaré had never existed. The following personal recollection can be thought as an illustration of this statement. When I discovered that the Solar System is chaotic, I was still a young scientist, at the lower level of the CNRS carrier, with the title of "Chargé de recherche" of second class. After being for four years in this position, I could apply for being upgraded to the first class on the same position, which had to be decided by a national committee of astronomy experts. I met my referee who told me that there should not be any problem as my records were very good and there was nine positions for only eleven applicants. Then, I told him that I had a new results, and that I had just shown that the Solar System is chaotic, and gave him the proofs of the Nature paper on this result, which was soon to be published (Laskar, 1989). The committee session occurred. Their global reaction was essentially to say that if it were true, it would be known, and that it is well-known since Laplace that the Solar System is stable. I did not get the promotion. Luckily, there were people like Michel Hénon, and his former student Claude Froeschlé, in Nice, that clearly stated the importance and the meaning of the result, so the next year it was known . . ., and I got promoted. It should be said that meanwhile I had presented these results in front of some of the most prestigious mathematicians, as Jurgen Moser who contributed to the KAM theory (Moser, 1962). For them, it was not such a surprise, as Poincaré work and KAM theory already provided the general setup of the story. I was just telling them that, instead of being very close to the central stable quasiperiodic trajectory, which would be the cases for extremely small values of the planetary masses, the actual Solar System was much further, in an area where resonances of large amplitude destroy a large amount of the regular trajectories, as forecasted by Poincaré. Maybe it is at this meeting in Luminy in 1990, or in an earlier meeting on dynamical systems, that Jurgen Moser spoke in very high terms of Michel to Alain Chenciner, saying Michel Hénon is a real scientist.

2. Twenty five years later, I had the opportunity to discuss with a member of this committee, who acknowledged that they had not understood at all the meaning of the result, which actually discredited me in their evaluation.
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