MATEMATICA

CORSO DI DOTTORATO DI RICERCA IN

XXXV

CICLO DEL CORSO DI DOTTORATO

Andrej N.Kolmogorov's 1954 theorem on the persistence of invariant tori: a historical perspective on its cultural roots and its meaning in the history of classical mechanics

Titolo della tesi

Isabella Fascitiello

Nome e Cognome del dottorando

firma

Luca Biasco
Ana Millán Gasca

Docente Guida/Tutor: Prof.

firma

Alessandro Giuliani

Coordinatore: Prof.

firma

Andrej N. Kolmogorov's 1954 theorem on the persistence of invariant tori: a historical perspective on its cultural roots and its meaning in the history of classical mechanics

Isabella Fascitiello

May 29, 2023

Ringraziamenti

Ringrazio i miei relatori, Ana Millán Gasca e Luca Biasco, per avermi guidata, supportata e stimolata in questo lungo e meraviglioso percorso. Senza i loro insegnamenti e incoraggiamenti continui, non ce l'avrei mai fatta a portare a termine questo lavoro e non avrei mai potuto terminare il dottorato con un tale bagaglio culturale. Ho scelto dei relatori, ma ho trovato degli amici, confidenti, compagni di viaggi (e chi lo dimentica il viaggio a Parigi con Ana!) e, soprattutto, dei professionisti. GRAZIE!

Ringrazio Antonio, mio meraviglioso marito, per il suo appoggio assoluto, senza riserve, senza condizioni. Lo ringrazio per la sua presenza costante, ma mai invadente, per la vicinanza e soprattutto per la forza che mi dà ogni volta che cado.

Ringrazio la mia famiglia: mamma, papà, i miei suoceri, mio cognato Roberto e mia cognata/cugina Alessia, mio fratello Paride e mia cognata Katia. Nonostante non abbiano ancora ben compreso cosa abbia studiato in questi tre anni, sono sempre stati felici della mia scelta.

Ringrazio tutto il gruppo "Dinamici a Roma Tre", per avermi accolta e insegnato tanto. In particolare, ringrazio Michela Procesi, per tutte le volte in cui ha ascoltato i miei dubbi e mi ha aiutata a scioglierli; Livia, per avermi fatto "le ossa" in teoria dei sistemi dinamici; Jessica, per aver seguito tutti i progressi di questo lavoro, ma, soprattutto, per essermi amica.

Ringrazio Luigi Chierchia, perché in questi tre anni è stata una figura sempre presente. Lo ringrazio per i suoi suggerimenti, per il continuo scambio di letture e per tutte le volte che, incontrandomi anche per caso, ha avuto sempre la premura di chiedermi come procedesse il lavoro.

Ringrazio Alfonso Sorrentino, per avermi fatto conoscere Yakov Sinai e ottenere così una sua preziosa intervista.

Ringrazio Paola Magrone, Shulamit, Fabrizio, semplicemente perché sono persone che hanno fatto parte di questo percorso sin dall'inizio e, sono sicura, rimarranno oltre.

Ringrazio le mie amiche, Ludovica e Valentina, perché ci sono sempre. E perché è così facile volergli bene.

Contents

Introduction

THURSDAY, 9th SEPTEMBER, AFTERNOON

2.30–3.30 Concertgebouw, van Baerlestraat

In the chair: J. A. Schouten

A. N. Kolmogorov, Obščaja teorija dinamičeskih sistem i klassičeskaja mehanika (Théorie générale des systèmes dynamiques et mécanique classique). *(Address by invitation of the Organizing Committee).*

3.45 Concertgebouw, van Baerlestraat

CLOSING SESSION*)

It was a surprise to me that I would have to present a paper at the final session of the Congress in this large hall, which had been known to me rather as a place for the performance of great musical compositions of the world conducted by Mengelberg. The paper which I have prepared, without taking into account that it would occupy such an honourable position in the programm of the Congress, is devoted to a rather special range of problems. My aim is to elucidate ways of applying basic concepts and results in the modern general metrical and spectral theory of dynamical systems to the study of conservative dynamical systems in classical mechanics. However, it seems to me that the subject I have chosen may also be of broader interest, as one of examples of the appearance of new, unexpected and profound relationships between different branches of classical and modern mathematics.

In his famous address at the Congress in 1900, D. Hilbert said that the unity of mathematics and the impossibility of its division into independent branches stem from the very nature of the science of mathematics.

From the 1957 published version of Andrei N. Kolmogorov's closing speech at the 1954 ICM Amsterdam (English translation by V. M. Volosov in [Tikhomirov 1991], vol. I, p. 355)[1]

With the above quoted words Andrei Nikolaevich Kolmogorov (1908-1984) started off his invited plenary lecture opening the last session of the International Congress of Mathematicians at Amsterdam on the afternoon of September 9th 1954, at the city Concert Hall (the Concertgebouw). It was the second meeting after the interruption of the series of congresses by the war, the first one in which a delegation from the USSR was present.

As the speaker who has opened the Congress on September 2th – the Hungarian born American mathematician John von Neumann (1902-1957) – Kolmogorov chose thus to start from and give further support to the claim about the unity of mathematics put forward by David Hilbert (1862-1943) in his famous conference of the Paris ICM in 1900. Hilbert's admonition to 20th century mathematicians was blend by Kolmogorov with a reference to the special kind of continuity between the past and the future in the science of mathematics, between, as he put it, classical mathematics and modern mathematics.

The title of his lecture, *The general theory of dynamical systems and classical mechanics*, made reference in fact both to the centenary tradition of mathematical study of motion, elasticity and an increasing number of physical phenomena using differential equations[2]– having a golden age in the 18th-19th centuries – and to the 20th century theory of dynamical systems[3],

[1]The Russian text was published in the Congress Proceedings, [Kolmogorov 1957].

[2]Phenomena "caused by the forces of nature", as Newton put it in the preface to the Principia. On the history of mechanics see [Duhem 1905], [Borel 1943], [Dugas 1957], [Truesdell 1976a,b], [Fraser 1994]. In the a recent essay on Modern classical mechanics [Halliwell, Sahakian 2020] remind that "motions within the Solar system were the most important tasting ground for classical mechanics in the first place" (p. xiv).

[3]The origins and development of the theory of dynamical systems – starting from the seminal contributions by Henri Poincaré and George David Birkhoff – has been the object

rooted in classical mechanics but at the same time a paradigmatic example of the new mathematical approaches (qualitative approaches) developed after 1900.

The adjective *classical* underscored the breakup between the tradition of "rational mechanics"[4] – from its Newtonian source to its reformulations by Joseph Louis Lagrange (1736-1813) and William Rowan Hamilton (1805-1865) – and the new theoretical physics (quantum mechanics and relativity) in the early 20th century[5]. Craig Fraser writes on this regard[6]:

> With the establishment of special relativity, it became necessary to introduce the adjective "classical" to delineate the vast range of mechanical doctrines from Newton to Einstein. Classical theories retain their validity and continue to be cultivated extensively today in mathematical engineering. Nevertheless, since Einstein, the classical viewpoint has lost its epistemological primacy as final description of material motion in space and time. [Fraser 1994, p. 984]

In a recent paper on Edmund Whittaker's *A treatise on the analytical dynamics of particles and rigid bodies* (1st edition 1904, and edition 1917) published in «Archive for the History of Exact Sciences», Severino Collier Countinho reminds the situation of classical mechanics in that period:

> Once a flourishing subject, where a remarkable cross-breeding of mathematics and physics took place, classical mechanics was considered by many to have reached a dead end by the first decades of the twentieth century, ex-

of some historical research since the 1990s, mainly thanks to a new attention to the issue of chaos as opposed to determinism: [Dahan-Dalmedico, Chabert, Chemla (eds.) 1992], [Aubin, Dahan Dalmedico 2002], [Holmes 2007]. On the contribution by Henri Poincaré see [Holmes 1990], [Barrow Green 1997]. On Birkhoff, see [Aubin 2005], [Dell'Aglio 2003].

4 This expression is uncommon in English; see [Fraser 1994] for a comment on this.

5 As a matter of fact, the advent of quantum mechanics in the early 20th century – together with that of relativity – had marked the emergence of a scientific community of theoretical physicists, culturally and institutionally strongly autonomous from the mathematicians world [Faddeev 1995].

6 In his contribution on Classical mechanics to Grattan Guinness Companion Encyclopedia of the History and Philosophy of Mathematical Sciences.

> cept for eventual applications to other.
>
> [...] So dramatic have been the changes that mechanics has undergone in the twentieth century that the style and even the contents of most books on dynamics written before the 1930s look hopelessly dated to present-day readers. But there are exceptions [Coutinho 2014, p. 356].

Let's also quote the provoking Clifford Truesdell in the final part *On the decline of classical mechanics* his 1976 essay History of classical mechanics.

> The word "classical" has two senses in scientific writing; (1) acknowledged as being of the first rank or authority, and (2) known, elementary, and exhausted ("trivial" in the root meaning of that word). In the twentieth century mechanics based upon the principles and concepts used up to 1900 acquired the adjective "classical" in its second and pejorative sense, largely because of the rise of quantum mechanics and relativity. "Fundamental" in physics came to mean "concerning extremely high velocities, extremely small sizes, or both". Physicists gave less and less attention to classical mechanics because they thought nothing more could be learned from it and nothing new discovered about it, although of course they continued to use it in the design of the experimental apparatus with which they claimed to controvert it. At about the same time "applied" in mathematics came to refer not to the object studied but to the originality and logical standards of the student, again in a pejorative sense.
>
> Engineers still had to be taught classical mechanics, because in terms of it they could understand the machines with which they worked and could devise new machines for new purposes. Research in mechanics came to be slanted toward the needs of engineers and to be carried out largely by university teachers who regarded mathematics as a scullery-maid, not a goddess or even a mistress. [Truesdell 1976, pp.127-128].

Physics' exciting new research contrasted with a deep theoretical impasse in classical mechanics, which emerged in Henri Poincaré's brilliant contributions in the final years of the 19th century to the three body problem of celestial mechanics: the belief that the mathematical structures designed to describe natural phenomena involved non-regularity or chaos, dramatically jeopardizing the possibility of forecasting time evolution[7].

This theoretical and epistemological crisis of classical mechanics challenged its central role in the mathematical world, also because of the vitality of modern algebra and branches of mathematics working in abstract universes investigated independently of possible links to physical phenomena, technological development and other applications. The new qualitative theory of differential equations (the theory of dynamical systems) displayed possible of applications to phenomena regarding life or social and economical human systems, as well as new engineering applications described by non linear differential equations. Recent studies on the origin of the theory of dynamical systems had shown light on the various research line which were displayed in the years of witnessing the "downfall of classical mechanics", involving the developing of the modellistic approach as well as efforts to apply the classical approach to mechanics to problems in biology, demography, and economics:

> From Poincaré to the have all contributed a stone to the final edifice. In fact, this history unfolds along various geographic, social, professional, and epistemological axes. It is punctuated by abrupt temporal ruptures and by transfers of methods and conceptual tools. It involves scores of interactions among mathematics, engineering science, and physics along networks of actors with their specific research agendas and contexts. Finally, it is characterized by countless instances of looping back to the past, to Poincaré's work in particular, which are so many occasions for new starts, crucial reconfigurations, and reappreciation of history. [Aubin, Dahan-Dalmedico 2002 pp. 278-279].

"The problem of integration of systems of differential equations of classi-

[7][Dahan Dalmedico et al 1992]; On mechanism as an underlying metaphysics of science showing the crucial role of classical mechanics in scientific thought see [Israel 2015].

cal mechanics", Kolmogorov reminded to the audience, had been a "focal point for the mathematics of the 19th century," as if we asking to renew ties with the past .

This area had been paid continuous attention in the Soviet Union, where Birkhoff's work was developed in connection with engineering applications (nonlinear mechanics)as Simon Diner has underscored[8]:

> le grand public en Occident ignore largement que ce sont essentiellement des savants russes qui ont pendant cinquante ans exploité la partie de l'héritage d'Henri Poincaré, concernant la "théorie qualitative des systèmes dynamiques" et la "mécanique non linéaire" dont le chaos déterministe n'est qu'un des aspects les plus spectaculaires.
>
> Situation créée par la conjonction de l'isolement relatif de l'Union soviétique et les mobiles internes du développement des mathématiques dans un univers de la physique où la mécanique quantique a ravi la vedette à la mécanique classique. Le langage de Poincaré semblait opaque et ses idées en ont souffert, d'autant plus que les applications qu'il envisageait ne concernaient que l'astronomie. [Diner 1992, pp. 331-332]

The goal of his conference was to show how metrical and spectral methods – 20th century measure theory and Hilbert spaces – could be applied to throw new light on the understanding of key open problems of classical mechanics, regarding Hamiltonian conservative dynamical systems central in celestial mechanics[9]. This area appeared neglected in the mathematical world since the late 1930s, when attention to celestial mechanics classical approaches flagged[10].

---

[8]In his contribution to the volume [Dahan Dalmedico et al eds 1992], Simon Diner stressed that the apparent gap between Poincaré's work and Steven Smale's contributions had been not so, but was the consequence of "ignorance of the Russian contributions" during the Cold War period, and the separation between the West (NATO area) and the area of Soviet Unione and the countries tied by the Warsaw Pact [Diner 1992].
See also: [Nemytskii 1957].

[9]On the meaning of Hamiltonian system see Appendix

[10]This historical transformation and change of status and reciprocal position of the mathematical disciplines in the early 20th century possibly deserves further attention

A crucial theorem had been published by Kolmogorov a few days before, on August 31st before his lecture in the proceedings of the Soviet Academy of Sciences («Doklady Akademii Nauk SSSR») in a paper entitled *On the preservation of conditionally periodic motions under small variations of the Hamilton function* [Kolmogorov 1954]. His theorem applied these methods and showed the path for further attainments. Moreover, the theorem suggested that the above mentioned impasse suggested by Poincaré – referred to the three body problem in celestial mechanics – could be overcome.

Séminaire de
MÉCANIQUE ANALYTIQUE et de MÉCANIQUE CÉLESTE
dirigé par
Maurice JANET

1re année : 1957/58

-:-:-:-

TABLE DES MATIÈRES

-:-:-:-

Table of contents: Séminaire Janet. *Mécanique analytique et mécanique céleste*, tome

1. The research issue

The theoretical meaning of this Kolmogorov theorem, in the context of the changing status of classical mechanics and celestial mechanics in the interplay between mathematics and physics, and its cultural origins – why and how Kolmogorov arrived to its formulation and demonstration – is the subject of the dissertation. Following the suggestion of Vladimir Arnold (1937-2010), I refer to it as the Kolmogorov theorem on the persistence of invariant tori under small perturbations in Hamiltonian dynamical systems[11]. In my investigation I have found crucial clue in a testimony by Arnold – a former student of Kolmogorov's – regarding a short conversation in 1984.

Let's start from the appreciation by Scott Dumas of the contents of this Kolmogorov's theorem and its key role from an epistemological point of view in the evolution of modern science:

> Right from the start, after enunciating his laws of mechanics and gravitation, Isaac Newton ran into difficulties using those laws to describe the motion of three bodies moving under mutual gravitational attraction (the so-called 'three body problem'). For the next two centuries, these difficulties resisted solution, as the best minds in mathematics and physics concentrated on solving other, increasingly complex model systems in classical mechanics (in the abstract mathematical setting, to 'solve' a system means showing that its trajectories move linearly on so-called 'invariant tori'). But toward the end of the 19th century, using his own new methods, Henri Poincaré confronted Newton's difficulties head-on and discovered an astonishing form of 'unsolvability,' or chaos, at the heart of the three body

[11] The origin of the name chosen for the theorem has been taken up by Arnold in [Arnold 1997]: *Kolmogorov's theorem of 1954 on the persistence of invariant tori under a small analytic perturbation of a completely integrable Hamiltonian system* [Arnold 1997, p. 742]; *[...] he [Kolmogorov] arrived at his 1954 theorem on the persistence of invariant tori.* in [Arnold 1997, p. 743].

problem. This in turn led to a paradox. According to Poincaré and his followers, most classical systems should be chaotic; yet observers and experimentalists did not see this in nature, and mathematicians working with model systems could not (quite) prove it to be true either. The paradox persisted for more than a half century, until Andrey Kolmogorov unraveled it by announcing that, against all expectation, many of the invariant tori from solvable systems remain intact in chaotic systems. These tori make most systems into hybrids – they are a strange, fractal mixture of regularity and chaos. [Dumas 2014, preface].

Kolmogorov's theorem would became the cornerstone of a new mathematical theory, the socalled KAM theory, from the initials of Kolmogorov as well as of Vladimir Arnold and Jürgen Moser (1928-1999) ([Celletti, Froeschlé, Lega 2003], [Chierchia 2008], [Chierchia 2012]; [Dumas 2014, chapter 4]; [Hubard 2014], [Diacu, Holmes 1996]);

At the origin of my research work was a call by the above quoted scholar Scott Dumas to develop a "story of KAM" in order to overcome a lack of awareness – among mathematicians and in the scientific world – of an area of research whose aim is the "true picture of classical mechanics – often thought to have been sketched in the 17th century — was not complete until the latter part of the 20th century. And although the mathematical theory is indeed mostly complete, certain applications to problems in physics (especially in celestial and statistical mechanics) have been developed only with great difficulty, and some remain controversial and uncertain even today." [Dumas 2014, preface, p 7].

Historiographical appears crucial in the attempt to develop a "storytelling" to be shared and become part of common awareness of breakthroughs in mathematics[12]. In fact, mathematical treatises tend to hide

[12] The initial impetus of my research came from Dumas's call to tell a story, deeply felt in the Rome scholarly group of mathematical analysts and physicists, where I have been welcomed for the three years a PhD student; however, it also stemmed from the conviction that the story of KAM could have a wider diffusion, thus contributing to the

the original context of discovering and "make up" mathematical achievements by presenting them in an normalized, logically perfected exposition. My contribution focuses on the understanding of the conditions and foundations of Kolmogorov's research resulting in his 1954 theorem on the preservation of invariant tori in Hamiltonian systems, as a crucial episode in the status of classical mechanics in the 20th century and also throwing light on the cultural conditions and often awkward paths of mathematical research.

2. Published sources and beyond

My research focuses on three papers, originally written and published in Russian, available in English through the reliable translation by Vladimir M. Volosov[13] (published in the 1991 English edition of vol. I of Kolmogorov's Selected works)[14]. Two of them appeared in the periodical "Doklady Akademii Nauk SSSR"[15] in November 1953 [Kolmogorov 1953] and August 1954 [the already mentioned Kolmogorov 1954, where he states the theorem on the persistence of invariant tori and discusses its demonstration]. The third, [Kolmogorov 1957], is the contribution published three years after the International Congress of Mathematicians in Amsterdam, in the Proceedings

culture of mathematics, only on the basis of a monographic historical hermeneutic reconstruction, carefully based on sources that included published documents and testimonials from the seminal years – the Dumas' essay essentially consists of a collection of these references

[13]Vladimir Markovich Volosov's (born 1928) scientific career, in the area of nonlinear mechanics ordinary and partial differential equations, was developed at the "M. V. Lomonosov" Moscow State University; he graduated in 1950 (Physical Faculty; Department of mathematics); Ph. D. 1956 and D. Sci. in 1961, then becoming a professor and member of the "P. P. Shirshov" Institute of Oceanology of the Academy of Sciences. He had previously translated a 1980 textbook by Viacheslav M. Starzhinskii published in English by MIR (Moscow) with the title Advanced course of theoretical mechanics for engineering studies (1982) (www.mathnet.ru/eng/person 12469)

[14]The three papers were included in the first volume of selected works by Kolmogorov published in Russian in 1985 by Nauka (Moscow), edited by Valdimir M. Tikhomirov. See introductory note to the Bibliography.

[15]Proceedings of the Soviet Union Academy of Sciences, founded in 1933.

of the Congress, edited by Gerretsen Johan C.H and Groot Johannes [Gerretsen, De Groot 1957], in Russian, whose translation in English there is selected works [Tikhomirov 1991].

The first two contributions are quite succinct and include a couple of references each. The written text of the Amsterdam conference, instead, presents a wide panorama opening to future research and includes a 23 bibliographical references spanning in the years 1917-1954[16]. On March 22, 1958 Kolmogorov gave a talk in Paris at the Analytical Mechanics and Celestial Mechanics Seminar leaded by Maurice Janet (1888-1983); a French translation (by Jean-Paul Benzécri) of the Proceedings contribution was published in the series of the Seminar [Kolmogorov 1958][17].

I argue – in Chapter 3 – that this third contribution put forward a research program for Hamiltonian conservative systems in classical mechanics – presenting it to the international audience that had recently mostly disregarded this area. In a nutshell, it was described by Kolmogorov with the following words:

> For conservative systems, the metrical approach is of basic importance making it possible to study properties of a major part of motions. For this purpose, contemporary general ergodic theory has elaborated a systems of notions whose conception is highly convincing from the viewpoint of physics. However, up to now the progress made towards the application of these modern approaches to the analysis of specific problems of classical

[16]The list includes also [Kolmogorov 1953] and [Kolmogorov 1954]. The oldest reference is to Émile Borel's *Leçons sur les fonctions monogènes uniformes d'une variable complexe* (1917) and the most recent to the 1954 paper by the Soviet mathematician Mstislav Igorevich Grabar (1925-2006) *On strongly ergodic synamical systems.*

[17]The contribution by Kolmogorov was published in the first volume, corresponding to the academic year 1957-58. Some scholars had of course the opportunity to attend the Amsterdam lecture, but, lacking any audio or video registration, this third paper is usually referred to as "the Amsterdam lecture." Further archival material regarding the writing of the contribution for the Amsterdam Proceedings, could make it possible to improve the analysis presented in this dissertation on the cultural origins of the theorem on the persistence of invariant tori.

> mechanics has been more than limited. [...] I believe that the time has now come when considerable more rapid progress can be made. [Kolmogorov 1957, pp. 356-357].

This research program was the focus of a Moscow seminar leaded by Kolmogorov in years 1957- 1958, from which originated further research (notably by Vladimir Arnold, who worked on the three body problem, and Yakov Sinai). This research program lies at the foundations of KAM theory, from a historical and epistemological point of view[18].

Kolmogorov research program could arrive as a surprise or somehow *démodé* as it regarded an area of problems in classical mechanics which had had little attention for more than fifteen years in the Soviet Union and abroad: issues regarding the dynamical systems in celestial mechanics, such as the crucial three body problem.

The issue of the reasons that led Kolmogorov to this intellectual and cultural gesture had been raised by Arnold, underlining it as an appealing enigma. Arnold has reported – in two different papers – about a conversation with Kolmogorov regarding this issue, dating back to 1984 (in [Arnold 1997] and then in [Arnold 2000]). One year after the reported conversation, in the first volume (1985) of selected works published in Moscow, the editor Vladimir Tikhomirov was able to include a short comment by Kolmogorov himself on the same issue. In my research, I have analyzed the scholars mentioned both in the two reports of Arnold of his 1984 conversation with Kolmogorov and in the 1985 comment by Kolmogorov himself.

Among the authors mentioned in the conversation with Arnold – also

[18] On KAM theory Dumas writes: "It is not a stretch to rank KAM theory alongside the revolutions in modern physics. But KAM theory [...] also had the misfortune of playing out over roughly the same interval during which the revolutions of modern physics took place. Not surprisingly, in that period, physicist abandoned classical mechanics to the few hardy mathematicians who reanimed interested in. The physicists returned with wondrous stories of their exploits in quantum mechanics, relativity, and nuclear physics. *The time has come for mathematicians to tell their tales from this period in a broad setting, too.* [Dumas 2014, preface, my emphasis]

comparing them with the references included in the three above mentioned contributions [Kolmogorov 1953, 1954 and 1957] – two groups can be identified.

On the one hand, scholars active in the area celestial mechanics after the publication of Henri Poincaré *Les méthodes nouvelles de la mécanique celeste* (1892-1899) [Barrow-Green 1997], notably Carl Vilhelm Ludvig Charlier (1862-1934), an outstanding member of Scandinavian research, author of *Die Mechanik des Himmels* (1902-1907) and Edmund Whittaker (1873-1956), who was the author of a report on the three-body problem on 1899 from which outgrew his classical *A treatise on the analytical dynamic* which was intended as an essay inspired by past achievements in classical mechanics presenting challenges for future research. Moreover, other more recent authors who lived in the age of emergence of quantum mechanics and relativity theory, while facing the challenges to classical mechanics involved in Poincaré research on the three body problem: Jean-François Chazy (1882-1955) in France, and a Soviet eclectical scholar close to Kolmogorov[19], the editor of the Soviet Encyclopedia Otto Yulyevich Schmidt (1891-1956).

On the other hand, in the note published in the first volume of his *Selected works*, Kolmogorov mentioned some contributions published in years 1931-37 exploring the approach to Hamiltonian systems in classical mechanics by means of Hilbert spaces and measure theory, in connection with development of the socalled modern ergodic theory. Hilbert spaces had been introduced in the 1920s by John von Neuman (1903-1957) (in the context of what he called the "theory of operators"), who has showed their suitability in the mathematical formalism of quantum mechanics.

A seminal paper was published in 1931 by Bernard O. Koopman (1900-1981), a collaborator of George Birkhoff (1894-1944), entitled Hamiltonian

[19]Two papers by Chazy of 1929 and 1932 are included in the references of [Kolmogorov 1957], together with a referene to a 1947 paper by Schmidt.

systems and transformations in Hilbert Space [Koopman 1931]. Let's use Kolmogorov's own words regarding this development in [Kolmogorov 1957]:

> After the work of H. Poincaré, the fundamental role of topology for this range of problems became clear. On the other hand, the Poincaré-Carathéodory recurrence theorem initiated the "metrical" theory of dynamical systems in the sense of the study of properties of motions holding for "almost all' initial states of the system. This gave rise to the "ergodic theory", which was generalized in different ways and became an independent centre of attraction and a point of interlacing for methods and problems of various most recent branches of mathematics (abstract measure theory, the theory of groups of linear operators in Hilbert and other infinite-dimensional spaces, the theory of random processes, etc.). [Kolmogorov 1957, pp. 355-356].

Furthermore, in the note included in the first volume of his Selected papers, Kolmogorov wrote:

> My papers on classical mechanics appeared under the influence of von Neumann's papers on the spectral theory of dynamical systems and, particularly under the influence of the Bogolyubov-Krylov paper of 1937. I became extremely interested in the question of what ergodic sets (in the sense of Bogolyubov-Krylov) can exist in the dynamical systems of classical mechanics and which of the types of these sets can be of positive measure at present this question still remains open). To accumulate specific infor- mation we organized a seminar on the study of individual examples. My ideas concerning this topic and closely related problems aroused wide re- sponse among young mathematicians in Moscow. [Kolmogorov 1991/1985, p. 521]

3. A journey in the mathematical biography of Andrej N. Kolmogorov[20]

[20]In the aftermath of Kolmogorov's death, Vladimir Mikhailovich Tikhomirov (born

These two groups of references suggest that in the early decades of the 20th century the young Kolmogorov followed contemporary developments in classical mechanics – especially regarding the central question of the three body problem in celestial mechanics – as well as the new approach developed by George Birkhoff, again in the wake of Poincaré, towards a general theory of dynamical systems based on qualitative analysis of systems of differential equations. Both trends had scholars involved in them in the Soviet Union. Soviet researchers in celestial mechanics included the above mentioned Schmidt, but also Boris Vasilyevich Numerov (1891-1941?), a leader of the flourishing Soviet astronomy supported by a network of observatories. Birkhoff approach was followed in the Soviet Union by several leading Soviet scholars, in particular working in nonlinear mechanics, such as Nikolay M. Krylov (1879-1955) in Kiev; Vyacheslav Vasil'evich Stepanov (1889-1950) in Moscow, who in 1930 started a seminar on the qualitative theory of differential equations, attended among many others by Kolmogorov [Nemytskii 1957][21].

In his early paper [Kolmogorov 1953] the book on qualitative theory of differential equations (in Russian) by Viktor Vladimirovich Nemytskii (1900-1967) and Stepanov is the only bibliographical reference[22].

1934) presented a short but very detailed essay "The life and work of Andrei Nikolaevich Kolmogorov" [Tikhomirov 1988], which includes a biography of the mathematician, a description of his works, as well as a list of all his pupils. In addition, we can see [Shyraev 1989], who draws from authobiographical memories included in Kolmogorov's book on mathematics [Kolmogorov 1988] (in Russian, no English translation available).

[21]Kolmogorov was a doctoral student of Nicolai Luzin, in that period the dominant figure in Moscow mathematics; but this is what the British David G. Kendall (1918-2007) wrote in a remembrance of Kolmogorov published in 1991:"A number of mathematicians stimulated Kolmogorov's earliest mathematical research, but perhaps his principal teacher was Stepanov. In 1922 Kolmogorov produced a synthesis of the French and Russian work on the descriptive theory of sets of points, and at about the same time he was introduced to Fourier series in Stepanov's seminar. This was when he made his first mathematical discovery -that there is no such thing as a slowest possible rate of convergence to zero for the Fourier cosine coefficients of an integrable function. "[Kendall 1991, p. 303] In [Sinai 1989] the author underlines how von Neumann's studies were followed in the Soviet Union of the 1930s.

[22]"Stepanov was among the first in our country to understand the significance of the

Two papers by Kolmogorov can be mentioned dating back to the 1930s that show his interest in the general theory of dynamic systems and in ergodic theory, fresh approaches to the classical problems of 19th century mechanics [Kolmogorov 1936] and [Kolmogorov 1937]. Years 1936-37 marked the sharpening of the Stalin's purges in the Soviet Union, including a massive attack to astronomers. A dark age initiated, which included the years of the Second world war 1941-1945, and was closed with Stalin's death in early 1953. The network of international connections of scientists from the Russian Empire was radically damaged. During these years Kolmogorov apparently carried on a silent work on Hamiltonian systems, as he confided to Arnold that "he had been thinking about this problem for decades, starting from his childhood" [Arnold 1997, p. 1] and Sinai wrote in 1989 that "apparently the interests of Kolmogorov in ergodic theory had already started in the 1930s." [Sinai 1989, p. 833].

Understanding the cultural origins of Kolmogorov theorem on the persistence of invariant tori in the framework of his research program for classical mechanics – the issue first raised by Arnold, that lies at the basis of Dumas' call for a better understanding of the relevance of KAM theory for modern science – has thus lead me to consider some aspects of intellectual and cultural evolution of a single, outstanding scholar of the 20th century, Kolmogorov, facing the radical transformation of the relationship between physics and mathematics and the changing status of classical mechanics – the heart of modern science.

My research has explored both the general context of Kolmogorov work regarding classical mechanics old open problems and new methods, and the biographical and intellectual reasons behind Kolmogorov involvement

---

metric theory of general dynamical systems begun in the works of Poincare and Birkhoff, and he made an essential contribution to it" [Myshkis, Oleinik, 1990, p. 180] Stepanov was the author of a textbook on differential equations published in 1936, and a second textbook on the qualitative theory of differential equations with Viktor Vladimirovich Nemytskii (1900-1967) first published in 1947, 2nd edition 1949 that would be translated in 1960 in English by Princeton University Press.

in this area of research in the 1950s, a choice that, as I will argue, had deep cultural and almost "political" value, in a time of reconstruction and – perhaps — of hope. Deeping our understanding the seminal work by Kolmogorov published in years 1953-54 thus can contribute to a better understanding of the evolution of mathematical thought in its relation to science as investigation of Nature in the 20th century.

4. Structure of the dissertation

In Chapter 1, *The mathematical landscape: general theory of dynamical systems and classical mechanics in the late 19th century and early 20th century*, some elements will be discussed regarding the general problem of dynamics as formulated by Poincaré in 1892 and researcher in celestial mechanics around 1900 that Kolmogorov – as I have just mentioned – considered as the background of his research program for dealing with open problems in classical mechanics. Moreover, the seminal research – by Koopman and von Neumann in the United States and by Krylov with Bogoliubov – on the metrical and spectral approach to dynamical systems and ergodic theory will be described. My analysis of this seminal research is intended to support my analysis and interpretation of Kolmogorov's research program, and it is open to further deepening in subsequent studies. My aim in this first chapter is to draw the main lines of the cultural landscape behind Kolmogorov's contribution, that was in place in the eve of the dramatic explosion of totalitarianism and war of the 1930s and 1940s.

Chapter 2, *Fascination and risk. Aspects of Andrej N. Kolmogorov's (1903-1987) life and times*, is dedicated to the biographical and cultural aspects relating to Kolmogorov's training which may have influenced his interest in celestial mechanics and the formulation of his research program for classical mechanics in the early 1950s. The above mentioned report's by Arnold of his 1984 short conversation with Kolmogorov is analyzed and investigated, also thanks to available literature on the evolution of science and scientific education in the Russian Empire from the zarist regime to the Soviet Union. Kolmogorov's participation in the scientific discussion around Lysenko's views– apart from its relevance from a political point of view – throws light on his views on the role of mathematics in the scientific study of natural phenomena, thus adding to our understanding of his attention to classical mechanics. Kolmogorov's silent research for a nice span of years can found also an explanation considering the purge of astronomers by Stalin, as well as the orientation of research on dynamical systems mainly towards the dissipative systems of technological applications.

On the basis of the framework established in Chapters 1 and 2, in Chapter 3, *Kolmogorov's theorem on the persistence of invariant tori: a look into the origins of KAM theory*, I analyze the seminal contribution contained in the above mentioned three papers, originally published in Russian, [Kolmogorov 1953, 1954, 1957]. For the purpose of the exposition, I first consider the research program for classical mechanics most explicitly put forward in [Kolmogorov 1957] (the written version of his 1954 lecture at Amsterdam ICM). Then, I will analyze Kolmogorov's discussion of the proof of his theorem in Kolmogorov 1954. Finally, I present an analysis of the Diophantine condition that has a key role in the proof, comparing its uses in a 1942 paper by the German scholar Carl Ludwig Siegel (1896-1981) [Ghys 2004]. My historical study, I argue, shows that speaking about a "KAM theorem" [Hubard 2004] hides the meaning of 1954 Kolmogorov theorem on the persistence of the invariant tori in the context of a research program for classical mechanics. Subsequently, Arnold applied Kolmogorov's research program to the study of the three-body problem [Arnold 1963b], [Arnold 2009]).

Chronology

1903, April 25 Andrei Nikolaevič Kolmgorov's birth

1906-10 Kolmogorov attends the experimental school run by his aunts, located in their home.

1917 October Revolution. Kolmogorov is a high school student at the Evgenja (Evgeniya) Albertovnava Repman private Institute in Moscow

1920 He begins his university studies in mathematics (Moscow University) and in metallurgy (I. D. Mendeleev Institute)

1922-25 He teaches mathematics and physics at the Potylokhin Experimental School in Moscow

1925 Graduation from Moscow University and beginning of postgraduate work

1927 *Dynamical systems*, George David Birkhoff (1884 - 1944)

1929 He finishes his university studies and becomes a researcher at the Institute of Mathematics and Mechanics of the Moscow University

1929-1930 Vyacheslav Vassilievich Stepanov's Seminar on Qualitative theory of differential equations at Moscow University.

1930, June - 1931, May journey trip to Germany and France with Pavel S. Aleksandrov (1896 - 1982)

1931 Abandonment of progressive tendencies in education in the Soviet Union

1931 *Hamiltonian systems and transformations in Hilbert space*, by Bernard Osgood Koopman (1900 - 1981).

1932 *Zur Operatorenmethode in der klassischen Mechanik*, by John von Neumann (1903 - 1957)

1932 *Proof of the quasi-ergodic hypothesis*, by von Neumann.

1932 *Dynamical systems of continuous spectra*, by Koopman.

1936 Luzin affair

1936, October 20 Arrest of Boris V. Numerov (1891-1941(?))

1937 *La théorie générale de da mesure dans son application à l'étude des systémes dynamiques de la mécanique non linéaire*, by Nikolay Mitrofanovitch Krylov (1879-1955) and Nikolay Nikolayevitcch Bogoliubov (1909-1992)

1937 Publication of *A simplified proof of the Birkhoff-Khinchin ergodic theorem* by Kolmogorov

1942 *Iteration of analytic functions*, by Carl Ludwig Siegel (1896–1981)

1942 Kolmogorov married Anna Dmitrievna Egorova.

1953, March 5 Stalin's death

1953, November 13 *On Dynamical systems with an integral invariant on the torus*, in Douklady Akademii Nauk SSSR [Kolmogorov 1953]

1954, August 31 *On the preservation of conditionally periodic motions under small variations of the Hamilton function*, in Douklady Akademii Nauk SSSR [Kolmogorov 1954]

1954, September 9 Plenary lecture at the International Congress of Mathematicians in Amsterdam: *The general theory of dynamical systems and classical mechanics* (the title was announced in Russian).

1957 Publication of *Proceedings of the International Congress of Mathematicians 1954, Amsterdam September 2—September 9*. In vol.1 the text of Kolmogorov's lecture was printed in Russian.

1957, Autumn Ph.D. course on the theory of dynamical systems in Moscow (among those present the students Vladimir Igorevich Arnold and Yakov Grigorevich Sinai)

1959 Vladimir Igorevich Arnold discusses his dissertation under the supervision of Kolmogorov.

1962 *On invariant curves of area-preserving mappings of an annulus*, by Jürgen KurtMoser (1928-1999)

1963 *Proof of a theorem of AN. Kolmogorov on preservation of conditionally periodic motions under small change in the Hamilton function* (In Russian) by Vladimir Igorevich Arnold (1937-2010).

1 The mathematical landscape: general theory of dynamical systems and classical mechanics in the late 19th century and early 20th century

Dans les théories physiques, il faut distinguer le fond et la forme. Le fond, c'est l'existence de certains rapports entre des objets inaccessibles. Ces rapports sont la seule réalité que nous puissions atteindre et tout ce que nous pouvons demander, c'est qu'il y ait les mêmes rapports entre ces objets réels inconnus et les images que nous mettons à leur place.

La forme n'est qu'une sorte de vêtement dont nous habillons ce squelette; ce vêtement, nous le changeons fréquemment, à l'étonnement des gens du monde, que cette instabilité fait sourire et qui proclament la faillite de la Science. Mais si la forme change souvent, le fond reste.

Les hypothèses relatives à ce que je viens d'appeler la forme ne peuvent pas être vraies ou fausses, elles ne peuvent être que commodes ou incommodes. Par exemple, l'existence de l'éther, celle même des objets extérieurs ne sont que des hypothèses commodes. C'est pour cela que l'on voit renaître de leurs cendres en se transformant certaines théories que l'on croyait définitivement abandonnées. C'est pour cela aussi qu'il y a certaines catégories de faits qui s'expliquent également bien dans deux ou plusieurs théories différentes, sans qu'aucune expérience puisse jamais décider.

Cela est vrai en particulier pour les théories mécanistes. On peut en effet démontrer que, si un phénomène comporte une explication mécanique, il en comportera une infinité.[23] [Poincaré 1921, p. 130]

[23]Eng. Tr.: In physical theories, it is necessary to distinguish between substance and form. The substance is the existence of certain relationships between inaccessible objects. These relations are the only reality that we can reach and all we can ask is that there be the same relations between these unknown real objects and the images that we put in their place.

The form is only a kind of garment with which we dress this skeleton; this garment, we change it frequently, to the astonishment of the people of the world, whom this instability makes smile and who proclaim the bankruptcy of Science. But if the shape changes often,

> The branch of physics known as "classical mechanics" originated in the seventeenth century, but wasn't called that until the discovery of quantum mechanics in the 1920s. It was quantum mechanics that most profoundly changed our understanding of how and why particles move as they do, and even what a particle is. Quantum mechanics was so completely different that the word "classical" had to be added to the older theory to make it clear which mechanics was meant. At the same time, quantum mechanics was heavily inspired by the formulations of classical mechanics by Lagrange and Hamilton dating back to the eighteenth and nineteenth centuries.
>
> In many situations, using quantum mechanics and/or relativity to study a physical system would be tantamount to shooting a fly with a catapult. Roughly speaking, classical mechanics works very well (i.e., agrees with experiments) for macroscopic objects that are moving at speeds much less than the speed of light, and where gravity is not too strong – and also where our experimental measurements are not too precise.
>
> Take the motions of the planets around the sun and moons round their planets, for example. Motions with the solar system were the most important testing ground for classical mechanics in the first place, and for nearly all purposes classical mechanics in this domain works as well now as it ever did. [Helliwell, Sahakian 2020, Preface, pp xiii-xiv].

At the end of the 19th century, the research of the French scholar Henri Poincaré (1854-1912) showed to the international scientific community that new mathematical tools or conceptual frameworks, new-style *clothes* offeri new theoretical perspectives to the study of mechanical phenomena

---

the background remains.

Assumptions about what I just called form cannot be right or wrong, they can only be convenient or inconvenient. For example, the existence of the ether, that even of external objects are only convenient hypotheses. This is why we see reborn from their ashes by transforming certain theories that we thought had been definitively abandoned. This is also why there are certain categories of facts which can be explained equally well in two or more different theories, without any experience ever being able to decide.

This is especially true for mechanistic theories. We can indeed demonstrate that, if a phenomenon has a mechanical explanation, it will have an infinity of them.

of celestial motion, *the most important testing ground for classical mechanics* ad Thomas M. Helliwell and Vatche V. Sahakian put in their recent *Modern Classical mechanics* (2020). In the evolution of mechanics from the studies of Galileo, and above all of dynamics starting from Isaac Newton, we witness a continuous evolution and improvement of mathematical approachs, in a tension between the physical objects and relationships that are the primary reason for research and one's own life who acquire mathematical objects and relationships, regardless of the roots they have in trying to understand and predict the natural phenomena of motion and stability.

During the 18th century, mathematical analysis developed in symbiosis with theoretical and applied mechanics, mainly adopting a variational approach. The formulation given by Joseph Louis Lagrange at the turn of the 1800s derived from discussions of principles such as the minimum action of Maupertuis. Was further refresh by William Hamilton and Carl Gustav Jacobi increasingly following internal mathematical logic, which are therefore intertwined with the nature of mechanics as knowledge about the physical world.

At the turn of the 1900s, abstract algebra, topology, functional analysis and probability developed as new fields of mathematics. Their current autonomy as purely mathematical research sectors can make us forget how deep connections exist in their origins with the open problems of mechanics, and in particular of celestial mechanics, starting from the problem of the three bodies.

The early decades of the twentieth century were years full of fruits in research in physics and mathematics, but also in disciplinary restructuring. The first steps of quantum mechanics and the theory of relativity were accompanied by significant efforts - that deserve further historiographical analysis - to open new perspectives to 19th century mechanical studies, i.e. to what was then beginning to be considered as classical mechanics. Celestial mechanics had long maintained among scholars of the late 19th century the fascination deriving from Isaac Newton's research on the physical world, the beating heart of modern mathematized science, and even earlier from the immemorial human aspiration to understand the positions of the stars and their variations on the celestial vault ([Diacu, Holmes 1996], [Wilson 1994]). With his work *Les méthodes nouvelles de la mécanique celeste* (1892-99), three volumes, however, Poincaré had opened the way, with his qualitative analysis of differential equations, not only to new mathematical methods for dynamics in the classical sense, but to the birth of a research sector, the general theory of dynamical systems, a mathematical theory distant from physics but susceptible to applications in several fields of study of evolution phenomena over time[24]

It is in this double framework – classical mechanics and the general theory of dynamical systems – that Kolmogorov will present his theorem oh the persistence on invariant tori in his closing lecture at the 1954 International congress of mathematicians.

In this chapter we present a synthetic overview of the research on the three-body problem – which had been one of the starting points of Hamilton's own contribution – at the end of the 19th century, including the contribution of Poincaré, then we will now analyze the works that have contributed most to the development in the field of classical and celestial mechanics between the end of the 19th century and the beginning of the 20th century. Theories that will be the starting point of Kolmogorov's studies: a sort of cultural landscape in which we find the scientists and works that inspired Kolmogorov for his research in classical mechanics:

> "I[25] had thought for a long time about problems in celestial mechanics, from childhood, from Flammarion, and then — reading Charlier, Birkhoff, the mechanics of Whittaker, the work of Krylov and Bogolyubov, Chazy, Schmidt. I had tried several times, without results. But here was a begin-

[24]In the research of mechanics in the wake of Poincaré there is the echo of both statistical mechanics and quantum mechanics.

[25]Arnold is quoting Kolmogorov's words.

ning." [Arnold 2000, p. 90].

This scenario allows us to reconstruct – this is the main purpose of the chapter – a kind of *research group* conducted around 1930 by applying new mathematical tools, starting from a work by Bernard Koopman (1900-1981) on the application of Hilbert spaces to classical mechanics, a former student of George David Birkhoff and professor at Columbia University, published in 1931 in the Proceedings of the NAS, in which both John von Neumann (1903-1957), who was then starting his visits to Princeton, and Nikolaj Mitrofanovitch Krylov (1879-1955), professor in Kiev, participated his student Nikolaj Nikolayevich Bogolyubov (1909-1992). Birkhoff's research was at the center of the seminar founded in Moscow by Vyacheslaw Vassilievich Stepanov (1889-1950), in which the young Kolmogorov took part.

1.1 Between past and future: celestial mechanics at the turn of the two centuries

> ASTRONOMY is not only one of the most ancient of the physical sciences, but also one of those which present the most alluring invitations to the contemplative mind. The starry heavens, spangling with countless luminaries of every shade of brilliancy, and revolving in eternal harmony round the earth, constitute one of the most imposing spectacles which nature offers to our observation. The waning of the placid moon, the variety and splendour of the constellations, and the dazzling lustre of the morning and evening star, must in all ages have excited emotions of admiration and delight.
>
> [Grant (1852), *History of physical astronomy: from the earliest ages to the middle of the 19th century, comprehending a detailed account of the establishment of the theory of gravitation by Newton, and its development by his successors, with an exposition of the progress of research on all the other subjects of celestial physics.* p. i]

The stars, the planets, these small dots, barely visible to the human eye, apparently wander undisturbed in our sky, always fascinating different peoples and cultures, who wondered about their nature and their movements.

The illusory regularity in the movement of celestial bodies, often attributed to ultraterrestrial and divine intervention, has been the object of study, understood as a search for arithmetical methods for forecasting lunar and planetary phenomena, since the most ancient civilizations - just think of astronomy Babylonian, who from the eighth century B.C. compiled astronomical diaries with daily collections of planetary positions and other occasional and important events, or to the Egyptians, who developed stellar clocks, i.e. tables that indicated the apparent displacement of 36 stars, the so-called "decans", found inside numerous sarcophagi. The interest in astronomical phenomena is due to various reasons: first of all, from the knowledge of the periodicity of the skies derived that of the cycles of the seasons, fundamental for agriculture; in addition, the desire for knowledge and understanding of visible reality was certainly not lacking.

Studies on astronomy and the collection of astronomical data has accompanied the evolution of science and mathematical theories. I could say that we are dealing with one of the many examples in the history of science in which two sciences feed on each other: the need to understand the behavior of celestial bodies has led scientists and mathematicians to develop new theories and, at the same time, the he advent of new mathematical studies has allowed the intuition and formulation of new astronomical theories, which followed one another from culture to culture, thus preparing the field for Newtonian dynamics and the advent of what we will today call modern astronomy, or physical astronomy, or even, by Pierre Simon Laplace, *celestial mechanics*.

When in 1687 Isaac Newton (1643-1727) published *Philosophiæ Naturalis Principia*, in which the law of universal gravitation is found, he was the son of a particularly lively cultural context. Just to name a few, Newton's work came after the works of Nicolaus Copernicus (1473 - 1543), Tycho Brahe (1546 - 1601), the author of *Astronomia Nova* (1609) Johannes Kepler (1571 - 1630), Christiaan Huygens (1629 - 1695), and again Giovanni Domenico Cassini (1625 - 1712), the French abbot and astronomer Jean - Felix Picard (1620 - 1682).

With the systematisation of mechanics into a single corpus and Newton's formulation of mutual gravitational attraction, the mechanical study of celestial bodies could be deepened.

Indeed, the planets of the Solar System are approximately spherical in shape and very small in size relative to their mutual distances. For this reason, they can be considered material points and Newton's laws can be applied to the system.

If only the interaction between the Sun and each planet were taken into account, then the motion of each would describe an elliptical orbit around the Sun, with the Sun occupying one of the foci (Kepler's Laws). The interaction between only two celestial bodies, and the relative time evolution of their orbits, goes by the name of the Two-Body Problem. This was only solved geometrically by Newton himself, but rigorous resolution came thanks to later contributions from Swiss mathematicians Johann Bernoulli (1667-1748) and Leonhard Euler.

Solved, in the sense of classical mechanics, means that the system of differential equations describing the two-body problem has been proven to be *integrable*, i.e. that the physically interesting parameters (the semi-major axis of the elliptical orbit and the eccentricity of the orbit) remain constant over time or, in mathematical terms, are constants of motion.

The two-body problem: The two-body problem can be schematised as two material points moving in a three-dimensional Euclidean space; each point is therefore identified by three coordinates and, for this reason, the problem has 6 degrees of freedom. If we call $x_1 = (x_{11}, x_{12}, x_{13})$ and $x_2 = (x_{21}, x_{22}, x_{23})$ the spatial coordinates of the two bodies 1 and 2, m_1 and m_2 the two masses, and F_1 and F_2 the forces acting on bodies 1 and 2, respectively, then the equations of motion are

$$\begin{cases} m_1 \frac{d^2 x_1}{dt^2} = F_1(|x_1 - x_2|) \\ m_2 \frac{d^2 x_2}{dt^2} = F_2(|x_1 - x_2|) \end{cases}$$

For its resolution, it is shown that the problem reduces to two decoupled problems, one of which is a trivial uniform rectilinear motion and the other becomes a 2-degree-of-freedom problem. This means that it becomes a system of two ordinary differential equations in two unknowns, one of which depends on a single variable, and the solution can be found.

In the Solar System, however, not only the Planet-Sun interactions count but, even if of lesser intensity, there are also the Planet-Planet interactions or, again, the interactions between a Planet and its Satellite. These forces "perturb" the elliptical orbits described by the individual planets and, although the effect is slow, catastrophic cases over very long periods of time cannot be excluded a priori, such as the collision between two planets or the escape of a planet from its orbit.

An integrable problem, such as the two-body problem, in which the equations of motion are solved exactly, is thus perturbed by the small perturbations deriving from the other gravitational interactions and the resulting problem is generally no longer integrable.

In fact, the two-body problem is one of the few cases of integrable systems whose equations of motion are solved exactly. One-dimensional Hamiltonian systems, such as the harmonic oscillator and the simple pendulum; the so-called Lagrange top, Kovalenskaja's top, geodetic motion on an ellipsoidal surface etc...[26], are integrable, but the motion of a planet

[26] [Arnold 1992], [Gentile 2021], [Gentile 2022].

in the Solar System, taking into account the gravitational interactions with the other planets or other celestial bodies, is a problem becomes practically intractable from the mathematical point of view.

The planets are in constant motion, their positions with each other change over time and the force each exerts on all the others changes in direction and intensity during their orbits. If these forces compensated for each other, the planets would continue in the same elliptical orbits observed by Newton for infinite times. Otherwise, if the small perturbations do not compensate, the end result would be a collision between planets or a departure into space of one of the planets.

Mathematically solving a differential equation problem with so many variables becomes extremely complex.

However, it should be noted that the planet-planet and planet-satellite interactions remain very small compared to the interactions of the planets with the Sun - because the forces of gravitational attraction depend on the masses of the bodies interacting with each other, and those of the planets are much smaller compared to that of the Sun[27]. We are therefore dealing with systems - usually written in the Hamiltonian formalism - differ little from an integrable systems, such as the two body problem. This is the so-called *perturbation theory*, already addressed by Newton from a geometric point of view and which became toe focus of celestial mechanics, starting from the second half of the 18th century, with the contribution by Laplace , Lagrange, Charles Eugène Delaunay (1816-1872)[28] and Urbain Le Verrier (1811-1877).[29].

Perturbation theory deals with problems in which a small parameter appears which represents the measure of the difference between the sys-

[27]The mass of the planets is about a thousandth part of the mass of the Sun

[28]Charles-Eugène Delaunay (April 9, 1816 – August 5, 1872) was a French astronomer and mathematician. I will provide more details in §1.4.2

[29]for example, Delaunay developed a very precise theory of the motion of the Moon, based on the theory of perturbations [Delaunay 1860-67].

tem to be studied and a similar, "near" system which can be integrated.

This was the starting point in Poincaré's work at the end of the century.[30]

The aim of Johan August Hugo Gyldén (1841– 1896), Finnish astronomer at the Pulkovo and Stockholm observatories, was to find, through perturbation theory, mathematical series that described the orbits of the planets, even for arbitrarily long periods of time. In this way it would have been possible to answer the question whether the Solar System is stable.[31].

Let's go into more detail, to better understand how the perturbation theory acts on the problem of the motion of planets in the Solar System.

As already noted, in a first approximation, the interactions other than planet-sun could be neglected, due to their smallness. Therefore, we can start by considering the system of differential equations which takes into account only the interactions of the single planets with the Sun. This problem can be integrated: as for the two-body problem, the planets describe elliptical orbits around the Sun. Now, assuming that we no longer want to neglect the smallest interactions, the orbits will undergo variations which, although it could be irrelevant for short times (for example of the order of thousands of years), could have catastrophic effects in very long times, such as the collision of planets, the fall of one of them into the Sun or the departure of one of them from the solar system.

The so-called n-body problem, with $n \geq 3$, is therefore considerably more complex than its reduction to just two bodies - it is enough to consider that for $n = 3$ there is not yet a general solution[32]. In his *A treatise on the analytical dynamics of particles and rigid bodies; with ad introduction on the problem of three bodies* [Whittaker 1917], first published in 1904, the English

[30]*Mais le savant qui a rendu à cette branche de l'Astronomie les services les plus éminents est sans contredit M. Gyldén*, said Poincaré, in the introduction to the first volume of Les méthodes nouvelles de la mécanique céleste [Poincaré 1892-99].

[31][Markannen 2007], [Bohlin 1897]

[32]See [Barrow-Green 1997], [Marcolongo 1915] and [Whittaker 1899].

mathematician Edmund Taylor Whittaker (1873 - 1956) introduces chapter XIII, The reduction of the problem of three bodies, defining the problem as *the most celebrated of all dynamical problems*:

> The most celebrated of all dynamical problems is known as the Problem of Three Bodies, and may be enunciated as follows: Three particles attract each other according to the Newtonian law, so that between each pair of particles there is an attractive force which is proportional to the product of the masses of the particles and the inverse square of their distance apart: they are free to move in space, and are initially supposed to be moving in any given manner; to determine their subsequent motion. The practical importance of this problem arises from its applications to Celestial Mechanics: the bodies which constitute the solar system attract each other according to the Newtonian law, and (as they have approximately the form of spheres, whose dimensions are very small compared with the distances which separate them) it is usual to consider the problem of determining their motion in an ideal form, in which the bodies are replaced by particles of masses equal to the masses of the respective bodies and occupying the positions of their centres of gravity. The problem of three bodies cannot be solved in finite terms by means of any of the functions at present known to analysis. This difficulty has stimulated research to such an extent, that since the year 1750 over 800 memoirs, many of them bearing the names of the greatest mathematicians, have been published on the subject. [Whittaker 1917, p 339]

To describe the problem in terms of differential equations, we can use the symbology adopted by Whittaker himself in [Whittaker 1917].

The three-body problem: Let m_1, m_2 and m_3 be the masses of three bodies and r_{23}, r_{13} and r_{12} the reciprocal distances between them.
Given an orthogonal system of Cartesian axes $Oxyz$, we can denote with $(q_{11}, q_{12}, q_{13}), (q_{21}, q_{22}, q_{23})$ and (q_{31}, q_{32}, q_{33}) the coordinates of the

positions of the three masses with respect to it.
The force of attraction between two masses m_i and m_j is $F = km_im_jr_{ij}^{-2}$, with k is a constant and, with a suitable choice of units, we can assume $k = 1$.
The kinetic energy and potential energy of the system of three mutually attracting masses are, respectively:

$$T = \frac{1}{2}\sum_{i=1}^{3} m_i(\dot{q}_{i1}^2 + \dot{q}_{i2}^2 + \dot{q}_{i3}^2)$$

and

$$V = -\frac{m_2m_3}{r_{23}} - \frac{m_1m_3}{r_{13}} - \frac{m_1m_2}{r_{12}}.$$

Thus the equations of motion of the system formed by the three bodies is:

$$m_i\ddot{q}_{ij} = -\frac{\partial V}{\partial q_{ij}} \qquad i, j = 1, 2, 3$$

These are 9 second-order differential equations, and therefore the system has order 18.
Lagrange will prove that this system can be reduced to a system of 6th order[a].
If we want to write the equations in Hamiltonian form[b], we can denote

$$H = \sum_{i,j=1}^{3} \frac{p_{ij}^2}{2m_i} + V$$

where $p_{ij} = m_i\dot{q}_{ij}$ denotes the j-th component of the momentum of the mass body m_i. So, the equations of the three body system are:

$$\frac{dq_{ij}}{dt} = \frac{\partial H}{\partial p_{ij}}, \qquad \frac{dp_{ij}}{dt} = -\frac{\partial H}{\partial q_{ij}},$$

with $i, j = 1, 2, 3$.

[a]See [Whittaker 1917, pp. 338-355]
[b]See the appendix

It is possible to explicitly find its general solution for all times?

One way to prove the integrability - and therefore its complete resolution - of the problem is the search for the so-called uniform integrals. A uniform (or prime) integral for a problem defined by a system of differential equations can be defined as a function that remains constant along the solutions of a system. For example, total energy is a uniform integral of the 3-body problem, because it holds constant.

The existence of a number of independent prime integrals equal to the order of the system of differential equation (i.e. the number of degrees of freedom of the problem) implies the integrability of the problem.

Therefore, if we wanted to prove the solvability of the three-body problem through the existence of uniform integrals, we should find eighteen of them, independent of each other:

> [...] the problem of three bodies possesses 10 known integrals: namely the six integrals of motion of the centre of gravity, the three integrals of angular momentum, and the integral of energy; these are generally called the *classical* integrals of the problem. [Whittaker 1917, p 358]

The German mathematician and astronomer Ernst Heinrich Bruns (1848 - 1919), in a 1887's paper [Bruns 1887], demonstrated that for the general problem of the three bodies there are no other uniform integrals other than the classical ones[33].

Given the difficulties of the three-body problem, the study of the sys-

[33]See [Whittaker 1899, pp 157-159]

tem has been reduced to a simpler case: the so-called *restricted problem of three bodies*, in which a particle of negligible mass moves subject to the attraction of two other bodies of positive mass rotating in circles about their center of gravity.

Nevertheless, shortly after Bruns' paper, Poincaré formulated in his memoire [Poincaré 1890] - and subsequently in the first volume of [Poincaré 1892-99] - an extension of Bruns' theorem, proving the non-existence of uniform integrals also for the restricted three-body problem. Therefore, the integrability problem cannot be addressed by going this route.

Another way is to use perturbation theory.

The solutions of the equations of motion to be described by means of formal power series depend on the perturbation which deviates the problem from the closest integrable one. In addition to Gyldén's contribution, there was another Swedish scientists, Anders Lindstedt (1854-1939), who developed one of the series of perturbations that describe the solutions, still most used in celestial mechanics today.

While Gyldén was an astronomer with a strong theoretical bias, Lindstedt combine with practice his theoretical interest in the problem of the three bodies. The Lindstedt series was the method most used by Poincaré and by his successors, including Kolmogorov.

The main issue was then to study the convergence of these series which, in most cases, seemed to be divergent. The reason for the lack of convergence was due to disturb caused by the socalled *small denominators* (or *small divisors*. In fact, the construction of the series implies that within the coefficients of the terms there are denominators that can be zero or dangerously close to 0, causing the coefficients to tend to infinity and, therefore, making diverge the series itself.

These denominators take the form of linear combinations of frequencies of non-perturbed motions with integers, of the type:

$$m_1\omega_1 + m_2\omega_2 + \dots m_n\omega_n, \tag{1}$$

where ω_i, $i = 1 \dots n$ are real numbers representing the frequencies of the planets and $m_1, \dots, m_n$ are integers numbers.

If the ratio of frequencies are a rational number, these denominators can cancel and the corresponding term of the perturbation theory series becomes infinite. This situation is described nowadays as exact resonance between the planets each other, after a certain number of periods, the initial configuration of their mutual positions repeats itself. In the vicinity of a resonance, i.e. when the frequencies are close to being commensurable, the small divisors continue to be very close to zero and, in general, it is not possible to predict the dynamic effects that follow. The repetition of *close* configurations amplifies the perturbation effect and, in many cases, causes the instability of the resonant orbit.

An example: the frequencies of the motions of Saturn and Jupiter. In their motion of revolution around the sun, every day Saturn and Jupiter move with frequencies equal respectively to approximately

$$\omega_S = 120'' \qquad and \qquad \omega_J = 299'' \tag{2}$$

The two frequencies are almost commensurable since

$$5\omega_S - 2\omega_J \approx 0$$

Now, the series that describes the motion of the two planets, deriving from the perturbation theory, is of the type

$$\sum_{m,n\neq 0} \frac{a_{nm}}{n\omega_S + m\omega_J} e^{i(n\omega_S + m\omega_J)t} \tag{3}$$

and, therefore, we find in the denominator a quantity that, for infinite

n and m integers, is close to 0.

Difficulties due to small denominators accompanied the theories of celestial mechanics during the first half of the 20th century. We will see in chapter 3 that Kolmogorov obviated this problem by adopting a necessary condition on the ratio between the frequencies of the motions.

1.1.1 "Properties holding for almost all the initial states of the system": Henri Poincaré recurrence theorem (1890) towards a metrical approach to dynamical systems

One hundred years before the International Congress of Mathematicians in Amsterdam in 1954, Jules Henri Poincaré was born in Nancy.

A pioneer in the use of algebraic geometry and topology in the study of celestial mechanics, His work provided developments in the study of the three-body problem, with the introduction of a new approach, known as the *qualitative study of differential equations*.

The term *qualitative* refers to the study of the behaviour of the solutions of a system of differential equations, obtained through a geometric approach, without knowing an explicit expression for these solutions. This turned out to be necessary above all in celestial mechanics after, as we have seen, it was not possible to determine the solutions of the system explicitly.

Although this novelty finds its greatest application in the three volumes *Les méthodes nouvelles de la mécanique céleste* [Poincaré 1892-99], the starting point of his research in this field can be traced back to more than ten years earlier, to the article *Mémoire sur les courbes définies par une équation différentielle (I)* [Poincaré 1881], in which the author himself introduces the adjective qualitative in reference to the geometric study of the curve defined by the function under consideration:

> Une théorie complète des fonctions définies par les équations différentielles serait d'une grande utilité dans un grand nombre de questions de Math-

ématiques pures ou de Mécanique. Malheureusement, il est évident que dans la grande généralité des cas qui se présentent on ne peut intégrer ces équations à l'aide des fonctions déjà connues, par exemple à l'aide des fonctions définies par les quadratures. If, therefore, we wanted to restrict ourselves to the cases which we can study with definite or indefinite integrals, the field of our researches would be singularly diminished, and the immense majority of the questions which arise in applications would remain insoluble. Il est donc nécessaire d'étudier les fonctions définies par des équations différentielles en elles-mêmes et sans chercher à les ramener à des fonctions plus simples [...].

Rechercher quelles sont les propriétés des équations différentielles est donc une question du plus haut intérèt. On a déjà fait un premier pas dans cette voie en étudiant la fonction proposée dans le voisinage d'un des points du plan. Il s'agit aujourd'hui d'aller plus loin et d'étudier celte fonction dans toute l'étendue du plan. Dans cette recherche, notre point de départ sera évidemment ce que l'on sait déjà de la fonction étudiée dans une certaine région du plan. L'étude complète d'une fonction comprend deux parties:

1° Partie qualitative (pour ainsi dire), ou étude géométrique de la courbe définie par la fonction;

2° Partie quantitative, ou calcul numérique des valeurs de la fonction.

[Poincaré 1881, pp. 375-376][34]

[34]Eng.tr.: A complete theory of the functions defined by the differential equations would be of great use in a large number of questions of pure Mathematics or Mechanics. Unfortunately, it is obvious that in the great generality of the cases which arises, it is not possible to integrate these equations using the functions already known, for example using the functions defined by the quadratures. If we therefore wanted to restrict ourselves to the cases which we can study with inte. Whether defined or indefinite, the field of our researches would be singularly diminished, and the immense majority of the questions which presently apply would remain insoluble. It is therefore necessary to study the functions defined by differential equations in themselves and without seeking to reduce them to simpler functions [...].

Researching what are the properties of differential equations is therefore a question of the highest interest. We have already taken a first step in this direction by studying the

His interest in the theory of differential equations accompanied much of his scientific production, from the first article that appeared in 1878 to the last in 1912. However, from 1885, it is clear that his interest shifted towards celestial mechanics. In fact, the output of articles from that year onwards on differential equations mostly concerned questions of celestial mechanics. In that year, he published the article entitled *Sur l'équilibre d'une masse fluide animée d'un mouvement de rotation* in volume 7 of the journal "Acta Mathematica". In the same volume, on the first six pages, we find the announcement written by the publisher Gösta Mittag-Leffler, *Mittheilung, einen von König Oscar II gestifteten mathematischen Preis betreffend*[35] [Mittag-Leffler 1885], concerning the prize announced by King Oscar II in which Poincaré will participate, winning the prize for the first of the proposed topics:

> Etant donné un système d'un nombre quelconque de points matériels qui s'attirent mutuellement suivant la loi de NEWTON, on propose, sous la supposition qu'un choc de deux points n'ait jamais lieu, de représenter les coordonnées de chaque point sous forme de séries procédant suivant quelques fonctions connues du temps et qui convergent uniformément pour toute valeur réelle de la variable. Ce probléme dont la solution étendra considérablement nos connaissances par rapport au système du monde, paraît pouvoir être résolu à l'aide des moyens analytiques que nous avons actuellement à notre disposition; on peut le supposer du memoires, car LEJEUNE-DIRICHLET a communiqué peu de temps avant sa mort à un géomètre de ses amis qu'il avait découvert une méthode pour l'intégration

proposed function in the neighborhood of one of the points of the plane. It is now a question of going further and of studying this function in the whole extent of the plan. In this research, our starting point will obviously be what we already know about the function studied in a certain region of the plane. The complete study of a function understood due party:

1° Qualitative part (so to speak), or geometric study of the curve defined by the function;

2° Quantitative part, or numerical calculation of the values of the function.

[35] Eng.tr.: *Communication concerning a mathematical prize donated by King Oscar II.*

> des équations différentielles de la mécanique, et qu'en appliquant cette méthode il était parvenu à démontrer d'une manière absolument rigoureuse la stabilité de notre système planétaire. Malheureusement nous ne connaissons rien sur cette méthode, si ce n'est que la théorie des oscillations infiniment petites parait avoir servi de point de départ pour sa découverte. On peut pourtant supposer presque avec certitude que cette méthode était basée non point sur des calculs longs et compliqués, mais sur le développement d'une idée fondamentale et simple, qu'on peut avec raison espérer de retrouver par un travail persévérant et approfondi. Dans le cas pourtant où le problème proposé ne parviendrait pas à être résolu pour l'époque du concours, on pourrait décerner le prix pour un travail, dans lequel quelque autre problème de la mécanique serait traité de la manière indiquée et résolu complètement.[36]

This represents only the first of the four problems proposed in the competition, proposed by Karl Weierstrass (1815 - 1897), a member of the prize commission together with Charles Hermite (1822 - 1901). Indeed, the ques-

[36]Eng.tr.: Given a system of any number of material points which mutually attract each other according to NEWTON's law, we propose, under the assumption that a collision of two points never takes place, to represent the coordinates of each point as form of series proceeding according to some known functions of time and which converge uniformly for any real value of the variable. This problem, the solution of which will considerably extend our knowledge in relation to the system of the world, seems capable of being solved with the aid of the analytical means which we currently have at our disposal; one can suppose it from the memoires, because LEJEUNE-DIRICHLET communicated shortly before his death to a geometrician of his friends that he had discovered a method for the integration of the differential equations of mechanics, and that by applying this method he had succeeded in demonstrating in an absolutely rigorous manner the stability of our planetary system. Unfortunately we know nothing about this method, except that the theory of infinitely small oscillations seems to have served as a starting point for its discovery. We can, however, assume almost with certainty that this method was based not on long and complicated calculations, but on the development of a fundamental and simple idea, which we can with reason hope to recover by persevering and thorough work. In the event, however, that the proposed problem does not succeed in being solved by the time of the competition, the prize could be awarded for a work in which some other problem of mechanics would be treated in the manner indicated and solved completely.

tion reflected Weierstrass's strong interest in the n-body problem. This question is explored further in [Barrow-Green 1997], where we read in a footnote, on page 70:

> In a letter dated 15 August 1878, Weierstrass told Kovalevskaya that he had constructed a formal series expansion for solutions to the problem but was unable to prove convergence, and in 1880/81 he gave a seminar on the problems of perturbation theory in astronomy. Despite Weierstrass' own difficulties with the problem, certain remarks made by Dirichlet in 1858 had led him to believe that a complete solution was possible, and hence his choice of the Problem as one of the competition questions. Weierstrass' interest in the problem is chronicled in [Mittag-Leffler 1912].

Weierstrass here refers to the Lindstedt series, discussed in the previous paragraph.

Poincaré won the prize in January 1889[37], although the result presented did not meet the question posed in the prize.

In fact, he concentrated only on the problem of three bodies and instead of demonstrating that the Lindstedt series converges, his research led him to hypothesize, without being able to prove it, that they diverged.

He was asked to produce his memoir for publication as soon as possible in Acta Mathematica. Thus, in volume 13 of the 1890 Acta Mathematica "Sur le problème des trois corps et les équations de la dynamique" was published.

Here are found the main ideas of Poincaré and will be considered as the foundation of his later monumental work *Les méthodes nouvelles de la mécanique céleste*, which appeared in three volumes in the seven years from 1892 to 1899. It is in the memoire [Poincaré 1890] that we find the first original formulation of the so-called *Poincaré Recurrence Theorem*, mentioned by Kolmogorov in his 1954 speech.[38]

[37] see [Barrow-Green 1997], [Diacu, Holmes 1996], [Dumas 2014] for full details

[38] In [Barrow-Green, p 113] the author underlines that the original formulation of the theorem is already found in the draft memoire of 1889, never published: *Sur le probléme*

The theorem, in its original formulation, is stated as follows:

Theorem 1 (Poincaré Recurrence) *Supposons que le point P reste à distance finie, et que le volume $\int dx_1 dx_2 dx_3$ soit un invariant intégral*[39]*; si l'on considère une region r_0 quelconque, quelque petite que soit cette région, il y aura des trajectoires qui la traverseront une infinité de fois.*[40]

The theorem, with its characteristic geometric nature, will be the forerunner of Birkhoff's studies and of the birth of the ergodic theory [Sinai 1976], [Barrow-Green 1993], [Chenciner 2012], as Kolmogorov will say in his Amsterdam speech:

> After the work of H. Poincare, the fundamental role of topology for this range of problems became clear. On the other hand, the Poincaré-Carathéodory recurrence theorem initiated the "metrical" theory of dynamical systems in the sense of the study of properties of motions holding for "almost all" initial states of the system. This gave rise to the "ergodic theory", which was generalized in different ways and became an independent center of attraction and a point of interlacing for methods and problems of various most recent branches of mathematics (abstract measure theory, the theory of groups of linear operators in Hilbert and other infinite-dimensional spaces, the theory of random processes, etc.). At the preceding International Congress in 1950 the extensive paper by Kakutani was devoted to general problems of ergodic theory. [Kolmogorov 1957, pp. 355-356].

This theorem, together with the theorem of non-existence of uniform integrals for the three-body problem, and with many other results developed in the memoirs, finds a more conscious accommodation in the three volumes of *Les Méthodes nouvelles de la mécanique céleste* [Poincaré 1892-99].

---

des trois corps et les équations de la dynamique avec des notes par l'auteur - mémoire couronné du prix de S. M. le Roi Oscar 11. Printed in 1889 but not published.

[39] It means that the volume of the region is conserved

[40] Eng.tr.: Suppose that the point P remains at a finite distance, and that the volume $\int dx_1 dx_2 dx_3$ is an integral invariant; if we consider any region r_0, however small this region may be, there will be trajectories which will cross it an infinity of times.

The introduction to the first tome of the work is a remarkable historical document: Poincaré traces the state of the art of celestial mechanics and describes the developments to which he contributed in a clear and concise manner.

I quote here a few passages that are particularly significant for my historical reconstruction of the evolution of dynamics:

> Le Problème des trois corps a une telle importance pour l'Astronomie, et il est en même temps si difficile, que tous les efforts des géomètres ont été depuis longtemps dirigés de ce côté. Une intégration complète et rigoureuse étant manifestement impossible, c'est aux procédés d'approximation que l'on a dû faire appel. [...] Le but final de la Mécanique céleste est de résoudre cette grande question de savoir si la loi de Newton explique à elle seule tous les phénomènes astronomiques; le seul moyen d'y parvenir est de faire des observations aussi précises que possible et de les comparer ensuite aux résultats du calcul. Ce calcul ne peut être qu'approximatif et il ne servirait à rien, d'ailleurs, de calculer plus de décimales que les observations n'en peuvent faire connaître. Il est donc inutile de demander au calcul plus de précision qu'aux observations; mais on ne doit pas non plus lui en demander moins.
>
> Aussi l'approximation dont nous pouvons nous contenter aujourd'hui sera-t-elle insuffisante dans quelques siècles.
>
> [...] Cette époque, où l'on sera obligé de renoncer aux méthodes anciennes, est sans doute encore très éloignée; mais le théoricien est obligé de la devancer, puisque son oeuvre doit précéder, et souvent d'un grand nombre d'années, celle du calculateur numérique.
>
> [...] Ces méthodes, qui consistent à développer les coordonnées des astres suivant les puissances des masses,, ont en effet un caractère commun oui s'oppose à leur emploi pour le calcul des éphémérides à longue échéance. Les séries obtenues contiennent des termes dits séculaires, où le temps sort des signes sinus et cosinus, et il en résulte que leur convergence

pourrait devenir douteuse si l'on donnait à ce temps t une grande valeur.

La présence de ces termes séculaires ne tient pas à la nature du problème, mais seulement à la méthode employée.

[...] Mais le savant qui a rendu à cette branche de l'Astronomie les services les plus éminents est sans contredit M. Gyldén[41]. Son oeuvre touche à toutes les parties de la Mécanique céleste, et il utilise avec habileté toutes les ressources de l'Analyse moderne. M. Gyldén est parvenu à faire disparaître entièrement de ses développements tous les termes séculaires qui avaient tant gêné ses devanciers. D'autre part, M. Lindstedt a proposé une autre méthode beaucoup plus simple que celle de M. Gyldén, mais d'une portée moindre, puisqu'elle cesse d'être applicable quand on se trouve en présence de ces termes, que M. Gyldén appelle critiques.

[...] Il m'a semblé, d'autre part, que mes résultats me permettaient de réunir dans une sorte de synthèse la plupart des méthodes nouvelles récemment proposées, et c'est ce qui m'a déterminé à entreprendre le présent Ouvrage.[42]

---

[41]Johan August Hugo Gyldén (May 29, 1841– November 9, 1896) was a Finnish astronomer primarily known for work in celestial mechanics. I will provide more details in §1.4.2

[42]Eng.tr.:The Three-Body Problem has such importance for astronomy, and it is at the same time so difficult, that all the efforts of geometers have long been directed in this direction. A complete and rigorous integration being obviously impossible, it is to the processes of approximation that one had to appeal. [...] The ultimate goal of Celestial Mechanics is to resolve this great question whether Newton's law alone explains all astronomical phenomena; the only way to achieve this is to make observations as precise as possible and then compare them with the results of the calculation. This calculation can only be approximate and it would serve no purpose, moreover, to calculate more decimals than the observations can make known. It is therefore useless to demand more precision from the calculation than from the observations; but neither should one ask less of it.

Also the approximation with which we can content ourselves today will be insufficient in a few centuries.

[...] This time, when we will be obliged to renounce the old methods, is doubtless still very distant; but the theoretician is obliged to precede it, since his work must precede, and often by a large number of years, that of the digital computer.

[...] These methods, which consist in developing the coordinates of the stars according to the powers of the masses, have in fact a common character which is opposed to their

[Poincaré 1892-99, vol I, pp 1-5.]

The first volume dealt with periodic solutions and the non-existence of uniform integrals, as well as asymptotic solutions to the three-body problem, while the second volume focused on the multiple perturbation series methods developed up to then by Newcomb, Gyldén, Lindstedt and Bohlin and their applications to the three-body problem.

Finally, the last one, which appeared six years after the second, delved into integral invariants, periodic solutions of the second kind and doubly asymptotic solutions, the latter introduced by Poincaré himself in the prize memoir.

The difficulties highlighted by the various methods listed by Poincaré on the convergence of power series are characteristic not only of problems of celestial mechanics, but of all problems "close" to integrable problems, with which perturbation theory deals.

For this reason, Poincaré defined on page 32 of volume 1 what he will call *Problème général de la Dynamique*. Let's see what it is, using the same nomenclature used by the French mathematician.

The general problem of dynamics: Let us consider the study of the movement of q material bodies, free to move in space; each of them

use for the calculation of long-term ephemeris. The series obtained contain so-called secular terms, where the time comes out of the sine and cosine signs, and it follows that their convergence could become doubtful if one gave this time t a large value.

The presence of these secular terms is not due to the nature of the problem, but only to the method employed.

[...] But the scholar who has rendered this branch of astronomy the most eminent service is without a doubt Mr. Gyldén. His work touches on all parts of Celestial Mechanics, and he skilfully uses all the resources of modern Analysis. Mr. Gyldén has succeeded in eliminating entirely from his developments all the secular terms which had so embarrassed his predecessors. On the other hand, Mr. Lindstedt has proposed another method much simpler than that of Mr. Gyldén, but of less import, since it ceases to be applicable when one finds oneself in the presence of these terms, which Mr. Gyldén calls criticism.

[...] It seemed to me, on the other hand, that my results allowed me to bring together in a kind of synthesis most of the new methods recently proposed, and this is what determined me to undertake the present work.

will be characterized by a mass $m_1, \ldots, m_q$, by the three spatial coordinates: (x_1, x_2, x_3) for the first body, (x_4, x_5, x_6) for the second body, ..., $(x_{3q-2}, x_{3q-1}, x_{3q})$ for the last body and by the three spatial coordinates of the momentum (y_1, y_2, y_3) for the first body, (y_4, y_5, y_6) for the second body, ..., $(y_{3q-2}, y_{3q-1}, y_{3q})$ for the last body, with respect to a fixed reference system.

Since Newton's formulation we have seen that the equations of motion correspond to a system of n second-order differential equations.

With the Lagrangian and Hamiltonian formalisms the equations take on a new form, becoming a system of first order differential equations in a space of 2n variables (double, with respect to the first), in which the coordinates of the points are identified by their positions and their momentum.[a]

A force will act on each body, resulting from the gravitational interactions between the masses, which is also vectorial and formed by spatial components along the three directions: (F_1, F_2, F_3) for the first body, (F_4, F_5, F_6) for the second body, ..., $(F_{3q-2}, F_{3q-1}, F_{3q})$ for the last body.

If the system is conservative, there will exist a function V, called the *force function*[b] such that

$$F_i = \frac{dV}{dx_i}$$

Also, we can define the live half force[c], that have the form:

$$T = \frac{y_1^2 + y_2^2 + y_3^2}{2m_1} + \frac{y_4^2 + y_5^2 + y_6^2}{2m_2} + \cdots + \frac{y_{3q-2}^2 + y_{3q-1}^2 + y_{3q}^2}{2m_q}$$

Thus, the equation of live forces can be written as

$$T - V = const.$$

and, written

$$T - V = F(x_1, x_2, \ldots, x_{3q}, y_1, y_2, \ldots, y_{3q}),$$

the equations of motion are described by

$$\frac{dx_i}{dt} = \frac{dF}{dy_i} \qquad \frac{dy_i}{dt} = -\frac{dF}{dx_i} \tag{4}$$

Now, proceeding in an analogous way to formalize the problem of the three bodies, Poincaré observed that, since two of them have much smaller masses than the third, their masses can be written as a product between a very small value μ and a finite value (for example: $m_1 = \mu\alpha_1$ and $m_2 = \mu\alpha_2$, with α_1, α_2 finite numbers).
Then it may be an advantage to develop F in increasing powers of μ

$$F = F_0 + \mu F_1 + \mu^2 F_2 + \ldots \tag{5}$$

with F_0 not depending on any variable y_i. Whatever μ, F is a periodic function of period 2π with respect to the variables y_i.
Thus Poincaré defines *The general problem of dynamics* as the study of the canonical equations (4), assuming that the function F can expand in powers series as (5) and supposing that the function F_0 depends only on the variables $x_1, x_2, \ldots$ and that the successive F_i are periodic of period 2π with respect to the variables y_i.

[a]In modern terms, the space formed by the pairs (x, y) with x position vector and y momentum vector, forms a differential manifold and is called *phase space*.
[b]Today it is called potential energy
[c]Today it is called kinetic energy

The general problem of dynamics is the form of the new methods presented by Poincaré in his classic three volumes essay.

To give an overview of Poincaré's research in this area, we can avoid ourselves of the words written by the mathematician himself, in the article *Analyse des travaux scientifiques de Henri Poincaré faite par lui-même*, published in 1921 in volume 38 of "Acta Mathematica" [Poincare 1921]. In the 133 pages, all of the French mathematician's publications are first listed and then what he defines as "Résumé analitique" is reported, divided into seven items that represent the areas in which he carried out his work:

> J'ai classé les travaux que j'ai à résumer sous les sept rubriques suivantes:
>
> 1°. Equations Différentielles.
>
> 2°. Théorie générale des Fonctions.
>
> 3°. Questions diverses de Mathématiques pures (Algèbre, Arithmétique, Théorie des Groupes, Analysis Situs).
>
> 4°. Mécanique Céleste.
>
> 5°. Physique Mathématique.
>
> 6°. Philosophie des Sciences.
>
> 7°. Enseignement, vulgarisation, divers (Bibliographie, rapports divers). [Poincaré 1921, p. 36][43].

In particupar, the section on celestial mechanics works begins on page 102, where he makes a very clear summary of his results. Divided into very short and discursive subsections, the first is entitled *Généralités sur les Équations de la Dynamique et de la Mécanique Céleste*:

> Les équations de la Dynamique présentent des propriétés remarquables

[43]Eng.tr.: I have classified the work I have summarised under the following seven headings:
1°. Differential equations.
2°. General Theory of Functions.
3°. Various questions of pure Mathematics (Algebra, Arithmetic, Group Theory, Analysis Situs).
4°. Celestial Mechanics.
5°. Mathematical Physics.
6°. Philosophy of Science.
7°. Teaching, popularisation, various (Bibliography, various reports).

qui ont été mises en évidence par JACOBI dans ses Vorlesungen[44].

Quelles sont les conséquences plus ou moins immédiates de ces propriétés? Quel partie peut-on en tirer pour la mise en équation des problèmes de Dynamique et en particulier des problèmes de Mécanique Céleste? Telle est la première question dont je veux parler ici.

J'ai été amené à passer en revue les principales propriétés des équations canoniques (183, 278). Les propriétés sont classiques; et je n'ai eu qu'a perfetionner certains détails; en me servant surtout du caractère bien connu qui permet de reconnaître si un changement de variables conserve la forme canonique des équations.

Ce genre de transformations facilite la mise en équation du problème des trois corps; c'est ce que j'ai montré (164, I87). On sait que dans le procédé classique on rapporte toutes les planètes à des axes mobiles passant par le Soleil. L'inconvénient est que la fonction perturbatrice n'est pas la même pour toutes les planètes. Un autre procédé consiste à rapporter chaque planète au centre de gravité du système formé par le Soleil et toutes les planètes inférieures à celle que l'on considère. L'inconvénient est évité, mais la fonction perturbatrice est un peu plus compliquée. J'ai proposé un troisième procédé, dans lequel les coordonnées de chaque planète sont rapportées au Soleil, et sa vitesse à des axes fixes.

Malgré les travaux dont les équations canoniques ont été l'objet depuis JACOBI, toutes leurs propriétés ne sont pas connues, ou plutôt on n'a pas insisté sur toutes les formes que peuvent revêtir ces propriétés et qu'il peut être utile de connaître. Si par exemple on étudie les équations aux variations des équations de la Dynamique, c'est à dire les équations qui définissent une solution infiniment peu différente d'une solution donnée, on rencontre des propositions importantes sur lesquelles j'ai attiré l'attention (183, 278).

D'un autre côté, j'ai été amené à introduire une notion nouvelle, celle

[44] *Vorlesungen uber dynamik* (Lectures on Dynamics) by Karl Gustav Jakob Jacobi (1804-1851), First published in 1866

des invariants intégraux (I83, 280). Ce sont certaines intégrales définies simples ou multiples qui demeurent constantes, quand le champ d'intégration varie conformément à une certaine loi définie par une équation différentielle. Si par exemple on envisage les équations différentielles au mouvement d'un fluide incompressible, le volume est un invariant intégral.

Les équations canoniques de la Dynamique possèdent des invariants intégraux remarquables et l'existence de ces invariants jette une grande lumière sur leurs propriétés.

Pour en finir avec ces généralités sur les équations de la Dynamique et le problème des 3 corps, je signalerai un dernier travail (166). On sait que BRUNS a démontré que le problème des 3 corps ne saurait admettre d'autre intégrale algébrique que les intégrales classiques. Malheureusement dans sa démonstration subsistait une lacune grave et particulièrement délicate à combler. J'ai été assez heureux pour mettre la belle et ingénieuse démonstration de M. BRUNS à l'abri de toute objection.[45]

[45]Eng. Tr.: The equations of Dynamics have remarkable properties that were highlighted by Jacobi in his Vorlesungen.
What are the more or less immediate consequences of these properties? What part can one draw from it for the setting in equation of the problems of Dynamics and in particular of the problems of Celestial Mechanics? This is the first question I want to discuss here.
I have been led to review the main properties of the canonical equations (183, 278). The properties are classic; and I only had to perfect certain details; by using above all the well-known character which makes it possible to recognize whether a change of variables preserves the canonical form of the equations.
This kind of transformation facilitates the equation of the three-body problem; this is what I have shown (164, I87). It is known that in the classical method all planets are related to moving axes passing through the Sun. The disadvantage is that the perturbation function is not the same for all the planets. Another method consists in relating each planet to the centre of gravity of the system formed by the Sun and all the planets below the one under consideration. The disadvantage is avoided, but the perturbation function is a little more complicated. I have proposed a third procedure, in which the coordinates of each planet are related to the Sun, and its speed to fixed axes.
In spite of the work that has been done on the canonical equations since Jacobi, not all their properties are known, or rather not all the forms that these properties can take and that it can be useful to know have been insisted on. If, for example, one studies the equations of variations of the equations of Dynamics, that is to say, the equations which

The proof to which he refers in the last sentence is contained in chapter 5 of the first volume of [Poincaré 1892-99], on page 233. Nowaday is called *Non-existence des intégrals uniforms*, [Fermi 1923b], [Benettin, et al. 1985],.

To this theorem must be added the discovery of some complex solutions - called by Poincaré *asymptotic* and *doubly asymptotic* (that is, in infinite time past and future) - and of some trajectories, called *homoclinic*, so complex that they cannot be drawn. Thus he marks the beginning of a very delicate moment in the history of celestial mechanics: once it became clear that the method of direct integration could no longer be pursued and that some particular solutions were anything but simple, the study of the qualitative and global aspects of motion became the new paradigm. Not only that: the possibility of being faced with a mathematical description that is anything but stable has made its way more and more, creating an ever-widening gap between astronomers and their direct measurements and mathematicians and their chaotic theories about the solar system.

---

define a solution infinitely little different from a given solution, one encounters important propositions to which I have drawn attention (183, 278).
On the other hand, I was led to introduce a new notion, that of integral invariants (I83, 280). They are certain simple or multiple definite integrals which remain constant, when the field of integration varies according to a certain law defined by a differential equation. If, for example, we consider the differential equations of motion of an incompressible fluid, the volume is an integral invariant.
The canonical equations of Dynamics have remarkable integral invariants and the existence of these invariants sheds much light on their properties.
To finish with these generalities on the equations of Dynamics and the 3-body problem, I will mention one last work (166). It is well known that Bruns proved that the 3-body problem cannot admit any other algebraic integral than the classical ones. Unfortunately, in his proof there was a serious gap that was particularly difficult to fill. I was fortunate enough to protect Mr. Bruns' beautiful and ingenious demonstration from any objections.

1.1.2 Ferrying classical mechanics to the 20^{th} century: Edmund Wittaker's A treatise on the analytical dynamics of particles and rigid bodies (1904)

At the end of the 19th century celestial mechanics and the problem of the stability of the solar system were at the center of interest of the international mathematical community and the revolutionary new theories of Henri Poincaré played a leading role in this field. The question posed by the same French mathematician *de savoir si la loi de Newton explique à elle seule tous les phénomènes astronomiques* still remains open. Although Poincaré's work was widely recognized during his lifetime, many parts of him remained enigmatic afterwards [Dumas 2014, p 43]. This mainly depended on two factors: on the one hand, the French mathematician did not make an effort to condense, polish and check his work [Dumas 2014]; on the other hand, mathematics itself in the 20th century was affected by the restructuring of ideas: previous theories were not rejected but assumed in new visions, becoming unrecognizable from the original ones of the mathematicians of the past. The field of classical mechanics itself has not been spared by the wave of change:

> Once a flourishing subject, where a remarkable cross-breeding of mathematics and physics took place, classical mechanics was considered by many to have reached a dead end by the first decades of the twentieth century, except for eventual applications to other fields.
>
> [...] So dramatic have been the changes that mechanics has undergone in the twentieth century that the style and even the contents of most books on dynamics written before the 1930s look hopelessly dated to present-day readers. But there are exceptions [Coutinho 2014, p. 356].

Severino Collier Coutinho refers to the works of the British mathematician, of Scottish origin, born in 1873, when Poincaré was just 18 years old: Edmund Taylor Whittaker (1873-1956).

In the year of the publication of the last of the three volumes of Les méthodes nouvelles, Whittaker was in Cambridge, England, when the British Association asked him for a report on the state of research on the three-body problem. The English mathematician and astronomer William Hunter McCrea (1904 - 1999) reported in [McCrea 1957] the notice:

> Whittaker's interests in dynamics and optics were closely linked with an interest in their astronomical applications. As early as 1898 the Council of the British Association resolved "that Mr. E. T. Whittaker be requested to draw up a report on the planetary theory". Besides, in those days an interest in astronomy was more general amongst mathematicians than it has since become, and most professional mathematicians in the country joined the Royal Astronomical Society. [McCrea 1957, p 236]

The following year, Whittaker wrote *Report on the Progress of the Solution of the Problem of Three Bodies*, a report covering the last thirty years of research, up to Poincaré's very recent studies:

> The Report attempts to trace the development of the subject in the last thirty years, 1868-98; this period opens with the time when the last volume of Delaunay's "Lunar Theory" was newly published; it closes with the issue of the last volume of Poincaré's "New Methods in Celestial Mechanics". Between the two books lies the development of the new dynamical astronomy.
>
> The work will be distributed under the following seven headings:
>
> §I. The differential equations of the problem.
>
> § II. Certain particular solutions of simple character.
>
> § III. Memoirs of 1868-89 on general and particular solutions of the differential equations, and their expression by means of infinite series (excluding Gyldén's theory).
>
> §IV. Memoirs of 1868-89 on the absence of terms of certain classes from the infinite series which represent the solution.
>
> § V. Gyldén's theory of absolute orbits.
>
> § VI. Progress in 1890-98 of the theories of §§ III and IV
>
> § VII. The impossibility of certain kinds of integrals. [Whittaker 1899, p.

122].

Section VI is dedicated to the developments of Poincaré in "Les méthodes nouvelles de la mécanique céleste". It provides a very accurate description of some of the aspects addressed by the French mathematician (such as periodic and asymptotic solutions and invariant integrals); he underlies the importance of some of his results, such as the recurrence theorem [Whittaker 1899, p 145], reporting the original formulation of the fundamental problem of dynamics [Whittaker 1899, p 147].

Whittaker held the position of secretary of the RAS from 1901 to 1906, became Astronomer Royal of Ireland, moved to Dunsink Observatory - the same observatory where Hamilton had worked - and was appointed Professor of Astronomy at Dublin University in 1906.

Five years after his report on the problem of three bodies, he published the first edition of his monumental work on analytical mechanics, entitled *A treatise on the analytical dynamics of particles and rigid bodies; with an introduction to the problem of three bodies*.

McCrea emphasises the importance of the figure of Whittaker for the British mathematics , especially with reference to his work in the field of dynamics:

> The name of Sir Edmund Whittaker will always hold a unique place in the history of British mathematics. It may reasonably be claimed that no single individual in this century or the last had so far-reaching an influence upon its progress. If such a claim comes as a surprise to some present-day readers, it is probably because we are apt to forget the part that Whittaker played personally in bringing about so many of the developments that we now take for granted.
>
> British nineteenth-century mathematics was deplorably insular, apart from the work of a very few of its most distinguished men in certain particular fields. Whittaker, more than anyone else, brought about the transformation to something that was more abreast of developments elsewhere

while, happily, still bearing characteristic features of its own.

[...] He was the first to make available in this country a comprehensive account of the special functions of analysis. Further, what Forsyth[46] and Whittaker did for analysis, Whittaker alone did for applied mathematics by his Analytical Dynamics.

[...] Moreover, with an inspired appreciation of what is in the best sense useful in mathematics, he has included in his books much that was found to be needed in the development of quantum mechanics and wave-mechanics more than twenty years afterwards. The part that British workers in particular were thus enabled to contribute to this development owes a debt to Whittaker which seems scarcely to have been sufficiently acknowledged. [McCrea 1957, p 234].

Whittaker's Analytical Mechanics was the first book to provide a systematic account in English of the theory arising from Hamilton's equations ([McCrea 1957]).

In a recent paper in "Archive for History of Exact Sciences", [Coutinho 2014], The author traces the history and the wide spatial and temporal diffusion of the essay. He has attempted to study the reasons that made this work so enduring, even in times when many of the contemporary works were shelved and deemed obsolete. Published in 1904, it had four editions, translations into German and Russian and is still in print today[47]:

What were the qualities that allowed Whittaker to write a book [...] that remains useful to mathematicians working in several different areas, more than one hundred years after it was written? [...] this was in good measure due to Whittaker's great knowledge of the literature and to his ability to organize this knowledge in a systematic way. Moreover, his reading was

[46] Andrew Russel Forsyth (Glasgow, 18 June 1858- South Kensington 2 June 1942) was a British mathematician, of whom Whittaker was the only (at least official) student. He wrote important works on analysis which were responsible for introducing foreign research to Britain.

[47] An edition dated 27 December 2022 by Cambridge University Press is currently on sale

not limited to contemporaneous works, it also encompassed the classics of the 18th and 19th centuries. [...] It seems to me that the success of Whittaker's books owes much to the fact that he was one of that rare breed, a scientist who is also a scholar, of which D'Arcy Thompson is probably the best known representative. People whose research may not have been exceptional, but whose great knowledge of the literature, including historical works, allowed them to "crystallize" in their books a vision of a whole subject that would greatly influence later generations. [Coutinho 2014, p 403]

1.1.3 The Scandinavian research tradition: Die Mechanik des Himmels (1902-07) by Carl Ludvig Charlier (1862-1934)

In the last decade of the 19th century, the theory of singularities in the three-body problem was developed by the French mathematician Paul Prudent Painlevé (1863–1933).

Singularities are closely related to collisions between bodies, since each collision corresponded to a singularity in the differential equations of the problem. Therefore, the goal was to try to eliminate singularities, so as to be able to study the motion of the system even after a possible collision. Furthermore, the question was raised whether singularities arise only from collisions or whether there are other phenomena connected to them.

In 1896, Painlevé published *Sur les singularités des équations de la Dynamique* [Painlevé 1896], an in-depth analysis of the study of being, where he demonstrated that the only possible singularities were those due to collisions.

The question concerning the singularities of motion in the three-body problem also found fertile ground in the Scandinavian scientific environment, which was to play a leading role in the history and evolution of astronomy and celestial mechanics in those years.

For almost two centuries, until 1809, the Finnish-Swedish union was not only geographical and political, but also manifested itself in the links between the universities and academies of the two countries. Astronomy and celestial mechanics represented privileged fields of study and from the second half of the eighteenth century, with the construction of the Stockholm, Uppsala and Lund Observatories, research intensified.

Given their geographical position, the two countries could count on collaborations with Germany towards the West and, subsequently, towards the East with Russia.

Furthermore, after Sweden's defeat against Russia in 1809 and the cession of Finland to Russia, becoming the Grand Duchy of Finland, new opportunities opened up for Finnish astronomers and celestial mechanics to collaborate with their Russian neighbours.

In fact, if in the eighteenth century most of the studies were of Swedish dominion, the situation of astronomical research changed together with the changing of the political balance.

Between 1831 and 1834, on the Ulrikasborg Hill (Observatory Hill Park) in Helsinki, the architect Carl Ludvig Engel (1778-1840), together with the collaboration of Professor Friedrich Wilhelm Argelander (1799-1875), completed the construction of one of the most modern observatories of that weather. The Helsinki observatory ended up influencing the next major observatory project in the Russian Empire, that of the main imperial observatory at nearby Pulkovo, just south of nearby St. Petersburg.

The ambitious Finnish astronomers, with their observatory and the newly created connections, had great opportunities to get an excellent education at Pulkovo, much more than if they had been under Swedish rule.

Representative of this scientific fervor is Karl Frithiof Sundman (1873 - 1949) Finnish mathematician and astronomer who, after graduating in 1897, went to the Pulkovo Observatory to continue his research on astronomy. He demonstrated the existence of a solution in convergent infinite series to the three-body problem, using analytical methods for the regularization of the motion, i.e. the elimination of singularities through a suitable series of transformations.[48].

On the other hand, from neighboring Sweden we cannot ignore the theories related to the names of Anders Lindstedt, Johan August Hugo Gyldén - names already encountered in the previous paragraphs - and Carl Ludwig Charlier.

Charlier (1862-1934) defends his thesis on *Untersuchung über die allgemeinen Jupiter-Störungen des Planeten Thetis*[49] in 1887, as a student of Gyldén, at Uppsala University. Thanks to the quality of this work he was immediately appointed professor at the same university[50]. In the autumn of 1898 he gave lectures on general celestial mechanics which contained - as he himself revealed in the preface of the first volume [Charlier 1902-7] - the main topics of his two volumes of *Die mechanik des Himmels*, published in 1902 and 1907.

In the preface on page iii he declares the main intent of the texts:

> Als Ziel habe ich mir gesteckt, eine möglichst einheitticlie Darstellung des jetzigen Standpunkts der Untersuch nugen über die Mechanik des Himmels, insofern sieb dieselbe mit der Bewegung von Massenpunkteu beschäftigt, zu geben. Es ist dabei mein Hauptstreben gewesen, die astronomisch wichtigen Resultate hervorzuheben, indem ich gleichzeitig die mathematische Eleganz und Schärfe, welche besonders die neueren Untersuchungen auf diesem Gebiete ermöglicht haben, zum Ausdruck zu bringen suchte.[51] [Charlier 1902-7, vol 1, p III]

Like and contemporary with Whittaker's works, the volumes represent a

[48][Sundman 1907], [Sundman 1910],[Sundman 1913]

[49]Eng.tr.: Investigation of the general disturbances of Jupiter of the planet Teti

[50][Wicksell 1935]

[51]Eng.tr.: My goal has been to provide as uniform a presentation as possible of the current point of view of the investigations of the mechanics of the heavens, regarding the movement of points of mass. My main goal has been to emphasize the astronomically important results, while trying to express the mathematical elegance and sharpness that especially the most recent investigations in this field have made possible.

clear and complete systematization of studies in celestial mechanics at the beginning of the new century.

And it is Whittaker who cites the results of the Swedish mathematician several times, already in his report for the British Association of 1899:

> Poincaré's paper gave a fresh stimulus to the investigation of periodic solutions. In 1890 v. Haerdtl[52] calculated numerically two cases of the restricted problem of three bodies. Charlier in 1892 discussed the same cases by means of expansions proceeding in ascending powers of the time, and the same author in 1893 found a set of periodic solutions of the problem of three bodies in a plane, whose expansion involves four arbitrary constants. [Whittaker 1899, p 151]

> Brown[53] in 1897 discussed the properties of the general solution in trigonometric series of the problem of three bodies, by supposing it to have been derived by integrating the Hamilton-Jacobi equation.
>
> [...] Researches relating to the convergence of the trigonometric series of dynamical astronomy were published in 1896 by Charlier and in 1898 by Poincaré. The former, by expanding in descending powers of m the coefficient of the mth term in such a series, arrived at the conclusion that the convergence can be augmented by dividing the function expressed into two parts, one of which depends on the first terms in these expansions of the coefficients. [Whittaker 1899, pp 156-157].

We will see later that the works of both mathematicians, Whittaker and Charlier, were among those that Kolmogorov will deepen before dedicating himself to the works on classical mechanics, the subject of this thesis.

[52]Eduard Freiherr von Haerdtl (1861- 1897) was an Austrian astronomer, who became the first professor of astronomy at the University of Innsbruck in 1892.

[53]Ernest William Brown (1866 - 1938) was an English mathematician and astronomer, known in the field of celestial mechanics for his studies on lunar movements.

1.1.4 Classical and modern mechanics: Jean-François Chazy (1882-1955) and the capture in the three body problem

The 20th century brings with it the advent of the theories of relativity and quantum mechanics, and celestial mechanics - like the other fields of classical mechanics - has been heavily affected by the ongoing restructuring of the sciences. Classical mechanics, an essential pillar of a 19th-century mathematician and the origin of the development of modern European mathematics, is marginalized - albeit with some notable exceptions.

On closer examination, one realizes that two fundamental issues contribute to the change that occurred at the beginning of the last century. On the one hand, using the words of Dumas in [Dumas 2014 p.7], *not surprisingly, in that period, physicists abandoned classical mechanics to the few hardy mathematicians who remained interested in it. The physicists returned with wondrous stories of their exploits in quantum mechanics, relativity, and nuclear physics.*

On the other hand, it is precisely with the formulation of the principles of the theory of relativity that shadows were cast on the validity of Newton's laws and Galilean transformations - transformations which relate the coordinates describing the same phenomenon from two distinct reference systems. Classical theories appeared obsolete and celestial mechanics perhaps had to be revised on the basis of the new relativistic ideas.

Although there is widespread belief in the clear separation between classical and modern theories - or in the replacement of the new paradigm at the expense of the previous one - what really occurred among the few mathematicians who continued to deal with celestial mechanics was a coexistence of the two. There is no shortage of examples of scholars who dedicated themselves both to celestial mechanics and to relativity.

Poincaré himself dealt with questions on the simultaneity of times and Lorentz's transformations - which replaced the Galilean ones - in [Poincaré 1900] even before the formulation of the principles by Albert Einstein in

1905. The Italian mathematician Tullio Levi-Civita (1873 -1941) and the French mathematician and astronomer Jean François Chazy (1882 - 1955) are just some of scholars who worked in both fields.

After having been mobilized in the French troops in 1914 and sent to the sound reconnaissance laboratory set up at the École Normale Supérieure in Paris, Chazy only returned to his research at the University of Lille in 1919. He published extensively on the three-body problem - such as [Chazy 1922], [Chazy 1924], [Chazy 1929] - worked also on the subject of relativity and its application in celestial mechanics, published the essay *La Théorie de la Relativité et la Mécanique céleste* [Chazy 1928-30], in two volumes[54].

Chazy's main contribution on the three-body problem regarded the final trend of the motion of the three-body problem, that is for times very close to infinity, even after a possible collision between the bodies themselves. He categorized the possible final moves - seven in all: hyperbolic motions, hyperbolic-elliptic motions, oscillating motions, constrained motions, parabolic-elliptic motions, hyperbolic-parabolic motions, parabolic motions - and analyzed each of them in detail.

In particular, Chazy theorized the impossibility of capture in the three-body problem.

The French mathematician Darmois, in drafting "Notice sur la vie et les travaux de Jean Chazy (1882-1955)", published in 1957, wrote about it:

[54]On trouve, dans les deux livres de Jean Chazy, toutes les notions nécessaires de géométrie différentielle générale, les méthodes générales de calculs et de formation des équations d'Einstein, l'étude des questions classiques.

Nous insisterons sur le problème du périhélie de Mercure qui avait été l'objet, nous l'avons vu, d'une discussion approfondie par Jean Chazy.[55] [Darmois 1957, pp 42-43] .

Georges Darmois (1888-1960) refers in particular to some applications of relativity to the motions of Mercury - the only planet in the solar system on which, as a consequence of its proximity to the Sun, the theory of relativity has obtained more precise results than the classical theories - which showed a still inexplicable body, despite the vain efforts to discover perturbing masses.

Les résultats ainsi obtenus, qui assujettissent le point représentatif à demeurer dans une région ou sur une surface, ont permis à Jean Chazy d'affirmer l'impossibilité dans certains cas de l'écartement indéfini correspondant à une dislocation d'un système. C'est ainsi que si l'un des corps vient de l'infini (dans une direction non parallèle au plan du mouvement des deux autres), il ne peut que s'en éloigner indéfiniment au bout d'un temps fini passé en leur voisinage. Les deux corps reviennent alors à un mouvement relatif elliptique.

Ce résultat généralisait et précisait une étude de Schwarzschild[56] faite dans le cas d'un troisième corps de masse nulle. Signalons que de nouvelles recherches sont entreprises, surtout en URSS, sur ce sujet.[57] [Darmois 1957, p 40].

We will see, in fact, that the developments in the USSR to which he referred had already been published in 1947 and 1953 by the scientists Kirill Aleksandrovich Sitnikov (1926 - ?) and Otto Yulyevich Schmidt(1891-1956) ([Sitnikov 1953] e [Schmidt 1947]), which found counterexamples to the validity of his capture theory in three-body problem, refuting the French mathematician.

1.1.5 Otto Yulyevich Schmidt (1891-1956): A Soviet contribution in 1947

Pour une époque comme le premier tiers du XXe siècle, il est en général difficile d'étudier la science astronomi. que à l'intérieur des frontières d'un

[56]It refers to Karl Schwarzschild (1873 – 1916), a German mathematician, astronomer and astrophysicist.

[57]End.tr.: The results thus obtained, which subject the representative point to remaining in a region or on a surface, enabled Jean Chazy to affirm the impossibility in certain cases of the indefinite separation corresponding to a dislocation of a system. Thus, if one of the bodies comes from infinity (in a direction not parallel to the plane of motion of the two others), it can only move away from it indefinitely after a finite time spent in their neighborhood. The two bodies then return to an elliptical relative motion.

This result generalized and specified a study by Schwarzschild made in the case of a third body of zero mass. It should be noted that new research is being undertaken, especially in the USSR, on this subject.

> pays. En effet, dès cette époque, l'astronomie est une science internationale du point de vue de la collaboration et de la coordination des recherches. Cette collaboration et cette coordination ont d'ailleurs été sensiblement renforcées après la constitution de l'Union Astronomique Internationale, en 1919.
>
> L'étude de l'astronomie en U.R.S.S. de 1917 à 1935 a attiré notre attention, car ce pays constituait une exception à cette règle.[58] [Nicolaïdis 1984, p. 6].

Although we will analyze in detail the Soviet scientific community of the early 20th century in the next chapter, and how this and the socio-political conditions influenced the education and choices of the young Kolmogorov, we want to focus here only on the figure of the Soviet mathematician, astronomer and explorer Otto Yulyevich Schmidt[59] (Mogilev, now Belarus 1891 - Zvenigorod, Russia 1956).

We will see in the following chapter that one of the pupil of Kolmogorov, V.I. Arnold, in an attempt to analyze the origins of Kolmogorov's works in classical mechanics, will report in [Arnold 2000] a direct testimony of his teacher; in this, Kolmogorov will assert that, together with others, one of the sources of inspiration was Schimdt himself, referring to the contributions he made in the field of celestial mechanics and, among these, he will cite *On possible capture in celestial mechanics*, published in Doklady Akademii Nauk SSSR in 1947 [Schimdt 1947] in the bibliography of the article published in the proceedings [Kolmogorov 1991/57].

After a short period as a professor of mathematics at the University

[58]Eng. tr.: For an era such as the first third of the 20th century, it is generally difficult to study astronomical science within the borders of one country. In fact, from that time onwards, astronomy was an international science from the point of view of collaboration and coordination of research. This collaboration and coordination was significantly strengthened after the establishment of the International Astronomical Union in 1919. The study of astronomy in the U.S.S.R. from 1917 to 1935 attracted our attention, as this country was an exception to this rule.

[59]often transliterated as Shmidt

of Kiev in 1915 (he graduated from the university in 1913), he became a professor of mathematics at the Moscow University in 1923, becoming in 1929 the head of the algebra department and founding an active school of group theory.

His professional life has always been divided between academic and administrative roles, carrying out various institutional positions including head of one of the divisions of the People's Commissariat for Food, created in 1917 after the dissolution of the Ministry of Food by the Bolsheviks.

In April 1924 he was appointed editor-in-chief of the Great Soviet Encyclopedia, which, as Laurent Mazliak defines it in the recent article [Mazliak 2018], *was a gigantic enterprise to the glory of "Marxist science" and of the Soviet regime. There were three editions: the first one was launched in 1926, the second one in 1949, the third one in 1977."* [Mazliak 2018, p 25]

Schmidt was chief editor until 1941, and Kolmogorov was one of the lead author for the Large Soviet Encyclopedia, publishing more than one hundred entries from 1937 to 1975. He was given the job of writing the entries "Mathematics", for all three editions , published in 1938, 1950 and 1974, respectively[60].
The collaboration for the Encyclopedia, as well as being colleagues at the same university, undoubtedly allowed the two mathematicians to relate and share their ideas. Several comments testify to this, including one written by Kolmogorov himself in [Tikhomirov 1991, p 902] regarding his interest in the theory of turbulence:

> In 1946 O. Yu. Shmidt suggested that I should head the Turbulence Laboratory in the Institute of Theoretical Geophysics, USSR Academy of Sciences. In 1949 this post was passed to Obukhov. I was not engaged in experimentation myself, but I worked extensively with other researchers on computation and graphical processing of the data.

[60] References in [Graham 1993] and [Mazliak 2018]).

Schimdt's name is now associated with a Russian mathematician, explorer and astronomer, but his research in the latter area is due only in the last decade of his life. In 1949 he will publish the book *A Theory Of Earth's Origin* in Russian, translated into English in 1958 by the Russian George H. Hanna, translator for the Foreign Languages Publishing House and also Radio Moscow.

The book contains the elaboration of four conferences held by the author at the Geophysical Institute of the USSR Academy of Sciences in 1948 on the author's hypothesis on the genesis of the Earth and other planets.

In the author's preface to the second edition, quoted on pag 7 of [Schimdt 1958] he writes:

> The problem of the origin of the Earth is one of such great importance to science that it possesses interest not only for the specialists — astronomers, geophysicists, geologists, geographers and others — but also for the general public. The Soviet people have made very considerable cultural progress so that it is only natural that they should show an interest in this problem and demand an answer from their scientists: the problem of the Earth's origin, say our people, must be solved as quickly as possible on account of its specific importance to the study of nature and from the standpoint of our philosophy of dialectical materialism.
>
> The author's hypothesis of the genesis of the Earth and other planets proposed in 1944 met with a wide response, gave rise to extensive criticism and discussion. In the course of time the hypothesis has developed and grown into a detailed theory.
>
> Apart from separate publications in scientific journals it became necessary to publish, at least, an interim report on basic results and methode: The First Edition of this little booklet was published in 1949: it consisted of four lectures which I deliveged at the Academy of Sciences Geophysical Institute in 1948.

One of the articles referred to by Schmitd is precisely [Schmdit 1947] - the latter cited from Kolmogorov.

The reason for the appearance of interest in celestial mechanics and astronomy only in the 1940s should probably be attributed to Schimdt's prudence in dealing with such a delicate subject in the 1930s and which, we will see in the next chapter, led to the dramatic vicissitudes of the purge of astronomers, which occurred between the years 1936-37.

His caution was also highlighted by Mazliak, who notes how Schimdt, unlike other members of the Large Soviet Encyclopedia, was spared from the political persecutions of the time, hypothesizing a reason for this:

> We shall briefly describe the first editorial board of the encyclopedia in the next subsection. We shall in particular see that a large majority of its members were victims of the political storms experienced by Soviet Union in the 1930s. It is therefore slightly surprising that Otto Schmidt could remain at the head of the enterprise almost until the end (he resigned in fact in 1941), despite his proximity with Bukharin and even, to a certain extent, with Trotsky. Maybe Stalin thought it was useless for the regime to touch an internationally too well-known scientist. But above all, Schmidt himself had the wisdom, as soon as the end of the 1920s, not only to make a brilliant come back to mathematics (he was appointed to the newly created Chair of higher algebra at Moscow university in 1929 and remained there until 1949), but also to participate to long-distance scientific exploratory expeditions such as the German-Soviet expedition to the Pamir (1928) and afterwards the long expedition in the Arctic (1930-1934), which maintained him far from the internal struggles tearing the party apart at the turn of the 1930s. [Mazliak 2018, p 35]

And, although far from the years of the purges, the attention with which he deals with topics of celestial mechanics is evident - suffice it to observe that in the introduction of the [Schmdit 1958] he underlines *its specific importance to the study of nature and from the standpoint of our philosophy of dialectical materialism*.

Despite the small number of works in this area, the contributions of the Russian scientist allowed a notable progress of celestial mechanics in the Soviet Union so much that, in a 1972 article by the Japanese astronomer Yusuke Hagihara entitled *Recent advances of Celestial Mechanics in the Soviet Union* [Hagihara 1972] the first section is dedicated precisely to the problem of captures, starting from Schimdt's contributions.

1.2 Metrical and spectral studies: The modern ergodic theory and the theory of dynamical systems in the 1930s

The essays by Whittaker and Charlier helped the international community of scholars to understand that the ancient and illustrious discipline of mechanics needed new horizons of theoretical development. What were the subsequent developments in the first decades of the 20th century? In the next section I will consider some future contributions, but inspired by Poincaré's approach, which Andrei Kolmogorov considered crucial for the development of his work on classical mechanics. In particular, he will focus on the evolution of the theory of dynamical systems at the beginning of the last century, on the birth of ergodic theory and on the contributions in the field of nonlinear mechanics in the 1930s in the Soviet Union.

The attention of historians of science has declined mainly in favor of the nascent physical theories of the twentieth century: the theory of relativity and the "new" quantum mechanics. The 1957 essay on the history of mechanics by René Dugas, in fact, closes its chapter on the evolution of classical mechanics just after Lagrange - with a chapter on the discussion of the energy theses of the Newtonian principles culminating with the reflections of the French scholars Poincaré, Painlevé and Duhem - and then moved on to the discussion of what he described as "modern physical theories of mechanics". Around this new approach, in fact, a new scientific community is gathering, the so-called theoretical physicists.

In a final note concluding his essay, Dugas considers Poincaré's attitude towards such modern theories, as expressed at a conference on new mechanics held in Lille on August 3, 1909. It was an attitude of acceptance, he says, but:

> It is true that in his conclusion, Poincaré very clearly fixes the limits of the new science and considers that it would be premature "in spite of the great value of arguments and facts raised against classical science, " to consider this latter as being definitely condemned. He shows that classical mechanics will remain that "of our practical life and of our terrestrial technique," and he emphasizes the necessity of a through knowledge of classical mechanics if we wish to understand the new mechanics. [Dugas 1957, p. 650]

The emergence of those modern theories led to the label "classical" for studies in mechanics following the 19th century mathematical tradition. While modern mechanics attracted attention even among the general public and relegate classical approaches to the background, research in the wake of Poincaré was carried on mainly in the USA and in the Russian Empire, two quite young mathematical communities, peripherical in the structure of international science. It was an age troubled by war and political totalitarism, yet the connection between the two countries were initially tight. Poincaré's "new methods" in celestial mechanics were a main source of ideas and inspiration: it was a time of change, in which the theory of dynamical systems was conceived, potentially enlarging the scope of differential equations to the study of phenomena of time evolution beyond inanimate body motion. As in the past, research in mechanics was tightly linked to research in mathematical analysis, which experienced in those years great advances.

The Hungarian scholar John von Neumann (1903-1957), a rising star in German mathematics between the two word wars, was active both in classical mechanics and in quantum mechanics. As a matter of fact, his contribution to classical mechanics appears to have been encouraged by his contact with his senior George Birkhoff (1884-1944), established in the late 1920s when he started his visits to the USA. As we shall see, a young American collaborator of Birkhoff's, Bernard Koopman (1900-1981), acted as bridge between Birkhoff and von Neumann. As early as 1911 Birkhoff had started his work in classical mechanics in the wake of Poincaré and laying the foundations of the theory of dynamical systems. In the years between the two world wars, he was the leading figure also for scholars working in the Soviet Union – thus helping the international recognition of the young United States mathematical community.

Yet, in spite of Poincaré's auspices, classical mechanics received less and less attention in the late 1930s and in the 1940s except for the Soviet Union, where the ideological framework encouraged continued attention to the mechanics "of practical life and terrestrial technique". As Simon Diner has emphasized in an essay published in 1992, it would be completely biased to consider that no results in classical mechanics were produced after the work of Birkhoff until the contributions by Stephen Smale in the 1960s; moreover, such a vision doesn't offer any explanation of the cultural origins of Komologorv's 1954 theorem of invariant tori and thus any understanding the birth of KAM theory as an expression of 20th century developments in classical mechanics.

No doubt theoretical physicist's mechanics attracted a lot of scientific talent, to the detriment of the classical approach. Moreover, the second World war and the breaking of the Cold War had a great impact on the network of scholars working in classical mechanics. Birkhoff died when he was sixty years old one year before the end of the war, and Von Neumann and Koopmann turned to other areas of mathematics, specially in connection to new applications such as computing or operations research. In the Soviet Union, celestial mechanics became a high risk activity, because of the suspicion on astronomy research during Stalinism, while research in the area of nonlinear systems went on, thanks to its connection to technological systems. Nevertheless, the international diffusion of Soviet research was hindered by the fragile connections of the Soviet scientific community with those in countries outside the Warsaw Pact. Thus, Kolmogorov's work on classical mechanics and dynamical systems continued, but he published on it again only in 1953, in the months after Stalin's death. He choose to present that topic to the 1954 international congress of mathematicians, the one marking the recovery of international mathematical relations.[61].

Kolmogorov used late 19th-century mathematics as an illustrative example to support his thesis on the union of mathematics and, in particular, suggested how this need arose after the works of Poincaré in the field of classical mechanics.

The example is certainly indicative. The legacy that Poincaré left is enormous and, as we have already said in the last paragraph, his works have opened the way to the qualitative study of dynamical systems, to ergodic theory, to chaos theory. In a 2002 essay entitled Writing the History of Dynamical *Systems and Chaos: Longue Durée and Revolution, Disciplines and Cultures*, mathematical historians David Aubin and Amy Dahan Dalmedico affirm that there *can be no doubt whatsoever that his œuvre is the point of origin of the domain under consideration here — dynamical systems and chaos — and the cornerstone on which it was built.* [Aubin, Dahan Dalmedico 2002, p 279].

> From Poincaré to the 1960s, the mathematical study of dynamical systems developed in the course of a longue-durée history that cannot be unfolded in a cumulative, linear fashion. In particular, this history is not reducible to that of a mathematical theory (which might be called "dynamical systems theory" or the "qualitative theory of differential equations") made by

[61]When Stephen Smale (1930 -) begin his studies on dynamical systems in the 1960s, his main reference would be Soviet mathematics

academic mathematicians who would have all contributed a stone to the final edifice. In fact, this history unfolds along various geographic, social, professional, and epistemological axes. It is punctuated by abrupt temporal ruptures and by transfers of methods and conceptual tools. It involves scores of interactions among mathematics, engineering science, and physics along networks of actors with their specific research agendas and contexts. Finally, it is characterized by countless instances of looping back to the past, to Poincaré's work in particular, which are so many occasions for new starts, crucial reconfigurations, and reappreciation of history. [Aubin, Dahan-Dalmedico 2002 pp. 278-279].

Therefore, starting from the works of Poincaré - obviously together with Whittaker's dynamics - we will try to outline the following developments in the field of celestial mechanics and dynamical systems.

In this paragraph, we will briefly analyze the contributions of Birkhoff, von Neumann and Koopman at the beginning of the twentieth century in the USA and we will draw a line that unites these mathematicians, with the works of the Russian mathematicians Nikolay Mitrofanovitch Krylov and Nikolay Nikolayevitch Bogoliubov, who worked together in the 1930s in Ukraine, then USSR, at the Ukrainian Academy of Sciences on the problems of linear and non-linear non-mechanical oscillations.

1.2.1 Towards the general dynamical systems: George David Birkhoff's (1884-1944) work on the wake of Poincaré in the years 1912-1927

In a paper recently published in the "Rendiconti del Circolo Matematico di Palermo" (vol. 33, 1912, pp. 375-407) and entitled *Sur un théorème de Géométrie*, Poincaré enunciated a theorem of great importance, in particular for the restricted problem of three bodies; but, having only succeeded in treating a variety of special cases after long-continued efforts, he gave out the theorem for the consideration of other mathematicians.

For some years I have been considering questions of a character similar to that presented by the theorem and it now turns out that methods which I have been using are here applicable. In the present paper I give the brief proof which I have obtained, but do not take up other results to which I have been led. [Birkhoff 1913, p 14]

The statement of the theorem to which Birkhoff refers is very simple and easy to understand.

Theorem 2 ***Poincaré's geometric theorem.*** *Let us suppose that a continuous one-to-one transformation T takes the ring R, formed by concentric circles C_a and C_b of radii a and b respectively $(a > b > 0)$, into itself in such a way as to advance the points of C_a in a positive sense, and the points of C_b in the negative sense, and at the same time to preserve areas.*
Then there are at least two invariant points[62].

George David Birkhoff (1884 -1944) was then 28 years old and had moved to Harvard that year for an assistant professorship. In [Diacu, Holmes 1996] the authors report an extract from the letter that Poincaré left to the editor of the journal Rendiconti of the Circolo Matematico of Palermo, in which the French mathematician seems to be aware of a possible imminent death:

> At my age, I may not be able to solve it, and the results obtained, which may put researchers on a new and unexpected path, seem to me too full of promise, in spite of the deceptions they have caused me, that I should resign myself to sacrificing them. In [Diacu, Holmes 1996, p 53]

Indeed, Poincaré died on July 17, 1912 and, after just three months, Birkhoff sent his work to the journal Transactions of the American Mathematical Society, where it will be published in January 1913.

The American mathematician's interest in Poincaré's work appears evident in his works and, above all, in the book *Dynamical Systems*, first published in 1927 by the American Mathematical Society. There are also some

[62]With *invariant point* we mean a point of the ring which remains fixed under the transformation T.

direct testimonies, including the mathematician Marston Morse[63] in the report *George David Birkhoff and his mathematical work* [Morse 1946], in which the author reports the mathematicians who most influenced his work:

> Birkhoff admired Moore[64] of Chicago, but not to the point of imitating him. He respected Bôcher[65] no less, and did him the honor next to Poincaré of following his mathematical interests. F. R. Moulton's[66] study of the work of Poincaré had something to do with Birkhoff's own intense reading of Poincaré. Poincaré was Birkhoff's true teacher. There is probably no mathematician alive who has explored the works of Poincaré in full[67] unless it be Hadamard, but in the domains of analysis Birkhoff wholeheartedly took over the techniques and problems of Poincaré and carried on. [Moore 1957, p 357].

Birkhoff's interest in topics related to Poincaré's works appears evident from some publications dating back to the years 1912-1915, such as *Quelques théorèmes sur le mouvement des systèmes dynamiques* [Birkhoff 1912] and *The Restricted Problem of Three Bodies* [Birkhoff 1915]. In the latter, in particular - awarded the Querini Stampalia Prize by the Royal Venice Institute of Science - we read the leitmotif that will push Birkhoff in his research, leading him over the following years to be increasingly independent of celestial mechanics - although an argument that will remain in its interest - and to the publication of Dynamical Systems:

> Thorough investigation of non-integrable dynamical problems is essen-

[63]Harold Calvin Marston Morse (1892-1977) was an American mathematician, known for developing variational theory in general with applications to equilibrium problems in mathematical physics, a theory which is now called Morse theory.

[64]He refers to Robert Lee Moore (1882-1974), an American mathematician known for his work in general topology.

[65]He refers to Maxime Bôcher (1867-1918), American mathematician who developed works in the field of differential equations, series and algebra.

[66]He refers to Forest Ray Moulton (1872-1952) an American astronomer best known for having formulated the so-called planetesimal hypothesis of the origin and evolution of the solar system.

[67]We will see in the next paragraphs that this assertion is not entirely true.

tial for the further progress of dynamics. Up to the present time only the periodic movements and certain closely allied movements have been treated with any degree of success in such problems, but the final goal of dynamics embraces the characterization of all types of movement, and of their interrelation. The so-called restricted problem of three bodies, in which a particle of zero mass moves subject to the attraction of two other bodies of positive mass rotating in circles about their center of gravity, affords a typical and important example of a non-integrable dynamical system. It is this problem which I consider in the present paper. [Birkhoff 1915, p. 265]

Insisting on considering general problems of dynamics rather than particular ones and looking globally at sets of motions rather than at particular orbits, Birkhoff's Dynamical Systems fully embraces the intentions of a qualitative analysis that Poincaré had for celestial mechanics, developing and using notions of general topology, but broadening the field beyond celestial mechanics[68]. All this emerges in a particular way in chapter VII of [Birkhoff 1927], entitled *General theory of dynamical systems*, in which we read the intentions of the author:

> The final aim of the theory of the motions of a dynamical system must be directed toward the qualitative determination of all possible types of motions and of the interrelation of these motions.
>
> The present chapter represents an attempt to formulate a theory of this kind.
>
> As has been seen in the preceding chapters, for a very general class of dynamical systems the totality of states of motion may be set into one-to-one correspondence with the points, P, of a closed n-dimensional manifold, M, in such wise that for suitable coordinates $x_1, \ldots, x_n$, the differential equations of motion may be written

$$dx_i/dt = X_i(x_1, \ldots, x_n), \qquad (i = 1, \ldots, n)$$

[68] A detailed account of the book can be found in [Aubin 2005]

in the vicinity of any point of M, where the X_i are n real analytic functions and where t denotes the time. The motions are then presented as curves lying in M. One and only one such curve of motion passes through each point P_0 of M, and the position of a point P on this curve varies analytically with the variation of P_0 and the interval of time to pass from P_0 to P. As t changes, each point of M moves along its curve of motion and there arises a steady fluid motion of M into itself.

By thus eliminating singularities and the infinite region, it is evident that we are directing attention to a restricted class of dynamical problems, namely those of 'non-singular' type.

However, most of the theorems for this class of problem admit of easy generalization to the singular case. The problem of three bodies, treated in chapter IX, is of singular type. [Birkhoff 1927, pp.189-190].

Meanwhile, in the first decade of the 20th century, a new mathematical tool was taking shape, and would prove to be indispensable in the theories of partial differential equations, quantum mechanics and ergodic theory: Hilbert spaces[69].

A Hilbert space is an infinite dimensional space whose points are numerical sequences $(x_1, x_2, \dots)$ for which the infinite series of squares $\sum_i x_i^2$ converges.

This established a new field in which mathematicians study the properties of fairly general linear spaces and has also provided a source for rich ideas in topology. Indeed, as a metric space, the Hilbert space can be considered a linear topological space of infinite dimension.

Their formulation marked the beginning of what at the time was called *Operator theory* - today it is commonly called *Functional analysis*, i.e. the study of linear operators (functions) defined in function spaces.

The theory of operators applied to the study of dynamical systems re-

[69]Designation used for the first time in 1929 by the Hungarian mathematician John von Neumann, in reference to the mathematician David Hilbert who, for the first time, described them.

sulted in a completely new idea of dealing with dynamics. And it is precisely on the basis of these intuitions that the ergodic theory developed in the 1930s, which involved many mathematicians, including Ludwig Eduard Boltzmann (1844-1906), Birkhoff himself, one of his pupils, Bernard Osgood Koopman (1900-1981), von Neumann.

We will see in the next paragraph the close connections linking the last three mathematicians listed above and the results obtained in this field, almost simultaneously. We will also focus on the preponderant use that von Neumann made of the theory of operators, not only in classical mechanics, but also to develop a mathematical corpus for quantum mechanics.

1.2.2 Bernard O. Koopman (1900-1981) paper Hamiltonian systems and transformations in Hilbert space (1931) and the role of John von Neumann (1903-1957)

> The invitation of the Organizing Committee for me to speak about "Unsolved problems in mathematics" fills me as it should with considerable trepidation and a prevailing feeling of personal inadequacy. Hilbert gave a talk on this subject at the similar congress about 50 years ago and this is a very formidable precedent. He stated about a dozen unsolved problems in another widely separated areas of mathematics, and they proved to be prototypical for much of the development that followed in the next decades. It would be absolutely foolish, if I tried to emulate this quite singular feat. In addition I do not know the future and the future at any rate can only be predicted ex post with any degree of reliability. I will, therefore, define what I am trying to do in a much more narrow way, hoping that in this manner I have a better chance of not failing. I will limit myself to a particular area of mathematics which I think I know and I will talk about it and about what its open ends appear to be, particularly in some directions which are not the ones that the evolution so far has mainly emphasized and which are, I think, quite important. *I will speak about operator theory and about its connec-*

> *tions with various areas and quite particularly about how it hangs together with a number of open questions in physics*[70] and how I think it hangs together or ought to hang together with a number of questions in logics and probability theory and questions of the foundations of these and certain reformulations of these which I think it puts into a quite different light from the one with which we usually look at these subjects.
>
> John von Neumann in [von Neumann 1954, p.231]

On 2 September 1954, the International Congress of Mathematicians began in Amsterdam. The first plenary on the same day from 3.00 to 4.00 pm was given by John von Neumann: *On unsolved problems in mathematics*. At the beginning of this paragraph we have quoted an extract from his introduction.

We could draw a parallel between the life of Andrei Nikolaevich Kolmogorov and that of the Hungarian mathematician John von Neumann (1903-1957): in addition to having in common the same year of birth, scientific interests, as well as their being reference figures for the international mathematical community of their era, both were involved in the dramatic events of the history of Europe, from the revolutionary upheaval of 1905 in the Russian Empire, to the two world wars and the rise of totalitarianism, and, in the middle decades of the century, to the tearing apart of political and international cultural heritage during the Cold War between the NATO area leaded by the USA and the Varsaw Pact area. Von Neumann in the USA - he became an American citizen in 1937 - and Kolmogorov in the USSR were somehow standard-bearer of Western science against Soviet science since the 1930s and in the following decades.

By the time of the plenary session at the ICM in Amsterdam, von Neumann had long since published his papers on operator theory, the first ones in 1929 [von Neumann 1929a] and [von Neumann 1929b], followed

[70] The italics are our license.

by the more complete work which appeared in 1932 [von Neumann 1932b] - which will be followed by a second work, together with the Hungarian mathematician Paul Richard Halmos (1916-2006), in English, with the same title as the first. Furthermore, in the same 1932 he had published his book "Mathematische Grundlagen der Quantenmechanik", in German: *The object of this book is to present the new quantum mechanics in a unified representation which, so far as it is possible and useful, is mathematically rigorous*, he will write in the first lines of the preface, and, he will add later, *a presentation of the mathematical tools necessary for the purposes of this theory will be given, i.e., a theory of Hilbert space*, wrote the author in the introduction.

von Neumann understood the importance of the theory of operators: a new tool capable of providing new contributions in various fields of mathematics. In those years, the young mathematician Koopman, a student of Birkhoof and a research doctor since 1926 at Columbia University, was also taking an interest in the theory of operators, and in particular in its application to Hamiltonian systems in classical mechanics. Philip M. Morse, in [Morse 1982] tells of a young Koopman who is dynamic and often travels for work. Among his most frequent trips were those to reach von Neumann and Birkhoff:

> Every summer he was off somewhere: California, Rome, the Alps, the Tetons-and always a week or month at Randolph. Even during term time he would travel: to Princeton, particularly after John von Neumann arrived there; and back to Harvard, to talk things over with Birkhoff. [Morse 1982, p. 419].

In 1931, Koopman published *Hamiltonian systems and transformations in Hilbert Space* [Koopman 1931], in which the intent, already evident in the title, is described by the same author in the body of the article:

> In recent years the theory of Hilbert space and its linear transformations has come into prominence. It has been recognized to an increasing extent that many of the most important departments of mathematical physics can

> be subsumed under this theory. In classical physics, for example in those phenomena which are governed by linear conditions-linear differential or integral equations and the like, in those relating to harmonic analysis, and in many phenomena due to the operation of the laws of chance, the essential role is played by certain linear transformations in Hilbert space. And the importance of the theory in quantum mechanics is known to all. It is the object of this note to outline certain investigations of our own in which the domain of this theory has been extended in such a way as to include classical Hamiltonian mechanics. [Koopman 1931, p. 315]

Koopman will prove that the functional operator induced by a measure-preserving transformation is unitary.

In mathematical terms, we are stating that, if T is a measure-preserving transformation on a measure space and U a transformation on a Hilbert space, and if for every function f in the Hilbert space the function Uf, defined by

$$Uf(x) = f(Tx)$$

is still in the Hilbert space, then can state that U is a unitary operator, i.e., it is an isomorphism between two Hilbert spaces that preserves the scalar product.

More simply, Koopman found a connection between the measure-preserving transformations and the unitary operators of a Hilbert space. Therefore, knowledge of the analytic theory of these operators will provide some information on the geometric behavior of the transformations[71].

This work was considered by all the protagonists of history - Koopman, Birkhoff and von Neumann - the beginning of what Halmos calls the modern ergodic theory[72].

[71][Halmos 1958] *von Neumann on measure and wrgodic theory*

[72]the term "modern" separates it from the first formulations in the early twentieth century due to Boltzmann

Morse, through the words of a friend and colleague of Koopman, Edgar Lorch, reconstructs the chronology of the articles on ergodic theory, the result of the continuous exchanges between Koopman with Birkhoff and von Neumann:

> [...] he [Koopman] was in close contact with John von Neumann and with G. D. Birkhoff. In his open way he discussed freely, during his visits, what was going on elsewhere.
>
> This put him in the middle in the controversy over the ergodic theorem. Questions of ergodicity had been in the foreground for many years and had attracted the attention of powerful mathematicians. Koopman was well versed in this domain and had discussed it with both Birkhoff and von Neumann. In March of 1931, Koopman published a note in the National Academy Proceedings, transforming the problem into one dealing with one parameter unitary groups in Hilbert space.
>
> Since these groups may be represented by self-adjoint transformations and since they were known to have a particularly decent structure, the door was open to rapid extension. Koopman communicated his ideas to von Neumann, who, in a short time, gave a proof of the ergodic theorem in a Hilbert space sense, establishing convergence in the mean but not actual convergence. In a state of considerable excitement Koopman told von Neumann's result to Birkhoff, who worked feverishly and succeeded in proving the theorem, establishing point-wise convergence almost everywhere. Birkhoff's notes were published in the late 1931 Proceedings of the Academy. Von Neumann's results, which had been obtained earlier, were published in the early 1932 Proceedings, seemingly a year later. Koopman, who had been the catalytic agent in the process, felt quite embarrassed. However the problem was clarified by the publication of three notes; one by Birkhoff and Koopman, another by Koopman and von Neumann and a third by von Neumann alone, setting the work in its proper order. All gave

priority of place to Koopman's original result[73] .

We will not go into the details of the ergodic theory, but we will try to briefly provide some salient points, which will be useful in understanding Section §3.1.

From a mathematical point of view, ergodic theory can be considered as generated by the interaction of measure theory and transformation group theory.

The existence of invariant measures (i.e. probability measure functions that remain unvaried under an automorphism) is a fundamental hypothesis of ergodic theory and classical conservative systems possess natural invariant measures[74], resulting a good field in which to apply the ergodic theory.

A motion of a given dynamical system is said to be *transitive* (or quasi-ergodic) if it is everywhere dense in the phase space Ω. If such motion exists, the dynamical system is said to be transitive. Birkhoff and the mathematician Paul Althaus Smith(1900-1980) first defined the concept of metric transitivity in a 1928 paper titled *Structure analysis of surface transformations*:

> A transformation will be called metrically transitive if there exists no measurable invariant set E such that $0 < m(E) < m(S)$. A transformation of this type is also transitive in the ordinary sense. [Birkhoff, Smith 1928, p 365]

The importance of ergodicity lies in the fact that it allows the study of dynamics, practically impossible when the number of degrees of freedom is high, to be replaced with the calculation of averages carried out with the invariant measure.

We give as an example of an ergodic ensemble an integrable Hamiltonian system:

[73] He referis to [von Naumann 1932a], [Birkhoff, Koompan 1932]. [Morse 1982, pp. 419-420].
Also, it can be read [Halmos 1958], [Moore 2015] and [Morse 1946].

[74] An example will be provided in the next frame.

An example of an ergodic system: the integrable systems of classical mechanics

Consider a dynamical system in a $2n$-dimensional phase space Ω whose elements are $(x_1, \dots, x_n, y_1 \dots, y_n)$. The equations of motion will be described by the Hamiltonian H such that:

$$\frac{dx_i}{dt} = \frac{dF}{dy_i} \qquad\qquad \frac{dy_i}{dt} = -\frac{dF}{dx_i} \tag{6}$$

where $i = 1, \dots, n$.

If the system is integrable, then the Ω phase space is shown to decompose into n tori with dimension n. On each torus it happens that a point that starts from it will follow a trajectory on the torus, without ever leaving it[a]. Therefore, the system admits a natural guiding measure, which is given by the volume element.

Now, if the frequencies $(\omega_1, \dots, \omega_n)$ of motion are rationally independent, i.e.,

$$m_1\omega_1 + \cdots + m_n\omega_n \neq 0$$

for any $(m_1, \dots, m_n) \in \mathbb{Z}$

the orbit of a point on a torus is said to be *quasi-periodic* and densely fills the torus, never passing through the initial point, but approaching it an infinite number of times.

The density of the orbits allows the equality of the temporal averages with the spatial ones and this means that the motions of an integrable Hamiltonian system are bounded (on the tori) and the system is ergodic.

[a]In mathematical terms, the torus is said to be invariant with respect to the flow

Thus, at the beginning of 1932 *Proof of the Quasi-Ergodic Hypothesis* [von Neumann 1932a] was published:

> The purpose of this note is to prove and to generalize the quasi-ergodic

> hypothesis of classical Hamiltonian dynamics (or "ergodic hypothesis," as we shall say for brevity) with the aid of the reduction, recently discovered by Koopman, of Hamiltonian systems to Hilbert space, and with the use of certain methods of ours closely connected with recent investigations of our own of the algebra of linear transformations in this space. [von Neumann 1932a, p 70].

Halmos analyzes von Neumann's real intentions on ergodic theory and observes that:

> It is therefore curious, but true, that von Neumann always looked at ergodic theory as a part of measure theory ; he never worked on the abstract versions. What fascinated him most was the delicate interplay between measure and spectrum. The ergodic theorem itself (mean or individual) was almost never needed in his later work; its main role was that of historical justification for studying measure-preserving transformations. [Halmos 1958, p.92].

In fact, the Hungarian mathematician highlighted this aspect in his article. On page 71 he stated:

> The pith of the idea in Koopman's method resides in the conception of the spectrum $E(\lambda)$ reflecting, in its structure, the properties of the dynamical system- more precisely, those properties of the system which are true "almost everywhere," in the sense of Lebesgue sets. The possibility of applying Koopman's work to the proof of theorems like the ergodic theorem was suggested to me in a conversation with that author in the spring of 1930.

The topic is broad and this is not meant to be the place for its rigorous treatment. Let's just try to explain the salient points briefly, reducing the symbolic math as much as possible.

Just two months after von Neumann's publication on the quasi-ergodic theorem, he and Koopman publish a second article entitled *Dynamical Systems of Continuous Spectra* [Koopman, von Neumann 1932], in which one can read in the first lines:

> In a recent paper by B. O. Koopman, classical Hamiltonian mechanics is considered in connection with certain self-adjoint and unitary operators in Hilbert space $\mathscr{S}$ (= $\mathscr{L}^2$). The corresponding canonical resolution of the identity E(λ), or "spectrum of the dynamical system," is introduced, together with the conception of the spectrum revealing in its structure the mechanical properties of the system. In general, E(λ) will consist of a discontinuous part (the "point spectrum") and of a continuous part.

The theorem proved in this article, which today takes the name of "shuffling theorem" relates particular geometric properties of a measure-preserving transformation T with the spectral properties of the corresponding unitary operator U in the Hilbert space.

The cases in which the spectrum is continuous or pure punctual are divided.

In case the spectrum is pure pointwise, then they show that, given two measure-preserving transformations S and T, both ergodic and with pure spectrum, then a necessary and sufficient condition for there to be a measure isomorphism between S and T is the unitary equivalence of the corresponding unitary operators on the Hilbert space.

In paragraph §3.1 we will analyze in detail Kolmogorov's articles from the years 1953-54 and we will see the topics treated by von Neumann and Koopman were of great inspiration to him, accompanying the evolution of the three articles, starting from the first of [Kolmogorov 1953].

1.2.3 Measure theory for the dynamical system of non linear mechanics (1937): the work of Nikolay M. Krylov (1879-1955) with Nikolay N. Bogolyubov (1909-1992)

> Dans la théorie des systèmes dynamiques un progrès très important a été réalisé ces derniers temps grâce aux travaux de B. O. Koopman, T. Carleman, E. Hopf, J. v. Neumann et G. D. Birkhoff qui ont établi une série

> de théorèmes remarquables dits ergodiques concernant certaines moyennes temporelles et leur connexion avec les moyennes spatiales pour une classe très étendue de systèmes dynamiques[75]. [Krylov, Bogoliubov 1937 p.65]

Mechanical studies in Russia find fertile ground, thanks to the tendency to bring theory and practice closer together. The phenomenon involves all the cultural centers of the Soviet Union, including the cities of Kazan, Kiev, Odessa and Kharkov.

This meant that "between the 1930s and 1970s an area of scientific culture was established in the Soviet Union, often isolated, where privileged topics will be developed within powerful scientific schools. The study of nonlinear dynamical systems and that of stochastic processes are among the most important topics." [Diner 1993 p. 336].

In the 1930s, the cultural fervor that involved mathematics and physics, with the aim of developing applicative theories, resulted in unparalleled evolution and growth, but at the same time a change of direction: while Europe and America they were intent on developing the nascent theories of relativity and quantum mechanics, in Russia there was a return to classical mechanics, faced from the point of view of the study of dynamic systems. The fields that have had greater prominence have been dissipative systems, since most of them have manifested applications in the technological field.

In the current Ukrainian capital, that in the 1930s mathematicians Krylov and Bololyubov developed new methods of non-linear mechanics with applications to the theory of dynamical systems.

The training of Nikolay Mitrofanovitch Krylov (St. Petersburg, 1879 - Moscow 1955) led him to enroll at the St. Petersburg Mining Institute, thus

[75]Eng.tr.: In the theory of dynamical systems a very important progress has been made recently thanks to the work of B. O. Koopman, T. Carleman, E. Hopf, J. v. Neumann and G. D. Birkhoff who have established a series of remarkable so-called ergodic theorems concerning certain time averages and their connection with spatial averages for a very wide class of dynamical systems.

obtaining the title of mining engineer ([Gruzin, Brega 2008])[76]. However, his contributions to mathematics were so undeniable that in 1917 the University of Kiev awarded him an honorary degree in mathematics.

In the early 1920s, he noticed potential in a young Russian, just fourteen: Nikolay Nikolayevich Bogolyubov (Novgorod, 1909-Moscow, 1992). Spurred on by Krylov himself to continue, in 1925 he was exceptionally accepted to the postgraduate course in mathematics of the Academy of Sciences of the Ukrainian SSR; just three years later, at the age of only nineteen, he defended his thesis entitled *The Application of the Direct Methods of the Calculus of Variations to Investigation of Irregular Cases of a Simplest Problem* and in 1930 obtained his doctorate in mathematics.

The collaboration between student and teacher manifested itself right away, when they developed the first results on the theory of non-linear oscillations - a subject that they themselves will call "non-linear mechanics" - ([Krylov, Bogolioubov 1933], [Krylov, Bogolioubov 1937], [Krylov, Bogolioubov 1950][77]).

In this context, we are interested in deepening some results developed by the two Ukrainian mathematicians which will be useful to explain the few lines written by Kolmogorov in 1985 [Kolmogorov 1991/1985] - already reported in the introduction of this thesis - in which he stated: "My papers on classical mechanics appeared under the influence of von Neumann's papers on the spectral theory of dynamical systems and, *particularly under the influence of the Bogolyubov-Krylov paper of 1937.* I became extremely interested in the question of what ergodic sets (in the sense of Bogolyubov-Krylov) can exist in the dynamical systems of classical mechanics and which of the types of these sets can be of positive measure (at

[76]He was denied access as free student of mathematics and physics at Kiev University for failing a course of study in classical languages

[77]It is the first book by Krylov and Bogoliubov, first published in 1934, in Russian, and translated into English by Russian naturalized American mathematician Solomon Lefschetz (Moscow, 1884 - Princeton, 1972) in 1942.

present this question still remains open)".

The work to which Kolmogorov refers is *La Theorie Generale De La Mesure Dans Son Application A L'Etude Des Systemes Dynamiques De la Mecanique Non Lineaire*, published January 1937 in Annals of Mathematics. In the introduction, as we have seen in the epigraph of this paragraph, the intention of the authors to the connection of this work with the words of Kolmogorov - also referring to the works of von Naumann - are clear.

Furthermore, it reads below:

> La seule condition restrictive vraiment essentielle dans leurs recherches consiste dans l'existence d'une mesure invariante la notion présentant une généralisation toute naturelle de celle d'un invariant intégral, utilisée jadis par H. Poincaré dans la démonstration de son théorème classique sur la récurrence des mouvements dans les systèmes de Liouville.
>
> Vu le grand intérêt théorique des théorèmes ergodiques et la variété de leurs applications physiques il était très désirable d'étendre le domaine de la validité de ces théorèmes sur les systèmes pour lesquels aucune mesure invariante n'est donnée à priori.
>
> C'est avec les systèmes dynamiques de ce dernier type qu'on a affaire en mécanique non linéaire dans différentes questions concernant les oscillations non linéaires[78]. [Krylov, Bogoliubov 1937 p.65]

---

[78]Eng. tr.:In the theory of dynamical systems a very important progress has been made recently thanks to the work of B. O. Koopman, T. Carleman, E. Hopf, J. v. Neumann and G. D. Birkhoff who have established a series of remarkable so-called ergodic theorems concerning certain time averages and their connection with spatial averages for a very wide class of dynamical systems.

The only really essential restrictive condition in their research consists in the existence of an invariant measure, the notion presenting a very natural generalisation of that of an integral invariant, used formerly by H. Poincaré in the proof of his classical theorem on the recurrence of motions in Liouville systems.

Given the great theoretical interest of ergodic theorems and the variety of their physical applications it was very desirable to extend the domain of validity of these theorems to systems for which no invariant measure is given a priori.

It is with dynamical systems of the latter type that one has to deal in nonlinear mechanics in various questions concerning nonlinear oscillations.

What they call a *condition restrictive* is that the hypothesis of the existence of invariant measures is not verified in dynamical systems describing dissipative phenomena, i.e. those dynamical systems in nonlinear mechanics that we find in various questions concerning nonlinear oscillations.

Krylov and Bogoliubov, with their work, have made it possible to extend the ergodic theory to such situations. In fact, they declare that the fundamental result of their work is the demonstration of the fact that it is always possible to construct, for such systems, invariant measures in the phase space and also transitive measures (theorems I, II and III in [Krylov and Bogoliubov (1937), pp. 92-95) . With their results, they succeeded in applying the ergodic theorems of G. D. Birkhoff et J. v. Neumann to the systems examined. Furthermore, they introduced the important concept of ergodic sets (definition IX in [Krylov and Bogoliubov (1937), p. 103]) - a concept in which Kolmogorov himself was interested - and proved that the phase space of a non-ergodic dynamical system can be decomposed (up to sets of zero measure) into a sum of subsets on which the system is ergodic.

For such a non-ergodic system, if for almost all of its ergodic components the dynamical system has a purely point-like spectrum (i.e. quasi-periodic motion), then the system is integrable.

This will be one of the fundamental points of Kolmogorov's theorem on invariant tori and the work of the Ukrainian mathematicians will serve him precisely to use the notions of ergodic theory for the qualitative study of the motions of analytical dynamical systems. I will deal with this aspect in detail in section §3.1.

2 Fascination and risk. Aspects of Andrej N. Kolmogorov's (1903-1987) life and times

The development of science in the Russian Empire during the final decades of the Tzarist monarchy and the Soviet regime has received increasing attention in recent years. In fact, within the lively intellectual environment, between petty nobility and cultured bourgeoisie (Westernists or Slavophiles), not only literary culture but also scientific culture flourished, so it is possible to speak of an intelligentsia science [Gordin, Hall, Kojevnikov 2008]. The scientific movement in cities such as St. Petersburg, Moscow, Kazan or Kiev had specific characteristics that deserve further study thirty years after the end of the political experience of the Soviet Union:

> The Russian Empire possessed ten universities at the beginning of World War I, the oldest (Moscow) dating to 1755. Its Imperial Academy of Sciences (1725) continued to sponsor valuable research throughout the nineteenth and early twentieth centuries. If nineteenth-century Russia was often thought of in the West as a country outside the scientific tradition, a nation where forms of Slavic mysticism and Orthodox Christianity[79] not conducive to science were the principal intellectual trends, it is quite clear, to the contrary, that by the end of that century Russia possessed a developing and capable scientific community already rooted in an institutional base. [Graham 1993, p 80]

This evolution can be seen as part of a more general trend towards modernization (industrialization, social progress, political evolution) in a cul-

[79]The case of Russia can be considered in the general framework of the issue of the cultural conditions of the development of science in the areas of the Christian Ortodoxy (see [Nicolaïdis 2011]). In the outstanding mathematical development the role of the Orthodox religious tradition was studied by Loren Graham Florenskij. On the evolution between the late 19th century and 20th century, see [Graham 1993], [Rabkin and Rajapolapan 2001], [Kojevnikov 2002] and the papers included in the monographic issue of Science in Context introduced by the late paper, and the papers included in the issue of Osiris devoted to Intelligentsia science. The Russian century 1860-1960 [Gordin, Hall, Kojevnikov 2008].

tural atmosphere of increasing cultural tights with other countries in Europe of the intelligentsia, since the late 19^{th} century reforms by Alexander II. The social and political tension against the autocratic regime was also linked to the diffusion of positivism and scientism:

> The cult of science flourished across Europe at the beginning of the twentieth century. It happened to be particularly prominent in the Russian empire, which had only recently embarked upon industrialization and modernization. Almost all parts of the political spectrum bought into it, although for different reasons. For Russian liberals, science was synonymous with economic and social progress; for the radical intelligentsia, including the yet utterly insignificant and marginal Bolsheviks on the very left, it was the closest ally of the revolution. Many among the monarchists, too, placed high hopes on modern science as a remedy for the country's relative economic backwardness vis- à- vis Germany, France, and Britain (other European countries rarely figured in the comparison). After the Great Reforms of the 1860s, they helped institutionalize science and promote the research imperative at Russian universities, hoping that at the very least it could distract unruly students from pursuing dangerous political temptations. [Kojevnikov 2008, pp. 115-116].

In fact, intelligentsia science was a complex phenomenon where several trends can be identified, from the philosophical and religious (the cultural background of Orthodox Christianity), to the patriotic and the utilitarian. Botany and chemistry were paid a great attention, for example, as both scientific areas had an impact on modernization of agriculture [Elina 2002]; mathematics was developed also in connection with religious worship [Graham, Kantor 2009]. Among outstanding, original figures, consider Vladimir Ivanovich Vernadskij (1863 - 1945), a Russian geochemist and mineralogist who developed a holistic vision of the planet and chemical and biological processes; and Lev Semënovich Vygotskij (1896 - 1934), who founded the research area on the child named pedology. The political evolution after the 1917 Revolution has hampered the diffusion of their ideas, and thus the understanding of the phenomenon of the modern spread of science in the Russian Empire. There are thus factors of continuity and of discontinuity between the evolution of science before and after the fall of the tzarist regime:

> The fact that the Soviet Communist regime placed extraordinarily high value and expectations upon science is, of course, rather well known. So much so, perhaps, that it has usually not been seen as a historical problem but has been taken for granted as something natural that does not ask for further discussion or inquiry. Behind the cover of obviousness, however, one can find a complex combination of historical choices and heterogeneous reasons—some ideological, some pragmatic, some accidental— that together may offer an explanation of why, among all the various political regimes and movements of the twentieth century, Communism, especially in its initial Soviet incarnation, happened to be the one most favor- ably predisposed toward science, believing most utterly, up to the point of irrationality, in science's power and value.
>
> To begin with, the Soviets mounted their belief in science on top of a preexisting and rather high foundation. [Kojevnikov 2008, pp. 115].

Lenin's and Stalin's policy was that of "preserving the old forms of intellectual and cultural institutions inherited from tzarism" even against the criticism from the left. The political evolution and ideological framework of the Soviet regime under Stalinism had a relevant impact on the evolution of research.

Soviet science should have a double mission: help the construction of material basis of the socialist regime and support the ideology, including the fight against religious beliefs. The large area of classical mechanics, between mathematics and physics, was a favorite area of scholarship because of its relevance to technological applications – in §1.4 we have considered the work by Krilov in Kiev. Celestial mechanics, as we have mentioned in Chapter 1, was overlooked in the early decades of the 20th century, but in the Soviet Union this situation could perhaps be felt as dangerous because of the attack against astronomy starting in 1936. Simon Diner[80], writes, in his essay on *Les voies du chaos déterminist dans l'école russe*:

> En 1985 est inaugurée à Moscou une série de petits ouvrages: "Problèmes contemporains des mathématiques. Orientations fondamentales." [...] Que les huit premiers volumes, ouvrant ce tour d'horizon exhaustif des mathématiques, soient consacrés aux "systèmes dynamiques" est une affirmation hautement significative de la puissance de l'école russe en ce domaine. Ces volumes sont plus souvent dirigés (et même rédigés) par les deux mathématiciens: V. I. Arnold et Y. G. Sinaï. La réputation de ces deux élèves de A. N. Kolmogorov (1903-1987), l'un des géants mathématiques du XX^e siècle, n'est plus à faire. Et pourtant, le grand public en Occident ignore largement que ce sont essentiellement des savants russes qui ont pendant cinquante ans exploité la partie de l'héritage d'Henri Poincaré, concernant la "théorie qualitative des systèmes dynamiques" et la "mécanique non linéaire" dont le chaos déterministe n'est qu'un des aspects les plus spectaculaires.
>
> Situation créée par la conjonction de l'isolement relatif de l'Union soviétique et les mobiles internes du développement des mathématiques dans un univers de la physique où la mécanique quantique a ravi la vedette à la mécanique classique. Le langage de Poincaré semblait opaque et ses idées en ont souffert, d'autant plus que les applications qu'il envisageait ne concernaient que l'astronomie.
>
> [...] Pendant tout ce temps l'URSS a vu éclore de nombreux travaux, dans des circonstances où ont simultanément joué des facteurs idéologiques et intellectuels, des traditions scientifiques nationales et la constitution d'écoles scientifiques pour suivant des programmes, pour ne pas dire des "plans".

[80]Simon Diner is a theoretical physicist who was research director of the CNRS, whose parents, both chemists, left Bessarabia in 1930

> [...] Dans les années 30 toutes ces écoles de physique sont d'une manière ou d'une autre engagées dans le grand mouvement international de la physique quantique, manifestant par là le niveau de formation des physiciens russes et le non-isolement initial de la Russie soviétique. [...] Mais la situation historique et politique de l'Union soviétique des années 30 va contribuer à créer comme una bulle fermée [...]. Sous l' influence de cette idéologie matérialiste, qui s'oppose d'une manière militante à l'ensemble des démarches idéalistes, positiviste et formalistes, dominantes dans les "sociétés bourgeoises", de nombreux savants et penseurs soviétiques privilégient las travaux qui cherchent à garder ou à restaurer une "image réaliste du monde". [Diner 1992, p 331-332, 335].

Andrej N. Kolmogorov's scientific biography is tightly intertwined with the development of mathematics in his country, including aspects such as mathematical education [Karp 2012], [Karp 2014], the political conditions [Lorentz 2002]; [Kutatelazde 2012], [Demidov, Levshiin 2016], [Mazliak 2018], the creation of research schools [Demidov 2004].

Kolmogorov's political views had been considered either that of a authentic Marxist [Graham 1993] loyal to the regime or that of a member of the intelligentsia science, who lived through the troubled 1930s and 1940s acting sometimes against his principles [Arnold 2000], [Lorentz 2002].

In the present chapter 2, I have gathered some elements that appear pertinent to the understanding of the cultural meaning – in the history of mathematics, in the history of science in Russia — of the aspect of Kolmogorov's contributions that he presented short after the death of Stalin. As I stated in the introduction, the investigation of these aspects was prompted by the reports by Vladimir Arnold of a conversation with Kolmogorov dating back to the 1984, thirty years after the publication of [Kolmogorov 1954] and his closing lecture at the Amsterdam ICM. I start from this testimony.

2.1 The testimony of a former student: A short conversation between Vladimir Igorevich Arnold and Kolmogorov in 1984

> He later related that he had been thinking about this problem for decades starting from his childhood when he had read Flammarion's Astronomy, but the success had come only after Stalin's death in 1953 when a new epoch had begun in the Russian life. The hopes this death raised had a deep impact on Kolmogorov, and the years 1953-1963 were one of the most productive periods in his life.
>
> V.I. Arnold in [Arnold 1997, p.1]

> "No", he [A.N Kolmogorov] answered ,"I was not at all thinking of that at the time. The main thing was that there appeared to be hope in 1953. From this I felt an extraordinary enthusiasm. I had thought for a long time about problems in celestial mechanics from childhood from Flammarion [...]. I had tried several times, without results But here was a beginning."
>
> V.I. Arnold in [Arnold 2000 p.90]

Vladimir Igorevich Arnold was born in Odessa on June 12, 1937 and grew up in Moscow[81].

The same year that Kolmogorov was to deliver his speech at the ICM in Amsterdam, he entered Moscow State University, being lucky enough to be the right age at the right time:

> I entered the Faculty for Mechanics and Mathematics of the Moscow State University in 1954 (before Stalin's death in 1953 or after the

[81] Nina Alexandrova Isakovich, her mother, belonged to a Jewish Odessa family (on Jewish Odessa see [Zipperstein 1985]. In 1937 the town belonged to the Ukrain Soviet Socialist Republic)

> invasion to Czechoslovakia in 1968, this would probably have been impossible for me because my mother was a Jew while my grandfather was shot dead in 1938 on the flagrantly false charge of espionage for England, Germany, Greece, and Japan). [Sevryuk 2014, p 3].

In 1959 he defended his thesis under the supervision of Kolmogorov and in 1961 he received the title of "candidate in physical-mathematical sciences", analogous to the PhD. in the West, at the Keldysh Applied Mathematics Institute in Moscow, with the dissertation containing his famous resolution of Hilbert's 13th problem. When he was 28 year old, he became a Professor in the Faculty of Mechanics and Mathematics at Moscow State University. In [Arnold 2000] he published some letters sent to him by Kolmogorov, which show the confidential relationship between the two of them.

In my effort to trace the cultural origins of Kolmgorov's theorem on the persistence of invariant tori under small perturbations in Hamiltonians systems, I have analyzed a testimony from Arnold, regarding a conversation with Kolmogorov on the later's interest in classical mechanics. The fact that this episode is mentioned twice by Arnold ([Arnold 1997] and [Arnold 2000]) with slightly different nuances but a hardcore, gives force to the testimony, a written source regarding a short, fleeting oral exchange. They were published three years apart, and in any case after Kolmogorov's death in 1987.

In both reports, a key information regards the circumstance that Kolmogorov affirmed that he had been interested in open issues in celestial mechanics – in modern language, Arnold spoke about "quasi-periodic motions in dynamical systems" – for decades; moreover, he links this interest to his readings in astronomy in childhood – specifically the well-known popularizer of astronomy Camille Flammarion (1842–1925), author of many multi-translated bestsellers. The first one [Arnold 1997] is concise:

> He later related that he had been thinking about this problem for decades starting from his childhood when he had read Flammarion's Astronomy, but the success had come only after Stalin's death in 1953 when a new epoch had begun in the Russian life. The hopes this death raised had a deep impact on Kolmogorov, and the years 1953-1963 were one of the most productive periods in his life.

The second report [Arnold 2000] was published in a contribution in the book Kolmogorov in perspective [Andrews et al 2000], including some testimonies of his private life written by students and colleagues. Here Arnold explains that he had tried to find an explanation on his own before asking Kolmogorov (here he also reports the year in which the conversation took place). I quote the report dividing it in two parts:

> I constructed for myself a theory of the origin of Andrei Nikolaevich's work on invariant tori: it began with his studies of turbulence. In the well-known work of Landau [82] (1943) it was invariant tori—attractors in the phase space of the Navier-Stokes equation—that were used to "explain" the onset of turbulence.[...] In a discussion at the Landau seminar Andrei Nikolaevich remarked that a transition to an infinite dimensional torus and even to a continuous spectrum can already take place for a finite Reynolds number. On the other hand, even if the dimension of the invariant torus remains finite for a fixed Reynolds number, the spectrum of a conditionally periodic motion on a torus of sufficiently high dimension contains so many frequencies that it is practically indistinguishable from a continuous spectrum. The question as to which of these two cases actually holds was asked more than once by Andrei Nikolaevich. A program for the seminar on the theory of dynamical systems and hydrodynamics was posted on a

[82] He refers to Lev Davidovič Landau (1908 -1968), Soviet physicist winner of the Nobel Prize in physics in 1962, one of the most important physicists of the 20th century. He wrote numerous treatises on mechanics, hydrodynamics, quantum physics and physical statistics.

> bulletin board in the Mechanics and Mathematics Department of Moscow State University at the end of the 1950's [...]. Andrei Nikolaevich chuckled about the tori of Landau: "He (Landau) evidently did not know about other dynamical systems." [Arnol'd 2000, p 89-90].

He refers to Lev Davidovich Landau (1908 -1968), winner of the Nobel Prize in physics in 1962, contributing to several areas in mathematical physics such as mechanics and hydrodynamics, as well as quantum physics and physical statistics. "The transition from the tori of Landau to dynamical systems on a torus would be a completely natural train of thought" was in the end Arnold's idea. But Arnold goes on:

> In the final analysis I almost believed in my theory and (in 1984) asked Andrei Nikolaevich whether it was really so. "No," he answered, "I was not at all thinking of that at the time. The main thing was that there appeared to be hope in 1953. From this I felt an extraordinary enthusiasm. I had thought for a long time about problems in celestial mechanics, from childhood, from Flammarion, and then –reading Charlier, Birkhoff, the mechanics of Whittaker, the work of Krylov and Bogolyubov, Chazy, Schmidt. I had tried several times, without results. But here was a beginning." [Arnol'd 2000, p 90].

Common to both reports is Kolmogorov's hint regarding "a hope" felt in 1953, a new epoch being opened for life in Russia – the mention of Stalin's death was perhaps Arnold's interpretation. In the period 1936 – March 1953 the population of the Soviet Union was bent by Stalin's Great Purges and the terror policy implemented. Internal tensions, the growing threat of a Second World War, the Iron Curtain, helped create distances and barriers between the USSR and the rest of the world. Serguei Demidov, in [Demidov 2009], describes the interruptions of travel and relations with France among mathematicians:

> À la fin des années trente, les savants soviétiques ne voyageaient presque plus à l'étranger, et les séjours de spécialistes occidentaux en URSS étaient

également devenus très rares. Cette restriction des contacts fut aggravée par la diminution graduelle du nombre de publications de savants soviétiques dans des revues scientifiques étrangères, jusqu'à l'interdiction totale.[83][Demidov 2009, p 133].

2.2 Reading Flamarion and Timirjazev. Kolmogorov as member of the Russian "intelligentsia science"

The young Kolmogorov, born two years before the 1905 upheavals that marked the final years of the Tzarist regime, was raised in his mothers' family of land owners, first at Tunoshna, close to Yaroslav, and then, when he was 6 years old, in Moscow. His mother, Marya Yakolevna Kolmogorova, died in childbirth, and her aunt Vera Yakolevna Kolmogorova (1863-1950) brought him up. She, as well as Kolmogorov's father, Nikolai Matveevich Kataev[84] were part of the radical Russian intelligentsia, learned people following ideals of justice and freedom, interested in the arts and the sciences, and believing in new or progressive education.

Andrej Nikolaevich attended a private gymansium in Moscow, founded by two woman, Evgenja (Evgeniya) Albertovnava Repman (1870-1937)[85]

[83]Eng.tr: By the end of the 1930s, Soviet scientists hardly ever travelled abroad, and visits to the USSR by Western specialists had also become very rare. This restriction of contacts was aggravated by the gradual decrease in the number of publications by Soviet scientists in foreign scientific journals, until they were completely banned.

[84]A cousin on his father side was the poet Ivan Ivanovich Kataev (1902-1937), who was a victim of the Stalinist period (dates of birth and death are from the Library of Congress catalogue where several of his books can be found). Kolmogorov's father (his parents were not married) was an agronomist and a writer (Tikhomirov 1988, p. 2). Tikhomirov reports this testimony: "In the thirties Andrei Nikolaevich stated in questionnaires that one of his grandfathers was a high ranking nobleman and the other a fatherly Archdeacon. He spoke of this with a touch of pride. I think htat the reason for Kolmogorov's pride here was that the position of his ancestors in the class hierarchy was not obvious enough and htat the did not demean himself by concealing the truth in these difficult years" (p. 2)

[85]Repman, founder and director of the school, was the eldest daughter of Albert Hristianovitch Repman (1834-1917), since 1889 direc- tor of the section on applied physics of

and Vera Fedorovna Fedorova, educating boys and girls together and following the principles of "experimental" pedagogy[86].

We have salient testimonies of the relevance of his childhood for this intellectual trajectory. He lost her mother at birth and very soon also his father – the 1917 fall of the monarchist regime and the war sweeping away institutions, people, and normal life, his father who he used to visit him from time to time became head of the educational division of the People's Commisssariat Narkomset and in 1919, having been attached to the Kursk government, he disappeared. Even so, it was a happy and stimulating period for the young Andrej Nikolaevich, and his friendship circle in adult life was linked to his school colleagues, including his future wife Anna Dmitriyevna Egorova (1903-1988), the daughter of the historian Dmitri Nikolaevich Egorov[87].

His former student and collaborator in the area of mathematics education since the 1960s Alexander Abramov (1926 - 2019) wrotes:

> [...] certain key events took place at various stages of Kolmogorov's life and had a particular influence on him. Both Kolmogorov's genius and his personality stem from his childhood, adolescence, and youth. In his articles, letters, and conversations, he often returned to the events of his early life. First, there was his early childhood. Left without a mother —

the Polytechnical Museum of Moscow (founded by the zar Alexander II in 1870 as Museum of Applied Knowledge). After 1917, the school was renamed Section grade school no. 23.

[86] See [Tikhomirov 1988]

[87] In his diary pages dating back to 1943 [Duzhin 2011], together with his companion of life Pavel Sergeyevich Alexandrov (1896-1982), her three aunts Vera, Nadya and Varya (he wanted to bring back the first two from Kazan where they had been evacuated are named, as well as his wife (he had married her in 1942) and two other friends from the school years, his wife's former husband, the mathematician and painter Sergei Mikhailovich Ivashyov-Musatov and the geneticist Dmitrii D. Romashov (1899-1963), a prominent scientist in the Soviet Union evolutionary biology school founded by Sergei Chetverikov (arrested by the secret police in 1929) The son of Musatov and her wife Anna, Oleg Sergeyevich Ivashyov-Musatov, is also named (he would who majored in mathematics with his stepfather.

Maria Kolmogorova died while giving birth to him — Kolmogorov was raised in an atmosphere of love and attention in a wealthy noble family that embraced the best traditions of the Russian intelligentsia, combining a deep interest in culture with respect for work and adherence to democratic principles. Kolmogorov's diligence, inquisitiveness, and talent began to take shape at a very early age. [Abramov 2010, p 89]

And this is how Tikhomirov descrives Kolmogorov internal connection to the experiences of his early days:

> Kolmogorov retained very clear memories of his early years. He was surrounded by love, kindness, attention, and care. Those close to him endeavoured to develop in the child curiosity and interest in books, science, and nature. Vera Yakolevna took the boy through fields and woods and talked to him of trees, flowers, herbs; she went on walks with him in the late evening and showed him the starry sky, named the constellations and the individual bright stars, told him of the universe; in the evening she read a lot – the stories of Hans Andersen, the tales of Selma Lagerlöf... [Tikhomirov 1988, p. 3]

Among books which left a trace in his mind we know from Camille Flammarion – as we have seen in §2.1 – and Kliment Arkadievich Timiryazev(1843-1920). Tikhomirov's quotes this word from Kolmogorov:

> The first deep impression of the power and significance of scientific research was made on me by K.A. Timiryazev's book Zhizn' rastenii (Plant life) [quoted in Tikhomirov 1988, p. 6]

Thus, the final school years and those as university stu- dent were marked by the end of Tzarist monarchy in October 1917 – he was 14 years old – and the rise of bolshevism under Lenin. He had to leave Moscow in 1918-20 with his family, as he himself recounts in [Kolmogorov 1988]. Tikhomirov writes:

> In the hard year of 1919 Kolmogorov was compelled to seek some paid work. He found work as a railwayman (both as librarian and stoker) on the

> train running between Kazan and Ekarterinburg (now Sverdlovsk). (The carriage containing the library stopped for some time at various small stations). At the same time he continued to study diligently, preparing to take and external examination for the secondary school. But somewhat to this disappointment, these efforts were of no use – in the summer of 1920 he was given a certificate stating that he had graduated from the 23^{rd} school of the second stage (the Repman grammar school had been renamed thus) without having an examination. [Tikhomirov 1988, p. 7]

In 1920 he enrolled both at the Physics and Mathematics Department of Moscow University and at the Institute of Chemical Engineering "D.I. Mendeleev". *Engineering was then perceived as something more serious and necessary than pure science*, he would say in 1963 during and interview with the magazine Ogonek riferimento [Kolmogorov 1963]. Furthermore in 1922, Kolmogorov was hired as a math and physics teacher and boarding school educator in a secondary school of the network under the administration of the People's Commissariat of Education (known as Narkompros), led by Anatoly Lunacharsky (1875-1933) with Lenin's wife, Nadezhda Konstantinovna Krupskaya (1869- 1939):

> Now I remember with great pleasure my work at the Potylikha Experimental School of the People's Commissariat of Education of the RSFSR. I taught mathematics and physics (at that time they were not afraid to entrust the teaching of two subjects to 19-year-old teachers at the same time) and took an active part in the life of the school (I was the secretary of the school board and a boarding school educator)[88] [Kolmogorov 1963, p 12].

Alexander Karp has described the influence on mathematical education in the Russian Empire of the "modern school" movement [Karp, Vogeli 2010], [Karp 2012] and [Karp, Schubring 2014]; his description help us

[88] Although perhaps this early involvement in elementary education began as a job out of necessity rather than will, [Abramov 2011] shows that in the 1960s and 1970s he returned to his interest in education, marked by his own experience, participating in efforts to improve the secondary mathematics education in the USSR.

understanding the cultural atmosphere in which the young Kolmogorov work as mathematics and physics teacher:

> In place of all existing types of educational institutions, a statute of 1918 established the so-called unified labor schools. These schools were divided into two stages, and the network of first-stage schools, which were far more numerous to begin with, continued to be intensively developed.
>
> [...] The goal was to eliminate from schools anything reminiscent of former discipline and drills, including exams, textbooks, and even separate subjects (including mathematics). The ideas of American progressive educators were taken up and developed in Russia (Soviet Union); schools made use of projects, laboratory work, group work, and, above all, "complexes".
>
> "Complexes" had to link through one overarching theme topics that had previously been studied in different subject classes. For example, teachers could use a theme such as "The Post Office" to get their students to do some writing, to perform some computations, to talk about geography, and even to discuss the difficult position of the working class in other countries. [Karp 2014, p. 315].

The deep roots that led Kolmolgorov to enroll in a degree in applied sciences were the same ones that probably drove him to take an interest in astronomy, as we have read from Arnold's words.

The references in the previous paragraph take us back to the astronomer Camille Flammarion (1842–1925), a famous French astronomer, publisher and science popularizer. A prolific and multi-translated author, during his career he published more than fifty works, among which the most famous were popular astronomy guides. In an obituary written by the English astronomer William Porthouse[89], he is described as an apostle of astronomy:

> Camille Flammarion might be described as the apostle of popular astronomy. His numerous literary works had for object primarily the popu-

[89] William Porthouse (1877-1964), a member of the Manchester Astronomical Society from 1905 until his death and editor of the Journal of the Manchester Astronomical Society from 1913 to 1924

larisation of astronomical study in all its manifolds branches [...].

Flammarion was not content to spread abroad the gospel of astronomy by book and pamphlet. He believed in the practical application of his theories for the spread of a universal knowledge of the sky. [Porthouse 1925, p 951]

Strongly convinced that the study of science was for everyone, Flammarion collaborated with a large number of magazines and newspapers, actively participating in the great scientific emancipation movement of the second half of the nineteenth century. His books are rich in figures and illustrations and are written with direct and persuasive communication, with a style capable of enthralling and enthusing the reader.

But which Flammarion's Astronomy could Kolmogorov have read? Analyzing the time span in which he as a child would have read the works and translations in Russian, we could restrict the field to two possibilities: one of which is the famous Astronomie Populaire, [Flammarion 1880] published in 1880 in Paris by the publishing house "C. Marpon et E. Flammarion"[90], translated for the first time into Russian as early as 1897 and in various subsequent editions. Divided into six chapters - The earth, The moon, The sun, The planetary worlds, The comets, The stars - it is intended to be a book aimed at everyone to teach the elementary knowledge of astronomy in an extremely didactic and popular form.

The opening words of chapter one is an introduction to the entire book:

Ce livre est écrit pour tous ceux qui aiment a se rendre compte des choses qui les entourent, et qui seraient heureux d'acquérir sans fatigue une notion élémentaire et exacte de l'état de l'univers.

N'est-il pas agréable d'exercer notre esprit dans la contemplation des grands spectacles de la nature? N'est-il pas utile de savoir au moins sur quoi nous marchons, quelle place nous occupons dans l'infini, quel est ce soleil dont les rayons bienfaisants entretiennent la vie terrestre, quel est ce

[90] Ernest Flammarion (1846 - 1936), French publisher and Camille's brother.

ciel qui nous environne, quelles sont ces nombreuses étoiles qui pendant la nuit obscure répandent dans l'espace leur silencieuse lumière? Cette connaissance élémentaire de l'univers, sans laquelle nous végéterions comme les plantes, dans l'ignorance et l'indifférence des causes dont nous subissons perpétuellement les effets, nous pouvons l'acquérir, non-seulement sans peine, mais encore avec un plaisir toujours grandissant. Loin d'être une science isolée et inaccessible, l'Astronomie est la science qui nous touche de plus près, celle qui est la plus nécessaire à notre instruction générale, et en même temps celle dont l'étude offre le plus de charmes et garde en réserve les plus profondes jouissances.[91]

Another probable reading, although less famous than the first but well known in the innovative circles of that time, is *Initiation Astronomique* [Flammarion 1908]. It was a work aimed at children, also with numerous illustrations, published in 1908 in Paris by Libraire Hachette et C^{Ie} and translated into Russian in the same year, when Kolmogorov was five years old and was in Tunoshna where he was studying in the innovative school of aunts.

The booklet was published in the series of "Initiations scientifiques" directed by the mathematician Charles-Ange Laisant[92] and, as he himself

[91]This work is written for those who wish to hear an account of the things which surround them, and who would like to acquire, without hard work, an elementary and exact idea of the present condition of the universe.

It is not pleasant to exercise our minds in the contemplation of the great spectacles of nature? It is not useful to know, at least, upon what we tread, what place we occupy in the infinite, the nature of the sun whose rays maintain terrestrial life, of the sky which surrounds us, of the numerous stars which in the darkness of night scatter through space their silent light? This elementary knowledge of the universe, without which we live, like plants, in ignorance and indifference to the causes of which we perpetually witness the effects, we can acquire not only without difficulty, but with an ever-increasing pleasure. Far from being a difficult and inaccessible science, Astronomy is the science which concerns us most, the one most necessary for our general instruction, and at the same time the one which offers for our study the greatest charm and keeps in reserve the highest enjoyments.
Populair Astronomy (1894), English version, translated by J. Ellard Gore, London, Chatto & Windus, Piccadilly, p.1

[92]Charles-Ange Laisant (1841-1920). French mathematician and politician, he was a

writes in the opening introductory pages, *Il est destiné, entre le mains de l'éducateur, à servir de guide pour la formation de esprit des tout jeunes enfants - de quatre à douze ans - afin de meubler leur intelligence de notions saines et justes, et de les préparer ainsi à l'étude, qui viendra plus tard*[93]. [Flammarion 1908, p. V].

We also find a brief introduction by the author who, in his words, expresses all his passion in this project and affirms the centrality of astronomy in scientific thought:

> J'ai toujours pensé aussi qu'il n'est pas nécessaire d'ennuyer le lecteur puor l'instruire, et que si pendant tant de siècles, l'Astronomie, la plus belle des sciences, celle qui nous apprend où nous sommes et qui nous dévoile les splendeurs de l'Univers, est restée à peu orès ignorée de l'immense majorité des habitants de notre planète, c'est parce qu'elle a toujours été mal enseignée dan les Ècoles. Aujourd'hui, enfin, on commence à la trouver intéressante, à lire le grand livre de la Nature, à vivre un peu plus intellectuellement. [94] [Flammarion 1908, p. VII]

Although it is not possible to ascertain with certainty which of the cited texts Kolmogorov read, it is clear that this author played a fundamental role in the birth of the interest of the child Kolmogorov in the stars and celestial mechanics.

---

deputy from Nantes and professor at the École polytechnique in Paris. He dealt with mechanics, geometry and algebra and, mainly, with the teaching of mathematics and related reform.

[93]Eng. tr.: It is intended, in the hands of the educator, to act as a guide for the formation of the mind of very young children - from four to twelve years old - in order to provide their intelligence with sound and correct notions, and thus prepare them for examination, which will come afterwards.

[94]Eng. tr.: I have also always thought that it is not necessary to bore the reader to instruct him, and that if for so many centuries astronomy, the most beautiful of the sciences, the one that teaches us where we are and that reveals the splendors of the Universe, has remained almost ignored by the vast majority of the inhabitants of our planet, it is because it has always been badly taught in schools. Today, finally, we begin to find it interesting, to read the great book of Nature, to live a little more intellectually.

2.3 A silent work. Kolmogorov's scientific life under Stalinism

The Kolmogorov's career as a research mathematician was already starting. In 1922 he proved his first famous result, in the field of trigonometric series, building an almost everywhere divergent Fourier-Lebesgue series; it was published the following year under the title *Une serie de Fourier-Lebesgue divergente presque partout* in the new *Polishjournal Fundamenta Mathematicæ*[95].

On the 21st January 1924 Lenin died. Under the first years of Joseph Stalin's rule Kolmogorov's career and prestige took off: he graduated in 1925, and after his postgraduate period, in 1929 began his teaching career ad the Moscow University Institute of Mathematics of Mechanics.

After ten years, in the dramatic years of the Stalinist terror, Kolmogorov was elected a full member of the USSR Academy of Sciences. In 1933 he had published, in German, his revolutionary treatise on the foundations of probability theory: *Grundbegriffe der Wahrscheinlichkeitsrechnung*, laying the axiomatic foundations of the theory of probability.

Kolmogorov's private life is inevitably intertwined with his professional one when he becomes a friend of the mathematician Pavel Sergeevič Aleksandrov[96]

> Our close friendship began in 1929. Now I am posting a description of my life with Aleksandrov in the first years of this friendship (1929-1931).
>
> [...] for me these 53 years of close and indissoluble friendship were the reason that my whole life was on the whole filled with happiness, and the basis of this happiness was Aleksandrov's unceasing concern. [Kol-

[95]This journal, founded in 1920 by a group of Polish mathematicians to strenghten the mathematical homeland culture in the years of restoration of Poland independence after the end of the First World War, was at the same time intended with a deep international vocation.

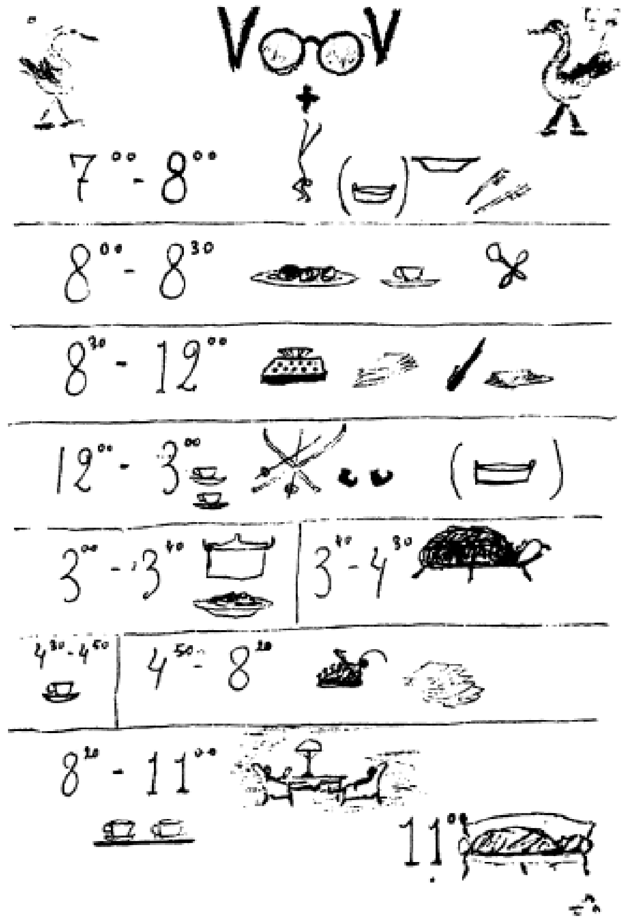
[96]Pavel Sergeevič Aleksandrov (1896-1982), a well-known Russian mathematician who made important contributions to general topology.

mogorov 1986, p.225]

In the thirties they bought a house in the village of Komarovka, near Bolshev where they spent their whole life, from 1942 also together with Kolmogorov's wife Anna Dmitrievna Egorova:

> In 1935 we acquired from the heirs of Konstantin Sergeevich Stanislavskii[97] part of an old manor house in the village of Komarovka near Bolshev (later we bought the whole house). This "house in Komarovka" satisfied all our requirements: there was room for a large library and we could put up our guests in separate rooms for several days and even for longer periods. By the end of the 30's we were both well settled in. As a rule, of the seven days of the week, four were spent in Komarovka.
>
> [...] One of our favourite ways of arranging ski-runs was this: we invited young mathematicians to, say, Kalistov, and from there we set out in the direction of Komarovka. Some who did not get as far as Komarovka caught a bus and set off for home. When we got to Komarovka it was suggested that we had a shower and then if one felt like it a romp in the snow and then dinner. In the golden age of the Komarovskii house the number of guests at the dinner table after skiing could be as many as fifteen. This was a typical day's programme at Komarovka. Breakfast at 8-9 o'clock. Study from 9 to 2. Second breakfast about 2. Ski run or walk from 3 to 5. When the organization was at its strictest, a pre-dinner nap of 40 minutes. Dinner 5-6 p.m. Then reading, music, discussion of scientific and general topics. And finally a short evening walk, especially on moonlight nights in winter. Bed between 10 and 11. There were two cases in which this arrangement could be altered; a) when scientific research became exciting and demanded an unlimited length of time; b) on sunny days in March when skiing was the only occupation. [Kolmogorov 1986, pp. 233-234].

[97] He refers to Konstantin Sergeevich Stanislavskii (1864- 1938), a Russian actor, theater director and teacher, known for being the creator of the Stanislavsky method.



In [Shiryaev 2000 p.38]

Despite the change of regime in the Russian Empire, in the 1920s and early 1930s scientist's in the USSR maintained and developed the ongoing international relationships, specially with Germany, France – a center of emigrés intellectuals after the 1905 upheaval –, and the United States. Legacy of the schools of Dmitry Fëdorovič Egorov[98] and Luzin, Kolmogorov had tight connections in France.[99]

[98] Dmitry Fëdorovič Egorov (Moscow, 1869- Kazan, 1931) was a Russian mathematician who mainly dealt with differential geometry and mathematical analysis.

[99] For more details on relations between France and Russia, see [Demidov 2009].

During his first academic trip, with Pavel Alexandrovich Aleksandrov, in Germany and France between 1930 and 1931, he forged links with many French and German mathematicians:

> From June 1930 to March 1931 he went on his first academic journey abroad—to Gottingen, Munich, and Paris. In the first 30 years of this century Gottingen was the Mecca for all mathematicians. Krein, Hubert, and other outstanding scholars worked there, and everyone who aspired to do research in the field of mathematics and its applications made it his goal. In the twenties and early thirties almost all the most powerful young mathematicians spent some time there, starting with von Neumann and Wiener.
>
> Kolmogorov had very many mathematical contacts in Gottingen "with Courant and his pupils in the field of limiting theorems, where diffusion processes proved to be limits for discrete random processes, with H. Weyl in intuitionistic logic, with E. Landau for questions in the theory of functions".
>
> He talked with Hubert, had scientific contacts with E. Noether, H. Lewy, Orlicz, and many others. [Tihkomirov 1988, p. 10]

Demidov asserts that the intense relations between these two countries seemed more flourishing than ever, with no clouds on the horizon:

> Dans les années 20, les mathématiciens soviétiques se rendirent souvent à l'étranger. Luzin continuait, beaucoup plus que les autres, à travailler en France, surtout à Paris: en 1925-1926, il y séjourna neuf mois, en 1926-1927, cinq mois, puis deux ans entre 1928 et 1930. Lors de ce dernier séjour, il rédigea son fameux ouvrage, *Leçons sur les ensembles analytiques et leurs applications*. En 1926-1928, ses élèves Men'šov, Lavrent'ev et Bari voyagèrent également à Paris. Bien que de plus en plus attiré, au début des années 20, par l'école mathématique de Göttingen, Aleksandrov se rendit quand même en France pendant son séjour européen. Durant celui-ci, il fut d'abord accompagné par Uryson; malheureusement, en 1924, ce jeune mathématicien russe fort prometteur se noya. En 1930-1931, Aleksandrov acheva à

> Paris puis sur la Côte d'Azur son périple entre l'Allemagne et la France, en compagnie de Kolmogorov.
>
> Ce dernier rendit visite à Borel et Lebesgue, discuta des problèmes de chaînes de Markov avec Fréchet, et s'entretint avec Paul Lévy. Dans l'introduction à leur ouvrage, Barbut, Locker et Mazliak montrent bien comment la correspondance entre Lévy et Fréchet nous plonge dans le climat de coopération entre mathématiciens français et russes sur la théorie des probabilités, dans les années 20 à 50. En 1926-1928, l'élève de Bernštejn, Vasilij L. Gončarov, travaille à Paris auprès de Montel. Pour compléter ce tableau, rappelons qu'à la fin de 1928, Vladimir A. Kosticyn, élève d'Egorov et spécialiste des équations intégrales, émigre en France puis entre au CNRS. Il deviendra célèbre pour ses travaux sur la biologie mathématique[100]. [Demidov 2009, pp. 129-130]

But in the time of the great purges starting at the mid 1930s the connections almost collapsed.

---

[100]Eng. tr.: In the 1920s, Soviet mathematicians often travelled abroad. Luzin continued, much more than the others, to work in France, especially in Paris: in 1925-1926, he stayed there for nine months, in 1926-1927, for five months, then for two years between 1928 and 1930. During this last stay, he wrote his famous work, *Lessons on analytic sets and their applications*. In 1926-1928, his students Men'šov, Lavrent'ev and Bari also travelled to Paris. Although in the early 1920s Aleksandrov was increasingly attracted to the mathematical school in Göttingen, he still went to France during his European stay. During his stay, he was first accompanied by Uryson; unfortunately, in 1924, this promising young Russian mathematician drowned. In 1930-1931, Aleksandrov completed his trip between Germany and France in Paris and then on the Côte d'Azur, accompanied by Kolmogorov.

Kolmogorov visited Borel and Lebesgue, discussed the problems of Markov chains with Fréchet, and talked with Paul Lévy. In the introduction to their book, Barbut, Locker and Mazliak show how the correspondence between Lévy and Fréchet immerses us in the climate of cooperation between French and Russian mathematicians on probability theory in the 1920s to 1950s. In 1926-1928, Bernštejn's pupil, Vasilij L. Gončarov, worked in Paris with Montel. To complete this picture, let us recall that at the end of 1928, Vladimir A. Kosticyn, a student of Egorov and a specialist in integral equations, emigrated to France and then joined the CNRS. He became famous for his work on mathematical biology.

2.3.1 The great purges of astronomers

> Suivant, bien qu'avec une certaine inertie, les événements politiques, l'astronomie soviétique comme en général les autres sciences, a connu en gros trois étapes dans son développement jusqu'à la mort de Staline: la première étape, qui s'étend de la stabilisation du régime soviétique jusqu'à la fin des années 20, ne comporte pas de changements idéologiques dans l'étude de l'astronomie, mais seulement de changements dans la structure organisationnel. le des établissements et dans les rapports entre les différentes institutions. C'est l'époque de l'appel du nouveau régime vers les spécialistes, les «spets» comme ils furent appelés. La deuxième étape, qui s'étend de la fin des années 20 au milieu des années 30, voit l'application de concepts idéologiques dans l'astronomie et de la ligne officielle de la supériorité de l'astronomie soviétique par rapport à l'astronomie dite bourgeoise. C'est l'époque du développement par Staline de la théorie de la construction du socialisme dans un seul pays. La troisième étape, du milieu des années 30 à la mort de Staline, voit les «grandes purges» des astronomes soviétiques et constitue un bouleversement sans précédent du personnel scientifique de l'astronomie en U.R.S.S.[101]. [Nicolaïdis 1984, p 6].

Pulkovo Observatory was one of the main astronomy centers of the Russian Academy of Sciences, which was inaugurated in August 1839 and still employs more than 250 workers today. The scientific activities of the Ob-

[101]Eng.tr.: Following, albeit with a certain inertia, political events, Soviet astronomy, like other sciences in general, went through roughly three stages in its development up to the death of Stalin: the first stage, which extends from the stabilization of the Soviet regime until the end of the 1920s, does not involve ideological changes in the study of astronomy, but only changes in the organizational structure. of the establishments and in the relations between the different institutions. It was the time when the new regime called for specialists, the "spets" as they were called. The second stage, which extends from the late 1920s to the mid-1930s, sees the application of ideological concepts in astronomy and the official line of the superiority of Soviet astronomy over astronomy called bourgeois. This is the time of Stalin's development of the theory of building socialism in one country. The third stage, from the mid-1930s to Stalin's death, saw the "great purges" of Soviet astronomers and constituted an unprecedented upheaval in the scientific personnel of astronomy in the U.S.S.R.

servatory have always covered the priority areas of fundamental research in astronomy: celestial mechanics and stellar dynamics, geodesy, astrometry, the Sun and solar-terrestrial relations, physics and evolution of the stars, as well as observational equipment and methods astronomical.

In [Nicolaïdis 1990], the author exposes the vicissitudes of the Observatory from the Revolution to the beginning of the 1930s, focusing in particular on the upheavals that occurred in conjunction with Stalin's rise to power in 1928.

As the Greek historian of science Efthymios Nicolaïdis points out, before the Revolution, the Observatory was structured according to the canons of most Western observatories. In fact there was a single director (in the period 1916-1919 he was the Russian astronomer Aristarkh Belopolsky) who had decision-making power both in the administrative and scientific fields. But the situation changed and, although for the first decade the regime's control affected only organizational matters, from 1928 onwards, the control of all the Soviet Union's observers, as well as scientific research and publications was centralized.

For the ideological line established by Stalin and the Bolsheviks, science had to be an important social, political and economic ally of the state. Rational science was supposed to oust the power of religion and superstition over people's minds.

Historically, it is well known that astronomy has been a unique discipline of its kind since its origins: its studies have served to change our view of the world and more generally of the entire Universe over the centuries. Many revelations have shocked society, putting a strain on the solidity of religions based on the centrality of man.

Nicolaïdis dwells precisely on the relationship between Soviet science, in general, and its relationship with Orthodoxy:

> Until 1928, the change of social order in Russia affected astronomy only with respect to organizational matters: the new state organization was re-

flected in astronomical institutions by the creation of the Soviets of astronomers. At the ideological level the revolutionary state did not attempt to interfere in astron-omy. Research and educational programs continued as before, except insofar as they concerned material problems.

After 1928 however, the Stalinist regime proclaimed a so called "Marxist" official ideological line concerning science. This ideological line became the official line of Soviet astronomy in 1931 Its principles were the following:

(1) *There are two sorts of astronomies Soviet and bourgeois*. This principle comes from the dogmatic principle that a capitalist regime restrains the scientific evolution while on the contrary, the construction of the socialist regime implies in addition the construction of a new, superior science. This principle of "two sciences" was the main Stalinist principle concerning all scientific fields. We will see that in astronomy, the application of this principle was to have terrible consequences for the leading Russian astronomers.

(2) Soviet astronomy must serve Soviet society more precisely *astronomy must serve ideology and the economy.*

But how could astronomy serve Stalinist ideology? [...] Astronomy was a scientific tool that would help to disprove what Stalinists called "religious myths". In a more specifically scientific field, soviet astronomy was ordered to fight against what was termed idealistic western cosmological theories, and especially against the theory of general relativity and the concept of a finite universe - because to put limits and an age to the universe would imply the Creation and so the existence of a God.

The relation between astronomy and the Soviet economy was a more complicated concept.

The general line that all activities in the USSR must serve the "building of socialism" implied that research in astronomy must also have industrial applications It was difficult to make applications concrete, and so the ideological line spoke about researches concerning Earth Sun relations and geodesy. [Nicolaïdis 1990, pp. 346-347]

The relations between the regime and science, between astronomy and orthodoxy, which in the 1930s led to inevitable friction between the regime and the academic world of stellar scholars, ended up leading, in the years 1936-37, to one of the most significant and dramatic episodes concerning the science at that time - the purge that devastated the powerful group of astronomers (more than two dozen) operating in the extensive network of astronomical observatories of the Russian Empire.

The process was slow, but with catastrophic final effects. The most important astronomers of the time - Aleksandr Aleksandrovich Ivanov[102], Boris Petrovich Gerasimovich [103], Boris Vasilyevich Numerov[104], just to name a few - were not willing to submit to the new ideological line dictated by the regime. While not openly opposing it, their astronomical work continued in the same directions as in previous decades. The only supporters of the new line were amateur astronomers or those little known abroad.

But, in the summer of 1936, the newspaper *Pravda*, the official press organ of the Communist Party of the Soviet Union from 1922 to 1991, decided to launch brutal attacks against the Pulkovo Observatory, and later the Tashkent Observatory and Leningrad Astronomical Institute were also involved. The Pulkovo's director at that time, Boris Petrovich Gerasimovich, and in general all the astronomers of the observatory, were accused of subservience to foreign science, justified by the publication in Russian of only three of the seventy-five articles.

[102] Aleksandr Aleksandrovich Ivanov (1867 - 1939), soviet astronomer and specialist in celestial mechanics and practical astronomy, was director of the Pulkovo Observatory from 1919 to 1930.

[103] Boris Petrovich Gerasimovich (March 31, 1889 - November 30, 1937), Soviet astrophysicist, he was active in a wide range of astronomical research areas, and he was director of the observatory since 1933.

[104] Boris Vasilyevich Numerov (January 29, 1891 — 1941 (?)) was a Russian astronomer, land-surveyor and geophysicist. The lunar crater Numerov and the minor planet 1206 Numerowia, discovered by the German astronomer Karl Reinmuth in Heidelberg in 1931, were named in his honour.

Simultaneously with the deterioration of the reputation of the Pulkovo Observatory, a young student denounced Boris Numerov, director of the Leningrad Astronomical Institute[105]. On 20 October 1936 he was arrested by the NKVD and, as often happened, during the interrogation, the accused was forced, under torture, to sign a document in which he accused his own collaborators of the observatory. This gave rise to the so-called *Numerov affair*, which between the end of 1936 and the first half of 1937, led to the arrest of about two dozen astronomers, leaving the Tashkent Observatory almost deserted.

Most of the astronomers arrested never returned; of Numerov himself, there was no more news after 1941.

The last rehabilitations of the personalities purged and survived the massacre date back to the years 1956-57, thus leaving the Soviet astronomical world in a climate of vulnerability for nearly twenty years.

As Eremeeva, historian of astronomy of the Shternberg State Astronomical Institute (GAISh) in Moscow, tells us in [Eremeeva 1995, p 318], only at the end of the 1960s the tragic events that happened were brought to light and it was possible to name the fallen astronomers in disgrace:

> The process of reclaiming the memory of the repressed astronomers "from oblivion" was uneven and difficult. At first it was forbidden even to mention them in print. Indeed, it was the aim of the authorities to expunge not only their scientific work but their very names from human memory.
>
> [...] Personal factors played an important role in the process of 'returning' the names of the repressed astronomers. Thus long before the rehabilitation process had begun, the names of the disgraced astronomers appeared in the 1948 jubilee compendium *30 years of astronomy in the USSR*. In an article about the development of fundamental astrometry, M. S. Zverev even mentioned the contributions of B. V. Numerov. S. A. Shorygin, who in his own time had suffered arrest, compiled the bibliography that included the works of B. P. Gerasimovich; the main text, however, included no mention

[105] See [McCutcheon 1991] and [Eremeeva 1995].

of Gerasimovich.

Only in 1964 did the historian of astronomy Iu. G. Perel' dare to publish the first brief notes about Gerasimovich in the Soviet *Astronomicheskii kalendar*. Khrushchev's 1956 "secret speech" detailing Stalin's crimes to the 20th Congress of the Communist Party had by now become public knowledge, and the 'thaw' of the 1960s had arrived. Thanks to the 'thaw' Perel was able to publish his article about Gerasimovich.

2.3.2 The Luzin affair and the Moscow mathematical world

The first repercussions of the period of Stalinist repression in the mathematical academic environment manifested themselves more than five years before the purge of astronomers, above all with the interruption of connections with foreign countries, until then guaranteed and important sources of exchanges and cultural growth.

As early as 1932, Luzin was denied participation in the International Congress of Mathematicians in Zurich and, two years later, Kolmogorov was unable to go to Paris, despite a scholarship granted by the Rockefeller Foundation. Since the late 1930s, scientist in the Soviet Union hardly traveled abroad.

Kolmogorov could not leave the Soviet Union until September 1954, when the Russians were finally able to take part again in the International Congress of Mathematicians in Amsterdam, albeit with a tiny delegation of four people (Alexandrov, Kolmogorov, V. Kozetsky e Sergej Michajlovich Nikolskij).[106]

Travel and attendance at conferences were prohibited. Demidov reports a series of examples, which show the sudden change of course compared to the situation that emerged in the previous extract, dating back to

[106] Already suspended in 1936, the congress was only reinstated in 1950, but, on that occasion, the entire Russian academic community did not participate in the event. For more details, see in the Proceedings of the ICM 1950, Cambridge, Massachusetts, [Graves, Hille, Smith, Zariski 1955, p 122]

a few years earlier:

> En 1932, Luzin n'a pas pu participer au congrès international des mathématiciens, à Zurich, car on lui a refusé l'autorisation de quitter le pays; en 1934, il ne put aller en cure en France, pour la même raison. La même année, quoique la fondation Rockefeller lui eût accordé une bourse, Kolmogorov ne fut pas autorisé à se rendre à Paris pour travailler près d'Hadamard. À la fin des années trente, les savants soviétiques ne voyageaient presque plus à l'étranger, et les séjours de spécialistes occidentaux en URSS étaient également devenus très rares. Cette restriction des contacts fut aggravée par la diminution graduelle du nombre de publications de savants soviétiques dans des revues scientifiques étrangères, jusqu'à l'interdiction totale.[107][Demidov 2009, p 133].

The difficulties in the mathematical environment did not end with the impossibility of travel and contacts: from the beginning of the 1930s, a series of persecutions of illustrious mathematicians began, fuelled by ideological militancy and internal and personal rivalries. One of the best-known cases are the arrest of Egorov in 1930 - which led to his death in 1931 through a hunger strike - accused for taking a stand against the repression of the Russian Orthodox Church and imprisoned as a "religious sectarian".

And, in the same year that the great purge of Soviet astronomers began, there was an event involving mathematicians and the research group of which Kolmogorov was a part: the *Luzin affair*.

The Luzin affair [108] started with anonymous accusation by the news-

[107]Eng.tr: In 1932, Luzin was unable to participate in the International Congress of Mathematicians in Zurich because he was refused permission to leave the country; in 1934, he was unable to go to a health resort in France for the same reason. In the same year, although the Rockefeller Foundation had granted him a scholarship, Kolmogorov was not allowed to go to Paris to work with Hadamard. By the end of the 1930s, Soviet scientists hardly ever travelled abroad, and visits to the USSR by Western specialists had also become very rare. This restriction of contacts was aggravated by the gradual decrease in the number of publications by Soviet scientists in foreign scientific journals, until they were completely banned.

[108][Levin 1990], [Lorentz 2002],[Katuteladze 2012], [Katuteladze 2013], [Demidov,

paper *Pravda* in eight long articles from July 2 to 16, 1936, as *Enemy under the guise of a Soviet citizen* [Kutateladze 2013, p.A86]. A commission was immediately appointed by the Academy of Sciences and the first session of the process began on July 7, 1936, which was followed by others in a few days. A sentence was reached, which proved to be less drastic than the tones of the articles and the trial, and the Soviet mathematician was acquitted of practically all charges. In this affair was involved Enrst Ko'lman, a Cezch emigrée of Jewish ascendence and Marxist convictions. Aleksey E. Levin writes in [Levin 1990]:

> Kol'man's formal position inside the party hierarchy was head of the mathematical section of the Communist Academy during the first half of the 1930s; he was subsequently promoted early in 1936 to head of the science department of the party's Moscow City Committee (he held this office until 1938 when he apparently lost some powerful protection and was sent to a teaching position). During the 1930s, Kol'man regularly published on the philosophy of mathematics and had many personal connections in mathematical circles, especially among politically active youth.
>
> Kol'man's status makes it unlikely that any public attack on Luzin would have been launched without his approval. [Levin 1990, pp 98-99]

A rift opened between Luzin and most of his students, including Kolmogorov and Aleksandrov, who played an active role in the commission set up for the trial. After the denouement of the matter, the split continued to manifest itself: ten years after the Luzin affair, he voted against the election of Alexandrov to become a full member of the Academy of Sciences:

> Lusin died in 1950, but not before a final violent collision with Aleksandrov and Kolmogorov. In 1946, the Academy had to elect a new group of members, this time with preference to the applied sciences. This allowed Lusin to vote against the topologist Aleksandrov. To everybody's consternation, as a reaction, Kolmogorov slapped Lusin's face on the floor of the

Lëvshin 2016]

Academy. The president of the Academy, S. I. Vavilov, was at a loss of what to do. Finally the incident was reported to the Kremlin. It was said that Stalin was not astonished. "This happens even among us," was his reply. In other words, Stalin recommended to do nothing.[109] [Lorentz 2002, p 207]

This episode strongly marked the Moscow Mathematical School. Everything contributed to creating a cautious and subdued climate, in which everyone was careful not to publish their work in a foreign language, openly reveal their reservations or make public aspects of their private life.

Kolmogorov's active participation in the trial against Luzin and his collaboration in the various editions of the Large Soviet Encyclopedia - which *was a gigantic enterprise to the glory of "Marxist science" and of the Soviet regime*[110] - meant that he was often credited with fully sharing the communist ideals of the Soviet regime. Our purpose is not to fully analyze this question[111]. However, there are two opposing interpretations of his behavior and writings that are worth mentioning. Loren Graham, in his book *Science in Russia and the Soviet Union. A short History* [Graham 1993], describes Kolmogorov as one of the outstanding figures in science of the USSR:

> Most people now assume that all influence of Marxism on Soviet science was deleterious. On the contrary, in the works of scientists such as L. S. Vygotsky, A. I. Oparin, V. A. Fock, O. Iu. Schmidt, and A. N. Kolmogorov, the influence of Marxism was subtle and authentic. [Graham 1993, p 3-4].

In his discussion of USSR mathematics, he writes:

> A. N. Kolmogorov, one of Shmidt's authors in the first edition of the *Large Soviet Encyclopedia*, wrote the entry "Mathematics." [...] I will briefly compare Kolmogorov's article with those in the *Encyclopedia Britannica* writ-

[109] This was the *fight* to which Kolmogorov referred in the words reported by Arnold

[110] [Mazliak 2018]

[111] The question of science in the Soviet Union, its impressive development, in the context of an evolution that began in the last fifty years of the tsarist regime, has received considerable attention since the dissolution of the USSR and the establishment of the Russian Federation [Kojelnikov 2002], [Gordin et al 2008].

ten by Frank Plumpton Ramsey and Alfred North Whitehead at approximately the same time as the first edition of Kolmogorov's article.

> The points of difference arise on the most essential questions of mathematics: What are the origins of mathematics? and What is the relationship between mathematics and the real world? According to Kolmogorov, mathematics is "the science of quantitative relations and spatial forms of the real world." It arose out of "the most elementary needs of economic life," such as counting objects, surveying land, measuring time, and building structures. In later centuries mathematics became so abstract that its origins in the real world were sometimes forgotten by mathematicians, but Kolmogorov reminded them that "the abstractness of mathematics does not mean its divorce from material reality. In direct connection with the demands of technology and science the fund of knowledge of quantitative relations and spatial forms studied by mathematics constantly grows." Kolmogorov then went on to sketch a history of mathematics in which its growth was intimately related to economic and technological demands. His views were consistent with Lenin's insistence on the material world as the source of human knowledge, and Engels's emphasis on technical needs as a motivating force in the development of knowledge. [Graham 1993, p 118]

In a paper published in 2002, [Lorentz 2002], based on his own experiences, George Gunther Lorentz (1910-2006) quotes the testimony and reflection of one of Kolmogorov's most famous students, Vladimir Igorevič Arnold (1937-2010), published in the volume *Kolmogorov in perspective* in 2000 by the American Mathematical Society and the London Mathematical Society [AA.VV. 2000]. Precisely in an attempt to reconstruct the reasons which prompted Kolmogorov in the two-year period 1953-54 to deal with one of the mathematical questions which, after probability, made him more famous - the one which today goes by the name of KAM theory - he writes:

> Although Andrei Nikolaevich himself regarded the hopes that appeared

in 1953[112] as the main stimulus for his work, he always spoke with gratitude about Stalin (following the old principle of saying only nice things about the dead): "First, he gave each academician a quilt in the hard year of the war, and second, he pardoned my fight in the Academy of Sciences, saying, 'such things happen also here'." Andrei Nikolaevich also tried to speak kindly about Lysenko, who had fallen into disfavor, claiming that the latter had sincerely erred out of ignorance (while Lysenko was in power, the relation of Andrei Nikolaevich to this "champion in the struggle against chance in science" was quite different).

[...] "Some day I will explain everything to you," Andrei Nikolaevich used to tell me after having done something contrary to his principles. Seemingly, pressure was exerted on him by some evil genius whose influence was enormous (the role of the group transmitting the pressure was played by well-known mathematicians). He hardly lived to the times when it became possible to speak of these things, and, like almost all people of his generation who lived through the 1930's and 40's, he was afraid of "them" to his last day. One should not forget that for a professor of that time not to tell the proper authorities about seditious remarks made by an undergraduate or graduate student not infrequently meant being accused the next day of having sympathy with the seditious ideas (in a denouncement by the very same student-provocateur). [Arnold 2000, p. 92].

2.3.3 Kolmogorov's attitude in the outbreak of the Lysenko affair

Kolmogorov has always had a strong interest in applied sciences. This is testified by the autobiographical accounts of the mathematician, in which he refers to his childhood interests both in astronomy - as we have already seen - and in biology:

[...] For a time, interest in other sciences took over. The first big impression on me of the strength and significance of scientific research was

[112] Stalin died in March 1953

made by the book by K. A. Timirjazev[113] The Life of Plants[114]. [Kolmogorov 1988].

His vision of the relationship between mathematics and science, necessary for a greater understanding of the natural world, seems to have encouraged him to participate in various discussions in the field of applied sciences, publishing various articles where probabilistic knowledge is intertwined with problems of biological, physical, geological etc...:

> The distinctive breadth of A. N. Kolmogorov's scientific interests is shown in his "more applied" work where the probabilistic approach is directed to problems of biology, genetics, physics, geology,.... Thus in his paper, "On the solution of a biological problem"[115] dealing with a simple model of the branching random process, Kolmogorov found the asymptotic behavior of the extinction probability as the number of generations increases.
>
> In discussion on genetics in the autumn of 1939 much attention was given to the validity of Mendel's laws (its simplest case means a splitting in the ratio 3: 1). In this connection Kolmogorov wrote "On a new confirmation of Mendel's law"[116], where he analyzed the statistical data of N. I. Ermolayeva, a pupil of T. D. Lysenko [...]. [Shiryaev 1989, p 896].

In addition to the works cited by Shiryaev, the seminal studies of Vito Volterra on population dynamics should also be underlined, as the beginning of a theoretical mathematical biology [Kolmogorov 1935, 1936].

Particular attention in this research was paid to the discussion about Mendelism, quoted in the previous excerpt, in which Kolmogorov took part. It fits into another thorny issue about science and ideology in the Soviet Union: the so-called Lysenko Affair. A violent campaign against

[113] He refers to Kliment Arkad'evič Timirjazev(1843-1920), a Russian plant physiologist and a major proponent of the Evolution Theory of Charles Darwin in Russia.

[114] The 23 editions published between 1898 and 1962 in 5 languages - therefore before and after October 1917 - of the famous book by the botanist Timiryazev show his adherence to the radical scientistic ideals and vision of science in the Soviet Union.

[115] He refers to the article [Kolmogorov 1938]

[116] He refers to the article [Kolmogorov 1940]

genetics, not considered to conform to dialectical materialism was one of the most devastating political intrusions into Soviet intellectual life under the Stalin regime.[117]

Trochym Denysovych Lysenko (September 29, 1898 Karlivka (Ukraine) - November 20, 1976, Moscow) exerted a growing influence on Russian biology from the mid-1930s onwards, until it reached the vertex of an ascending parabola in the late 1940s, with the approval of Joseph Stalin[118] himself, only to hit rock bottom after more than thirty years, in 1965, when geneticists called him an impostor and attributed to him all the damage caused to Soviet agriculture - just think that in the thirty years 1935-65 Russia, from Europe's granary became an importing country, after a series of failed crops.

Among the tenets of what will be termed Lysenkonism are the critique of Mendelism, the denial of the applicability of chemistry, physics and mathematics to the solution of any biological problem [Lysenko 1940], as well as the promise to quickly solve all agricultural tasks set by the party.

In his book *The Lysenko affair* (1970), the American historian of science David Joravsky wrote:

> Thus the Lysenko affair has been pictured as a latter-day version of Galileo versus the Church, or Darwin versus the churches: new science denounced to save old theology. The historical reality was far less high-minded, far more serious. Lysenko's school did not derive from a moribund tradition in science; it rebelled against science altogether. Farming was the basic problem, not theoretical ideology. Not only genetics but all the sciences that impinge on agriculture were tyranically abused by quacks and time-servers for about thirty-five years. The basic motivation was not a dream of human perfectibility but a selfdeceiving arrogance among politi-

[117] To name a few, see [Joravsky 1970], [Graham 1993], [Graham 2016], [Ptushenko 2021]

[118] At the Congress of the Communist Party of the Soviet Union in 1935, Lysenko delivered a speech which ended with the applause of the audience and the emblematic exclamation of Joseph Stalin, present at the congress: "Bravo, comrade Lysenko, bravo!"

cal bosses, a conviction that they knew better than scientists how to increase farm yields. The Lysenko affair, in short, was thirty-five years of brutal irrationality in the campaign for improved farming, with severe convulsions resulting in the academic disciplines that touch on agriculture. [Joravky 1970, p. vii]

Graham devoted chapter 6 of his book *Science in Russia and the Soviet Union: A Short History* [Graham 1993] to this topic:

> The roots of Lysenkoism lie not in Marxist ideology, but in the social and political context of Soviet Russia in the 1930s. Lysenko originated his ideas outside the circles of Marxist philosophers and outside the community of established geneticists. He was a simple agronomist who developed ideas about plants not very different from those of many practical selectionists of the late nineteenth and early twentieth centuries, but who was able to promote those ideas to an unheralded prominence because of the political and social situation in which he found himself. An extremely shrewd but basically uneducated man, he learned how to capitalize on the opportunities that the centralized bureaucracy and ideologically charged intellectual atmosphere presented. Seeing that his ideas would fare better if they were dressed in the garb of dialectical materialism, with the help of a young ideologist he recast his arguments in Marxist terms.
>
> [...] Alarmed that the science of genetics itself might be eclipsed, Vavilov[119] abandoned the effort to compromise with Lysenko and pointed out the errors in his biological views.
>
> [...] A few other brave people continued to speak up against Lysenko. At a conference on genetics in December 1936, A.S. Serebrovskii, an outstanding geneticist and sincere Marxist, called Lysenko's campaign "a fierce attack

[119] It refers to Nikolai Vavilov (November 25, 1887 - January 26, 1943), a Russian agronomist, botanist and geneticist, famous for his expeditions around the world in search of varieties of agricultural plants. He was an exemplary researcher with encyclopedic knowledge: he contributed to genetics, botany, plant physiology, plant breeding, plant systematics and evolution and biochemistry.

> on the greatest achievements of the twentieth century...an attempt to throw us backward a half-century." [Graham 1993, p. 124, 129-130]

Lysenko's most famous opponent, Nikolai Vavilov, paid with his life for a heroic attempt to publicly defend the achievements of biology hitherto achieved in the Soviet Union, and to oppose Lysenko's scientifically unsubstantiated ideas. As early as 1935 Lysenko began to attack his colleague, accusing him of having hindered the development of agricultural production in Russia. And, although in 1939 Vavilov was elected president of the VII International Congress of Genetics, it was not enough to maintain his prestige. Accused Vavilov of defending classical Mendelian genetics, considered by party ideologues a "bourgeois pseudoscience", he was imprisoned in 1940 and a year later sentenced to death. He died of starvation in 1943 in the Russian prison of Saratov. He was on of the many biologists arrested, exiled and repressed.

In a recent book, *Lysenko's ghost*, [Graham 2016], considering recent genetical research, has developed a synthetic but thorough examination of genuine scientifc aspects of the discussion on epigenetics together with the evolution of repression and terror under Stalinism. In the conclusion he writes :

> With the realization that the inheritance of acquired characteristics might happen after all, was Lysenko right? No, he was not. Some people may think so because they mistakenly link Lysenko uniquely to the doctrine of acquired characteristics, a belief that has been around for serveral thousand years. Lysenko was a very poor scientist, and the inheritance of acquired characteristics was actually a small part of what he claimed.
>
> The fathers and mothers of epigenetics did not use Lysenko's results but developed their views on the basis of molecular biology. [...] Lysenko disregarded the action of genes [...].
>
> Does this mean that Lysenko was totally worthless as a practical plant breeder, especially in his early years? No. Lysenko had talents in the field

[...]. If Lysenko had lived in a normal democratic country, he would be remembered, if at all, as a talented farmer working away in his fields, employing idiosincratic methods but never garnering much support. None of his methods are employed in Russia today. But in the Soviet Union in the 1930s, a country suffering from famine (caused in large part by the disastrous collectivization effort), the need for quick agricultural remedies was acute, and Lysenko offered them. [Graham 2016, pp. 139-140].

On January 29, 1939 Kolmogorov was elected a full member of the Academy of Sciences of the USSR. In the same year, the scholar N.I. Ermolaeva published in Russian an article entitled, translated into English, *Once more on the "laws of peas"*, which went against the validity of Mendel's principle and where, in particular, it ended that Mendel's principle that self-pollination of hybrid plants resulted in 3: 1 segregation ratios was false.

The article was brought to Kolmogorov's attention by geneticist Aleksandr Sergeevich Serebrovskii [120]. In a paper published in 1940[121] in the reports of the USSR Academy of Sciences, he discussed Ermolaeva data as well as T. K. Enin data as discussed by Kol'Man:

> In the discussion on genetics that took place in the autumn of 1939 much attention was paid to checking whether or not Mendel's laws were really true. In the basic discussion on the validity of the entire concept of Mendel, it was quite reasonable and natural to concentrate on the simplest case, which, according to Mendel, results in splitting in the ratio $3:1$. For this simplest case of crossing $Aa \times Aa$, with the feature A dominating over the feature a, it is well known that Mendel's concept leads to the conclusion that in a sufficiently numerous progeny (no matter whether it consists of

[120][Kolmogorov 1940, p 222]. Serebrovskii (Kusrk, February 18, 1892 - Bolshevo, June 26, 1948) was a prominent Russian geneticist, the founder of the Department of Genetics of Moscow University. His major contributions are due to the genetic study of chicken breeds and the development of poultry farming.

[121]the episode took place at the turn of the world war - after the sanctioning of the Russo-German pact of August 1939 and a few months before the entry into the war of the USSR after the German invasion of June 1941

one family or involves many separate families resulting from various pairs of heterozygous parents of type Aa) the ratio between the number of individuals with the feature A (that is, the individuals of the type AA or Aa) to the number of individuals with the feature a (aa type) should be dose to the ratio $3 : 1$. T.K. Enin, N.I. Ermolaeva and E. Kol'man have concentrated on checking this simplest consequence of Mendel's concept. However, Mendel's concept not only results in this simplest conclusion on the approximate ratio $3 : 1$ but also makes it possible to predict the average deviations from this ratio. Owing to this it is the statistical analysis of deviations from the ratio $3 : 1$ that gives a new, more subtle and exhaustive way of proving Mendel's ideas on feature splitting. In this paper we will try to indicate what we think to be the most rational methods of such checking and to illustrate these methods on the material of the paper by N.I. Ermolaeva. In contrast to the opinion of Ermolaeva herself, this material proved to be a brilliant new confirmation of Mendel's laws.

And he adds in conclusion a strong attack against Kol'man's contribution:

> Kol'man's paper referred to in the beginning of this note does not contain any new facts; it only analyses Enin's data and is based on a complete misunderstanding of the circumstances set forth in this paper. [Kolmogorov 1940, p. 227].

Kolmogorov applied a statistic test, now called the *Kolmogorov* or *Kolmogorov-Smirnov test* in order to analyze in this more polished way the validity of Mendel's ratio. Kolmogorov's paper prompted a reaction by Kol'man and by Lysenko himself.

Mathematicians Alan Stark and Eugene Seneta, in the article *A.N. Kolmogorov's defense of Mendelism* published in 2011 [Stark, Seneta 2011], examined Kolmogorov's paper and reused the test used by the Russian mathematician in the data collected by Ermolaeva. They have shown that there were errors not only in Ermolaeva's obviously incorrect calculations - and in the consequent deduction of the inadequacy of Mendel's law - but also in the experiment itself performed by Kolmogorov:

> In the above brief χ^2 analysis we have attempted to use an essentially equivalent test to Kolmogorov's inasmuch as it relies on the approximate standard normality of the Δ's, after "cleaning" the data appropriately. So while the conclusion drawn by Kolmogoroff (1940) confirms what is now totally accepted, the evidence in support of this conclusion is not as strong as his paper presents. Of course his statistical technology was well beyond the understanding of Lyssenko (1940) and Kolman (1940), who could hardly argue on the grounds of its incompletely justified application and possible arithmetic error, to data which may have been poorly prepared. Seneta (2004) describes Kolman's leading role in the attacks on mathematicians and traditional pure mathematics in the Soviet Union during the Stalinist era.

Futhermore, at the end of the article they write:

> In his defense of Mendelism, Kolmogorov [...] relied simply on data. As we have seen, he ignored the fact that, strictly speaking, his test of him assumed continuous data while the actual data was discrete and in some cases based on inappropriately small numbers [Stark, Seneta 2011, p. 185]

Kolmogorov had not only corrected the results obtained by Ermolaeva, but his defense against Mendelism first involved Kol'man and Lysenko (even if he was not directly mentioned), as Ermolaeva was a student of him. In fact, Lysenko responded in a comment published in the reports of the Academy:, in *In Response to the Article by A. N. Kolmogorov*, [Lysenko 1940]:

> In "Doklady Akademii Nauk SSSR", Volume XXVII, N 1 of 1940, an article by academician A.N. Kolmogorov "On a new confirmation of Mendel's laws". In this article, the author, wanting to prove the "correctness" and inviolability of Mendel's statistical law, gives a number of mathematical arguments, formulas and even curves. I don't feel competent enough to

> understand this system of mathematical evidence. Besides, I, as a biologist, don't care whether Mendel was a good or a bad mathematician. I have already published my assessment of Mendel's statistical work several times, stating that he had nothing to do with biology. In this note I would just like to note that even the above-mentioned article by the famous mathematician A.N. Kolmogorov has nothing to do with biological science.
> [...] That is why we biologists do not take the slightest interest in mathematical calculations that confirm the useless statistical formulas of the Mendelists. [Lysenko 1940, p 834-835]

Also Kol'man also supported the agronomist's point of view[122].

It could have ended much worse for the Russian mathematician, but he was spared from the great purge. There is no doubt that at that time Kolmogorov's fame in the field of probability theory - a field with a strong Russian tradition for more than a century - was undisputed, and this may have favored him over the fates of other academics; moreover the cautious and submissive behavior maintained by Kolmogorov and his friend Aleksandrov during the period of the Stalinist regime - the same behavior which has led some to believe that they were "friends of the regime" - has to be taken into account. Consider also in [Levin 1990], the description of Kol'man previous attitude to the two mathematical friends:

> When the campaign was over[123] Kol'man's monograph, *Predmet i metod sovremennoi matematiki*, was published. The scholarly qualities of this very primitive and inaccurate book are of no relevance here, although it is worth noting that the author expressed his gratitude to A. N. Kolmogorov and P. S. Aleksandrov for reading a draft manuscript.

Therefore, only hypotheses, but no certainty about the reasons for his salvation. Nevertheless, quoting Lorentz's words in [Lorentz 2002, p 183] *The voice of Kolmogorov raised in defense of Mendel's laws was ignored*.

[122] As stated in the note 84 in [Joravsky 1970], p. 414.
[123] He refers to the campaign against Luzin

What prompted the reaction of Kolmogorov to a paper by a student of Lysenko, who was in those years reaching a great influence? No doubt he was encouraged to do so by a fellow Academician, the genetist Serebrovskii, who could thought that the mathematical authority of Kolmogorov regarding numbers in Mendel's laws of heredity could help their defense of Mendelism and maintain USSR biolgoy in connection with the international accepted ideas. Moreover, the role of mathematical studies in the discussions on evolution was growing in the late 1930s[124].

Kolmogorov was sympathetic with this research trend, and this can be linked to his vision of Mathematics as present in the Soviet Encyclopedia.

The reaction of Lysenko's against mathematics in biology can be understood as an underlying aspect of the harsh exchange in the Stalinst Russia of 1940.

[124] See [Kingsland 1985]; [Israel 1993]; [Israel, Millán Gasca 2002]

3 Kolmogorov's theorem on the persistence of invariant tori: a look into the origins of the KAM theory

> The statement of the problem of the motion of systems that are close to the systems of classical mechanics, including the problems of orbit evolution in the three-body problem, dates back to Newton [1][125]. Laplace [2][126] stated explicitly the theorem on stability of the semimajor axes of Keplerian ellipses, which is a forerunner of Kolmogorov's theorem on preservation of tori, but proved it only in terms of approximate perturbation theory. On analyzing numerous attempts to justify and improve Laplace's argument, Poincaré [3][127] stated the problem in its modern form (to study the motion of a system whose Hamiltonian $W(p)+\theta S(q,p,\theta)$ is periodic in q) and called it the basic problem of dynamics (see [3], Chapter 1, §13). In the papers under consideration Kolmogorov solves this problem for the majority of initial conditions in the generic case ($det\partial^2 W/\partial p^2 \neq 0$). [Arnold 1991, p 504]

Four mathematicians from the USSR took part in the International Congress of Mathematicians in Amsterdam in 1954, the second held after the end of the Second World War, but the first in which Soviet mathematicians could participate[128].

---

[125]He refers to "I. Newton, Philosophicae naturalis principia mathematica, London, 1686."

[126]He refers to "P.S. de Laplace, Traité de mécanique céleste, Vol. 1, Paris, 1799."

[127]He refers to "H. Poincaré, Les méthodes nouvelles de la mécanique céleste, Vol. 1, Paris, 1892."

[128]Already suspended in 1936, the congress was reinstated only in 1950, but, on that occasion, the entire Russian academic community did not participate in the event. In the proceedings of the ICM held in Cambridge, Massachusetts, in the Secretary's report section, we read:
Shortly before the opening of the Congress, the following cable was received from the President of the Soviet Academy of Sciences: *The USSR Academy of Sciences appreciates having received a kind invitation for Soviet scientists to participate in the International Congress of Mathematicians to be held in Cambridge. Soviet mathematicians are very busy with their regular work, unable to attend the congress. I hope that the upcoming congress will be a significant*

During the closing plenary conference on September 9, Kolmogorov was able to present his ideas on the research program in the field of classical mechanics. He had been thinking about it for a long time, as we saw in the testimonies reported in the previous chapter, and the contents exposed embraced those of the two articles *On dynamical systems with integral invariant on the torus* [Kolmogorov 1953] and *On the conservation of conditionally periodic motions under small variations of the Hamilton function* [Kolmogorov 1954], recently published in the Soviet journal *Doklady Akademii Nauk*, on November 13, 1953 and just nine days before the conference, respectively.

The main fulcrum of his studies is represented by the Theorem on the persistence of invariant tori presented in the 1954 article, which we will analyze in detail in the following paragraph. The fate of this theorem appears rather singular: very often it is confused or unified with the subsequent contributions due to Arnold [Arnold 1963] and Moser [Moser 1962], denoting the three results under a single theorem with the name "KAM Theorem". This is due to a still open historiographical question, which concerns the validity of the proof given by Kolmogorov for his theorem.

The “KAM theorem” [Hubard 2004] hides the meaning of Kolmogorov’s 1954 theorem on the persistence of the tori invariant in the context of a research program for classical mechanics. Therefore, my goal is to deepen and bring to light the salient points of the research program described by Kolmogorov in his speech, highlighting the cultural roots and the connections with the works of the previous decades, already analyzed in chapter 1. Here we will provide an original formulation of Kolmogorov’s theorem, present in the ’54 article [Kolmogorov 1954] and its second formulation presented at the International Congress of Mathematicians in Amsterdam.

Finally, I will present an analysis of the Diophantine condition, which plays a key role in the proof, comparing its uses in a 1942 paper by the

event in mathematical science. Desire for success in congress activities. S. Vavilov, President, USSR Academy of Sciences. [Graves, Hille, Smith, Zariski 1955, p 122]

German scholar Carl Ludwig Siegel (1896-1981) and, subsequently, I will analyze some future directions of the theorem and the research program, through the works of students of Kolmogorov, Arnold and Sinai and, of course, of the mathematician Jurgen Moser.

3.1 Kolmogorov's research program for classical mechanics: the metric and spectral approach

> I will consider my objective accomplished if I have managed to convince the audience that, in spite of the great difficulties and the limited nature of the results already obtained, the problem I have posed of using general notions of modern ergodic theory for qualitatively analyzing motion in analytic and, particularly, canonical dynamical systems deserves great attention of scientists capable of comprehending the many-sided interrelations with the most varied branches of mathematics revealed here.
>
> [Kolmogorov 1957, pp. 372-373]

Thus Kolmogorov concluded his speech at the ICM in Amsterdam.

The complete speech is reported in Russian in the Proceedings of the international congress of mathematician of 1954 [Gerretsen, De Groot 1957], in French in the *Séminaire Janet. Mécanique analytique et mécanique céleste, tome 1 (1957-1958)* [Kolmogorov 1957-58] and in two English translations: one from 1972 of the *Nasa Technical Translation* [Kolmogorov 1972], and the other from 1991 in volume 1 of the *Selected works* [Tikhomirov 1991], edited by Tikhomirov in Russian in 1985 and translated by Volosov in 1991 [Kolmogorov 1957]. The two English translations are different, not in content, but in the words used, even in the translation of the paragraph titles[129].

The complete index of his speech, taken from [Kolmogorov 1957], was as follows:

[129] We mainly used the latter English translation for our research.

The general theory of dynamical systems and classical mechanics

Introduction

§1. Analytic dynamical systems and their stable properties

§2. Dynamical systems on a two-dimensional torus and some canonical systems with two degrees of freedom

§3. Are dynamical systems on compact manifolds "in general" transitive, and should we regard the continuous spectrum as the "general" case and the discrete spectrum ad an "exceptional" case?

§4. Some remarks on the non-compact case

§5. Transitive measures, spectra, and eigenfunctions of analytic systems

Conclusion

In the scientific literature, which we have already listed in the introduction of this thesis, the theorem on the persistence of invariant tori is reported, but no space is given to the program that Kolmogorov declares to the mathematicians present at the conference and which seems to be the main reason for his push dealing with classical mechanics.

In fact, he had a broader project in mind: his program envisaged a wide-ranging study of dynamical systems, not dwelling on a particular case or on a dynamical system that describes a single real event, but his goal was to establish a method to be applied to dynamical systems to establish which properties can be considered "general" or "exceptional" (in the sense of measure theory), both for the function that defines the system and for the orbits it must describe:

> My aim is to elucidate ways of applying basic concepts and results in

the modern general metrical and spectral theory of dynamical systems to the study of conservative of conservative dynamical systems in classical mechanics.

[...] the Poincaré-Carathéodory recurrence theorem initiated the "metrical" theory of dynamical systems in the sense of the study of properties of motions holding for "almost all" initial states of the system. This gave rise to the "ergodic theory", which was generalized in different ways and became an independent centre of attraction and a point of interlacing for methods and problems of various most recent branches of mathematics (abstract measure theory, the theory of groups of linear operators in Hilbert and other infinite-dimensional spaces, the theory of random processes, etc.).

[...] For conservative systems, the metrical approach is of basic importance making it possible to study properties of a major part of motions. For this purpose, contemporary general ergodic theory has elaborated a system of notions whose conception is highly convincing from the viewpoint of physics. [Kolmogorov 1957, p 354-355]

Through a problem, i.e. a motion defined on an s-dimensional manifold Ω^{2s} through a Hamiltonian system $H(q_1, \ldots, q_s, p_1, \ldots, p_s)$, where $(q_1, \ldots, q_s)$ are positions and $(p_1, \ldots, p_s)$ are momentum, he shows the modus operandi he intends to follow in his project.

Assuming that the motion admits k prime integrals[130]:

$$I_1 = C_1, \ldots, I_k = C_k,$$

then, these integrals, being constant functions along the motions, lower the degree of freedom of the system, bringing it from $2s$ to $2s - k$, and identify in the phase space an analitic manifold M^{2s-k}.

An invariant density can be defined on this manifold, which Kolmogorv will denote by $M(x)$, which is the key to apply the methods of measure theory of dynamical systems to motions on M^{k-2s}:

[130]Kolmogorov's prime integrals are Poincaré's invariant integrals

> It is reasonable to resort to these more modern means when, apart from the integrals $I_1 = C_1, \ldots, I_k = C_k$, there are no single-valued analytic first integrals independent of the former or when their determination encounters severe difficulties and other classical methods for completing the integration of the system also prove inapplicable. In such cases it is necessary to use a qualitative approach in order to find out whether the motion on M^{k-2s} is transitive (that is, whether almost the entire manifold M^{k-2s} consists of a single ergodic set) and then, in the transitive case, to determine the nature of the spectrum or, in the absence of transitivity, to study, to within a set of measure zero (or at least to within a set of small measure), the decomposition of M^{k-2s} into ergodic sets and the nature of the spectrum on these ergodic sets.
>
> There are only two specific problems of classical mechanics known to me where this programme has been realized to a certain degree.
>
> [...] However, I believe that the time has now come when considerably more rapid progress can be made. [Kolmogorov 1957, p 357]

The end of his introductory speech gives way to the next sections, in which he puts his intent into action.

Section §1 mainly consists of introductory notions that will be used in the following paragraphs. Here he symbolically supplies the mathematical objects that he will use later:

1. A dynamical system of classical mechanics - which, Kolmogorov underlines is *a special case of analytic dynamical system with an integral invariant* - is defined by the differential equation

   $$\frac{dx_\alpha}{dt} = F_\alpha(x_1, \ldots, x_n)$$

   on a manifold Ω^n, where $\alpha = 1, \ldots n$.

2. The invariant measure is defined by the integral

$$m(A) = \int_A M(x) dx_1 \dots dx_n$$

where M(x) is the invariant density defined above.

3. A canonical system is defined as a dynamical system represented by a Hamiltonian function in the variables $(q_1, \dots, q_s)$ and $(p_1, \dots, p_s)$ on a manifold Ω^{2s} such that

$$\frac{dq_\alpha}{dt} = \frac{\partial H}{\partial p_\alpha}, \qquad \frac{dp_\alpha}{dt} = -\frac{\partial H}{\partial q_\alpha}$$

and with the invariant density equal to one:

$$M(q, p) = 1.$$

Having introduced the mathematical objects, he summarizes the modus operandi:

> Particular attention will be paid to finding which of the properties of dynamical systems are "typical" for "arbitrary" F_α and M (or an "arbitrary" function $H(q,p)$ in the case of canonical systems) and which of them can manifest themselves only by way of an "exception". However, this is quite an intricate problem. The approach from the standpoint of the category of corresponding sets in the spaces of systems of functions $\{F_\alpha, M\}$ (or functions H), despite the well-known achievements in this direction obtained in the general theory of abstract dynamical systems, is of interest rather as a means for proving existence than as a direct way for solving actual problems set by researchers in physics and mechanics, however stylized and idealized their statement may be. By contrast, the approach from the standpoint of measure theory appears to be quite reasonable and natural as viewed from physics (for instance, as it was set forth forcibly by von Neumann [1]), but its application is hampered by the absence of a natural

measure in function spaces.

We will follow two routes. First, to obtain positive results establishing that a certain type of dynamical systems should be recognized as being essential, not "exceptional", and from any reasonable point of view, should not be "neglected" (in the way that sets of measure zero are neglected), we will use the notion of stability in the sense of preservation of a certain type of behaviour of a dynamical system under small variation of the functions F_α and M or of the function H. From this standpoint, any type of behaviour of a dynamical system for which there exists at least one example of its stable realization should be recognized as being important and not negligible. [Kolmogorov 1957, p 358-359].

Section §2 *Dynamical systems on a two-dimensional torus and some canonical systems with two degrees of freedom*, contains the study of a dynamical system defined on a two-dimensional manifold, in particular a two-dimensional torus T^2.

Kolmogorov justifies the choice of such an example, pointing out that several important real situations are represented by such a system of equations:

Therefore the real significance for classical mechanics of the above analysis of dynamical systems on T^2 depends on whether there are sufficiently important examples of canonical systems with two degrees of freedom, not integrable by classical methods [Kolmogorov, 1957, p 363]

It is here that we find more references to the two previously published articles and an application, to the specific case under examination, of the theorem on the persistence of invariant tori, published nine days earlier in general form.

We will deepen this last aspect in the following paragraph, dedicated to the theorem and its formulations.

The following section, entitled with the questions *Are dynamical systems on compact manifolds "in general" transitive, and should we regard the continu-*

ous spectrum as the "general" case and the discrete spectrum in an "exceptional" case?, represents the fulcrum of his speech; in an attempt to find general or exceptional properties of a generic dynamical system, these questions perfectly reflect his research program.

In particular, the connection with the ergodic theory[131] emerges (through the concept of transitivity) and, the negative answer to both questions, sets a limit to the conjecture according to which the ergodic hypothesis was valid for any dynamical system:

> This contradicted claims which one could often see in the physical literature according to which any typical Hamiltonian system with interaction should be ergodic[132]. [Sinai 1989, p 838].

In [Arnold 1991] the author underlines that some hypotheses proposed by Kolmogorov in this paragraph regarding particular systems in which cases of mixing on bulls would occur - initially discussed in [Kolmogorov 1953] - find confirmation in two articles dated 1966 and 1967 by Sinai and Anosov[133]:

> Systems with stable transitivity and mixing on the energy level surfaces which Kolmogorov discusses at the end of §3 of the lecture at the Amsterdam Congress (paper No. 53) actually exist. Sinai and Anosov proved that geodesic flows on compact manifolds of negative curvature (along each two-dimensional direction) possess these properties [46-48][134]. Moreover, these properties are preserved under small perturbations not only in the class of Hamiltonian systems but also in the class of general dynamical systems. [Arnold 1991, p 510]

Section §4 is devoted to the discussion of a system defined on a noncom-

[131] See paragraph §1.2

[132] One of the paper that aims to demonstrate the opposite is "Dimostrazione che in generale un sistema meccanico è quasi ergodico" (Proof that in general a mechanical system is quasi-ergodic) by Enrico Fermi in *Nuovo Cimento*, [Fermi 1923a]

[133] Dmitri Victorovich Anosov (1936-2014), Russian mathematician known for his contributions to the theory of dynamical systems. He was a student of Lev Pontryagin.

[134] He refers to [Sinai 1966] and [Anosov 1967]

pact manifold. In an attempt to extend the results obtained in the previous paragraphs, Kolmogorov uses the measure theory of the Ukrainian mathematicians Krylov and Bogolyubov, already analyzed in chapter 1.

Here the problem of ultimate motions in the three-body problem is addressed, which was later thoroughly studied by the students of Kolmogorov, Sitnikov and Alekseev.

Finally, in the last section §5 Kolmogorov delves into the aspects that are closest to the overseas works of Brikhoff, Koopman and von Naumann, concerning spectra and transitive measures:

> The spectral properties of transitive measures in analytic systems have not been studied enough. [Kolmogorov 1957, p 371]

In particular he hypothesizes a stability for a dynamical system with continuous spectrum and that a countable discrete spectrum is "typical" in analytic dynamical systems:

> It is not impossible that only these cases (a discrete spectrum with a finite number of independent frequencies and a countably-multiple Lebesgue spectrum) are admissible for analytic transitive measures or that, in a sense, only they alone are general typical cases. [Kolmogorv 1937, p 371]

It is again Sinai and Asonov who prove the first of Kolmogorov's hypotheses, but regarding the typicality of the countable spectrum, there is still no confirmation or refutation:

> Kolmogorov's conjecture (§5 of paper No. 53) on the stability of a continuous (more precisely, countably-multiple Lebesgue) spectrum was proved by Sinai and Anosov [46, 47]. Thus far the conjecture that a discrete spectrum with a finite number of independent frequencies (not exceeding the phase space dimension) and a countably-multiple Lebesgue spectrum is typical has not been refuted for analytic systems. [Arnold 1991, p 513.]

In the last two sections emerge all the influences that we have explored in chapter 1 and that have created the cultural landscape in which this work resides. We will go into more detail on this aspect in section §3.3. of this thesis.

3.2 A historical analysis of Kolmogorov's Theorem on the persistence of invariant tori in Hamiltonian Systems: formulation, proof, and meaning

The original article in Russian, published in *Doklady Akademii Nauk, 1954, vol.98(4)*, is just four pages long[135], is mainly technical and mainly focuses on the formulation of the Theorem on the persistence of invariant tori and the his proof. The name chosen for the theorem in this thesis was given by Arnold[136]- and so we decided to use the same wording[137].

Although the notations used are quite different from those currently used in more recent texts, we are interested in reporting the original statement below, reported in [Kolmogorov 1954].

The theorem is then reformulated in the article published in the Proceedins of the International Congress of Mathematicians in Amsterdam, firstly in the section §2, in application to a Hamiltonian system defined on a two-dimensional torus, to then be stated in the general case of a manifold $2s$-dimensional in the next section.

3.2.1 Kolmogorov flips his cards: the publication of the Theorem on the persistence of invariant tori in 1954 in the "Doklady Academii Nauk SSSR"

The short article *The preservation of conditionally periodic motions under small variations of the Hamilton function* [Kolmogorov 1954], can be summarized

[135] The English translation consists of six pages

[136] "Kolmogorov's 1954 theorem on the persistence of invariant tori under a small analytical perturbation of a fully integrable Hamiltonian system", and "he [Kolmogorov] arrived at his theorem of 1954 on the persistence of invariant tori." in [Arnold 1997, pp 742-743].

[137] In modern literature, it is often referred to as the KAM Theorem.

in a very dry structure: after a brief introduction in which the mathematical objects that will be involved are introduced, the statement of the theorem is introduced, quite long and detailed. The immediately following part is a small observation on the very meaning of the theorem, to then discuss its proof point by point, without dwelling on the individual logical and mathematical passages.

We could divide the demonstration part into three moments:

1. a brief initial explanation of the method used;

2. a more technical central part, in which some mathematical passages of the proof are explained;

3. a final part, always discursive, where mathematical rigor leaves room for an explanation that begins with "*It is easy to see that* $[\dots]$" [Kolmogorov 1954, p 352].

To use the same nomenclature used by Kolmogorov, we consider a region $G \subset$ in phase space Ω^{2s} represented as the product of an $s-$dimensional torus T by a region S in an $s-$dimensional Euclidean space.

In this way, the points of the torus will be characterized by periodic 2π, $q_1, \dots, q_s$, and the coordinates of a point p belonging to S will be indicated by the vector $p_1, \dots, p_s$.

So, we consider a Hamiltonian H in G having the canonical form

$$\frac{dq_\alpha}{dt} = \frac{\partial}{\partial p_\alpha} H(q,p), \qquad \frac{dp_\alpha}{dt} = -\frac{\partial}{\partial q_\alpha} H(q,p)$$

and suppose that

- H also depends on a parameter θ (perturbative parameter), where $\theta \in (-c; c)$ and is independent of time;

- H is analityc in the variables (q, p, θ).

From now on, we will consider the Hamiltonian function H defined on G, with $\theta = 0$ having the form:

$$H(q,p,0) = m + \sum_{\alpha} \lambda_\alpha p_\alpha + \frac{1}{2}\sum_{\alpha\beta} \Phi_{\alpha\beta}(q) p_\alpha p_\beta + O(|p^3|),$$

where, m is a real constant and represents the constant energy of the system, $\alpha, \beta \in (1, \ldots, s)$ are integers, $(\lambda_1, \ldots, \lambda_s)$ are the frequencies of motions, the sum $\sum_\alpha \lambda_\alpha p_\alpha$ coincides with the scalar product between the vector of the frequencies $(\lambda_1, \ldots, \lambda_s)$ and the vector $(p_1, \ldots, p_s)$ of the coordinates of a point belonging to S.

The meaning of $\Phi_{\alpha\beta}$ will be clearer in the statement of the theorem.

Finally, we will denote with

$$(x, y) = \sum_{\alpha} x_\alpha y_\alpha, \qquad |x| = +\sqrt{(x,x)}$$

and with T_c an s-dimensional torus in region G, formed by the set of points (q, p) with $p = c$ constant.

We will assume that S contains the point $p = 0$, that is, $T_0 \subseteq S$.

Kolmogorov's Theorem on the persistence of invariant tori:

Theorem 3 [a] *Let*

$$H(q,p,0) = m + \sum_{\alpha} \lambda_\alpha p_\alpha + \frac{1}{2}\sum_{\alpha\beta} \Phi_{\alpha\beta}(q) p_\alpha p_\beta + O(|p^3|), \tag{7}$$

where m and λ_α are constants, and let the inequality

$$|(n, \lambda)| \geq \frac{c}{n^\eta} \tag{8}$$

be fulfilled for a certain choice of the constants $c > 0$ and $\eta > 0$ and *all integral vectors n. Morover, let the determinant formed from the average values*

$$\Phi_{\alpha\beta}(0) = \frac{1}{2\pi^s}\int_0^{2\pi}\int_0^{2\pi}\Phi_{\alpha\beta}(q)dq_1 dq_\alpha$$

of the function

$$\Phi_{\alpha\beta}(q) = \frac{\partial^2 H}{\partial p_\alpha \partial p_\beta}(q,0,0)$$

be non zero:

$$|\Phi_{\alpha\beta}(0)| \neq 0. \tag{9}$$

Then there exists analytic functions $F_\alpha(Q,P,\theta)$ and $G_\alpha(Q,P,\theta)$ defined for all sufficiently small θ and all point (Q,P) belonging to a neighbourhood V of the set T_0 that determine a contact transformation

$$q_\alpha = Q_\alpha + \theta F_\alpha(Q,P,\theta), \qquad p_\alpha = P_\alpha + \theta G_\alpha(Q,P,\theta)$$

of V into $V' \subseteq G$ reducing H to the form

$$H = M(\theta) + \sum_\alpha \lambda_\alpha P_\alpha + O(P^2) \tag{10}$$

($M(\theta)$ does not depend on Q or P).

[a]Original version found in [Kolmogorov 1957, p 349-350].

We will deal with some details on the proof of the theorem present in the original article in section §3.3.1.

Now we will limit ourselves to trying to explain the theorem obtained and its meaning, which is connected both to the works in classical mechanics - in particular celestial mechanics - and to those in the field of ergodic theory.

Kolmogorov himself provides an explanation of the theorem obtained, even before setting about demonstrating it:

The significance of Theorem 1 in mechanics can easily be understood. It shows that, under conditions (2) and (3)[138], an $s-$parameter family of conditionally periodic motions

$$q_\alpha = \lambda_\alpha t + q_\alpha^{(0)}, \quad p_\alpha = 0,$$

existing at $\theta = 0$ cannot disappear under a small variation of the Hamilton function H; namely, the variation results only in a displacement of the $s-$dimensional torus T_0, along the trajectories of the motions: it is transformed into a torus $P = 0$, which is filled with trajectories of conditionally periodic motions with the same frequencies $\lambda_1, \ldots, \lambda_s$. [Kolmogorov 1957, p 350].

Condition (8) is what is now called the *diophantine condition* and it is a condition that can be required for an irrational real number. It can be shown that this condition holds for almost all irrational real numbers, up to a set of Lebesgue measure zero. Thus, Kolmogorov had stated that, for most of the initial frequencies - i.e. for all those satisfying condition (8) - the motions of the perturbed Hamiltonian system remain quasi-periodic and the torus that foliate the phase space when the Hamiltonian is unperturbed, are not destroyed by the perturbation, but are transformed into other invariant toruses, close to the unperturbed ones, on which the motions are quasi-periodic with same frequencies $\lambda_1, \ldots, \lambda_s$.

Like Poincaré, Kolmogorov considered small perturbations of integrable systems and proved that most invariant tori in the measure-theoretic sense are preserved under small perturbations. [Sinai 1989, p 838]

Indeed, his theorem provides an important contribution to the "General problem of dynamics", defined by Poincaré - which we reported in this work in section §1.2.1 - and subsequent developments, left unsolved for more than fifty years. In fact, under the conditions imposed by Kolmogorov, and for a relatively small perturbation θ, nearly integrable perturbed Hamil-

[138] In our case the conditions are (8) and (9)

tonian systems are stable for $s \leq 2$ and, for $s \geq 3$ the majority of initial data generates solution stable for all times.

3.2.2 The presentation of the Theorem on the persistence of invariant tori during the ICM Amsterdam (September 9, 1954)

Section §3 of [Kolmogorov 1957], *Are dynamical systems on compact manifolds "in general" transitive, and should we regard the continuous spectrum as the "general" case and the discrete spectrum as an "exceptional" case?* reflect Kolmogorov's research program: try to find general or exceptional properties of any dynamical system.

So, he immediately clarifies that both questions are connected with issues in the ergodic theory, already detailed in this work in section 1.3.2[139].

> In application to analytical canonical systems, the answers to both questions are negative. [Kolmogovor 1957, p 365].

To provide this answer, Kolmogorov had just shown a particular case of the theorem on the persistence of invariant tori applied to a Hamiltonian system perturbed with two degrees of freedom on a two-dimensional torus.

The proof does not want to be rigorous: he limits himself to listing the salient points of it and the differences with respect to previous attempts in the field of perturbation theory:

> The method of proof consists in studying the behaviour of the original tori T^2_c with frequencies $\lambda_\alpha(c)$ satisfying condition (2)[140] under variation of θ[141] and establishing that for sufficiently small ϵ each of the tori is not destroyed and is merely displaced in Ω with preservation of trajectories of

[139]Remember that in 1.3.2. we have seen that if T is a measure-preserving transformation on a space X, then T is ergodic (or transitive) if and only if it has only trivial invariant sets, i.e. if and only if $m(E) = 0$ or $m(X - E) = 0$ whenever E is a measurable set invariant under T.

[140]Diophantine condition

[141]That is the small perturbations of the system

conditionally periodic motions with constant frequencies λ_α on it.

Probably many of you will already have guessed that, in essence, what we are talking about is a certain modification of the idea of the possibility of avoiding the appearance of abnormal "small divisors" when calculating disturbed orbits, which has been extensively discussed in the literature on celestial mechanics. However, in contrast to ordinary perturbation theory, we obtain exact results instead of the conclusion that the series of some approximation of finite order (relative to θ) are convergent. This is achieved because instead of calculating the disturbed motion for fixed initial conditions, we change the initial conditions themselves so that, with varying θ, we always deal with motions having normal frequencies λ_α(in the sense of condition (2)).

So, Kolmogorov observes that the theorem obtained holds for any number of degrees of freedom[142]:

In application to analytic canonical systems, the answers to both questions are negative, since the theorem on the stability of the decomposition into tori which we stated for systems with two degrees of freedom remains valid for any number of degrees of freedom as well.

[...] Thus, under small variations of H the dynamical system remains non-transitive and the region G continues to be decomposable, to within a residual set of small measure, into ergodic sets with discrete spectra (of the indicated specific nature). [Kolmogorov 1957, p 365-366].

Thus, in general, canonical Hamiltonian systems are not transitive and, in general, do not have a continuous spectrum.

The theorem obtained by Kolmogorov, therefore, inevitably raises questions about the validity of the ergodic hypothesis, in general.

However, under the condition that the number of degrees of freedom is finite, the main applications of the ergodic hypothesis do not fall, i.e. those of statistical mechanics, where the number of degrees of freedom is

[142]Finite number of degrees of freedom, let us add.

very high and often tends to infinity.

Furthermore, it is Kolmogorov himself who points out:

> No similar results regarding the stability of a certain general type of behavior of non-canonical dynamical systems with an integral invariant and a compact phase space Ω^n are known to me. [Kolmogorov 1957, p 367].

3.2.3 The Diophantine condition: from Carl Ludwig Siegel to Kolmogorov

In 1942 Carol Ludwig Siegel (1896-1981) published the article *Iteration of Analytic Functions* in the Annals of Mathematics. In an attempt to solve convergence problems of a Fourier series, he used the same Diophantine condition used twelve years later by Kolmogorov in his works and which represents one of the keys to solving the problem left open for so many years. The question of Kolmogorov's knowledge of this article still seems to be not fully clarified.

Siegel's works on analytic functions

In the spring of 1940 Carl Ludwig Siegel left Germany to go to America, to the Institute for Advanced Studies in Princeton. Here, after having been a fellow for five years, he became a permanent member in 1946, although in 1951 he returned definitively to Göttingen - where he had already held a professorship in 1938. His "escape" from World War II Germany is reported by himself in [Siegel 1979], an article which contains the "Address Given on June 13, 1964, in the Mathematics Seminar of the University of Frankfurt on the Occasion of the 50th Anniversary of the Johann-Wolfgang-Goethe-University Frankfurt". The speech focuses on the fate of some of his colleagues who actively contributed to the seminar, especially those mathematicians who were victims of racial persecution:

Paul Epstein[143], Ernst David Hellinger[144] and Max Dehn[145].

The story of his friend Dehn is intertwined with Siegel's last days before leaving for the United States of America:

> Dehn and his wife went to Copenhagen in January 1939 and later to Trondheim in Norway, where he took over the post of a vacationing colleague at the Technical University.
> [...] When I visited him there in March, 1940, he seemed to have an air of renewed hope about him after the sad events of the previous years, and he was happy to be lecturing again. While walking together one day we noticed several seemingly deserted merchant ships in the harbor flying the German flag. Dehn told me that they had been there quite some time already, reportedly with engine trouble. They were called pirate ships by the locals on account of the somewhat frightening impression they made. Because I left a few days later for a self-imposed exile in America, I learned only later the reason for those mysterious ships' presence, They were filled with war material for the German soldiers who suddenly occupied Trondheim on the day of the invasion of Norway.

At that time he had already become one of the leaders in the development of number theory, but it was in the 1940s that his interest in the theory of analytic functions emerged, probably influenced by his great passion for celestial mechanics. In fact, Siegel gave several lectures on celestial mechanics in Frankfurt, Main, Baltimore, Princeton and Göttingen. In Göttingen, with Moser as a student, he gave a series of lectures during the

[143] Paul Epstein (Frankfurt, Germany 1871- Frankfurt 1939) was a German mathematician of Jewish origin, best-known for his contributions to number theory; he committed suicide in 1939 with a lethal dose of Veronal, after receiving a summons from the Gestapo.

[144] Ernst David Hellinger (Striegau, Germany (now Strzegom, Poland) 1883 - Chicago 1950) was a German mathematician of Jewish origins, analyst and historian of mathematics, known for having introduced a new type of integral that today bears his name: Hellinger integral

[145] Max Dehn (Hamburg, Gemany 1878 - Black Mountain, North Carolina) was a German mathematician of Jewish origins most famous for his work in geometry, topology and geometric group theory.

winter semester of 1951-52. And it is precisely from the notes taken by the student, that Siegel published the first edition, in 1956, of *Lectures on celestial mechanics* [Siegel, Moser 1995/1971].

His articles [Siegel 1941], [Siegel 1942] focus on a classic linealization problem, *related to the important researches of Delaunay, Hill and Poincaré in celestial mechanics*, [Siegel 1941, p 807]. In fact, in the first of the two articles he demonstrates that every convergent integral (solution) of a given canonical system can be written as a power series in a single variable. However, it is Siegel himself who warns the reader:

> This elegant method of solution has also been generalized to the case of a function H which contains explicitly the variable t, in periodical form, and is closely related to the important researches of Delaunay, Hill and Poincaré in celestial mechanics. However, there is a serious objection: The question of convergence has never been settled.

In the 1942 article, entitled *Iteration of analytic functions* he goes one step further: the analytic power series

$$f(z) = \sum_{k=1}^{\infty} a_k z^k$$

with the assumption that a_1 is a number such that $|a_1| = 1$ and $a_1^n \neq 1$ for $n = 1, 2. \ldots$ and

$$log|a_1^n - 1| = O(log\ n) \tag{11}$$

is convergent.

It is then Siegel himself who states that the hypothesis (1) on a_1 is equivalent to stating that, written a_1 in the exponential form $a_1 = e^{2\pi\omega}$, then

$$|\omega - \frac{m}{n}| > \lambda n^{-\mu} \tag{12}$$

for arbitrary integers m and n, $n \geq 1$, where λ and μ are positive numbers depending only upon ω.

This is clearly the same Diophantine condition (8) that Kolmogorov imposed on the frequency of motion to obviate the problem of small divisors which would have interfered with the convergence of the series.

Did Kolmogorov know the Siegel's works?

Although the demonstrative techniques used by Siegel in 1942 and by Kolmogorov in 1954 are completely different, the coincidence of the Diophantine hypothesis used inevitably raises the question that entitles this subsection. Indeed, it is clear from some studies ([Dumas 2014, p 15, 35, 64, 81], [Goldstein 1980, p 530]) that there is a belief among many scholars that Kolmogorov was familiar with the work of his German colleague.

Dumas in [Dumas 2014] refers several times to the question. To cite a few excerpts:

> Occasionally, disagreement erupts over how much Kolmogorov proved in 1954. [...] Still others think that C.L. Siegel's name should be attached to the theorem. [Dumas 2014, p 15]

And, again:

> Kolmogorov (may have) adapted this step from Siegel's work, as described above. [Dumas 2014, p 64]

The author of "The KAM story", in note 1 on page 81, clarifies the reason for that "(may have)":

> In describing the first solutions of small divisor problems, many references say something like "Kolmogorov adapted Siegel's techniques," as I do here. However, in the sequel I'll qualify this with 'perhaps,' because, while there is no doubt that Siegel's work on small divisors preceded Kolmogorov's by a dozen years, there does not seem to be direct evidence that Kolmogorov knew about Siegel's work.

And, to support his statement, he refers to the article [Arnold 1997], already cited here. In fact, on p. 738 we find the reference that Dumas himself reports, in abbreviated form:

> I started inquiring whether somebody had examined all these questions between A. Denjoy's work of 1932 and my work of 1958. Among others, I found C. L. Siegel's papers on the linearization of holomorphic mappings near fixed points. To be more precise, I first invented this problem myself (as a simplified model of the problem of circle mappings) and solved it by Kolmogorov's method. Only later on, I discovered Siegel's work who had obtained the same result by another method in about 1940.
>
> "We are in a good company," Kolmogorov told me when I let him know of my bibliographic findings. As far as I understand, he was aware of neither Siegel's works nor J. E. Littlewood's[146] works on the exponential slowness of an increase in perturbations. [Arnold 1997, p 738].

There is a fine, often blurred line between attributing developments in classical mechanics to Siegel and Kolmogorov's use of his work.

In this regard, Arnold in [Arnold 1963] also attributes merit to the two mathematicians, placing their names side by side in the first lines of the introduction to his work:

> The difficulty of qualitative problems of classical mechanics is well known. In spite of prolonged efforts by many mathematicians most of these problems still await solution. Only in recent times, beginning with the work of C.L. Siegel (1942) and A.N. Kolmogorov (1954), has some progress been made in solving problems on the stability of motion of dynamical systems.

But, as we have seen, Kolmogorov's pupil thought that his mentor did not know the work of his German colleague.

In another more recent testimony by Arnold from 2004, [Arnold 2004], he returns to the question again and his conviction on the connection between the works of the two mathematicians appears more

[146]He refers to John Edensor Littlewood (Rochester, England 1885 - Cambridge 1977), English mathematician best known for his contributions in the field of function theory, series theory, many of which obtained together with the mathematician Godfrey Harold Hardy.

evident:

> Just at this time Kolmogorov was giving a course at Moscow University on his work on small denominators and on Hamiltonian systems and on what is now called KAM theory. [...] I came to Kolmogorov with my theorems. "Well," he said, "here is my paper in Doklady '54. I think it will be good if you continue with this problem, try to think of applications to celestial mechanics and rigid body rotation. I am very glad that you have chosen a good problem." [...] I read other people's works and I finally discovered some papers by Siegel, who was a personal friend of Kolmogorov when they stayed in Göttingen in the 1930s. Kolmogorov was not aware that Siegel had later worked on the small denominators problem. Siegel's paper was published in 1941 but was unknown to Kolmogorov. He knew about the works of Poincaré, of Denjoy, and of Birkhoff, but not about Siegel. So he told me that we were in very good company: "Siegel is really serious," he said. I had discovered the Siegel theorem related to the normal forms for circle rotations because of the system of education at Moscow University, which was different from that in America. I think it followed the German tradition that, when you have a result and wish to publish it, you first have to check the literature to see whether someone else has ever studied it. We were told this in our first introductory course in library work, in which we were taught how to find, starting from zero information, everything needed. There was no Internet at that time of course, but still we were able to find the references, and this is how I discovered that Siegel existed. [Arnold 2004, p 615]

Here, even, a friendly relationship between the two mathematicians is reported, dating back to the times of the trip to Germany and France in 1930-31 made by Kolmogorov together with Aleksandrov. We know that Siegel defended his doctoral thesis in Göttingen in 1920 and was then appointed lecturer at the Johann-Wolfgang-Goethe University of Frankfurt in 1922, where he remained until 1938, when he accepted a professorship in Göttingen. In [Shiryaev 1989], where some details of the trip and the places visited are reported, neither Siegel nor Frankfurt is mentioned. It is possible that, on the occasion of the arrival of the two Russian mathematicians, Siegel decided to meet his Russian colleagues visiting Göttingen and that was an opportunity to get to know each other, but we have no certain references in this regard.

Another opportunity to investigate the matter occurred to me when, on May 28, 2021, I had the opportunity to interview Jakov Grigorevich Sinai (1935 -), a pupil and then collaborator of Kolmogorov. The first question posed concerned precisely the relationship between the works of Kolmogorov and Siegel:

> ME: The first question is about Siegel's work on Diophantine estimates. These techniques are also used by Kolmogorov in his 1954 proof of the theorem but he does not mention them in the bibliography. Do you know if Kolmogorov knew this work?
>
> SINAI: In my opinion, he didn't know Siegel's work. Siegel's work was discussed at Arnold's seminar and I assume that Arnold explains Siegel's work to Kolmogorov which, as you know, also uses small denominators.
>
> ME: And do you know what Kolmogorov's inspiration for Diophantine estimates is?
>
> SINAI: I'm not so sure about that.

As has already been underlined in the conversation with Sinai, Kolmogorov never mentions Siegel's works, neither in the three articles nor in the brief statements subsequently made, which we have extensively analysed; when Siegel publishes his 1942 work, mathematics is only the backdrop to world events of World War II and he was already in America, a continent with which connections were difficult from Russia. It is at least plausible that the German mathematician's article did not cross Russian borders for several years.

Finally, we recall that Jürgen Moser was a student of Sinai when he was asked by Mathematical Review to review the work [Kolmogorov 1957] related to the 1954 ICM conference. At that time he had already dealt with stability problems of fixed points mapping ellipticals preserving area, under the exhortation of Siegel, [Moser 1999]. It seems rather strange that in the review he wrote there is no connection with the works of his master.

However, it should be emphasized that the purpose of this research is not to provide an absolute truth. Kolmogorov's knowledge or otherwise of Siegel's work would in no way invalidate the importance and impact that his work had on the development of KAM theory.

3.3 The roots of Kolmogorov-Arnold-Moser theory (KAM theory)

> I think that it is precisely them we must number among those of his predecessors to whom he turned most of all. Somewhat surprising is the absence of references to Poincare. This is largely because Kolmogorov learnt of Poincare's ideas by reading the works of Chazy and Charlier. The other mathematicians to whom Kolmogorov refers were part of the current scientific scene. Here we must mention the great influence which the works of Krylov-Bogolyubov and de Rham had on him. [Tikhomirov 1988, p. 23].

Let us analyze the points in Kolmogorov's speech in which the influences of the mathematicians mentioned in the conversation with Arnold are evident, and then briefly describe the future directions of his research program.

One of Kolmogorov's most claimed sources of inspiration was the work of Bogolyubov and Krylov in the field of nonlinear mechanics. Their contribution appears evident when Kolmogorov is preparing to make some considerations on dynamical systems defined in noncompact spaces, §4. The study of these specific dynamical systems appears simplified by the extension of the ergodic theory that Ukrainian mathematicians developed in [Krylov, Bogolyubov 1937]. In their work, through the construction of invariant and transitive measures even in cases where they are not present, they extend the field of validity of the ergodic theory.

On this subject Kolmogorov makes only a few considerations, without arriving at a definitive answer as had happened in the compact case, and hypothesizes some scenarios that appear more probable:

> The arguments which, in the case of a compact Ω, can be given in favour of the idea that a compact dynamical system of "general type" is transitive, when applied to non-compact dynamical systems, leads to the hypothesis that "in general" one of the following two cases holds: either the system is dissipative (that is, almost all its points recede) or the measure m is ergodic (obviously, in the latter case the receding points constitute only a set of measure zero).
>
> [...] When it is known in advance that there is a set of positive measure consisting of receding points, then in accordance with what has been said, the conjecture arises that the system is dissipative. Probably Birkhoff's assumption that the three-body problem is dissipative is based on some consideration of this kind. [Kolmogorog 1957, p 369].

Therefore, the study of this particular case appears interesting as soon as one thinks of the repercussions it entails in the field of celestial mechanics: in the three-body problem - as Kolmogorov underlined - but also in the cases of capture or receding problems of a celestial body . Regarding the latter aspect, he points out that in fact there are very few scholars attracted by these topics. In confirmation of what has been stated, it shows the significant example of the disapproval of some considerations made in the thirties by the astronomer Chazy - which we have studied in depth in 1.2.3 -, concerning the conjecture on the impossibility of the capture phenomenon in the three-body problem, takes place only after almost twenty years by O. Yu. Schimtd first and Sitnikov later:

> We note that, among more elementary problems, particular problems

> dealing with receding trajectories of various specific types attract little attention of specialists in the qualitative theory of differential equations. A spectacular example of this is the fact that a disproof of Chazy's assertions that no "ex-change" and "capture" are possible in the three-body problem [17, 18] was first carried out in a way which is cumbersome (and logically unconvincing without precise error estimates), using numerical integration (see Bekker [19] and Shmidt [20]), and only recently has Sitnikov [21] constructed an example of "capture" in a very simple manner and almost without calculations. [Kolmogorov 1957, p 370].

In paragraphs §3 and §4 of his work [Kolmogorov 1957], Kolmogorov will deepen the questions of the spectrum and its properties for a transitive system and of the existence of transitive measures even in cases where the phase space where the dynamical system is defined is not compact.

Indeed, we are not surprised by Kolmogorov's interest in measure theory, given his work in this area and his work *Foundations of the Theory of Probability* [Kolmogorov 1933]. But, the mathematician Jan von Plato even hypothesizes an interest in measure theory resulting from an interest in physics, and not the other way around:

> Two works precede Grundbegriffe's axiomatization of measure theory [Kolmogorov, 1929, 1931]. In the latter, there was a physical motivation for constructing a theory of probability, namely the need to handle schemes of statistical physics in which time and state space are continuous. [von Plato 2005, p 962].

We saw in Chapter 1 that these questions were also of great interest to von Neumann - and to Birkhoff and Koopman, of course - and Sinai confirms his master's interest in these topics:

> Apparently the interests of Kolmogorov in ergodic theory had already started in the 1930s. In mathematical Moscow it was a period of construction of the foundations of the theory of stationary random processes. One

> might recall the paper by Khintchine [11][147] at that time dedicated to the spectral theory of such processes. [...] The paper by Khintchine [10][148], where he gave a purely metric proof of the Birkhoff ergodic theorem, belonged to ergodic theory itself. In view of this paper the ergodic theorem on a.e. convergence of time averages is often called the Birkhoff-Khintchine theorem at least in the Russian literature. In the 1930s, the well-known paper by Krylov and Bogolyubov [12][149] on invariant measures for groups of homeomorphisms of topological spaces was written.
>
> In the beginning of the 1930s, there appeared the famous paper by von Neumann [21] [150], where the general notion of the metric isomorphism of one-parameter groups of measure-preserving transformations was introduced. Also in [21] von Neumann proved a basic theorem of metric isomorphism of ergodic dynamical systems with pure point spectrum. This theorem showed that for this class of systems the spectrum is the complete metric invariant. Since that time the problem of metric classification of dynamical systems became one of the central ones in ergodic theory. The scientific activity of von Neumann was always under close attention. It is not surprising that this problem became well known quite soon in Moscow and several mathematicians spent a lot of effort trying to make some progress here.
>
> [...] For Kolmogorov the end of the 1930s was the beginning of his classical works on hydrodynamics and turbulence. His first publication which can be considered as relating to ergodic theory goes back to 1937[151], where he exposed the Birkhoff-Khintchine theorem in probabilistic terms. [Sinai 1989, p 833]

In these sections we find full confirmation of the words written by the

[147]He refers to [Khintchine 1938]
[148]He refers to [Khintchine 1933]
[149]He refers to [Krylov, Bogolyoubov 1937]
[150]He refers to [von Neumann 1932b]
[151]He referso to *A simplified proof of the Birkhoff–Khinchin ergodic theorem* [Kolmogorov 1937]

Russian mathematician in the commentary on his works on classical mechanics. In the excerpt that we have already reported in this work, he stated that his works in this area were influenced, among others, by von Neumann's writings on the spectral theory of dynamical systems. Like the American of his time, Kolmogorov tackles the problem from a broad point of view, trying to provide general answers, also through the study of the system through ergodic theory and spectral theory. Proof of this is also a testimony from Sinai, in which the mathematician reports information about a seminar held in Moscow in 1957, *Kolmogorov's seminar on selected problems of analysis*, where Kolmogorov addresses once again the work of von Neumann.

> In the autumn of 1957, Kolmogorov organized his famous seminar on dynamical systems and gave a lecture course on the same subject. Among the participants and listeners there were Alekseev, Arnol'd, Girsanov, Meshalkin, Pinsker, Sinai, Sitnikov, Tikhomirov and others.
>
> [...] The lectures by Kolmogorov started with the proof of the metric isomorphism of dynamical systems with pure point spectrum. He gave an entirely probabilistic exposition of the corresponding theorem by von Neumann.
>
> [...] In the seminar the participants discussed in much detail the construction of Ito's multiple stochastic integrals and the ergodic properties of Gaussian stationary processes. It is well known that such processes can be obtained as natural limits of quasi-periodic processes, that is, processes corresponding to dynamical systems with pure point spectrum. A general feeling at that time was that there exist some boundary separating dynamical systems of probability theory and dynamical systems appearing in ordinary differential equations, classical mechanics and hydrodynamics or, as we call them sometimes, classical dynamical systems. [Sinai 1989, p 834].

One of the first future directions of his research program was carried out by Sinai himself after Kolmogorov published an article on the entropy of dynamical systems at the end of 1957. This work is strongly connected to ergodic theory and classical mechanics works and will lead to future developments of his student in his papers [Sinai 1959] and [Sinai 1964].

There are also future works, more properly related to the Theorem on the persistence of invariant tori.

> Kolmogorov's classical papers Nos. 52 and 53 produced a very strong effect on the subsequent development of the theory of dynamical systems, and at present there are dozens of books developing or presenting the material of these papers.
>
> In this brief commentary it is impossible to embrace all applications of these results and we confine ourselves to some improvements introduced into the theory after 1954. [Arnold 1991, p. 504].

On the one hand, Arnold decided to devote himself to the topics contained in the conference: in 1963 he published his formulation of Kolmogorov's theorem [Arnold 1963a] and subsequently extended the results obtained to some important cases of degenerate Hamiltonian systems[152]. This will prove to be fundamental for the subsequent development in celestial mechanics since, although Kolmogorov's work can be traced back to the works of Poincaré, the necessary hypothesis of his theorem, i.e. the non-degenerate condition (9) is not respected by the system representing the motions of our solar system and, in particular, of the three-body problem.

Almost simultaneously with the works of Arnold, in 1962 the German mathematician Jürgen Kurt Moser (1928-1999) published the paper *On invariant curves of area-preserving mappings of an annulus* after being asked in 1959 to review Kolmogorov's work on the speech at the ICM. He contained a theorem dealing with Hamiltonian systems that have only a finite number of derivatives (333 derivatives) and are not necessarily analytic - as Kolmogorov had instead imposed.

[152] [Arnold 1963b] and [Arnold 1964]

Their contributions were intended to make an important contribution in the studies of the general theory of dynamical systems, leading to the birth and development of a new approach for the study of such problems: the KAM theory. One of the peculiar characteristics of this new approach is precisely the demonstration technique based on the construction of an iterative algorithm that converges very rapidly which allows to neutralize the influence of the aforementioned small denominators and to prove, under some conditions, the stability of the studied problem.

3.3.1 KAM theorem or Kolmogorov's theorem?

> The proof of this theorem was published in Dokl. Akad. Nauk SSSR 98 (1954), 527–530, but the convergence discussion does not seem convincing to the reviewer.) This very interesting theorem would imply that for an analytic canonical system which is close to an integrable one, all solutions but a set of small measure lie on invariant tori. [Moser 1959]

> This theory is called KAM, or Kolmogorov–Arnold–Moser, and people say that there is even a KAM theorem. I was never able to understand what theorem is it. [Arnold 2004 p. 622].

When Jürgen Kurt Moser sent his review to Mathematical Review he had just turned thirty. As he will write in [Moser 1999], acting as referee for Kolmogorov's intervention in the proceedings will fill him with enthusiasm, since he had found someone else who was dealing with Hamiltonian mechanics in a historical moment in which there were fewer and fewer scholars interested in the subject:

> Some 40 years ago, when I was at MIT, the Mathematical Reviews asked me to review the famous lecture of Kolmogorov, held at the International Congress 1954 in Amsterdam. This is how I first learned about this work and I was very excited about it. At that time there were few mathematicians interested in Hamiltonian mechanics, and it was encouraging to me

to find others working in this field. The significance of this fundamental work was indeed apparent to me, since I had been working on the stability problem of elliptic fixed points of area-preserving mappings, a problem C.L. Siegel had urged me to pursue. Naturally, I was disappointed that neither Kolmogorov's address nor his Doklady announcement contained a proof. Therefore I wrote to Kolmogorov asking for the argument. I never received a reply, and I had to write my review not knowing whether this theorem was actually true. I never believed in proof "by authority"! I also had no doubt that Kolmogorov knew how to prove his claims, but that did not help me! [Moser 1999, p 19].

Indeed, in the article of the proceedings, Kolmogorov states the theorem in section 3, without giving any proof, recalling it from his recently published article [Kolmogorov 1954]. Therefore, the proof of the theorem must be sought in this article: here the author does not make a real rigorous demonstration, but exposes the essential passages on which it is based:

The transformation

$$(Q, P) = K_\theta(q, p)$$

whose existence in asserted in Theorem 1 can be constructed as the limit of transformations

$$(Q^{(k)}, P^{(k)}) = K_\theta^{(k)}(q, p),$$

where the trasformations

$$(Q^{(1)}, P^{(1)}) = L^{(1)}(q, p), \qquad (Q^{(k+1)}, P^{(k+1)}) = L_\theta^{(k+1)}(Q^{(k)}, P^{(k)})$$

are found by a "generalized Newton's method". In this paper, we confine ourselves to the construction of the transformation $K_\theta^{(1)} = L_\theta^{(1)}$, which makes it possible to understand the role of conditions (3) and (4)[153] of The-

[153] It refers to the conditions that in our work are denoted by (9) and (10) in paragraph 3.2

orem 1. [Kolmogorov 1954, pp. 350-351].

The proof of the theorem is substantially based on two fundamental steps, described by Luigi Chierchia (born 1957) in his article *Kolmogorov's 1954 Paper on Nearly-Integrable Hamiltonian Systems* [Chierchia 2008]:

1. the construction of the successive transformations of variables (q, p), obtained each from the previous one through Newton's quadratic approximation method;

2. the convergence of the product iteration process.

So, one of the peculiar characteristics of this new approach is the construction of an iterative algorithm that converges very rapidly (inspired by Newton's tangent method for finding the solutions of an algebraic equation). Indeed, this convergence makes it possible to neutralize the influence of small denominators.

Kolmogorov gives all the steps for constructing step 1 of his iterative process, but doesn't say much about a second step where he was supposed to ensure convergence of the product iteration process. The only passage in this regard can be found in the few papers *Only the applications of condition (3)*[154] *in the proof of the convergence of the mapping $K_\theta^{(k)}$ to an analytic limit mapping K_θ is somewhat more intricate* [Kolmogorov 1954, p 352].

Therefore, he almost takes for granted one of the most important aspects for proving the thesis, the analytical convergence of the sequence of functions $K_\theta^{(k)}$ to a function function K_θ, assuming only the Diophantine hypothesis on frequencies as sufficient. Instead, a detailed proof of the theorem requires more arguments in this regard, such as for example the introduction of a decreasing sequence of Banach spaces (of increasingly smaller dimensions) where it is possible to ensure the convergence of the single functions $K_\theta^{(k)}$ at each step. A complete demonstration, which however attempts to retrace the original version, can be found in [Chierchia

[154] Diophantine condition

2008]. Here, the author's attempt is to highlight the missing arguments in Kolmogorov's article with the aim of showing that all passages must have been well known to Kolmogorov:

> We point out that step (ii)[155] – which consists in introducing a scale of Banach spaces, giving recursive estimates and deducing from such estimates the convergence of the scheme – is based on very classical tools (such as Cauchy estimates for analytic functions or the classical Implicit Function Theorem) obviously well known to Kolmogorov. [Chierchia 2008, p 130]

It is possible that the actual lack of some demonstrative details, combined with the revision made by Moser, have contributed to create some uncertainty about it, so much so that most of the secondary sources on the KAM theory ([Dumas 2014], [Charpentier, Lesne, Nikolski 2004], [Diacu, Holmes 1996], for example) report the story of a theorem enunciated by Kolmogorov, but proved after almost 10 years by Arnold and Moser (although the latter in a different version) and a sort of unification of the three results reported in [Kolmogorov 1954], [Moser 1962] and [Arnold 1963] under the name of the KAM Theorem[156].

What is not clear is whether Kolmogorov really took the missing aspects for granted, perhaps considering them trivial or of little importance, or whether, instead, he did not notice the demonstrative "flaw".

One could also wonder why, when Moser aroused doubts about his result, by writing to him for further clarifications, he did not provide the necessary evidence to put an end to the impasse, definitively validating his thesis.

A possible explanation is provided by Arnold. Starting from the small extract reported at the beginning of this paragraph and reporting an ex-

[155] He refers to the convergence of the product iteration process.

[156] Although Dumas is more cautious in his statements and writes: *Occasionally, disagreement erupts over how much Kolmogorov proved in 1954 (some say his sketch-of-a-proof had such big gaps that it wasn't a proof at all; others say that it was complete enough to drop the A and M and simply call the KAM theorem 'Kolmogorov's theorem').* [Dumas 2014, p 15]

tension of his writing, we read:

> I turn now to KAM theory. This theory is called KAM, or Kolmogorov–Arnold–Moser, and people say that there is even a KAM theorem. I was never able to understand what theorem is it. In 1954 Kolmogorov proved his marvellous theorem on the preservation of tori in Hamiltonian systems for the case when the Hamiltonian is almost integrable and all functions are analytic. What I contributed was the study of some degenerate cases-when one of the frequencies is zero in the nonperturbed system or when the vicinity of the fixed points or periodic points or tori is of a smaller dimension-and then applications to celestial mechanics. All these cases are separate theorems. My main contribution was the discovery (in 1964) of the universal mechanism of instability in systems with many degrees of freedom, close to integrable (later called "Arnol'd diffusion" by physicists).
>
> In 1962 Moser extended Kolmogorov's theorem to the case of smooth functions." In the first papers of Moser the number of derivatives was enormous. Now we know that in the simplest case of plane rotation you only need three derivatives, and this is just the limit, the critical number of derivatives. But in the beginning the number was 333. For Kolmogorov, this was like a complete change of philosophy (he told me) because he expected, and even claimed in his Amsterdam talk, that the result would be wrong even in C^∞ and that one would need analyticity or something close to it, something like the Gervais condition[157].
>
> Moser complained that a proof of the theorem in the case of analytic Hamiltonians was never published by Kolmogorov. I think that Kolmogorov was reluctant to write down the proof because he had other things to do in his remaining years of active work-which is a challenge when you are sixty. According to Moser, the first proof was published by Arnol'd. My opinion, however, is that Kolmogorov's theorem was proved by Kolmogorov.

[157]It refers to a condition introduced by the mathematician Maurice-Joseph Gevrey (1884-1957), which defines an intermediate space between the spaces of smooth (i.e. C^∞) functions and real-analytic functions.

[Arnold 2004, pp. 622-623]

Whatever may have been the reasons that led Kolmogorov not to publish a more rigorous result than the Theorem on the persistence of invariant tori, what is certain is that the theorems of the three mathematicians involved are actually different: Kolmogorov and Arnold adopt different demonstrative techniques and even in [Moser 1962] the hypotheses of the theorem stated are different from those of the original result of Kolmogorov.

Surely all three have contributed to the development of the KAM theory which today appears to be a point of reference among the demonstrative techniques used to research the convergence and stability of dynamical systems or partial differential systems, but perhaps it would be more correct in the future the theorems of the three mathematicians had different names and references.

Concluding Remarks

1. "Problème général de la Dynamique": Did Kolmogorov give a resolution?

More than sixty years before the formulation of the theorem on the persistence of invariant tori, Poincaré had defined the General problem of dynamics [Poincaré 1892-99, tome I] as the study of the quasi-periodic motions of a perturbed system written in Hamiltonian form (paragraph 1.3 of this work). He had realized that the problem, written in its general form

$$F = F_0 + \mu F_1 + \mu^2 F_2 + \ldots,$$

where F_0 denotes the Hamiltonian function of the unperturbed system and μ the perturbation parameter, did not concern only aspects of celestial mechanics - so dear to Poincaré - but all those problems of mechanics "close" to integrable problems.

His approach would have brought about developments in many areas of mechanics, managing to provide information on all those problems which can be connected, through the theory of perturbations, to the few known integrable systems.

In fact we have already observed that, while completely integrable systems are very few (paragraph 1.2 note 9), those close to integrable systems, in the sense of perturbation theory, are many. Therefore it is easy to understand the reasons that led Poincaré to define this problem as "general". His approach is very ambitious. To date, without suitable additional hypotheses on the unperturbed system or on the magnitude of the perturbation, a result is not known.

However, we can say with certainty that Kolmogorov has provided an important contribution to the opening of the problem and subsequent developments. Given the condition of non-degeneracy on the unperturbed Hamiltonian system F_0 - which corresponds to condition (10) present in paragraph 3.2, albeit with different notations -, given a very large set of frequencies in the set of real numbers (its complement is a set of zero Lebesgue measure) - all frequencies satisfying the Diophantine condition (9) in §3.2 -, and given a sufficiently small perturbation μ[158] , the most of the invariant tori present in the integrable unperturbed Hamiltonian system continue to survive. Each torus will deform only slightly with respect to the unperturbed torus having the same frequency and so, in the phase space of the perturbed system, there are equally invariant tori, over which the motions are nearly periodic. Furthermore, in the phase space of the perturbed system it turns out that the invariant tori are the majority, in the sense that the Lebesgue measure of the complement of their union is small and depends on the perturbation μ of the system.

2. A micro-community of mathematicians connected by common re-

[158]In the Kolmogorov version, the perturbation corresponds to the parameter θ

searchs

If the line connecting Kolmogorov's works with some results and conjectures left open by Poincaré appears quite clarified and known also in various secondary sources ([Diacu, Holmes 1996], [Barrow-Green 1997], [Dumas 2014]), during the research for this thesis, the awareness of a further invisible thread linking Kolmogorov to other almost contemporary scholars became increasingly vivid, in a sort of small scientific community, geographically separated, but strongly united in the intentions and research methods used.

The apparent hiatus of more than fifty years between Kolmogorov and Poincaré is filled by this network which, in the 1930s, connects works of classical mechanics with similar research approaches and methods, which goes even beyond the qualitative methods outlined in Poincaré: let's talk by von Neumann, Birkhoff, Koopman, Krylov and Bogolyubov up to, of course, Kolmogorov.

This mathematical micro-community, although divided geographically (three in the United States, two in Ukraine and one in Russia), has decided to pursue a new study approach to mechanical systems: through measure theory, already used by Poincaré for the qualitative study of mechanical systems, and the nascent theory of operators, increased by the developments introduced in mathematics by the Hilbert spaces, the study of a dynamical system was transferred from an analytical method to a geometric approach, up to the study of the properties of functions defined on a Hilbert space , which linked back to the original problem.

As we have seen in this work, this was the approach used by Koopman, Birkhoff and von Neumann, who saw the evolution of the ergodic theory and the formalizations of the homonymous theorems - by Birkhoff and von Neumann - driven by the theorem obtained by Koopman connecting Hamiltonian systems with unitary operators. And in this wake, von Neumann's interest in a further connection emerges: the spectral theory, which connects a dynamical system to its spectrum, in an attempt - again - to provide information on the studied system by deriving it from properties (continuous or discrete spectrum) concerned some operators connected to it (the eigenfunctions defined by the eigenvalues of the spectrum).

Just a few years later, in an attempt to bring about developments in non-linear mechanics, which was so popular in the Soviet Union in the 1930s because it was so present in the surrounding reality - unlike the recent theories of relativity or quantum mechanics - the only objective of science Soviet Union at the time of dialectical materialism, the Ukrainians Krylov and Bogolyubov take up the work of the three American mathematicians and extend them to more general systems. In fact, we have seen that in conservative Hamiltonian systems there is a natural measurement function (the conservation of volume, for example) whose existence is a necessary condition for the development of the techniques developed by von Neumann in the field of ergodic theory, in non- linear, which often represent a dissipative dynamic, this measure is not present spontaneously.

Thus, in an attempt to apply the same approaches of their overseas colleagues, Krylov and Bogolyubov construct a measure function in nonlinear systems, with the same properties as the one existing for Hamiltonian systems, starting from which all the approaches used for the latter, they can also be transferred to the study of non-conservative systems.

This new method, which transforms the study of classical mechanics into a study in the field of operator theory and spectral theory, was the key to Kolmogorov's work: his research program, which we have had the opportunity to deepen, perfectly reflects this methodology of researches and follows all the research carried out by the colleagues just mentioned. Furthermore, the study of the possible transitivity of the system (or ergodicity) and the study of its spectrum (continuous or discrete), allow us to answer questions about "general" or "typical" properties of almost integrable perturbed Hamiltonian systems, and not simply to make a contribution to a single specific case.

3. A new paradigm in classical mechanics

> [...] it seems to me that the subject I have chosen may also be of broader interest, as one of the examples of the appearance of new, unexpected and profound relationships between different branches of classical and modern mathematics.
>
> In his famous address at the Congress in 1900, D. Hilbert said that the unity of mathematics and the impossibility of its division into independent branches stem from the very nature of the science of mathematics. The most convincing evidence for the correctness of this idea is the appearance of new focal points at each stage in the development of mathematics, where, in the solution of quite specific problems, notions and methods of quite different mathematical disciplines become necessary and are involved in new interrelations. [Kolmogorov 1957, p 355].

Kolmogorov was well aware of the coexisting interrelationships between the various branches of mathematics and the natural development of the discipline, in which the introduction of new mathematical formalisms implies different approaches and new methods. But how to make all this tangible? The natural progression of the mathematical discipline often leads to new formalizations, which hide the laborious process of knowledge creation. It is with the elaboration of a story that, through an epistemological and historical reflection that accompanies the evolution of the research, we draw on the true meaning of the intellectual enterprise, bringing to light reformulations of previous ideas, which are not rejected, but assumed in visions new and which often make the original ideas of the mathematicians of the past unrecognizable.

The contributions of the 1930s and 1950s just mentioned in the previous point depend on all the previous contributions. The new mathematical formalisms introduced at the beginning of the twentieth century have brought with them a strong change in the approach to classical mechanics, but it is doubtful that they conceal all the contributions made by the developments of Lagrange, Hamilton, Jacobi, Poincaré etc.

To use a metaphor, it is like trying to conquer a fortress by attacking from different points of the enclosure and with ever more sophisticated weapons. In our case, the fortress is the study of classical mechanics and the different weapons, were the various evolutions from the direct approaches first of the eighteenth and nineteenth century mathematicians and physicists, to then move on to the qualitative approach from a more geometric and topological theory conceived by Poincaré at the end of the 19th century, to then again address qualitative issues, but with an approach in the field of functional analysis.

Each evolution contains the previous ones but, when consolidated and permeated in the studies of the mathematicians who follow them, they take the form of a new paradigm which in a certain sense replaces the previous one.

4. Future research

The main objective of this work seems to have been achieved. The cultural origins of Kolmogorov's works appear more defined and the connections between his direct testimonies and the theories developed by his predecessors who he identifies as sources of inspiration for him appear clear. It is equally clear that we are dealing with a very multifaceted problem that encompasses very broad mathematical developments, with connections and abrupt ruptures which were certainly influenced by the socio-political conditions of the West and of the Soviet Union in the early 1950s, but which probably they are also daughters of the natural course of mathematics as a constantly evolving discipline. We feel we have provided a first step, not only in the direction of the contribution that Dumas had hoped for in

The KAM story, but also from a broader point of view, as an addition to the history of mechanics, up to now not developed in the main textbooks. There are several points which deserve further study and which we aim to address in the immediate future.

1. Kolmogorov's scientific personality still appears to be incompletely defined. While this work has allowed us to dig deeper through the profile of this mathematician who was active in so many different disciplines of mathematics, the connections between scientific interests as well as his personal trajectory need further investigation.
   One could wonder if, for example, his knowledge in the field of measurement and probability theory, seen from a purer mathematical point of view, then contributed to the birth of the interest in mechanics which then led to the works analyzed in this thesis - as one might think by chronologically retracing the published works - or if, on the contrary, Kolmogorov's interests, which had existed for "decades" before the 1950s, towards aspects of mechanics and celestial mechanics, and in general of mathematical physics , have pushed Kolmogorov to deepen the theory of probability starting from his studies in the field of measure theory. This second hypothesis finds a possible ally in von Plato's 2005 contribution, within the work published by Grattan Guinness *Landmark Writings in Western Mathematics 1640-1940*.
   At the moment we are unable to provide an exhaustive answer, but it certainly remains an aspect worthy of study.

2. The connection attempt that Arnold makes between the invariant tori of Landau in the field of turbulence theory and those of Kolmogorov, discussed in §2.1, although not shared by Kolmogorov himself, seems to us not negligible and worthy of attention. We intend to study Landau's works in this area and try to trace the similarities that convinced Arnold so much, before being denied by his

master.

Appendix: Hamiltonian dynamical systems

The Solar system is paradigmatic of the general concept of "a handful" of material bodies in motion due to reciprocal interactions constituting a system with their positions evolving in time.

The word «system» indicates an assembly, a whole made up of several components, from the Greek verb $\sigma\upsilon\nu\iota\sigma\tau\eta\mu\iota$ meaning "to put together, to gather"[159]. In celestial mechanics, celestial bodies are considered as points – reducing the complexity of the actual situation in order to formulate a mathematical description.

The word «dynamical» makes reference to motion/change (from the Greek $\delta\upsilon\nu\alpha\mu\iota\varsigma$), indicating the vigor making those inanimate bodies appear as having life, that are embedded in the modern concepts of "force" and "energy" for this motion. In *Über die Erhaltung der Kraft* Hermann von Helmholtz (1821-1894) first name his famous 1847 "On the conservation of *force*", then changing it to "On the conservation of *energy*" (from the Greek work $\varepsilon\nu\varepsilon\varrho\gamma\varepsilon\iota\alpha$ «activity», derived from $\varepsilon\varrho\gamma o\nu$ «endeavour»]

The modern concept of dynamical systems is extended to any system evolving in time as for example population dynamics in the life sciences or economic evolution.

Differential equations were the mainly mathematical tool to describe the evolution in time: those are equations involving functions and its derivatives. A different approach, what we now call Lagrangian mechanics, is a mathematical formalism developed first by Euler and Lagrange, in which the equations of motion are described by means of the so-called variational Euler equations[160][Fraser 1994].

[159]see Vocabolario della lingua italiana, Rome, Istituto della Enciclopedia Italiana, ad vocem.

[160]The establishment of variational mechanics was largely the work of Euler and Joseph Louis Lagrange. Although Lagrange's Méchanique analytique (1788) is usually cited as the definitive presentation of the subject, the theory was developed earlier, by Euler between 1740 and 1750 and by Lagrange between 1760 and 1780. Euler provided some of

In the variational approach, the representation of the motion of a physical system depends on the positions it can assume and on the respective speeds, and the trajectory of the system will be given by the solution of the variational equations involving scalar quantities and no more, as happened in Newtonian mechanics, from the equations of the forces acting on the system itself, which are vector quantities.

Lagrangian formulation of the Newtonian law of motion[a]

Newton's equations of the Second Law of dynamics, for a material point $q = (q_1, q_2, q_3)$ of mass m, free to move in three-dimensional space $\mathbb{R}^3$ under the action of a force $F = (F_1, F_2, F_3)$ is

$$F_i = m\ddot{q}$$

where $i = 1, 2, 3$ and $\ddot{q} = \frac{d^2 q_i}{dt^2}$.

We consider here, for simplicity, a motion that occurs along only one of the three dimensions (one-dimensional motion). Suppose it occurs along the q_1 direction and, again for convenience, we denote by q, neglecting its subscript. So $q = (q, 0, 0)$ and $F = (F, 0, 0) = F(q)$.

The first step is to introduce a scalar quantity and describe the force in terms of that quantity. We will say that a force F is *conservative*, if there exists a scalar function $V = V(q)$, called *potential energy*, such that

$$F(q) = -\frac{dV}{dq}.$$

Then, if the force F is conservative, we can write Newton's equation in the form

the essential ideas, while the systematic mathematical elaboration of the theory was Lagrange's achivement. Variational mechanics had its origins in the rule for equilibrium know as the principle of virtual velocities. [Fraser 1994, p. 975]

$$m\ddot{q} = -\frac{dV}{dq}. \tag{13}$$

It is shown that this equation is equivalent to

$$\frac{d}{dt}\left(\frac{\partial \mathcal{L}}{\partial \dot{q}}\right) - \frac{\partial \mathcal{L}}{\partial q} = 0, \tag{14}$$

where $\mathcal{L}$ is a function $\mathcal{L} = \mathbb{R} \times \mathbb{R} \to \mathbb{R}$, which depends by $(q, \dot{q})$ and with the form

$$\mathcal{L} = T - V = \frac{1}{2}m\dot{q}^2 - V(q).^b$$

$\mathcal{L}$ is called the *Lagrangian* and corresponds to the difference between kinetic energy and potential energy.

The equivalence of the two formulas is easy to prove. In fact, it suffices to calculate the partial derivatives of $\mathcal{L}$ with respect to q and $\dot{q}$ and substitute the results obtained within the expression (14):

$$\frac{\partial \mathcal{L}}{\partial \dot{q}} = m\dot{q} \qquad \textit{and} \qquad \frac{\partial \mathcal{L}}{\partial q} = -\frac{dV}{dq}.$$

So, the equation (14) becomes:

$$\frac{d}{dt}\left(\frac{\partial \mathcal{L}}{\partial \dot{q}}\right) - \frac{\partial \mathcal{L}}{\partial q} = \frac{d}{dt}(m\dot{q}) + \left(\frac{dV}{dq}\right) = m\ddot{q} - F = 0,$$

which is the Newton's equation.

[a] Since the objective of this framed is intended to be a mathematical understanding of the derivation of the Lagrangian formulation from Newton's equations, we use here a modern language, more congenial to this purpose.

[b] T represents the kinetic energy.

This new approach was perfected by William Hamilton and Carl Gustav Jacobi. The philosophical foundation lied in Maupertuis minimal action principle. Dynamical systems described by this mathematical formalism are modernly called Hamiltonian systems, because a function name

Hamiltonian is central to the description of evolution of the system in time.

René Dugas in his *A history of mechanics* (1957) explains how Hamilton managed to reduce the number of differential equations to be studied for determining the evolution of a system:

> Hamilton recalled that the determination of the motion of a system of free particles, subject only to their mutual attraction or repulsion, depended on the integration of a system of $3(n-1)$ ordinary differential equations of the second order or, by a transformation due to Lagrange, on a system of $6(n-1)$ ordinary differential equations of the first order.
>
> Hamilton reduced this problem to the "search and differentiation of a single function" which satisfied two equations of the first order in the partial derivatives[161]. [Dugas 1957, p. 395]

First of all, we consider a system of n-coordinates $q_1, \dots q_n$ and we define

$$p_i = \frac{\partial \mathcal{L}}{\partial \dot{q}_i} = m\dot{q}_i$$

where $i = 1, \dots, n$. p_i are called the momentum.

Denoted by Ω it is a $2n$-dimensional differentiable manifold, on which are assigned the coordinates $(q,p) = (q_1, \dots, q_n, p_1, \dots, p_n)$, the evolution of the system is represented by the functions $q(t), p(t)$, where t, the time, varies in a real interval.

This evolution is determined by the *Hamiltonian function* $H = H(q,p)$, where $H : \Omega \to \mathbb{R}$, through the equations:

$$\frac{dq_i}{dt} = \frac{\partial H}{\partial p_i}, \qquad \frac{dp_i}{dt} = -\frac{\partial H}{\partial q_i} \tag{15}$$

where $i = 1, \dots, n$.

H is explicitly defined as

$$H = p\dot{q} - \mathcal{L}(q,\dot{q}) = m\dot{q}^2 - \frac{1}{2}m\dot{q}^2 + V(q) = \frac{1}{2}m\dot{q}^2 + V(q) = \frac{p^2}{2m} + V(q),$$

[161] Actually, they are $2n$-equations, as we will see below.

As the sum of the kinetic and potential energies of the system.

This Hamiltonian, or *canonical*, formalism must now be considered as the main tool in the study of the dynamics of conservative systems and in the development of perturbation theory.

In general, H could depend on time t and in this case $H = H(q, p, t)$ is a function from the Cartesian product $\Omega \times \mathbb{R}$ in $\mathbb{R}$. If this does not happen - that is, if $H = H(q, p)$ is as we defined it above - the system defined by equations (15) is called *autonomous*.

The Hamiltonian of an autonomous canonical system is a first integral, i.e. it is constant along the solutions of the system; the system is called *conservative* and H is called the energy of the system:

> This equation can be written
>
> $$H(q_1, q_2, \ldots, q_n, p_1, p_2, ..., p_n) = h$$
>
> and this is the integral of energy, which is possessed by the dynamical system when the function H does not involve the time explicitly. For natural problems, it follows [...] that H is the sum of the kinetic and potential energies of the system. [Whittaker 1917, p.265].

Bibliography

Introductory note

Three volumes of Selected works by Andrej N. Kolmogorov were published in 1985, 1986 and 1987 by Nauka (Moscow). Thus, the volumes production started and was almost finished at the time of his death in 1987. The editors were Vladimir Mikhailovich Tikhomirov (vol.1)and Albert Nikolaevich Shiryaev (voll. 2-3).

The full English translation of the three volumes was published in 1991, 1992 and 1993 by Kluwer Academic Publishers (Dordrecht) and it's currently on sale (Springer Science+Business Media) [Kolmogorov, 1991-1993]. The translators were three mathematicians: Vladimir M. Volosov (vol. 1), Anders Gunnar Lindquist (vol. 2) and Alexei Sossinski (vol. 3). On Volosov, who translated the first volume which includes contributions to mechanics, see note xx (in the Introduction).

A complete list of Kolmogorov's scientific output was published in 1989 in «The Annals of Probability» of the Institute of Mathematical Statistics (17(3), pp. 945-964, section Memorial Articles)

The bibliography is divided in two parts: Sources and Studies. Obituaries and notes by Kolmogorov's contemporaries are included in Sources.

In 1988 the journal «Uspekhi Matematicheskikh Nauk» devoted the last issue (no. 6, December) to the commemoration of Kolmogorov with 11 contributions, including the biographical essay [Tikhomirov 1988].

In 1990 a 70-page obituary-study edited by David Kendall was published by the London Mathematical Society including 11 contributions [Kendall 1990 ed.]; for KAM Theory see [Moffatt 1990]. The reference is included in the Sources.

In 2000 the American Mathematical Society published a volume entitled Kolmogorov in perspective [Andrews et al. 2000, 8 contributions]. The reference is included in the Sources.

A second collective book, L'héritage de Kolmogorov en mathématiques was published by Belin (Paris) in 2004 and translated into English in 2007 including fifteen contributions [Charpentier et al. 2004][162]; for classical mechanics see [Ghys 2004] and Hubard 2004]. The twin book L'heritage de Kolmogorov en physique by the same French publisher [Livi, Vulpiani eds 2003], translated into English in 2010, devoted part I to Chaos and dynamical systems ([Livi, Ruffo, Shepelyansky 2003], [Celletti, Froeschlé, Lega 2003]. The reference are included in the Studies.

In 2003 an international conference was held in Moscow, to commemorate the centenary of his death, on "Kolmogorov and contemporary mathematics" [Shirayev 2004]; the proceedings were published in Moscow (editors: Urij Sergeevich Osipov, Viktor Antonovic Sadovnicij e Albert Nicolaevich Shirayev).

Some Kolmogorov's letters addressed to Arnold have been published in [Arnold 2000]. In 2002 the Moscow publisher Fizmatlit has published Kolmogorov (3 voll., edited by A. N. Shirayev, with the contribution of Natal'ya Grigor'evna G.. Khimchenko (1937-2007)) including excerpts both from the correspondence Kolmogorov–Alexandrov (vol. 2), and from Kolmogorov private diaries (vol. 3) (see [Duzhin 2011] for the English translation of some 1943 annotations; see [Khimchenko 2001]).

The website https://www.kolmogorov.com includes a great variety of materials on Kolmogorov.

[162] A second book was published by Belin (Paris) on Kolmogorov's legacy in physics : Livi, Vulpiani

Sources[163]

[Alekseev 1967] ALEKSEEV Vladimir Mikhailovich (1967) *Quasirandom vibrations and the problem of capture in the bounded three-body problem*. (In Russian) Doklady Akademii Nauk SSSR, 177(4), pp 751-754.

[Alekseev 1969] ALEKSEEV Vladimir Mikhailovich (1969) *On the possibility of capture in the three-body problem with a negative value for the total energy constant*. (In Russian) Uspekhi Matematicheskikh Nauk 24(1), pp 185-186.

[Alekseev 1981] ALEKSEEV Vladimir Mikhailovich (1967) *Final motions in the three-body problem and symbolic dynamics*.Russian Mathematical Surveys, 36(4), pp 181–200.

[Alexandrov 1981] ALEXANDROV Pavel Sergeyevich (1981) *In Memory of Emmy Noether*. In [Dick 1981], pp 153–179.

[Andrews et al. 2000] ANDREWS George E., DAUBEN, Joseph, CHAIR Karen P., SELIGMAN George B., CHAIR David F., GRAY Jeremy J., PATTERSON S. J. (2000) *Kolmogorov in perspective*. Providence, R.I. American Mathematical Society, London Mathematical Society.

[Anosov 1967] ANOSOV Dmitri Victorovich (1967) *Geodesic flows on closed Riemannian manifolds of negative curvature*. Trudy Matematika Instituta Akademii Nauk, SSSR, 90

[Arnold 1963a] ARNOLD Vladimir Igorevič (1963) *Proof of a theorem of A. N. Kolmogorov on the preservation of conditionally-periodic motions under a small perturbation of the Hamiltonian*. Russian Math. Surveys, 18(1963) no. 5, pp. 9-36. (Translated from Russian from Uspekhi Mat. Nauk 18 (1963) no. 5, 13-40).

[163]Obituaries and notes by contemporaries are considered as sources.

[Arnold 1963b] ARNOLD Vladimir Igorevič (1963) *Small denominators and problems of stability of motion in classical and celestial mechanics* (in Russian) Uspekhi Matematicheskikh Nauk 18(6), pp. 91–192. [Eng tr. in Arnold 2009, pp 306–412].

[Arnold 1964] ARNOLD Vladimir Igorevič (1963) *On instability of dynamical systems with many degrees of freedom* (in Russian) Doklady Akademii Nauk SSSR, 156(1), 9-12.

[Arnold 1988] ARNOLD Vladimir Igorevič (1988) *A few words on Andrei Nikolaevich Kolmogoroc*. Russian Mathematical Surveys, 43(6), pp 43-44

[Arnold 1991] ARNOLD Vladimir Igorevič (1991) *Classical mechanics*. In [Tikhomirov 1991], pp. 504-518.

[Arnold 1992] ARNOLD Vladimir Igorevič (1992) *Metodi matematici della meccanica classica*. Editori Riuniti, Edizioni Mir. [Italian translation from the Russian 1979 edited by Roberto Bernieri and Roberto Tirozzi].

[Arnold 1997] ARNOLD Vladimir Igorevič (1997) *Vladimir Igorevič Arnold. Selected-60*. Phasis, Moscow, pp. 727-740. (in Russian). [Eng. tr: (2014) *From Superpositions to KAM Theory*, trasleted by SEVRYUK Mikhail Borisovich, (2014) Regular and Chaotic Dynamics 19(6) pp. 734-744].

[Arnold 2000] ARNOLD Vladimir Igorevič (2000) *On A. N. Kolmogorov*. [in AA.VV. *Kolmogorov in perspective*], pp. 89-108.

[Arnold 2004] ARNOLD Vladimir Igorevič (2004) *From Hilbert's Superposition Problem to Dynamical Systems*. The American Mathematical Monthly 111(7), pp. 608-624.

[Arnold 2007] ARNOLD Vladimir Igorevič (2007) *Yesterday and long ago*. Springer/Phasis.

[Arnold 2009] ARNOLD Vladimir Igorevič (aut.), GIVENTAL Alexander B. et al (eds.) (2009) *Collected Works: Representations of Functions, Celestial Mechanics and KAM Theory, 1957–1965*. Berlin Heidelberg, Springer-Verlag.

[Birkhoff 1912] BIRKHOFF George David, (1912) *Quelques théorèmes sur le mouvement des systèmes dynamiques*. Bulletin de la Société Mathématique de France, 40, pp. 305-323.

[Birkhoff 1913] BIRKHOFF George David, (1913) *Proof of Poincaré's Geometric Theorem*. Transactions of the American Mathematical Society, 14(1), pp. 14-22.

[Birkhoff 1915] BIRKHOFF George David, (1915) *The Restricted Problem of Three Bodies*. Rendiconti del Circolo Matematico di Palermo, 39, pp. 265-334.

[Birkhoff 1927] BIRKHOFF George David, (1927) *Dynamical Systems*. Providence RI, American Mathematical Society. Colloquium publications, volume IX.

[Birkhoff, Smith 1928] BIRKHOFF George David, SMITH Paul Althaus (1928) *Structure analysis of surface transformations*. Journal de Mathématiques Pures et Appliquées 9[e] série, 7, pp. 345-379.

[Birkhoff, Koopman 1932] BIRKHOFF George David, KOOPMAN Bernard Osgood (1932) *Recent Contributions to the Ergodic Theory*. Proceedings of the National Academy of Sciences of the United States of America Vol. 18(3), pp. 279-282.

[Bruns 1887] BRUNS Ernst Heinrich (1887) *Uber die integral des vielkorperproblems*. Acta Mathematica 11, pp 25–96.

[Charlier 1902-7] CHARLIER Carl Ludwig (1902-7) *Die Mechanik des Him-*

mels. Vorlesungen. Leipzig Verlag von Veit Comp. 2 Vols.

[Chazy 1921] CHAZY Jean-François (1921) *Sur les fonctions arbitraires figurant dans le ds^2 de la gravitation einsteinienne*. Paris, Comptes rendus de l'Académie des Sciences 173, pp. 905-907.

[Chazy 1922] CHAZY Jean-François (1922) *Sur l'allure du mouvement dans le problème des trois corps quand le temps croît indéfiniment*. Annales scientifiques de l'É.N.S. 3e série, tome 39, pp. 29-130.

[Chazy 1924] CHAZY Jean-François (1924) *Sur le champ de gravitation de deux masses fixes dans la théorie de la relativité*. Bulletin de la Société mathématique de France, pp. 17-38.

[Chazy 1928-30] CHAZY Jean-François (1928-30) *La Théorie de la Relativité et la Mécanique céleste*. Paris, Gauthier-Villars, 2 vols.

[Chazy 1929] CHAZY Jean-François (1929) *Sur l'allure finale du mouvement dans le problème des trois corps*. Journal de Mathématiques Pures et Appliquées 8, pp. 353-380.

[Conway, Mc Connell 1931] CONWAY Arthur William, MCCONNELL Albert Joseph (eds.) (1931) *The mathematical papers of Sir. William Rowan Hamilton. Vol I, Geometrical optics*. London, Cambridge University Press.

[Conway, Mc Connell 1940] CONWAY Arthur William, MCCONNELL Albert Joseph (eds.) (1940) *The mathematical papers of Sir. William Rowan Hamilton. Vol II, Dynamics*. London, Cambridge University Press.

[Darmois 1957] DARMOIS Georges (1957) *Notice sur la vie et les travaux de Jean Chazy (1882-1955)*. Paris, Palais de l'Institut.

[Delaunay 1860-67] DELAUNAY Charles Eugène (1860-67) *Théorie du mou-*

vement de la lune. Paris, Mallet-Bachelier. Imprimeur-Librair des Comptes Rendus Hebdomadaires Séances de l'Academie des Sciences. 2 vols.

[Dick 1981] DICK Auguste (1981) (ed.) *Emmy Noether 1882–1935*. Birkhäuser Boston.

[Fermi 1923a] FERMI Enrico (1923) *Dimostrazione che in generale un sistema meccanico è quasi ergodico*. Nuovo Cimento 25, pp 267–269.

[Fermi 1923b] FERMI Enrico (1923) *Generalizzazione del teorema di Poincaré sopra la non esistenza di integrali uniformi di un sistema di equazioni canoniche normali*. Nuovo Cimento 26, pp. 105–115.

[Ferrini 2010] FERRINI Maria Fernanda (ed.) (2010) *[Aristotele] Meccanica*. Milano, Bompiani Testi a fronte.

[Flammarion 1880] FLAMMARION Camille (1880) *Astronomie Populaire: Description générale du ciel*. Paris, C. Marpon et E. Flammarion Éditeurs.

[Flammarion 1908] FLAMMARION Camille (1908) *Initiation Astronomique*. Collection Des Initiations Scientifiques fondée par C.-A. Laisant, Paris, Librairie Hachette et $C^{I}e$.

[Gerretsen, De Groot 1957] GERRETSEN Johan C.H., DE GROOT Johannes (eds.) (1957) *Proceedings of the International Congress of Mathematicians 1954 (Amsterdam September 2 - 9)*. Groningen/Amsterdam, Erven P. Noordhoff N.V./North-Holland Publishing Co.

[Grant 1852] GRANT Robert (1852) *History of physical astronomy: from the earliest ages to the middle of the 19th century, comprehending a detailed account of the establishment of the theory of gravitation by Newton, and its development by his successors, with an exposition of the progress of research on all the other subjects of celestial physics*. London, Henry G. Bohn, York Street, Covent

Garden.

[Graves, Hille, Smith, Zariski 1955] GRAVES Lawrence M., HILLE Einar, SMITH Paul A., ZARISKI Oscar (eds.) (1955) *Proceedings of the International Congress of Mathematicians 1950 (Cambridge, Massachusetts, U.S.A. 1950*. Providence, RI, American Mathematical Society.

[Halmos, von Neumann 1942] HALMOS Paul Richard, VON NEUMANN John, (1942) *Operator Methods in Classical Mechanics, II*. Princeton, Annals of Mathematics, 43(2), pp. 332-350.

[Hamilton 1828] HAMILTON William Rowan (1828) *Theory of systems of rays*. Dublin, The Transactions of the Royal Irish Academy, 15, pp. 69-174. In [Conway, Mc Connell 1931, vol I pp 1-87.]

[Hamilton 1833a] HAMILTON William Rowan (1833) *General Method for Expressing the Paths of Light and Planets by the Coefficients of a Characteristic Function*. Dublin, Dublin University Review and Quarterly Magazine, 1, pp. 795–826.

[Hamilton 1833b] HAMILTON William Rowan (1833) *Problem of Three Bodies by my Characteristic Function*. Note-book 29. In [Conway, Mc Connell 1940, vol II pp 1-102].

[Hamilton 1834] HAMILTON William Rowan (1834) *On a General Method in Dynamics; by which the Study of the Motions of all free Systems of attracting or repelling Points is reduced to the Search and Differentiation of one central Relation, or Characteristic Function*. London, Philosophical Transactions of the Royal Society part II, pp. 247-308. In [Conway, Mc Connell 1940, vol II, pp 103-161].

[Hamilton 1835] HAMILTON William Rowan (1835) *Second essay on a General Method in Dynamics*. London, Philosophical Transactions of the Royal

Society, part I, pp. 95-144. In [Conway, Mc Connell 1940, vol II, pp 162-211].

[Husson 1932] HUSSON Éduard (1932) *Les trajectoires de la dynamique*. Paris, Gautier-Villars et C^{ie} Éditeurs, Philosophical Mémorial des sciences mathématiques, fascicule 55.

[Jacobi 1837] JACOBI Carl Gustav Jacob, (1837) *Zur Theorie der Variations-Rechnung und der Differential-Gleichungen*. Berlin, Crelle Journal für die reine und angewandte Mathematik, Bd. XVII, p. 68-82.

[Kendall (ed.) 1990] KENDALL David G. (1990) (ed.) *Obituary Andrej Nikolaevich Kolmogorov (1903-1987). A tribute to his memory*. Bulletin of the London Mathematical Society, 22, pp. 31-100 .

[Kendall 1990] KENDALL David G. (1990) *Kolmogorov, the man and his work*. In [Kendall (ed.) 1990], pp.31-47

[Khensin, Tabachnikov 2014] KHESIN Boris Aronovich, TABACHNIKOV Sergei L'vovich (2014) *Arnold: swimming against the tide*. Providence RI, American Mathematical Society.

[Khensin, Tabachnikov 2018] KHESIN Boris Aronovich, TABACHNIKOV Sergei L'vovich (2018) *Vladimir Igorevich Arnold. 12 June 1937—3 June 2010*. Biographical Memoirs of Fellows of the Royal Society, vol. 64, pp 7-26.

[Khintchine, 1933] KHINTCHINE Aleksandr Yakovlevich (1933) *Zur Birkhoff's Lbsung des Ergodenproblems*. Mathematische Annalen, 107, pp. 485-488

[Khintchine, 1938] KHINTCHINE Aleksandr Yakovlevich (1938) *The correlation theory of stationary stochastic processes*. (In Russian), Uspekhi Matematicheskikh Nauk, 5, pp. 42-51.

[Khovanskii, Varchenko 2014] KHOVANSKII Askold, VARCHENKO Alexander (2014) *Arnold's Seminar, First Years*. [in KHESIN Boris A., TABACHNIKOV Serge L. (2014)], pp. 157-163.

[Kolmogorov 1923] KOLMOGOROV Andrei Nikolaevich (1923) *Une série de Fourier-Lebesgue divergente presque partout*. Warsaw, Fundamenta Mathematicae 4, pp. 324-328.

[Kolmogorov 1933] KOLMOGOROV Andrei Nikolaevich (1933) *Grundbegriffe der Wahrscheinlichkeitsrechnung*. (In German). Springer, Berlin. [Russian ed., (1936) ONTI, Moscow.; English ed., (1950), Chelsea, New York; 2nd English ed., (1956), Chelsea, New York; 2nd Russian ed., (1974).]

[Kolmogorov 1936] KOLMOGOROV Andrei Nikolaevich (1936) *Sulla teoria di Volterra della lotta per l'esistenza*. Giornale dell'Istituto Italiano degli Attuari Attuari, vol 7 pp. 74-80.

[Kolmogorov 1937] KOLMOGOROV Andrei Nikolaevich (1937) *A simplified proof of the Birkhoff–Khinchin ergodic theorem*. Uspekhi Matematicheskikh Nauk, 7, pp. 52-56.

[Kolmogorov 1938] KOLMOGOROV Andrei Nikolaevich (1938) *On the solution of a biological problem*. (In Russian). Izv. NII Mat. Mekh. Tomsk. Univ, 2(1), pp. 7-12.

[Kolmogorov 1940] KOLMOGOROV Andrei Nikolaevich (1940) *On a new confimation of Mendel's Laws* (in Russian). Doklady Akademii Nauk SSSR, 27(1940), 38-42. [English translation in: *Shiryaev 1992*, pp. 222-227].

[Kolmogorov 1953] KOLMOGOROV Andrei Nikolaevich (1953) *On dynamical systems with an integral invariant on the torus* (in Russian). Doklady Akademii Nauk SSSR, 93(5), pp. 763-766. [English translation in [Kolmogorov 1991-93], pp. 344-348.

[Kolmogorov 1954] KOLMOGOROV Andrei Nikolaevich (1954) *On the preservation of conditionally periodic motions under small variations of the Hamilton function* (in Russian). Doklady Akademii Nauk SSSR, 98(4), pp. 527-530. [English translation in [Kolmogorov 1991-93] pp. 349-354].

[Kolmogorov 1957] KOLMOGOROV Andrei Nikolaevich (1957) *The general theory of dynamical systems and classical mechanics*, in [Tikhomirov 1991, vol.1, pp.355-374]. Original edition in Russian in GERRETSEN JOHAN C.H., DE GROOT JOHANNES(1957) *Proceedings of the International Congress of Mathematicians 1954 (Amsterdam September 2 - 9)*, North Holland, Amsterdam, vol. 1, pp. 315–333.

[Kolmogorov 1957-58] KOLMOGOROV Andrei Nikolaevich (1957) *Théorie générale des systèmes dynamiques de la mécanique classique*. Séminaire Janet. Mécanique analytique et mécanique céleste, tome 1 (1957-1958), exp. no 6, p. 1-20. (Translated from the Russian by Jean-Paul Benzécri)

[Kolmogorov 1963] KOLMOGOROV Andrei Nikolaevich (1957) *How I became a mathematician*. Ogonek No. 48, 12-13. (In Russian.)

[Kolmogorov 1972] KOLMOGOROV Andrei Nikolaevich (1957) *The general theory of dynamic systems and classical mechanics*. Translation from the Russian published in National Aeronautics and Space Admistration, Washington D.C. 20546, June 1972. [Kolmogorov 1986] KOLMOGOROV Andrei Nikolaevich (1986) *Memories of P. S. Aleksandrov* Russian Mathematical Surveys 41(6), pp. 225-246.

[Kolmogorov 1988] KOLMOGOROV Andrei Nikolaevich (1988) *Mathematics: science and profession* (Russian) Moscow, Kvan Library n.64, Nauka. *https://sheba.spb.ru/za/kvant64-matprof-1988.htm*

[Kolmogorov 1991/1985] KOLMOGOROV Andrei Nikolaevich (1991/1985)

On the papers on classical mechanics (in Russian). [In *Selected works of A.N. Kolmogorov*, vol. I edited by V. M. Tikhomirov Dordrecht, Kluwer Academic Publishers, 1991, pp. 503-504].

[Kolmogorov 1991-93] SELECTED WORKS, Dordrecht, Kluwer Academic Publishers, 3 voll. [English translation of the original edition Nauka, Moscow], ed. THIKOMIROV Vladimir M., SHIRYAEV Albert N.[164].

[Koopman 1931] KOOPMAN Bernard Osgood (1931) *Hamiltonian Systems and Transformation in Hilbert Space*. Proceedings of the National Academy of Sciences, 17(5), pp. 315-318.

[Koopman, von Neumann 1932] KOOPMAN Bernard Osgood, VON NEUMANN John (1932) *Dynamical Systems of Continuous Spectra*. Proceedings of the National Academy of Sciences, 18(3), pp. 255-263.

[Krylov, Bogolyoubov 1933] KRYLOV Nikolai Mitrofanovich, BOGOLYOUBOV Nikolaj Nikolaevich (1933) *Problèmes fondamenaux de la mécanique non linéaire*. Revue générale des sciences pures et appliquées vol. 44, pp. 9-19

[Krylov, Bogolyoubov 1937] KRYLOV Nikolai Mitrofanovich, BOGOLYOUBOV Nikolaj Nikolaevich (1937) *La Theorie Generale De La Mesure Dans Son Application A L'Etude Des Systemes Dynamiques De la Mecanique Non Lineaire*. Princeton University, Annals of Mathematics, Second Series, vol. 38(1), pp. 65-113.

[Krylov, Bogolyoubov 1950] KRYLOV Nikolai Mitrofanovich, BOGOLYOUBOV Nikolaj Nikolaevich (1937) *Introduction to non-linear mechanics: A free translation by Solomon Lefschetz of excerpts from two Russian monographs.* Princeton/London, Princeton University Press/Oxford University Press.

[164] See the introductory note of the bibliography

[Lagrange 1811] LAGRANGE Joseph-Louis (1811) *Analytical mechanics*. Second edition. [COHEN Robert S. (ed.) (1997), Translated from the Mecanique analytique, novelle edition of 1811. Springer+Science-Business Media Dordrecht].

[Lindstedt 1883] LINDSTEDT Anders (1883) *Beitrag zur Integration der Differentialgleichungen der Störungstheorie.* Mémoires de l'Académie impériale des sciences de St-Pétersbourg, vol. 31(4), pp. 1–20.

[Lysenko 1940] LYSENKO Trochym Denysovych (1940) *In Response to the Article by A. N. Kolmogorov* ibid., 28(9), pp. 832-3.

[McCrea 1957] MCCREA William Hunter (1957) *Edmund Taylor Whittaker*. London, London Mathematical Society, 32, pp. 234-256.

[Mittag-Leffler 1885] MITTAG-LEFFLER Gösta, (1885) *Mittheilung, einen von König Oscar II gestifteten mathematischen Preis betreffend*. Stockholm, Acta Mathematica, 7, pp. I-VI.

[Moser 1959] MOSER Jürgen Kurt, (1959), *Rewiew of "Théorie générale des systèmes dynamiques et mécanique classique. (French) 1957"*, American Mathematical Society, Mathematical Review MR0097598.

[Moser 1962] MOSER Jürgen Kurt, (1962), *On invariant curves of area-preserving mappings of an annulus*, Göttingen, Nachrichten von der Akademie der Wissenschaften II, pp. 1-20.

[Moser 1999] MOSER Jürgen Kurt, (1999) *Recollections*. In [Bierstone, Khesin, Khovanskii, Marsden 1999 pp. 19–21].

[Nemytskii 1957] NEMYTSKII Viktor Vladimirovich, (1957) *Seminar on Qualitative Theory of Differential Equations in Moscow University* (in Russian). YMH, tome 12(4), pp.235-239.

[Nemytskii, Stepanov 1961] NEMYTSKII Viktor Vladimirovich, STEPANOV Vyacheslav Vassilievich (1961) *Qualitative Theory of Differential Equations*. Princeton Mathematical Series 22.

[Painlevé 1896] PAINLEVÉ Paul Prudent (1896) *Sur les singularités des équations de la Dynamique*. Comptes Rendus de l'Académie des Sciences de Paris, 123, pp. 636-639.

[Poincaré 1881] POINCARÉ Jules Henri, (1881) *Mémoire sur les courbes définies par une équation différentielle (I)*. Paris, Journal de mathématiques pures et appliquées 3e série, tome 7, pp. 375-422.

[Poincaré 1886] POINCARÉ Jules Henri, (1886) *Sur une méthode de M. Lindstedt*. Paris, Bulletin astronomique, 3, pp. 57–61.

[Poincaré 1890] POINCARÉ Jules Henri, (1890) *Sur le probléme des trois corps et les équations de la dynamique*. Acta Mathematica 13, pp. 1–270.

[Poincaré 1891] POINCARÉ Jules Henri, (1891) *Sur le problème des trois corps*. Paris, Bulletin astronomique, Observatoire de Paris, 8, pp. 12-24.

[Poincaré 1892-99] POINCARÉ Jules Henri, (1892-99) *Les Méthodes nouvelles de la mécanique céleste*. Paris, Gauthier-Villars, 3 vols.

[Poincaré 1900] POINCARÉ Jules Henri, (1900) *La théorie de Lorentz et le principe de réaction*. Archives néerlandaises des sciences exactes et naturelles 5, pp. 252–278.

[Poincaré 1921] POINCARÉ Jules Henri, (1921) *Analyse des travaux scientifiques de Henri Poincaré faite par lui-même.* Acta mathematica, 38, pp. 1–135.

[Porthouse 1925] PORTHOUSE William, (1925) *Obituary: M. Camille Flammarion*. Nature, 115(2903), pp.951-952.

[Schmidt 1947] SCHMIDT Otto Yulyevich, (1947) *On possible capture in celestial mechanics* (in Russian). Doklady Akademii Nauk SSSR 58(2), pp. 213-216.

[Schmidt 1958] SCHMIDT Otto Yulyevich, (1958) *A Theory Of Earth's Origin*, (trasleted from Russian Third Edition by George H. Hanna). Foreign Languages Publishing House.

[Shiryaev 1988] SHIRYAEV Albert Nikolayevich (1988) *On the scientific heritage of A.N. Kolmogorov*. Russian Mathematical Surveys, 43(6), pp. 211-212.

[Shiryaev 1992] SHIRYAEV Albert Nikolayevich (1992) (ed.) *Selected Works of A. N. Kolmogorov, vol II*. Dordrecht, Springer Science+Business Media.

[Shiryaev 2004] SHIRYAEV Albert Nikolayevich (2004) *International Conference "Kolmogorov and contemporary mathematics"*. Russian Mathematical Surveys, 59(1), pp. 193-194.

[Siegel 1941] SIEGEL Carl Ludwig, (1941) *On the Integrals of Canonical*. Annals of Mathematics 42(3), pp. 806-822

[Siegel 1942] SIEGEL Carl Ludwig, (1942) *Iteration of analytic functions*. Annals of Mathematics 43(2), pp. 607–612.

[Siegel 1979] SIEGEL Carl Ludwig, (1979) *On the history of the frankfurt mathematics seminar*. The Mathematical Intelligencer 1(4). [Eng. tr. by LENZEN Kevin M.]

[Siegel, Moser 1971] SIEGEL Carl Ludwig, MOSER Jürgen Kurt (1971) *Lectures on celestial mechanics*. Berlin/Heidelberg/New York, Springer-Verlag.

[Sinai 1959] SINAI Yakov Grigorevich (1959) *On the concept of entropy for* *dynamic system*. (In Russian). Doklady Akademii Nauk SSSR, 124, pp. 768-771.

[Sinai 1989] SINAI Yakov Grigorevich (1989) *Kolmogorov's Work on Ergodic Theory*. The Annals of Probability, 17(3), pp. 833–839.

[Sitnikov 1953] SITNIKOV Kirill Aleksandrovich, (1953) *on possible capture in the three body problem* (in Russian). Matematicheskii Sbornik, 32(3), pp. 693-705.

[Sundman 1907] SUNDMAN Karl Frithiof, (1907) *Recherches sur le problème des trois corps*. Acta Societatis Scientiarum Fennicae, 34(6), pp 1-43.

[Sundman 1910] SUNDMAN Karl Frithiof, (1910) *Sur les singularités réelles dans le problème des trois corps*. Stockholm, Actes du Congrès des Mathématiques Scandinaves (Stockholm), pp. 62-75.

[Sundman 1913] SUNDMAN Karl Frithiof, (1913) *Mémoire sur le problème des trois corps*. Acta Mathematica, 36, pp. 105-179.

[Tikhomirov 1988] TIKHOMIROV Vladimir Mikhailovich (1988) *The life and work of Andrei Nikolaevich Kolmogorov*. Russian Mathematical Surveys, 43(6), 1-39

[Tikhomirov 1991] TIKHOMIROV Vladimir Mikhailovich (1991) *Selected works of A.N. Kolmogorov, vol. I*. Dordrecht, Kluwer Academic Publishers.

[von Neumann 1929a] VON NEUMANN John, (1929) *Allgemeine Eigenwerttheorie Hermitescher Funktionaloperatoren*. Mathematische Annalen, 102 , pp. 49-131.

[von Neumann 1929b] VON NEUMANN John, (1929) *Zur Algebra der Funktionaloperatoren und Theorie der normalen Operatoren*. Mathematische Annalen, 102 , pp. 370-472.

[von Neumann 1932a] VON NEUMANN John, (1932) *Proof of the Quasi-Ergodic Hypothesis*. Proceedings of the National Academy of Sciences, 18(1), pp 70-82.

[von Neumann 1932b] VON NEUMANN John, (1932) *Zur Operatorenmethode In Der Klassischen Mechanik*. Princeton, Annals of Mathematics, Second Series 33(3), pp. 587-642.

[von Neumann 1932c] VON NEUMANN John, (1932) *Mathematische Grundlagen der Quantenmechanik*. Berlin, Springer. [Eng. Tr. by BEYER Robert T, (1955), Princeton, Princeton University Press.]

[von Neumann 1954] VON NEUMANN John (1954) *Unsolved problems in mathematics*. in [Rédei, Stö1tzner 2001], pp. 231-248.

[Whittaker 1899] WHITTAKER Edmund Taylor, (1899) *Report on the progress of the solution of the problem of three bodies*. British Association Report, pp. 121–159.

[Whittaker 1917] WHITTAKER Edmund Taylor, (1917) *A treatise on the analytical dynamics of particles and rigid bodies; with an introduction to the problem of three bodies*. Cambridge, University press, Second Edition.

[Wicksell 1935] WICKSELL Sven (1935) *Carl Wilhelm Ludvig Charlier*. Scandinavian Actuarial Journal, 1935(1-2), pp. 130-133.

[Youschkevitch 1983] YOUSCHKEVITCH Adolph Pavlovich (1983) *A. N. Kolmogorov: Historian and philosopher of mathematics on the occasion of his 80th birthday*. Historia Mathematica 10(4), pp. 383-395.

Studies

[Abramov 2010] ABRAMOV Alexander, (2010) *Toward a History of Mathematics Education Reform in Soviet Schools (1960s–1980s)* in [Karp, Vogeli (2010)], pp. 87-140.

[Arnold, Avez 1968] ARNOLD Vladimir Igorevič, AVEZ Andre (1968) *Ergodic Problems of Classical Mechanics*. Library of Congress Catalog Card Number 68-19936, Manufactured in the United States of America.

[Aubin, 2005] AUBIN David (2005) *Chapter 68 - George David Birkhoff, Dynamical systems*. In [Grattan-Guinnes 2005], pp. 871-881.

[Aubin, Dahan-Dalmedico 2002] AUBIN David, Dahan-Dalmedico Amy (2002) *Writing the History of Dynamical Systems and Chaos: Longue Durée and Revolution, Disciplines and Cultures*. Historia Mathematica 29, pp. 273–339.

[Barrow-Green 1997] BARROW-GREEN June (1997) *Poincaré and the Three Body Problem*. Providence RI, American Mathematical Society/History of Mathematics 11.

[Barrow-Green 2003] BARROW-GREEN June (2003) *Il problema dei tre corpi e la stabilità del Sistema solare*. in S. Petruccioli (dir.) Storia della Scienza, Roma, Istituto della Enciclopedia Italiana, vol. L'Ottocento, *https://www.treccani.it/enciclopedia/l-ottocentoastronomia-il-problema-dei-tre-corpi-e-la-stabilita-del-sistema-solare_*

[Benci, Cerrai et al. 2003] BENCI Vieri, CERRAI Paola, FREGUGLIA Paolo, ISRAEL Giorgio, PELLEGRINI Claudio (eds.) (2003), *Determinism, Holism, and Complexity*. New York, Springer Science+Business Media.

[Benettin, et al. 1985] BENETTIN Giancarlo, GALGANI Luigi, GIORGILLI Antonio (1985), *Poincaré's non-existence theorem and classical perturbation the-*

ory for nearly integrable Hamiltonian systems. Advances in Nonlinear Dynamics and Stochastic Processes (R. Livi and A. Politi, Eds.), World Scientific, Singapore, pp. 1–22.

[Bensaude-Vincent 1989] BENSAUDE-VINCENT Bernadette, (1989), *Camille Flammarion, prestige de la science populaire*. Romantisme 65, pp. 93-104.

[Bierstone, Khesin, Khovanskii, Marsden 1999] BIERSTONE Edward, KHESIN Boris, KHOVANSKII Askold, MARSDEN Jerrold E. (eds.) *The Arnoldfest: Proceedings of a Conference in Honour of V. I. Arnold for his Sixtieth Birthday*. Fields Institute Communications, 24, American Mathematical Society.

[Blay, Nicolaïdis 2001] BLAY Michel, NICOLAÏDIS Efthymios (2001) *L'Europe des sciences: constitution d'un espace scientifique*. Paris, Éditions du Seuil.

[Bohlin 1897] BOHLIN Karl (1897) *Hugo Gyldén*. Acta Mathematica, 20(1), pp. 397-404.

[Borel 1943] BOREL Émilie (1943) *L'évolution de la mécanique,* . Paris, Flammarion.

[Celletti, Froeschlé, Lega 2003] CELLETTI Alessandra, FROESCHLÉ Claude, LEGA Elena (2003) *Des mouvements reguliers aux mouvements chaotiques a travers le travail de Kolmogorov*. In [Livi, Vulpiani 2003], pp. 46-71

[Celletti, Chierchia 2005] CELLETTI Alessandra, CHIERCHIA Luigi (2005) *KAM Stability for a three-body problem of the Solar system*. ZAMP, 57(1), pp. 33-41.

[Celletti, Perozzi 2007] CELLETTI Alessandra, PEROZZI Ettore (2007) *Celestial Mechanics: The Waltz of the Planets*. Springer-Praxis.

[Charpentier, Lesne, Nikolski 2004] CHARPENTIER Eric, LESNE Annick, NIKOLSKI Nikolai Kapitonovich (eds.) (2004) *L'héritage de Kolmogorov en mathématiques*. Paris, Éditions Belin [eng. tr. Berlin Heidelberg, Springer Verlag 2007].

[Chern, Hirzebruch 2001] CHERN Shiing Shen, HIRZEBRUCH Friedrich (2001) (eds.) *Wolf Prize in Mathematics, vol. 2*. Singapore, World Scientific.

[Chierchia, Falcolini 1994] CHIERCHIA Luigi, FALCOLINI Corrado (1994) *A direct proof of a theorem by Kolmogorov in hamiltonian systems*. Pisa, Annali della Scuola Normale Superiore di Pisa, Classe di Scienze 4^e série, tome 21(4), pp. 541-593.

[Chierchia 2008] CHIERCHIA Luigi (2008) *A.N.Kolmogorov's 1954 paper on nearly-integrable Hamiltonian systems*. Regular and Chaotic Dynamics 13(2), pp. 130-139.

[Chierchia 2012] CHIERCHIA Luigi (2012) *Kolmogorov-Arnold-Moser (KAM) theory* in MEYERS Robert A. (ed.) *Mathematics of complexity and dynamical systems*, New York, Springer, ad vocem.

[Coutinho 2014] COUTINHO Severino Collier, *Whittaker's analytical dynamics: a biography*. Archive for History of Exact Sciences 68(3), pp. 355-407.

[Dahan-Dalmedico, Chabert, Chemla 1992] DAHAN DALMEDICO Amy, CHABERT Jean-Luc, CHEMLA Karine (1992) *Chaos et déterminisme*. Paris, Éditions du Seuil.

[Dell'aglio 2003] DELL'AGLIO Luca, *Probabilistic Aspects in George D. Birkhoff's Work* (2003), in [Benci, Cerrai et.al. 2003] pp 327–337 [Demidov 2004] DEMIDOV Sergej Sergeevich (2004) *La seconda rivoluzione scientifica: matematica e logica. La scuola matematica di Mosca*, in *Storia della Scienza*, Roma, Istituto della Enciclopedia Italiana *https://www.treccani.it/enciclopedia/la-seconda-rivoluzione-scientifica-matematica-e-logica-la-scuola-matematica-di-mosca_%28Storia-della-Scienza%29/*

[Demidov 2009] DEMIDOV Sergej Sergeevich (2009) *Les relations mathématiques Franco-Russes entre les deux guerres mondiales*. Revue d'histoire des sciences, 2009/1 Tome 62, pp. 119-142.

[Demidov, Lëvshin 2016] DEMIDOV Sergej Sergeevich, LËVSHIN Boris V. (eds.) (2016)*The Case of Academician Nikolai Nikolaevich Luzin*. Providence, Rhode Island, American Mathematical Society.

[Diacu, Holmes 1996] DIACU Florin HOLMES Philip (1996) *Celestial encounters: The origin of chaos and stability*. Princeton, New York, Pricenton University Press.

[Diner 1992] DINER Simon (1992) *Les voies du chaos déterministe dans l'école russe*. In [Dahan Dalmedico, Chabert, Chemla 1992], pp 331-370.

[Dugas 1957] DUGAS René (1957) *A history of mechanics*. London, Routledge & Kegan Paul LTD. [Trasleted into English by J.R.Maddox of DUGAS René (1950) *Histoire de la mécanique*, Switzerland, Neuchâtel, Éditions du Griffon]

[Duhem 1905] DUHEM Pierre Maurice Marie (1905) *L'évolution de la mécanique,* . Paris, Librairie Scientifique A. Hermann.

[Dumas 2014] DUMAS H. Scott (2014) *The KAM story a friendly introduction to the content, history, and significance of classical Kolmogorov-Arnold-Moser theory*. Singapore, World Scientific Publishing.

[Duzhin 2011] DUZHIN Fedor (2011) *Excerpts from Kolmogorov's diary*. Asia Pacific Mathematics Newsletter, 1(2), pp. 22-24.

[Elina 2002] ELINA Olga (2002) *Planting Seeds for the Revolution: The Rise of Russian Agricultural Science, 1860–1920*. Science in Context, 15(2), 209-237.

[Eremeeva 1995] EREMEEVA A. I. (1995) *Political repression and personality: the hostory of political repression against soviet astronomers*. Journal for the History of Astronomy 26(4), pp 297-324.

[Faddeev 1995] FADDEEV Ljudvig Dmitrievič (1995) *40 years in mathematical physics*. Singapore, World scientific series "20th century mathematics", vol. 2.

[Fraser 1994] FRASER Craig G. (1994) *Classical mechanics*. In [Grattan Guinness 1994], vol 2, pp. 971-986.

[Gentile 2021] GENTILE Guido (2021) *Introduzione ai sistemi dinamici vol.1 - Equazioni differenziali ordinarie, analisi qualitativa e alcune applicazioni*. Milano, Springer.

[Gentile 2022] GENTILE Guido (2022) *Introduzione ai sistemi dinamici vol.2 - Formalismo lagrangiano e hamiltoniano*. Milano, Springer.

[Ghys 2004] GHYS Étienne (2004) *Resonances and small divisors*. In [Charpentier, Lesne, Nikolski 2004], pp. 187-214.

[Giorgilli, Locatelli 1997] GIORGILLI Antonio, LOCATELLO Ugo (1997 *Kolmogorov theorem and classical perturbation theory*. ZAMP 48, pp. 220–261.

[Glimm, Impagliazzo, Singer 1990] GLIMM James, IMPAGLIAZZO John, SINGER Isadore (eds.) (1990) *The Legacy of John von Neumann*. Proceedings of Symposia in Pure Mathematics, Providence RI, American Mathematical Society, vol.50.

[Goldstein 1980] GOLDSTEIN Hebert (1980) *Classical mechanics. Second edi-*

tion. Addison-Wesley publishing company. [First Edition was published in 1950].

[Gordin, Hall, Kojevnikov 2008] GORDIN Michael, HALL Karl, KOJEVNIKOV Alexei (eds.) (2008) *Intelligentsia Science: The Russian Century, 1860-1960*. Chicago, Chicago University Press, Osiris, Vol.23(1).

[Graham 1993] GRAHAM Loren R. (1998) *Science in Russia and the Soviet Union: A Short History*. New York, Cambridge University Press.

[Graham 2016] GRAHAM Loren R. (2016) *Lysenko's ghost. Epigenetics and Russia*. Cambridge/London, Harvard University Press.

[Graham, Kantor 2009] GRAHAM Loren R., KANTOR Jean - Michel (2009) *Naming Infinity: A True Story of Religious Mysticism and Mathematical Creativity*. Cambridge/London, Harvard University Press.

[Grattan-Guinnes 1994] GRATTAN-GUINNESS Ivor (ed.) (1994) *Companion encyclopedia of the hystory and philosophy of the mathematical sciences*. London/New York, Routledge, 2 vols.

[Grattan-Guinnes 1994] GRATTAN-GUINNESS Ivor (1994) *Higher education and institutions. Russia and the Soviet Unione*. In [Grattan Guinness 1994], vol. 2, pp. 1477-1483.,

[Grattan-Guinnes 2005] GRATTAN-GUINNESS Ivor (ed.) (2005) *Landmark Writings in Western Mathematics 1640-1940*. Amsterdam, Elsevier Science.

[Grenier, Grignon, Menger 2001] GRENIER Jean-Yves, GRIGNON Claude, MENGER Pierre-Michel (eds.) (2001) *Le modèle et le récit*. Paris, Éditions de la Maison des sciences de l'homme.

[Halmos 1958] HALMOS Paul Richard (1958) *Von Neumann on measure and* *ergodic theory*. Bulletin of the American Mathematical Society 64(3), pp. 86-94.

[Hagihara 1972] HAGIHARA Yusuke (1972) *Recent advances of celestial mechanics in the soviet union*. Vistas in Astronomy 13, pp. 15-48

[Helliwell, Sahakian 2020] HELLIWELL Thomas M., SAHAKIAN Vatche V. (2020) *Modern Classical Mechanics*. Cambridge University Press.

[Hockey, Trimble, Williams, Bracher, Jarrell, Marché, Ragep 2007] HOCKEY Thomas, TRIMBLE Virginia, WILLIAMS Thomas R., BRACHER Katherine, JARRELL Richard A., MARCHÉ Jordan D. II, RAGEP F. Jamil (2007) *The Biographical Encyclopedia of Astronomers*. Springer Science+Business Media, LLC.

[Holmes 2007] HOLMES Philip (2007) *A short history of dynamical systems theory: 1885-2007*. Princeton, New York, Princeton University Press.

[Hubbard, Ilyashenko 2003] HUBBARD John ILYASHENKO Yulij (2003) *A proof of Kolmogorov's theorem. Dedicated to Prof. Vishik on the occasion of his 80th birthday*. Springfield (USA), Discrete and continuous dynamical systems 4(4), pp. 1–20.

[Hubbard 2004] HUBBARD John H. (2004) *The KAM Theorem*. In [Charpentier, Lesne, Nikolski 2004] pp. 215-238.

[Israel 1993] ISRAEL Giorgio (1993) *The emergence of biomathematics and the case of population dynamics: a revival of mechanical reductionism and Darwinism*. Science in context, 6, 469-509.

[Israel 2004] ISRAEL Giorgio (2004) *Technological innovation and new mathematics: van der Pol and the birth of non-linear dynamics*. In [Lucertini, Millán Gasca, Nicolò 2004], pp-129-158.

[Israel 2015] ISRAEL Giorgio (2015) *Meccanicismo. Trinfi e miserie della visione meccanica del monto*. Bologna. Zanichelli editori S.p.a.

[Israel, Millán Gasca 2008] ISRAEL Giorgio, MILLÁN GASCA Ana (2008) *Il mondo come gioco matematico. La vita e le idee di John von Neumann*. Torino, Bollati Boringhieri.

[Israel, Millán Gasca 2002], ISRAEL Giorgio, MILLÁN GASCA Ana (2002) *The Biology of numbers. The correspondence of Vito Volterra on mathematical biology* Basel, Birkhäuser.

[Joravsky 1970] JORAVSKY David (1970) *The Lysenko affair*. Chicago/London, University of Chicago Press.

[Karp 2012] KARP Alexander (2012) *Soviet mathematics education between 1918 and 1931: a time of radical reforms*. Karlsruhe, ZDM Mathematics Education 44, pp. 551–561.

[Karp 2014] KARP Alexander, (2014) *Mathematics Education in Russia* in [Karp, Schubring 2014] pp. 303-322.

[Karp, Schubring 2014] KARP Alexander, SCHUBRING Gert (edited by) (2014) *Handbook on the History of Mathematics Education*. New York, Springer.

[Karp, Vogeli 2010] KARP Alexander,VOGELI Bruce (eds.) (2010) *Russian mathematics education: history and world significance*. Series of mathematics educations, vol. 4 London/New Jersey/Singapore, World Scientific Publishing Co Pte Ltd.

[Kendall 1991] KENDALL David G. (1991) *Kolmogorov as I Remember Him*. Statistical Science, 6(3) pp. 303-312.

[Khimchenko 2001] KHIMCHENKO Natal'ya Grigor'evna (2001) *From the* *"last interview" with A.N. Kolmogorov*. The Mathematical Intelligencer 23, pp. 30–38.

[Kojevnikov 2002] KOJEVNIKOV Alexei (2002) *Introduction: A New History of Russian Science*. Cambridge University Press. Science in Context 15(2), pp.177–182.

[Kojevnikov 2008] KOJEVNIKOV Alexei (2008) *The Phenomenon of Soviet Science*. In [Gordin, Hall, Kojevnikov 2008], pp. 115-135.

[Kosygin, Sinai 2004] KOSYGIN Denis V., SINAI Yakov G. (2004) *From Kolmogorov's work on entropy of dynamical systems to non-uniformly hyperbolic dynamics*. In [Charpentier, Lesne, Nikolski 2004] pp. 239-252.

[Kutateladze 2012] KUTATELADZE Semën Samsonovich (2013) *The Tragedy of Mathematics in Russia*. Siberian Electronic Mathematical Reports, Vol. 9, pp. A85–A100.

[Kutateladze 2013] KUTATELADZE Semën Samsonovich (2013) *An epilog to the Luzin case*. Siberian Electronic Mathematical Reports, Vol. 10, pp. A1–A6.

[de La Cotardière, Fuentes 1994] DE LA COTARDIÈRE Philippe, FUENTES Patrick (1994) *Flammarion*. Paris, Ed. Flammarion.

[Levin 1990] LEVIN Aleksey E. (1990) *Anatomy of a Public Campaign: "Academician Luzin's Case" in Soviet Political History*. Slavic Review Vol. 49(1), pp. 90-108

[Livi, Vulpiani 2003] LIVI Roberto, VULPIANI Angelo (2003) *L'héritage de Kolmogorov en physique*. Paris, Éditions Belin.

[Lorentz 1990] LORENTZ George Gunter (2002) *Mathematics and Politics in* *the Soviet Union from 1928 to 1953*.Journal of Approximation Theory 116, pp. 169–223.

[Lucertini, Millán Gasca, Nicolò 2004] LUCERTINI Marco, MILLÁN GASCA Ana, NICOLÒ Fernando (2004) *Technological concepts and mathematical models in the evolution of engineering systems, controlling-managing-organizing*. Basel-Boston-Berlin, Birkhäuser Verlag.

[Lui 1997] LUI Simon H. (1997) *An interview with Vladimir Arnol'd*. Notices American Mathematical Society, vol. 44(4), pp.432-438.

[Mackey 1990] MACKEY George W. (1990) *Von Neumann and the early days of ergodic theory*. In [Glimm, Impagliazzo, Singer 1990], pp.25-38.

[Marcolongo 1915] MARCOLONGO Roberto (1915) *Il Problema dei Tre Corpi da Newton (1686) al Nostri Giorni*. Il Nuovo Cimento, vol. 9, pp. 89–130.

[Mazliak 2003] MAZLIAK Laurent (2003) *TAndrei Nikolaevitch Kolmogorov (1903–1987). Un aperçu de l'homme et de l'œuvre probabiliste*. Paris, Cahiers du CAMS-EHESS.

[Mazliak 2018] MAZLIAK Laurent (2018) *The beginnings of the Soviet encyclopedia. The utopia and misery of mathematics in the political turmoil of the 1920s*. Centaurus 60(1-2), pp 25-51.

[Markkanen 2007] MARKKANEN Tapio (2007) *Gyldén, Johan August Hugo*. In [Hockey et al. 2007], pp. 452-453.

[McCutcheon 1991] MCCUTCHEON Robert A. (1991) *The 1936-1937 Purge of Soviet Astronomers*. Cambridge University Press, Spring 50(1), pp. 100-117.

[Myshkis, Oleinik 1990] MYSHKIS Anatolii Dmitrievich, OLEINIK Olga Arsen'evna (1990) *Vyacheslav Vasil'evich Stepanov (on the centenary of his birth)*. Russian Mathematical Surveys, 1990, Volume 45(6), Pages 179–182.

[Moesgaard 1994] MOESGAARD Kristian Peder, (1994) *Astronomy*. In [Grattan Guinness 1994], vol 1, pp. 240-250.

[Moffatt 1990] MOFFATT Keith H., (1990) *KAM-theory*. In [Kendall (ed.) 1990], pp. 71-73.

[Molland 1994] MOLLAND Andrew George, (1994) *Medieval and Renaissance mechanics*. In [Grattan Guinness 1994], vol 1, pp. 229-239.

[Moore 2015] MOORE Calvin C., (2015) *Ergodic theorem, ergodic theory, and statistical mechanics*. Proceedings of the National Academy of Sciences, 112(7), pp. 1907-1911.

[Morse 1946] MORSE Marston (1946) *George David Birkhoff and his mathematical work*. Bulletin of the American Mathematical Society, 52(5.P1), pp. 357-391.

[Morse 1982] MORSE Philip M., (1982) *In Memoriam: Bernard Osgood Koopman, 1900-1981*. Operations Research, 30(3) pp. viii+417-427.

[Nicolaïdis 1984] NICOLAÏDIS Efthymios (1984) *Le developpement de l'astronomie en U.R.S.S. 1917-1935*. Paris, Observatoire de Paris.

[Nicolaïdis 1990] NICOLAÏDIS Efthymios (1990) *Astronomy and Politics in Russia in the Early Stalinist Period - 1928-1932*. Journal for the History of Astronomy 21, pp 345-351.

[Nicolaïdis 2011] NICOLAÏDIS Efthymios (2011) *Science and Eastern Orthodoxy: From the Greek Fathers to the Age of Globalization*. Johns Hopkins University Press

[Novikov 1988] NOVIKOV SERGEJ PETROVIČ (1988) *Remembrances of A. N. Kolmogorov*. Uspekhi Matematicheskikh Nauk 43(6), pp 35-36.

[Oliveira 2017] OLIVEIRA Agamenon Rodrigues Eufrásio, (2017) *History of Krylov-Bogoliubov-Mitropolsky Methods of Nonlinear Oscillations*. Advances in Historical Studies, 6, pp 40-55.

[Ptushenko 2021] PTUSHENKO Vasily V. (2021) *The pushback against state interference in science: how Lysenkoism tried to suppress Genetics and how it was eventually defeated*. Genetics 219(4).

[Rabkin, Rajagopalan 2001] RABKIN Yakov M. RAJAGOPALAN Sumitra (2021) *Les sciences and Russie: entre ciel et terre*. In [Blay, Nicolaïdis 2001], pp. 217-261.

[Rédei, Stö1tzner 2001] RÉDEI Miklós, STÖLTZNER Michael (eds.) (2001) *John von Neumann and the foundations of quantum physics*. Netherlands, Vienna Circle Institute Yearbook [2000] 8, Springer Science+Business Media B.V.

[Shiryaev 1989] SHIRYAEV Albert Nikolayevich (1989) *Kolmogorov: Life and Creative Activities*. The Annals of probability 17(3) pp. 866-944.

[Shiryaev 2000] SHIRYAEV Albert Nikolayevich (2000) *Andrei Nikolaevich Kolmogorov (April 25, 1903 to October 20, 1987) A Biographical Sketch of His Life and Creative Paths*. in [AA.VV. 2000], pp. 1-88.

[Sinai 1964] SINAI Yakov Grigorevich (1964) *Weak isomorphism of measure-preserving transformations*. (In Russian). Matematicheskii Sbornik, 64, pp.23-42.

[Sinai 1966] SINAI Yakov Grigorevich (1966) *Chemical dynamical systems with countable Lebesgue spectrum*. (In Russian). Izvestiya Akademii Nauk

SSSR, 30(1), pp 15-68.

[Sinkevich 2015] SINKEVICH Galina (2015) *The fate of Russian translations of Cantor*. Notices of the ICCM, 3(2), pp.74-83.

[Stark, Seneta 2011] STARK Alan, SENETA Eugene (2011) *A.N. Kolmogorov's defence of Mendelism*. Brazil, Genetics and Molecular Biology vol. 34(2), 177-186.

[Tikhomirov 2001] TIKHOMIROV Vladimir Mikhailovich (2001) *Andrej Nikolaevich Kolmogorov*. In [Chern, Hirzenbuch 2001], pp 119-164.

[Truesdell 1976a] TRUESDELL Clifford (1976) *History of Classical Mechanics. Part I, to 1800*. Switzerland, Naturwissenschaften 63, pp. 53-62.

[Truesdell 1976b] TRUESDELL Clifford (1976) *History of Classical Mechanics. Part II, the 19th and 20th Centuries*. Switzerland, Naturwissenschaften 63, pp. 119-130

[Ulam 1958] ULAM Stanislaw (1958) *John von Neumann, 1903-1957*. Providence, Rhode Island, Bulletin of the American Mathematical Society 64(3).

[von Plato 2005] VON PLATO Jan (2005) *A.N. Kolmogorov, Grundbegriffe der wahrscheinlichkeitsrechnung (1933)*. In [Grattan-Guinnes 2005, pp. 960-969]

[Wilson 1994a] WILSON Curtis, (1994) *The dynamics of the Solar System*. In [Grattan Guinness 1994], vol.2, pp. 1044-1053.

[Wilson 1994b] WILSON Curtis, (1994) *The three-body problem*. In [Grattan Guinness 1994] vol.2, pp. 1054-1062.

[Zdravkovska, Duren 2007] ZDRAVKOVSKA Smilka, DUREN Peter Larkin (eds.) (2007) *Golden Years of Moscow Mathematics*. Second edition, American Mathematical Society, History of Mathematics, vol.6.

[Zipperstein 1985] ZIPPERSTEIN Steven, J.(1985) *The Jewish of Odessa. A cultural history 1794-1881*. Stanford, Stanford University Press.