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# Poincaré and the Three Body Problem

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A thesis submitted for the degree of Doctor of Philosophy in the  
Department of Mathematics of The Open University.

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# Abstract

The purpose of the thesis is to present an account of Henri Poincaré's famous memoir on the three body problem, the final version of which was published in *Acta Mathematica* in 1890 as the prize-winning entry in King Oscar II's 60th birthday competition. The memoir is renowned both for its role in providing the foundations for Poincaré's celebrated three volume *Méthodes Nouvelles de la Mécanique Céleste*, and for containing the first mathematical description of chaotic behaviour in a dynamical system.

A historical context is provided both through consideration of the problem itself and through a discussion of Poincaré's earlier work which relates to the mathematics developed in the memoir. The organisation of the Oscar competition, which was undertaken by Gösta Mittag-Leffler, is also described. This not only provides an insight into the late 19th century European mathematical community but also reveals that after the prize had been awarded Poincaré found an important error in his work and substantially revised the memoir prior to its publication in *Acta*. The discovery of a printed version of the original memoir personally annotated by Poincaré has allowed for a detailed comparative study of the mathematics contained in both versions of the memoir. The error is explained and it is shown that it was only as a result of its correction that Poincaré discovered the chaotic behaviour now associated with the memoir.

The contemporary reception of the memoir is discussed and Poincaré's subsequent work in celestial mechanics and related topics is examined. Through the consideration of sources up to 1920 the influence and impact of the memoir on the progress of the three body problem and on dynamics in general is assessed.



*To the memory of my father and my brother  
Patrick Barrow-Green and Andrew Barrow-Green.*

# Acknowledgements

I am delighted to be able to express my thanks to all those people whose contributions, academic or otherwise, enabled me to complete this thesis.

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# 1. Introduction

## 1.1 Aims and organisation of the thesis

The main purpose of my research is twofold. First I provide a full account of Henri Poincaré's famous memoir on the three body problem which was published in *Acta Mathematica* at the end of 1890 as the prize winning entry of the important international competition sponsored by King Oscar II of Sweden and Norway to celebrate his 60th birthday. The memoir's publication had been eagerly awaited by the mathematical community since January 1889 when the results of the competition were announced. Its appearance fulfilled expectations for it contained substantial new and exciting results pertaining to a longstanding and important problem, and it has continued to be lauded by succeeding generations of mathematicians as a milestone in the study and development of both celestial mechanics and dynamics. In 1902 F. R. Moulton wrote:

*"The methods employed by Poincaré were incomparably more profound and powerful than any previously used in Celestial Mechanics, and mark an epoch in the development of the science."* [1914, 320],

and in 1925 George Birkhoff introduced a paper with:

*"Le Problème de (sic) trois corps ... contained the first great attack upon the non-integrable problems of dynamics. ... Acta Mathematica has had many*

*remarkable articles, but perhaps none of larger scientific importance than this one.*" [1925, 297].

Much more recently, Philip Holmes [1990] described the memoir as:

*"... the first textbook in the qualitative theory of dynamical systems ...".*

In his introduction to the memoir Poincaré stated that he had revised the paper for publication, but he did not document the nature and extent of his alterations. However, the discovery of a printed version of Poincaré's original memoir annotated personally by him, reveals that the paper which appears in *Acta* differed quite dramatically from the paper which actually won the prize<sup>1</sup>. Some of the principal results for which the paper is best known today are nowhere to be found in the original version. I make a comparison between the two versions which shows that their differences are not merely the result of refinements and additions but they exist largely as a consequence of corrections which Poincaré made upon the discovery of an important error after the prize had been awarded, and I show that the correction of the error is critical with regard to our perception of the memoir today.

Second, I give a description of the mathematical environment in which the work was produced and published. By placing [P2] into a historical context, I demonstrate that it marks an important change in approach to dynamical problems and can be regarded as a starting point for the beginnings of dynamical systems theory. In association with this second objective, I indicate how the many new and innovative ideas which appear in [P2] have been extended and developed, not only in the context in which they were created, but also in other branches of mathematics.

I begin by providing a mathematical framework for [P2] both in terms of the three body problem itself and in terms of Poincaré's earlier related work. In order to clarify the mathematical difficulties associated with the problem, I give a modern mathematical description before giving an outline of its history. With regard to the historical development of the problem, I look particularly at the progress made in the 19th century and consider the contributions made by the mathematical astronomers Delaunay, Gylden, and Lindstedt, whose work on infinite series solutions was of special interest to Poincaré, and I pay close attention to the research

---

<sup>1</sup> Henceforth I shall refer to the published version of the Memoir as [P2], the first printed version of the Memoir as [P1] and the copy of [P1] personally annotated by Poincaré as [P1a].

of the American G. W. Hill whose ground-breaking investigation into the theory of periodic solutions was a fundamental influence on Poincaré's work.

With respect to Poincaré's earlier work, I concentrate particularly on the celebrated memoirs on curves defined by differential equations, as it was these memoirs which provided the first qualitative account of the theory of differential equations, and formed the foundation for a large part of the mathematical theory to be found in his celestial mechanics which received its first exposure in [P2]. In addition, I also examine papers in which Poincaré either addressed specific aspects of the three body problem, such as his first paper on periodic solutions, or treated related mathematical techniques, such as his paper on asymptotic series.

Then I give an account of the circumstances which led up to and surrounded the final publication of the memoir. The organisation of the Oscar competition was the responsibility of the Swedish mathematician and editor of *Acta*, Gösta Mittag-Leffler, who engaged the support of two of the leading analysts in Europe, Charles Hermite and Karl Weierstrass, to make up the prize commission. From the competition's beginnings in 1884 until the appearance of Poincaré's memoir in *Acta* in October 1890, Mittag-Leffler was beleaguered by problems which reached a climax with Poincaré's discovery of the error, and an examination of the surviving documents provides an interesting insight into the life of the late 19th century European mathematical community.

The central core of the thesis is a description and analysis of [P2] in conjunction with a comparison with [P1]. In the analysis of the memoir, I follow Poincaré closely in order to describe clearly the significant differences between [P1] and [P2]. I have used [P2] as the basis, inserting descriptions from [P1] when these differences arise. It will be seen that in both versions Poincaré divided the memoir into two parts: theory and application, and in each case he applied the theory to a simplified form of the three body problem now known as the restricted three body problem.

The major part of the development of the theory is taken up with the two important topics of invariant integrals and periodic solutions. Although Poincaré had touched on the subject of invariant integrals in earlier papers, it is here that he provided the first detailed account of the theory, and included for the first time his famous recurrence theorem. With regard to the periodic solutions, Poincaré had earlier demonstrated the potential of these solutions for resolving qualitative questions in the theory of differential equations, but it was in [P2] that this potential was



successfully put to the test and led to the discovery of his exciting new theory of asymptotic solutions.

The second part of [P2] is largely concerned with the application of the theory of asymptotic solutions to the restricted three body problem, but it also contains Poincaré's theorem concerning the non-existence of any new transcendental integrals of the restricted three body problem, and his proof of the divergence of Lindstedt's series. In his application of the theory of asymptotic solutions Poincaré proceeded by a series of approximations which involved taking account of an increasing number of terms in the power series expansions used to describe the solutions. I show that although it was his geometrical description of the asymptotic solutions which was affected most dramatically by the discovery and correction of the error, the error itself arose in the preceding analysis.

Briefly, in his original account Poincaré did not draw a distinction between autonomous and non-autonomous Hamiltonian systems of differential equations, and as a result drew mistaken conclusions about the convergence of the series used to describe the asymptotic solutions of the problem. In [P1] Poincaré believed that the series were convergent and led to asymptotic trajectories with behaviour which was easily understood. In [P2] he showed that the series were actually asymptotic expansions with the result that the behaviour of the trajectories was anything but easy to describe and in fact was what today would be called chaotic. Thus contrary to what is generally believed, I show that Poincaré did not win the Oscar prize for his discovery and analysis of the behaviour of what he called *doubly asymptotic* solutions (and later called *homoclinic* solutions), but rather he won the prize for the underlying theory which eventually led to his correct description of these solutions.

Since the objective of my discussion of the memoir is to give an analysis of the new ideas contained in it and to make clear the nature and extent of Poincaré's error, I have not given the details of mathematical arguments where they are either well known or uncontentious. For further details the reader is referred to the copy of [P2] in Volume VII of the Poincaré *Œuvres*.

In discussing the reception of the memoir, I look first at the prize commission's views on [P1] as revealed through the correspondence and then describe the response to the news of Poincaré's success in the competition. Considering the period after the publication of [P2], I examine commentaries on the memoir as well as the contemporary response to some of the ideas contained in it.

With regard to Poincaré's work after 1890, I describe the relationship of [P2] to his renowned three volume *Méthodes Nouvelles* published between 1892 and 1899, the first and last volumes of which are largely an elaboration and refinement of [P2]. I then examine other papers on the three body problem and celestial mechanics which are either related to or develop ideas contained in [P2]. These range from specific corrections and criticisms of the work of other mathematicians and astronomers through to articles of a general nature on the stability of the solar system.

I also consider two later papers in which Poincaré developed his ideas about the existence of periodic solutions in the three body problem within a topological framework. The first of these, which was his first on periodic solutions after an interval of more than ten years, was a study of geodesics on a convex surfaces which was published in 1905, appearing shortly after the final paper in his fundamental series on the study of topology (or *Analysis Situs* as it was then called). The second paper, which was published in 1912 only shortly before he died, contained his famous "last geometric theorem", a complete proof of which sadly had eluded him, but which was supplied shortly afterwards by the young American mathematician George Birkhoff.

The latter part of the thesis is devoted to an examination of both the influence of [P2] on the progress of the three body problem, and the memoir's role in the foundation of dynamical systems theory. I consider the period up until 1920 by which time not only had a function theoretical proof to the three body problem been obtained, but also dynamical systems theory started to move into a new phase no longer centred around problems in celestial mechanics. Part of the reason for this change was the inadequacy of computing techniques which meant that verification of the accuracy of the predictions generated by the theory was not feasible, comparable quantitative analysis only becoming possible with the development of the modern digital computer. Not only did this lack of computing power help to engender a move towards more generalised problems but also astronomy itself was changing. The announcement of Einstein's general theory of relativity in 1915 and the rise of quantum mechanics in the 1920s created additional diversions away from some of the traditional problems in celestial mechanics with mathematicians eager to find applications for the new methods.



Prior to examining the progress of the three body problem, I extend the context in which Poincaré's work was produced, by first considering Alexander Liapunov's qualitative study of stability theory. Although Liapunov's memoir, which appeared in 1890, was produced independently of [P2], it provides an interesting alternative account of one of the topics discussed by Poincaré. Also in connection with stability theory I consider briefly the ideas of Tullio Levi-Civita.

With regard to the three body problem, I begin by discussing the regularisation of the equations, a process which was begun by Paul Painlevé in 1895. In particular I describe the important contribution made by Levi-Civita, and the final resolution of the problem achieved by Karl Sundman in 1912. I then look at the influence of Poincaré's ideas on the quantitative development of the problem by examining the work of Sir George Darwin on periodic orbits.

Turning to more general problems of dynamics, I show that it was Poincaré's methods which, being characterised by a global geometric viewpoint, led to the opening up of a new qualitative approach to the subject. In particular I examine the related work of both Jacques Hadamard and George Birkhoff each of whom were greatly influenced by Poincaré and professed the greatest admiration for his work.

Hadamard's ideas were presented in two seminal papers in which he discussed geodesics on surfaces and which were published in 1897 and 1898. The first, for which he was awarded the Prix Bordin de l'Académie des Sciences, deals with the case when the surfaces are everywhere of positive curvature, while the second is concerned with surfaces of negative curvature. With regard to these papers the importance of topology in the study of differential equations as initiated by Poincaré is clearly evident.

George Birkhoff made outstanding progress in the field of dynamics and the general theory of orbits. He was especially influenced by Poincaré's *Méthodes Nouvelles*, and recorded particular successes in geometrical aspects of dynamics, notably in providing a proof for Poincaré's last geometric theorem. I look at how Birkhoff expanded Poincaré's ideas in his own work on the restricted three body problem and dynamical systems to become one of the founders of modern dynamics.

Finally, I end by signalling the work of the next generation of mathematicians working in the same tradition. I mention here the work of Marston Morse, who was a student of Birkhoff's and took up both his ideas and those of Hadamard, and, moreover, described himself as a "*mathematical descendant of Poincaré*". He

understood topology through dynamics and in 1917 wrote papers on dynamics and geodesic flow which can be considered the starting point for symbolic dynamics. I also look forward to the work Kolmogorov, Arnol'd and Moser on the existence of quasi-periodic solutions of Hamiltonian systems.

References are cited in the [year] or [year, *page number*] form, for example Whittaker [1937, 339] refers to E. T. Whittaker *A treatise on the analytical dynamics of particles and rigid bodies*, page number 339. All page numbers in references to works of Poincaré, Birkhoff, Darwin, Hadamard and Hill refer to the collected works where applicable. References to the thesis will be by chapter and section number. The references, together with a name and date index, are collected at the end of the thesis. Unless otherwise stated all translations are my own.

## 2. Historical Background

### 2.1 Introduction

The three body problem, described by Whittaker as "*the most celebrated of all dynamical problems*"<sup>1</sup>, and which for Hilbert fulfilled the necessary criteria for a good mathematical problem<sup>2</sup>, can be simply stated: three particles move in space under their mutual gravitational attraction; given their initial conditions, determine their subsequent motion.

However, like many mathematical problems, the simplicity of its statement belies the complexity of its solution. For although the one and two body problems can be solved in closed form by means of elementary functions, the three body problem is a complicated non-linear problem and no similar type of solution exists.

Apart from the intrinsic appeal of such a simple to state problem, there is another compelling reason for wanting to study three body problem and that is its intimate link with the fundamental question of the stability of the solar system. Over the years attempts to find a solution have spawned a wealth of research. Between 1750 and the beginning of this century more than 800 memoirs relating to the problem

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<sup>1</sup> Whittaker [1937, 339].

<sup>2</sup> See the introduction to Hilbert's famous speech on mathematical problems given at the Paris Congress in 1900 which first appeared in *Gottinger Nachrichten* 1900, 253-297; and in *Archiv der Mathematik und Physik* (3) 1 (1901), 44-63 and 213-237; and translated for the *American Mathematical Society Bulletin* 8 (1902), 437-479, by Mary Winston Newson.

were published, invoking a roll call of many distinguished mathematicians and astronomers<sup>3</sup>. The interest generated by the problem has resulted in significant advances in mathematics in many different fields including, in recent times, the theory of dynamical systems.

At the beginning of this century a Finnish mathematical astronomer, Karl Sundman, mathematically “solved” the problem by providing a convergent power series solution valid for all values of time<sup>4</sup>. However, since Sundman’s solution gives no qualitative information about the behaviour of the system, and the rate of convergence of the series is considered to be too slow to be of any real practical use, it leaves plenty of issues surrounding the problem still unresolved.

It is helpful to begin by giving a current mathematical description in order to provide a context for the historical development.

## 2.2 Mathematical description of the three body problem

### 2.2.1 The differential equations of the problem

Let  $P_i$  represent the three particles with masses  $m_i$ , distances  $P_i P_j = r_{ij}$ , and coordinates  $q_{ij}$  ( $i, j = 1, 2, 3$ ) in an inertial coordinate system. The equations of motion are

$$\begin{aligned} \frac{d^2 q_{1i}}{dt^2} &= k^2 m_2 \frac{(q_{2i} - q_{1i})}{r_{12}^3} + k^2 m_3 \frac{(q_{3i} - q_{1i})}{r_{13}^3} \\ \frac{d^2 q_{2i}}{dt^2} &= k^2 m_1 \frac{(q_{1i} - q_{2i})}{r_{12}^3} + k^2 m_3 \frac{(q_{3i} - q_{2i})}{r_{23}^3} \\ \frac{d^2 q_{3i}}{dt^2} &= k^2 m_1 \frac{(q_{1i} - q_{3i})}{r_{13}^3} + k^2 m_2 \frac{(q_{2i} - q_{3i})}{r_{23}^3} \end{aligned} \quad (i = 1, 2, 3) \quad (2.2.i)$$

where  $k$  is the gravitational constant. The problem is therefore described by nine differential equations each of second order.

---

<sup>3</sup> Whittaker [1937, 339].

The history of the three body problem has been well documented, most notably by Gautier [1817], Cayley [1862], Whittaker [1899], Lovett [1912] and Marcolongo [1919]. Whittaker’s famous report describes the situation from 1868 to 1898, while that of Lovett is concerned with the following decade. Marcolongo’s book is a full account, superbly referenced and deserves to be better known.

<sup>4</sup> Sundman’s work is discussed in Chapter 8.

If the units are chosen such that  $k^2$  is equal to one, the force of attraction between the  $i$ th and  $j$ th particles becomes  $m_i m_j / r_{ij}^2$ , and the corresponding term in the potential energy becomes  $-m_i m_j / r_{ij}$ . The potential energy  $V$  of the whole system is therefore given by

$$V = -\frac{m_2 m_3}{r_{23}} - \frac{m_3 m_1}{r_{31}} - \frac{m_1 m_2}{r_{12}}.$$

Writing

$$p_{ij} = m_i \frac{dq_{ij}}{dt}$$

and

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

the equations take the Hamiltonian form

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

and these are a set of 18 first order differential equations.

So for a closed solution to the problem the system needs 18 integrals. However, as described below, it is only possible to reduce the system to one of order six. Briefly, this is achieved through the use of the so-called ten classical integrals - the six integrals of the motion of the centre of mass, the three integrals of angular momentum, and the energy integral - together with the elimination of the time and the elimination of what is called the ascending node. Bruns [1887] proved that when the rectangular coordinates are chosen as dependent variables the ten classical integrals are the only independent algebraic integrals of the problem and all others can be formed by a combination of these. It will be seen later how Poincaré extended Bruns' result by establishing that no new single-valued transcendental integrals of the problem exist, provided the masses of two of the bodies are very small when compared with the third.

### 2.2.2 Reduction to 6th order

If the  $ij$ th equation of equations (2.2.i) is multiplied by  $m_i$ , a summation can be performed to give three equations



$$\sum_{i=1}^3 m_i \frac{d^2 q_{ij}}{dt^2} = 0. \quad (j = 1, 2, 3)$$

These equations can be integrated twice to give the equations

$$\sum_{i=1}^3 m_i q_{ij} = A_j t + B_j \quad (j = 1, 2, 3)$$

in which the  $A_j$  and  $B_j$  are constants of integration. This set of equations shows that the centre of mass of the three particles either remains at rest or moves uniformly in space in a straight line, which is as expected since there are no forces acting except the mutual attractions of the particles. The six constants serve to describe the motion of the centre of mass in the original arbitrary inertial coordinate system and they play no part in the motion of the bodies about the centre of mass.

If in the first set of equations (2.2.i) the first equation is multiplied by  $-q_{12}$ , the second equation by  $-q_{22}$  and the third equation by  $-q_{32}$ , and in the second set the first equation is multiplied by  $q_{11}$ , the second equation by  $q_{21}$  and the third equation by  $q_{31}$ , and these two sets are added together, then this gives

$$\sum_{i=1}^3 m_i q_{i1} \frac{d^2 q_{i2}}{dt^2} - \sum_{i=1}^3 m_i q_{i2} \frac{d^2 q_{i1}}{dt^2} = 0,$$

and two similar equations can be obtained by a cyclic change of the variables. The three equations can be integrated to give

$$\sum_{i=1}^3 m_i \left( q_{i2} \frac{dq_{i3}}{dt} - q_{i3} \frac{dq_{i2}}{dt} \right) = C_1$$

$$\sum_{i=1}^3 m_i \left( q_{i3} \frac{dq_{i1}}{dt} - q_{i1} \frac{dq_{i3}}{dt} \right) = C_2$$

$$\sum_{i=1}^3 m_i \left( q_{i1} \frac{dq_{i2}}{dt} - q_{i2} \frac{dq_{i1}}{dt} \right) = C_3.$$

These equations represent the conservation of angular momentum for the system, i.e. they show that the angular momentum of the three particles around each of the coordinate axes is constant throughout the motion.



Looked at geometrically, the terms in brackets represent the projections of the areal velocities of the various bodies upon the three coordinate planes and hence the integrals are also known as the integrals of area<sup>5</sup>.

Since

$$\frac{\partial}{\partial q_{ij}} \left( \frac{1}{r_{ik}} \right) = - \frac{q_{ki} - q_{ij}}{r_{ik}^3},$$

the equations of motion can be written in the form

$$m_i \frac{d^2 q_{ij}}{dt^2} = - \frac{\partial V}{\partial q_{ij}}.$$

Multiplying by  $\frac{dq_{ij}}{dt}$ , and summing, gives, since  $V$  is a function of the coordinates only

$$\sum_{i,j=1}^3 p_{ij} \frac{d^2 q_{ij}}{dt^2} = - \frac{dV}{dt}.$$

This equation can be integrated to give

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

where  $C$  is a constant of integration. This is the expression for the Hamiltonian with  $H = C$ . The left hand side of the equation is the kinetic energy  $T$  of the system, hence the integral can be put into the form  $T - F = C$  which expresses the conservation of energy<sup>6</sup>.

Two further reductions can be made to the order of the system. Firstly the time can be eliminated by using one of the dependent variables as an independent variable, and secondly a final reduction can be made by the so-called elimination of the nodes, a procedure made first explicit in Jacobi [1843], although indicated in Lagrange [1772].

The first stage of Jacobi's process is to make a linear change of variables which effectively changes the configuration to one in which two "fictitious" bodies orbit a third. Since the change of variable is linear, the form of the integrals of angular

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<sup>5</sup> Relative to a fixed origin, the areal velocity of a point is the area swept out by the radius vector per unit time.

<sup>6</sup> Sometimes called the "Vis Viva" integral.

momentum are unchanged and the total angular momentum vector  $c$  remains constant and perpendicular to an invariant plane (see FIG. 2.2.i). Jacobi showed that the intersection between the orbital planes of the two bodies remains parallel to this invariant plane, which gives the result that the difference in longitude between the ascending nodes is always  $\pi$  radians.

Using these two last integrals in conjunction with the ten classical integrals, reduces the original system of order 18 down to a system of order 6. This result can be generalised to the  $n$  body problem in which the differential equations constitute a system of order  $6n$ . Using the same integrals this system can be reduced to system of order  $(6n - 12)$ .

$c$ : total angular momentum vector

$AB$ : lines of nodes

$r$ : radius vector of inner orbit

$R$ : radius vector of exterior orbit

$h$ : longitude of the node of the inner orbit

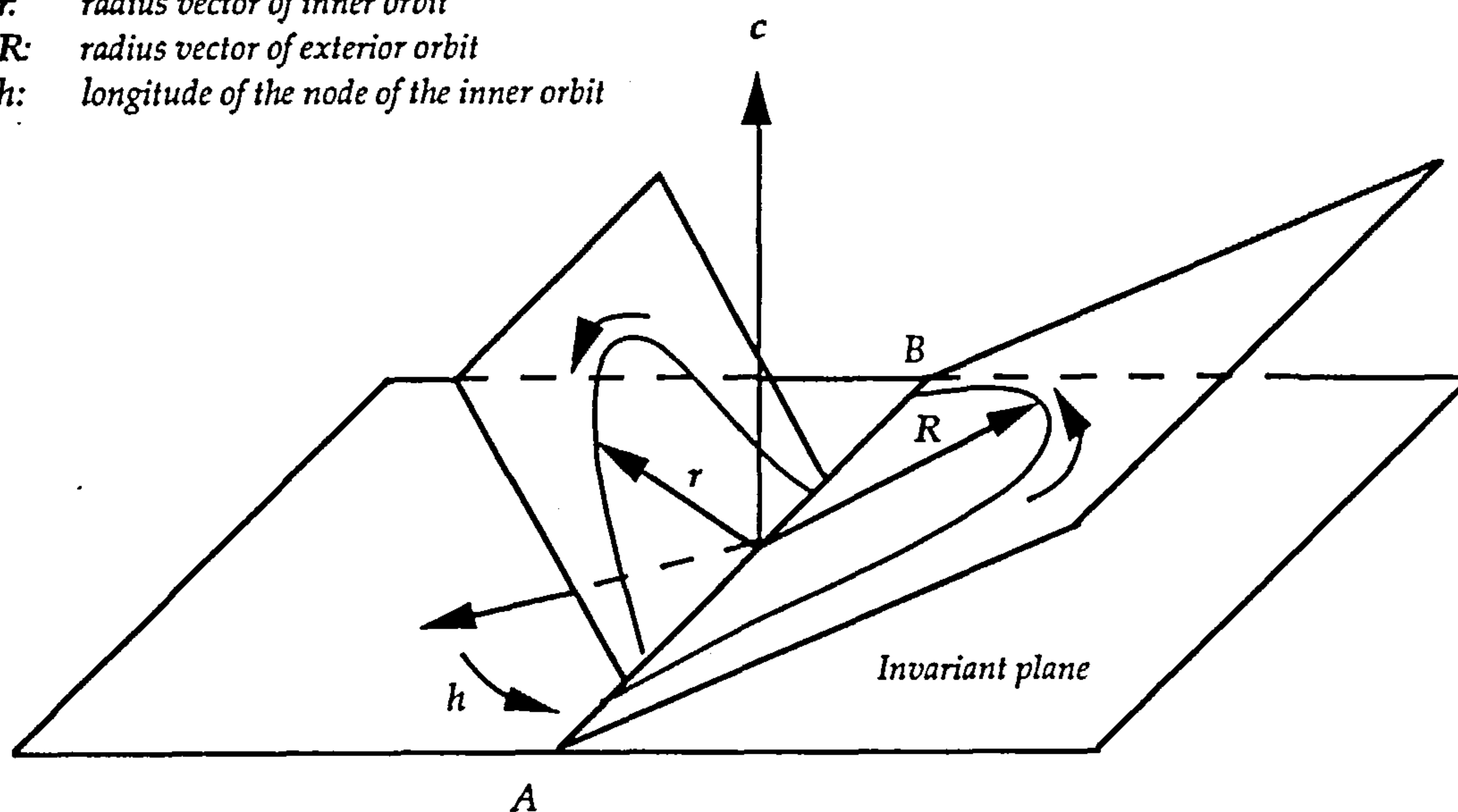


FIG. 2.2.i. Elimination of the nodes

### 2.2.3 The restricted three body problem

A special case of the three body problem which, due to its simplified form and its practical applications, has featured prominently in research is what today is known as the "restricted" three body problem<sup>7</sup>. In this formulation two of the bodies

<sup>7</sup> The term "problème restreint" was first used by Poincaré in [MN III, 69].

revolve around their centre of mass in circular orbits under the influence of their mutual gravitational attraction and thus form a two body system in which their motion is known. A third body (generally known as the planetoid), assumed massless with respect to the other two, moves in the plane defined by the two revolving bodies and, while being gravitationally influenced by them, exerts no influence of its own. The problem is then to ascertain the motion of the third body.

This particular case of the three body problem is the simplest one of importance, and, with regard to what is to follow, is especially significant since most of the results in Poincaré's memoir pertain to this formulation. Apart from its simplifying characteristics, it also provides a good approximation for real physical situations, as, for example, in the problem of determining the motion of the moon around the earth, given the presence of the sun. In this instance, the problem is almost circular (the eccentricity of the earth's orbit is approximately 0.017), almost planar (both the earth's orbit and the moon's orbit are nearly in the plane of the ecliptic), and the values of the mass ratios and the mean distances between the bodies satisfy the conditions.

The differential equations of motion for the problem can be derived as follows.

Let  $m_1 = 1 - \mu$  and  $m_2 = \mu$  denote the masses of the two finite bodies, choose the unit of distance so that the constant distance between the two finite bodies  $a$  is unity, and choose the unit of time so that the gravitational constant  $k^2$  is also unity. If the coordinates of  $m_1$ ,  $m_2$  and the planetoid  $P$  are  $(\xi_1, \eta_1)$ ,  $(\xi_2, \eta_2)$ , and  $(\xi, \eta)$  respectively and

$$r_1 = \sqrt{(\xi - \xi_1)^2 + (\eta - \eta_1)^2}$$

$$r_2 = \sqrt{(\xi - \xi_2)^2 + (\eta - \eta_2)^2}$$

then the equations of motion of the planetoid are

$$\frac{d^2\xi}{dt^2} = -(1 - \mu)\frac{(\xi - \xi_1)}{r_1^3} - \mu\frac{(\xi - \xi_2)}{r_2^3} \quad (2.2.ii)$$

$$\frac{d^2\eta}{dt^2} = -(1 - \mu)\frac{(\eta - \eta_1)}{r_1^3} - \mu\frac{(\eta - \eta_2)}{r_2^3}.$$

By Kepler's third law, the mean angular motion  $n$  of the finite bodies is

$$n = k \frac{\sqrt{(1 - \mu) + \mu}}{a^{3/2}}$$

which, due to the way the units have been chosen, is equal to unity.

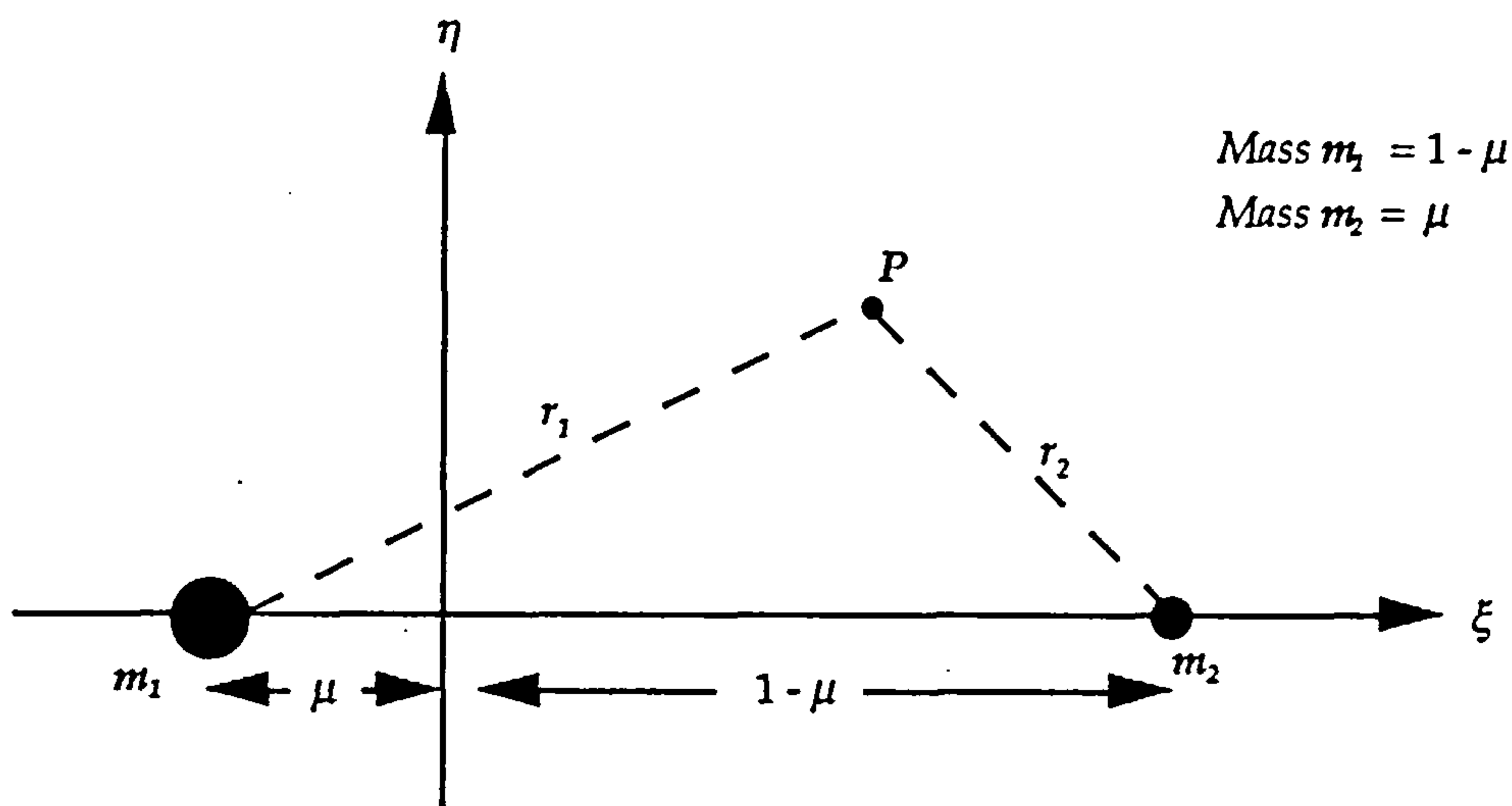


FIG. 2.2.ii. The restricted three body problem in rotating coordinates

If the motion of the bodies is now referred to a new system of axes  $x$  and  $y$  having the same origin as the old but rotating in the  $\xi\eta$  plane in the direction in which the finite bodies move with uniform angular velocity, then the coordinates in the new system are defined by

$$\xi = x \cos t - y \sin t \quad (2.2.iii)$$

$$\eta = x \sin t + y \cos t$$

with similar sets of equations for  $(\xi_1, \eta_1)$  and  $(\xi_2, \eta_2)$ . Differentiating equations (2.2.iii) twice, substituting the resulting expressions in equations (2.2.ii), multiplying the two equations by  $\cos t$  and  $\sin t$  respectively and adding, and then multiplying them by  $-\sin t$  and  $\cos t$  respectively and adding, gives

$$\begin{aligned} \frac{d^2x}{dt^2} - 2\frac{dy}{dt} &= x - (1 - \mu) \frac{(x - x_1)}{r_1^3} - \mu \frac{(x - x_2)}{r_2^3} \\ \frac{d^2y}{dt^2} + 2\frac{dx}{dt} &= y - (1 - \mu) \frac{(y - y_1)}{r_1^3} - \mu \frac{(y - y_2)}{r_2^3} . \end{aligned}$$

Since it is always possible to choose the direction of the  $x$  axis so that the two finite bodies lie on it, in which case  $y_1 = y_2 = 0$ , the equations then become

$$\begin{aligned} \frac{d^2x}{dt^2} - 2\frac{dy}{dt} &= x - (1 - \mu) \frac{(x - x_1)}{r_1^3} - \mu \frac{(x - x_2)}{r_2^3} \\ \frac{d^2y}{dt^2} + 2\frac{dx}{dt} &= y - (1 - \mu) \frac{y}{r_1^3} - \mu \frac{y}{r_2^3} \end{aligned} \quad (2.2.iv)$$

These differential equations, which have the important property that they do not involve  $t$  explicitly, are the equations of motion of the planetoid with respect to the

rotating coordinates. Since they are a set of two second order equations they represent a system of order 4. However, they do admit a solution, known as the Jacobian integral<sup>8</sup>, which reduces the system to one of order 3.

If a function  $U$  is defined by

$$U = \frac{1}{2}(x^2 + y^2) + \frac{1-\mu}{r_1} + \frac{\mu}{r_2}$$

then equations (2.2.iv) can be written

$$\begin{aligned} \frac{d^2x}{dt^2} - 2\frac{dy}{dt} &= \frac{\partial U}{\partial x} \\ \frac{d^2y}{dt^2} + 2\frac{dx}{dt} &= \frac{\partial U}{\partial y} \end{aligned} \tag{2.2.v}$$

and multiplying these by  $2\frac{dx}{dt}$ , and  $2\frac{dy}{dt}$  respectively gives

$$2\frac{dx}{dt}\frac{d^2x}{dt^2} + 2\frac{dy}{dt}\frac{d^2y}{dt^2} = 2\frac{dx}{dt}\frac{\partial U}{\partial x} + 2\frac{dy}{dt}\frac{\partial U}{\partial y} \tag{2.2.vi}$$

which is an exact differential since  $U$  is a function of  $x$  and  $y$  alone.

Integrating equation (2.2.vi) gives

$$\left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 = 2U - C$$

where  $C$  is a constant of integration and the left hand side is the square of the velocity of the planetoid in the rotating frame. If the latter is denoted  $V^2$  then

$$V^2 = 2U - C$$

and this solution is known as the Jacobian integral of the restricted three body problem. It is sometimes misleadingly called the energy or relative energy integral, but this terminology is erroneous as the integral does not express conservation of energy. Although the total energy of the original two body system remains constant, that of the planetoid does not, and so the total energy in the restricted problem is not constant. As Szebehely points out, the solution should be regarded purely as an integral of the equations of motion of the restricted three body problem using rotating coordinates<sup>9</sup>.

Equations (2.2.ii) can also be written

<sup>8</sup> Jacobi [1836] first announced his integral in an inertial coordinate system.

<sup>9</sup> See Szebehely [1967, 12-13, 38].



$$\frac{d^2\xi}{dt^2} = \frac{\partial F}{\partial \xi}, \quad \frac{d^2\eta}{dt^2} = \frac{\partial F}{\partial \eta}$$

where  $F = \frac{m_1}{r_1} + \frac{m_2}{r_2}$ .

If  $\xi = q_1$ ,  $\eta = q_2$ ,  $\frac{d\xi}{dt} = p_1$ , and  $\frac{d\eta}{dt} = p_2$ , then the Hamiltonian form of the equations is

$$\frac{dq_i}{dt} = \frac{\partial H}{\partial p_i}, \quad \frac{dp_i}{dt} = -\frac{\partial H}{\partial q_i} \quad (i = 1, 2)$$

where  $H = \frac{1}{2}(p_1^2 + p_2^2) - F$ .

Since the function  $F$  is not only a function of the variables  $q_i$  but it is also a function of the time  $t$ , and hence

$$H = \text{constant}$$

is not a solution of the system. However, if, as in the previous formulation, a transformation is made such that the axis of rotation and a line perpendicular to this through the centre of gravity of the two bodies become the coordinate axes, then a solution can be found. As Whittaker has shown [1937, 354], this can be done by applying the contact transformation defined by the equations

$$q_i = \frac{\partial W}{\partial p_i}, \quad P_i = \frac{\partial W}{\partial Q_i}$$

where

$$W = p_1(Q_1 \cos nt - Q_2 \sin nt) + p_2(Q_1 \sin nt + Q_2 \cos nt)$$

and  $n$  is the uniform angular velocity of the rotating axis which, due to the choice of units, is equal to unity<sup>10</sup>.

The new Hamiltonian, which has no explicit dependence on time, is then given by

$$H' = H - \frac{\partial W}{\partial t},$$

and

$$H' = \text{constant}$$

is a solution of the system corresponding to the Jacobian integral.

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<sup>10</sup> The idea of contact transformations originated with Sophus Lie in the early 1870s. See Hawkins [1992].



## 2.3 History of the three body problem

### 2.3.1 Origin of the three body problem

Since bodies in the solar system are approximately spherical and their dimensions extremely small when compared with the distances between them, they can be considered as point masses, and so the origin of the problem can be thought of as being synonymous with the foundation of modern dynamical astronomy. This part of celestial mechanics, which connects the mechanical and physical causes with the observed phenomena, began with the introduction of Newton's theory of gravitation. From the time of the publication of the *Principia* in 1687, it became important to verify whether Newton's law alone was capable of rendering a complete understanding of how celestial bodies move in space, and, in order to pursue this line of investigation, it was necessary to ascertain the relative motion of  $n$  bodies attracting one another according to the Newtonian law.

Newton himself had geometrically solved the problem of two bodies for two spheres moving under their mutual gravitational attraction [1934, I, section XI] while in 1710 Johann Bernoulli had proved that the motion of one particle with respect to the other is described by a conic section<sup>11</sup>. In 1734 Daniel Bernoulli won a French Academy prize for his analytical treatment of the two body problem<sup>12</sup>, and the problem was solved in detail by Euler [1744]. Although these results cleared the way for the natural extension of the problem from two to three bodies, there was already a special interest in the three body problem generated by the needs of navigation for knowledge about the motion of the moon. The system formed by the sun, the earth and the moon was already a focus of attention and the lunar theory (as the study of the moon's motion came to be called) dominated the early research into the problem.

### 2.3.2 Early attempts at solution

The investigations arising from a search for a solution to the problem led in two directions: those which were concerned with finding general theorems concerning the motion, and those which were searching for good approximations for the solutions

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<sup>11</sup> See Wintner [1941, 420].

<sup>12</sup> See Kline [1972, 492].

which would hold for a given period of time starting from an instant at which data was available.

Newton himself was the first to treat the problem and he achieved results in both types of investigation. On the one hand, having shown that the centre of mass of  $n$  bodies moves with uniform speed in a straight line, he made a general investigation into the motion of attracting bodies [1934, I, Section XI], while on the other, using an essentially geometric approach to the method of variation of parameters, he applied perturbation theory to the motion of the moon. Having treated the motion of the moon about the earth and obtained an elliptical orbit, he considered the effect of the sun on the moon's orbit by taking account of variations in the latter. However, the calculations caused him great difficulties and his computation for the motion of the lunar apsides<sup>13</sup> gave a value which was approximately half that of the observed value. A fact which he encapsulated in later editions of the Principia in the brief sentence: "*The apse of the moon is about twice as swift.*" [1934 I, 147]. Indeed, the problems he encountered were such that he was prompted to remark to the astronomer John Machin that "... *his head never ached but with his studies on the moon.*"<sup>14</sup>.

During the 18th century, the gradual recognition of the power of analytic methods meant that dynamics in general, and celestial mechanics in particular, began to break free from the constraints of geometry. With this freedom came the realisation of the impossibility of finding a closed solution in terms of elementary functions. Clairaut in [1747] announced the first successful approximate resolution to the lunar problem by using infinite series solutions to an improved simplification of the differential equations. However, the difficulty he had in explaining the motion of the lunar perigee was such that he even considered a modification to the inverse square law. But in [1749], by carrying his original approximation one step further, he reached results which almost accounted for the motion, and in 1752 his *Théorie de la Lune* won the St. Petersburg Academy prize. The value of Clairaut's methods was amply confirmed by his prediction of the date of the perihelion passage of Halley's comet in 1759 which was accurate to within one month, almost exactly the margin of error he had allowed himself. (In 1872 it was discovered that Newton, in

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<sup>13</sup> The two points in the orbit of the moon at which it is respectively at its greatest and least distance from the earth, also called the apogee and the perigee.

<sup>14</sup> Keynes MSS 130.6, Book 3; 130.5, Sheet 3 - Newton Ms in the Keynes collection in the library of King's College, Cambridge. See Westall [1980, 544].

an unpublished manuscript in the Portsmouth Collection, had in fact corrected his original calculations for the motion of the lunar perigee by including the second order perturbations, although the correction was completely unknown until the manuscript's discovery<sup>15</sup>.)

Meanwhile, Euler [1748] was the first to use the method of variation of parameters to treat perturbations of planetary motion and in [1753] he published his first lunar theory. His second lunar theory [1772] which, jointly with a memoir by Lagrange, shared the *Prix de l'Académie de Paris*, contained many new and important features, including the first formulation of the restricted problem based on a rotating coordinate system and for which he found particular solutions<sup>16</sup>.

Lagrange's prize winning memoir [1772] was an analysis of the three body problem in which he showed that the problem could be reduced from a system of order 18 to a system of order seven<sup>17</sup>. His method was first to determine the mutual distances between the bodies, then to determine the plane of the triangle in space, and finally to determine the orientation of the triangle in the plane. In addition, he also found two types of particular solution to the general problem. Jacobi in [1843], unaware of Lagrange's work, achieved an explicit reduction of the general problem to a 6th order system through the elimination of the nodes.

### 2.3.3 Particular solutions

Particular solutions are those in which the geometric configuration of the three bodies remains invariant with respect to time. Thus, either the configuration simply rotates in its own plane around the centre of mass, or an expansion or contraction takes place in which the mutual distances between the three bodies remain in fixed ratios to each other. If there is no change in scale, the solutions are called stationary.

The particular solutions found by Euler in his revolving coordinate system were collinear solutions, while those of Lagrange took the form of both collinear and

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<sup>15</sup> Catalogue of the Portsmouth Collection of Books and Papers written by or belonging to Sir Isaac Newton, Cambridge, 1888, xi-xiii, xxvi-xxx, section 1, div ix, numbers 7, 12. See Newton [1934, 649].

<sup>16</sup> Whittaker [1899, 123] incorrectly credits the first discussion of the restricted problem to Jacobi.

<sup>17</sup> According to Whittaker [1937, 341], the reduction from 7th to 6th order through the elimination of the nodes is implicit in Lagrange's memoir.

equilateral configurations. In the collinear case, the bodies are all set in motion from positions on a straight line and, given appropriate initial conditions, they continue to stay on that line while the line rotates in a plane about the centre of mass of the bodies. In the equilateral case, the initial positions of the three bodies are at the vertices of an equilateral triangle and the bodies continue to move as though attached to the triangle which rotates about the centre of mass.

Associated with these particular solutions are five equilibrium points, also called Lagrangian or libration points,  $L_1 - L_5$  (FIG 2.3.i).

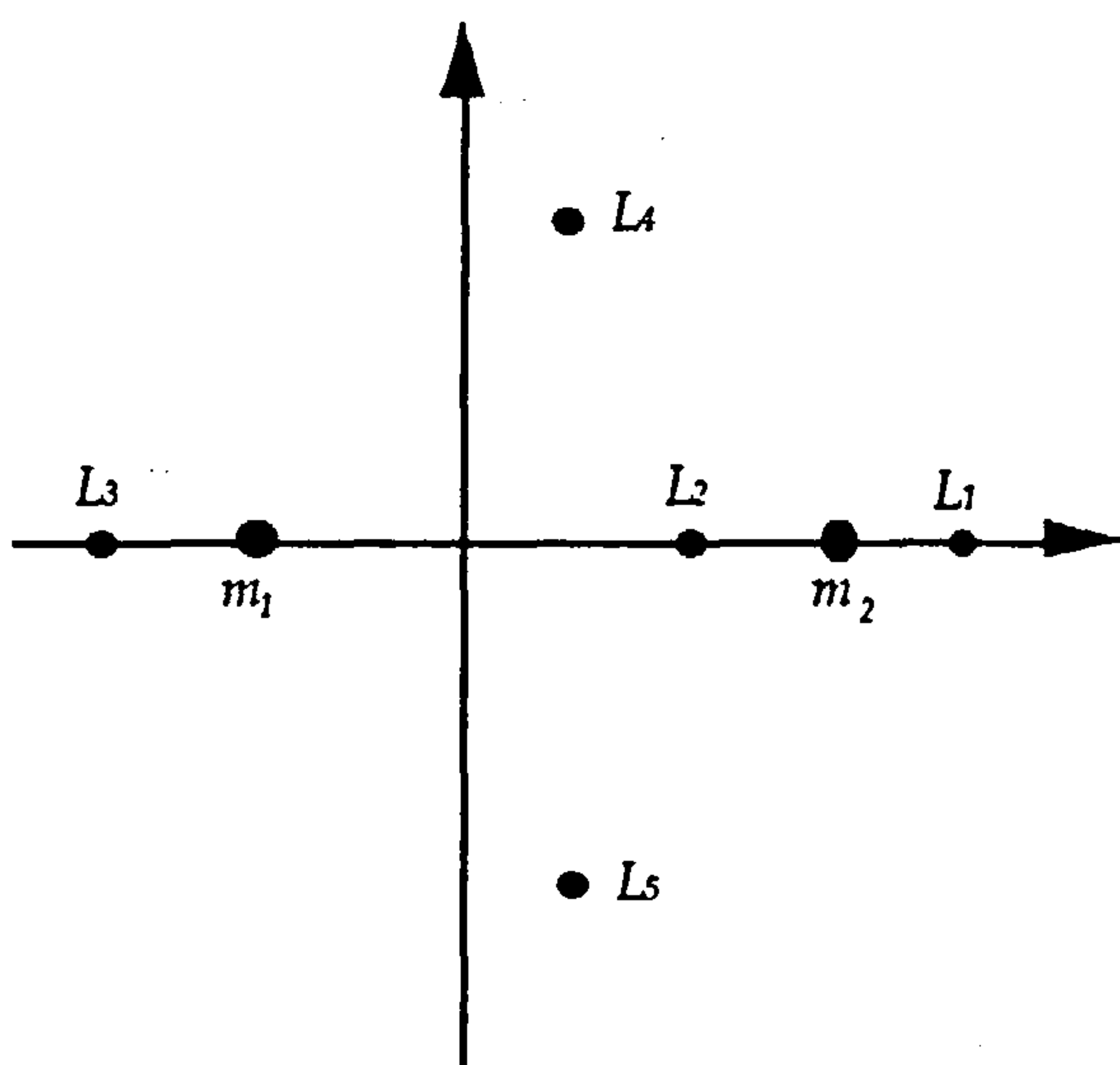


FIG. 2.3.i. Lagrangian points of the three body problem

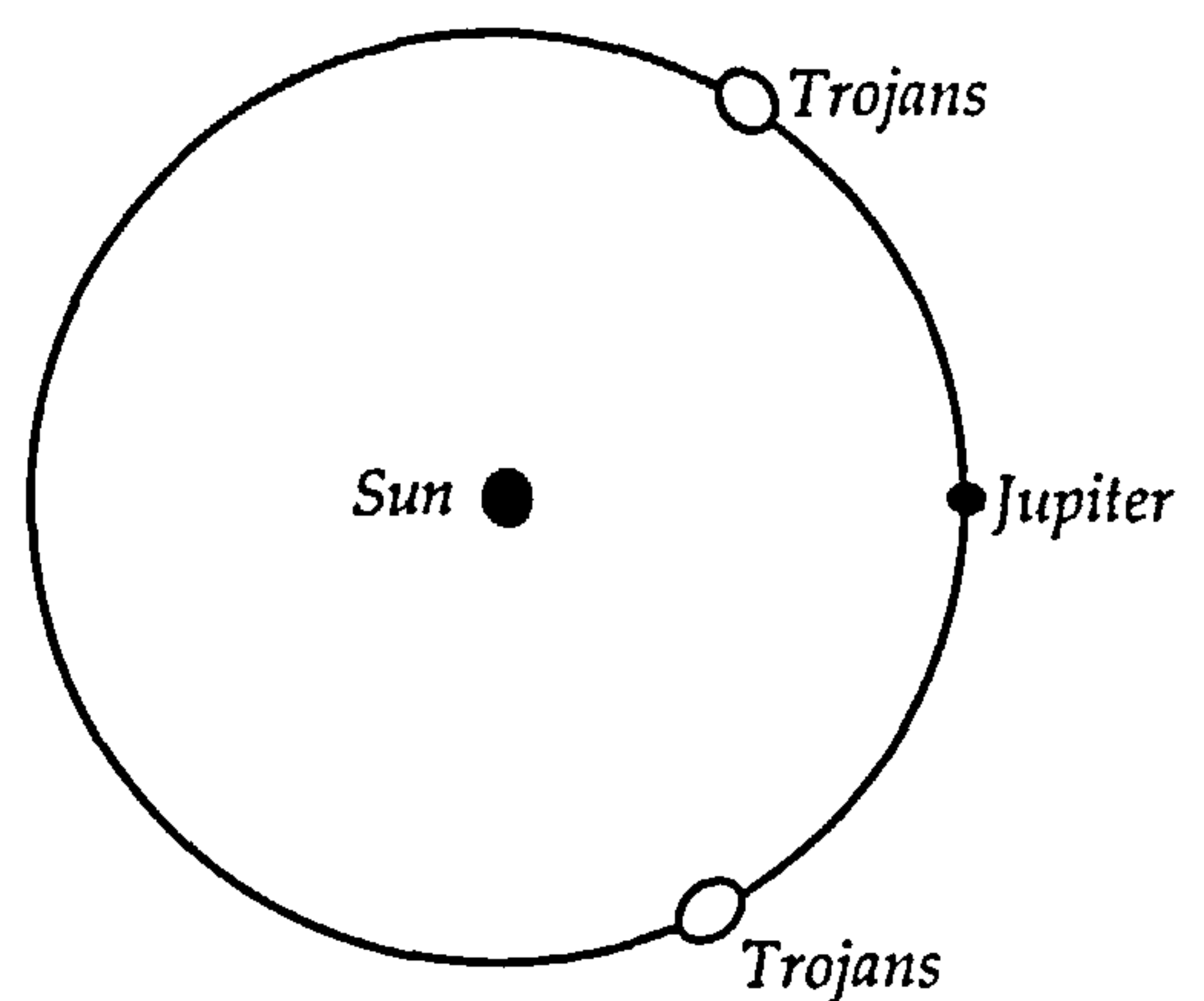


FIG. 2.3.ii. Jupiter's orbit and the Trojan asteroids

From a physical point of view, the libration points are the points where the forces acting on the third body in a rotating system are balanced and so there is no motion relative to the rotating system, and only the gravitational and centrifugal forces have to be considered. Lagrange proved the existence of triangular equilibrium points in the Sun-Jupiter system, and in so doing predicted the presence of the Trojan asteroids (FIG 2.3.ii), observational verification of which was not made until 1906 when Max Wolf discovered Achilles.

#### 2.3.4 "Small divisors"

In 1785 Laplace announced the resolution of several of the outstanding anomalies in the theory of the solar system, one of which concerned the observed deviations from



Keplerian orbits of the planets Jupiter and Saturn. He had discovered a long-period inequality in the motions of these two planets which was due to terms of the third order in the eccentricities.

In planetary perturbation theory the disturbing function can be expanded in a series of periodic terms which when integrated produces terms of the form

$$\frac{A_{jk}}{(jn_1 + kn_2)} \sin [(jn_1 + kn_2)t + B_{jk}]$$

where  $j, k$  are integers,  $n_1, n_2$  are the mean motions of the planets,  $A$  and  $B$  are constants, and the order of magnitude of  $A$  diminishes rapidly as  $j$  and  $k$  increase, and hence these terms arise in the expressions for the Keplerian elements. If the mean motions are commensurable then terms with argument  $[(jn_1 + kn_2)t + B]$  contribute to the secular term, but if they are incommensurable then the denominator can become arbitrarily close to zero. In practice, of course, the mean motions are determined by observation and given to a certain number of significant figures and hence it is always possible to find values for  $j$  and  $k$  so that the denominator is arbitrarily small. Nevertheless, the problems only really arise when the mean motions are not only incommensurable but can be closely approximated by the ratio of two small integers, since in this case the amplitude  $A$  remains large. It is this situation which gives rise to the phenomena known as the effect of *small divisors*, a notorious problem in celestial mechanics. Put in more general dynamical terms the problem of small divisors translates into the problem of resonance caused by near commensurability of the natural frequencies.

In the case of Jupiter and Saturn, the mean motions are approximately in the ratio of 5:2, and the expansion of the disturbing function gives a term with  $j = -2$  and  $k = 5$  in its argument which is of the order of three in the eccentricities. Although the value of the term in the disturbing function is very small, its effect becomes very prominent in the perturbation of the mean longitude since this contains the square of its reciprocal. Furthermore, the period of the perturbation is  $2\pi/(jn_1 + kn_2)$ , hence the term *long-period* inequality. In this particular case the period is approximately 900 years.

### 2.3.5 The restricted three body problem

As previously mentioned, the restricted formulation was first proposed by Euler in [1772]. An important insight into this formulation was provided by Jacobi [1836] who showed that it could be represented by a system of fourth order differential



equations, one solution of which (the Jacobian integral) could be found. G. W. Hill, the American mathematician and astronomer, was the first to show that an important application of the Jacobian integral is in establishing the regions of motion for the planetoid [1878]. Hill's idea was later used to great effect by George Darwin [1897] in his quantitative investigations into periodic orbits.

### 2.3.6 Series representation

By the middle of the 19th century, it was clear that the possibility of finding a closed solution to the problem was becoming increasingly unlikely. Consequently, the objective became to improve the approximations which resulted from the solution of the differential equations being given as infinite series. This involved attempting to eliminate the *secular* terms, that is those terms which increase or decrease indefinitely with time (the existence of which ultimately leads to an entirely new configuration of the system), in order to try to confine the expansion to series in which the time only occurs within the arguments of the periodic terms. (It is easy to see that the presence of secular terms can be extremely misleading: for example, if the true solution contains a term involving  $\sin at$ , then the series will contain terms in odd powers of  $at$ .)

However, in conjunction with the progress being made, a concomitant problem evolved. The people who were working on the topic were primarily astronomers rather than mathematicians and as such were interested in numerical rather than theoretical research. This variance between the two disciplines resulted in a different understanding of the meaning of the word convergence. For the astronomers a series was considered to be convergent if all the terms they calculated decreased rapidly, despite having no knowledge of the behaviour of subsequent terms which, in the long term, might or might not decrease. While for the mathematicians, for a series to be convergent it had to be rigorously proved to be so. Later it became clear that in general the series being proposed as solutions were not convergent in a rigorous mathematical sense.

For all practical purposes the calculation of the first terms provided a very satisfactory approximation, but if the series were intended for the establishment of rigorous theoretical results, such as the question of the stability of the solar system, the divergence of the series posed a serious problem. Furthermore, if the series were divergent then, in general, they were not capable of providing an arbitrarily close approximation. Poincaré, while giving due credit to the value of the results of the

astronomers - he singled out the achievements of Delaunay, Lindstedt and, in particular, Gylden, as worthy of special mention - was the first to prove the general divergence of their series.

### 2.3.7 Delaunay

Delaunay, using the method of variation of parameters, completed the first total elimination of the secular terms in the problem of lunar theory by forming a purely trigonometric series which formally satisfied the equations of motion [1860, 1867]. Beginning with the three dimensional elliptical restricted three body problem<sup>18</sup>, he completely developed the disturbing function  $R$  up to the seventh order of the small parameters and then repeatedly applied canonical transformations in order to eliminate the more important terms of  $R$ . The number of calculations involved in the project was immense and it took Delaunay over twenty years to complete. He announced an outline of the principle of his method in [1846], but his final results, which occupied two large volumes, did not appear until 1860 and 1867.

Delaunay's variables were three canonically related pairs of orbital elements which gave the equations of motion Hamiltonian form. If  $a$  is the semi-major axis,  $e$  the eccentricity,  $i$  the inclination of the orbit to a fixed plane,  $\mu$  the sum of the masses of the bodies whose relative motion is being considered,  $l$  the mean anomaly<sup>19</sup>,  $g$  the angular distance of the lower apsis from the ascending node,  $h$  the longitude of the ascending node, then putting  $L = \sqrt{\mu a}$ ,  $G = L\sqrt{1 - e^2}$ ,  $H = G\cos i$ , and  $F = R + \frac{\mu^2}{2L^2}$ , the equations are<sup>20</sup>

$$\begin{aligned} \frac{dL}{dt} &= \frac{\partial F}{\partial l}, & \frac{dG}{dt} &= \frac{\partial F}{\partial g}, & \frac{dH}{dt} &= \frac{\partial F}{\partial h}, \\ \frac{dl}{dt} &= -\frac{\partial F}{\partial L}, & \frac{dg}{dt} &= -\frac{\partial F}{\partial G}, & \frac{dh}{dt} &= -\frac{\partial F}{\partial H}. \end{aligned}$$

The Hamiltonian  $F$  was expanded as an infinite series (Delaunay's expansion contained 320 terms) and, using successive canonical transformations, the important periodic terms of the disturbing function  $R$  were eliminated term by term and the equations solved.

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<sup>18</sup> The two primaries describe elliptic orbits, while the motion of the planetoid does not take place in the plane defined by the motion of the primaries.

<sup>19</sup> The mean anomaly is the angle the radius vector would have described if it had been moving uniformly with average speed  $2\pi/\text{period}$ .

<sup>20</sup> For a complete derivation of the equations see Hill [1876].

Although the method was accurate up to one second of arc, its practical use was hampered by the progressively increasing complexity of the expressions involved, combined with the slow convergence of its series. Nevertheless, since its publication Delaunay's formulation in terms of canonical systems of elements has been recognised as one of the important landmarks in the analytical development of the lunar theory. In particular it was admired by Hill, whose own research was to provide inspiration to Poincaré, and whose work is discussed at the end of this chapter. In the introduction to Hill's collected works Poincaré [1905] described Delaunay's use of canonical variables in perturbation theory as the greatest contribution to celestial mechanics since Laplace.

### 2.3.8 Gylden

1881 saw the publication of the first of a long series of papers by Hugo Gylden, the Director of the Observatory at Stockholm, in which he developed his theory of absolute orbits for calculating the motion of planetary bodies<sup>21</sup>. These papers, founded on the use of elliptic functions, culminated in 1893 in the publication of the first volume of what was intended to be a three volume work devoted to the theory, the remaining volumes of which Gylden never completed due to his death in 1896<sup>22</sup>.

Gylden's system consists of the sun and two planets, of which one planet is designated the *disturbing* planet while the other is designated the *disturbed* planet. The differential equations which represent the motion of the disturbed planet are solved by means of sums of periodic terms whose arguments are linear functions of a quantity which Gylden called the planet's true longitude. The terms which vanish when the disturbing mass is equal to zero are called *coordinated* terms and are equivalent to the *periodic inequalities* of the classical theory. Those which do not vanish when the disturbing mass is equal to zero but coalesce with those which represent the elliptic motion of the disturbing planet around the sun are called *elementary* terms and correspond to the *secular inequalities* of the classical theory. Those which have the disturbing mass in the denominator of their coefficients are called *hyper-elementary* (they do not occur in the final result);

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<sup>21</sup> For full references to Gylden's papers see Whittaker [1899].

<sup>22</sup> A second volume of Gylden's work on planetary theory edited by Bäcklund with assistance from Sundman and von Zeipel was published in 1908. See Marcolongo [1919, 72].



while those of long period (which occur when the period of two planets are nearly commensurable) are called *semi-elementary* or characteristic terms.

If in the expression for the coordinates, all the coordinated terms are removed, then the modified expression, which will only contain elementary terms, will define a new orbit very close to the true one (the order of difference between them being of the same magnitude as the disturbing forces) and this new orbit is called the *absolute orbit*. The solution of the differential equations is obtained by substituting expansions of the disturbing function into the differential equations and integrating. The six arbitrary constants of integration are the elements which fix the absolute orbit of the disturbed body.

Using the planet's longitude as the independent variable throughout the integrations involves a large number of complicated transformations and these, combined with the necessity of keeping the elementary and non-elementary terms separate, mean that the whole process is extremely complex. So much so that Hill, when writing about the method, said:

*"A degree of complexity is thus imparted to the subject, which makes it difficult to see when one has really gathered up all the warp and woof of it. Professor Gyldén has nowhere removed the scaffolding from the front of his building and allowed us to see what architectural beauty it may possess; ... The advantages claimed for the method are that it prevents the time from appearing outside the trigonometrical functions, and that it escapes all criticism on the score of convergence. The first is readily conceded, but many simpler methods possessing this advantage are already elaborated, and it is not clear that the second ought to be granted. No completely worked out example of the application of this method has yet been published. The great labor involved will naturally deter investigators from employing it."*  
[1896a, 131].

Since Gyldén's constants of integration represented hitherto undefined quantities, an added difficulty in comprehension was provided by the new terminology which Gyldén invented to describe them.

With regard to the convergence of the series, in order to counteract the problem of small divisors (divisors of the order of the planetary masses), Gyldén modified the coefficient of the first power of the dependent variable through the incorporation of a function which he called the *horistic* (or limiting) function [1893]. Nevertheless,

despite his efforts which resulted in several lengthy papers, he did not in fact prove convergence, as Poincaré [1905a] was later to show.

However, prior to Gylden's introduction of the horistic function in 1893, the question of convergence of Gylden's series had already been a cause for contention between Gylden and Poincaré. In 1889 Gylden on learning of the content of Poincaré's Oscar prize-winning paper had immediately claimed priority with his own paper of 1887. The controversy which quickly became a subject for public debate is discussed in 6.3.

In spite of their differences, Poincaré was a great admirer of Gylden's methods, and publicly acknowledged his contribution to celestial mechanics on several occasions. For evidence of which, there is no need to look further than the introductions to both the first and second volumes of his *Méthodes Nouvelles*. Poincaré opened the latter, which was essentially an exposition of the work of astronomers, with the paragraph:

*"The methods which I want to discuss in this second volume are due to the efforts of a great number of contemporary astronomers, but it is the methods of Gylden which are the best and to which I shall give the greatest exposure."* [MN II, v].

### 2.3.9 Lindstedt

Another Swedish astronomer, Anders Lindstedt, was apparently prompted to get involved in the search for trigonometric series solutions in order to simplify the work of his compatriot Gylden<sup>23</sup>. Lindstedt's first paper to attract attention appeared in 1883 and introduced a method for integrating an important class of differential equations which frequently occur in perturbation theory in celestial mechanics [1883]. These equations, which essentially represent a perturbed harmonic oscillator, and had arisen in Gylden's researches, were of the general form

$$\frac{d^2x}{dt^2} + n^2x = \alpha\Phi(x, t)$$

where  $\alpha$  is very small and  $\Phi(x, t)$  is a function expanded in powers of  $x$  with coefficients which are periodic functions of  $t$ . Subject to certain restrictions concerning the symmetry of the coefficients (Lindstedt had thought that the perturbing forces should be either odd or even functions of the angle variable

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<sup>23</sup> See von Zeipel [1921, 328].



involved), the method avoids secular and mixed secular terms and shows how the equations can be satisfied by  $x$  expanded as a trigonometric series.

Later the same year, Lindstedt applied his method in order to find trigonometric series solutions for the three body problem [1883a]. He began from the equations in Lagrange's 1772 paper and, making the assumptions that the eccentricities, the ratio of the radius vectors and the inclinations of two of the bodies were sufficiently small, he reduced the system to four second order differential equations, which he then solved by successive approximations, eventually eliminating all the secular terms so that the time only appeared in the arguments of the periodic functions. This gave him the coordinates of the three bodies as trigonometric series of four arguments, each of which was a linear function of time. He then simply assumed that it was possible to choose the constants of integration in such a way that convergence was assured.

Lindstedt was not the first astronomer to provide such a series, chronologically the credit is due to Simon Newcomb [1874] who proved that the differential equations describing the motion of the planets could formally be satisfied by trigonometric series. However, since Lindstedt's method was the less complex of the two and consequently capable of greater generalisation, it became the more widely known. Lindstedt's method was also considerably simpler than Gylden's, although it was also correspondingly less powerful as the increased simplification brought with it an accompanying reduction in the range of its application.

#### 2.3.10 Hill

Shortly before Gylden and Lindstedt started publishing in earnest, there appeared two papers which were to have a profound effect on the future development of celestial mechanics in general and the three body problem in particular. In 1877 Hill privately published an exceptional paper on the motion of the lunar perigee [1877]. In it appeared the first new periodic solutions to the three body problem since Lagrange's discovery of special periodic solutions in 1772. In the following year, 1878, the first issue of the *American Journal of Mathematics* contained another important paper by Hill on the lunar theory which included a more complete derivation of the periodic solutions [1878]. These two famous papers have long been acclaimed for the originality and elegance of the mathematical methods they contain, as well as for their substantial contribution to the progress of celestial mechanics.

The paper of 1877 is plainly a logical continuation of the researches contained in the paper of 1878, and it is unclear why Hill should have chosen to publish them in apparently reverse order. Furthermore, since Hill's work was characterised by modesty of style and brevity of expression, to grasp an understanding of the innovatory ideas contained in the first paper without the support of the second paper would have added an extra difficulty. With regard to this point, Ernest Brown, the eminent astronomer who later took up and continued Hill's work, providing an almost exhaustive treatment of the lunar problem, remarked:

*"Hill was not a great expositor; even for those familiar with the subject, his work is often difficult and sometimes obscure ... he is rarely anything else but concise."* [1916, 293].

Nevertheless, Hill's discovery and use of a new class of periodic solutions was a turning point in the history of the three body problem and dynamical systems in general. So original was Hill's approach that in the introduction to Volume I of the *Méthodes Nouvelles*, Poincaré made the observation:

*"In this work ... it is possible to see the germ of most of the progress that science has since made."*,

and Whittaker was prompted to suggest that the publication of Hill's paper in 1877 signified:

*"... the beginning of the new era of Dynamical Astronomy."* [1899, 130].

The prevailing influence on Hill's work came from Delaunay, whose two volume work provided him with a major stimulus. He professed considerable admiration for Delaunay's method and, apart from employing it in the lunar theory, outlined ways in which it could be applied to other problems. Hill was also considered to be one of the few people who fully comprehended the work of another lunar theorist, the German astronomer P. A. Hansen, whose main work concerning the motion of the moon became the basis for extensive tables of lunar motion, published at the expense of the British Government in 1857.

The problem that concerned Hill in the paper of 1877 was the discrepancy between the theoretically computed values for the motion of the lunar perigee and those values derived from observation. Did this discrepancy arise because the approximations were not continued far enough or was it because there were other forces acting on the moon which had not yet been considered? Since the question

could only be answered if a limit could be put on the error incurred by the approximation method, Hill set out to compute the value of the motion of the lunar perigee:

*"... so far as it depends on the mean motions of the sun and moon, with a degree of accuracy that shall leave nothing further to be desired."* [1877, 1].

Hill's innovation was to abandon the idea of using an elliptic orbit for the Moon as a starting point, i.e. abandon the idea of neglecting the action of the sun as a first approximation, and instead begin with a circular orbit. He then used the effect of solar perturbation to vary the circular orbit before varying it again by the introduction of the eccentricity of the lunar orbit. In essence, he began by solving a modified version of the restricted three body problem before making a variation in order to attempt the general problem. Previous efforts had always begun by first solving the two body problem and then making the appropriate variation.

Hill had recognised that of the five parameters which were involved in Delaunay's series for the longitude, latitude and parallax, the one whose expansion provided the slowest rate of convergence in the series was the ratio of the mean motions of the sun and the moon. This gave him the idea of beginning his attempt on the problem by neglecting the other lunar inequalities and finding the series in powers of this one alone. He embarked on this first stage of the problem in [1878], while he dealt with the second stage, which was to take account of the lunar eccentricity, in [1877]. His original intention had been to treat all five different classes of lunar inequalities (as listed by Euler in [1772]), i.e. the two mentioned above, the lunar inclination, the solar eccentricity and the solar parallax, but shortly after [1877, 1878] had been published, Simon Newcomb, the director of the American Ephemeris, persuaded Hill to become involved in the theories of the motion of Jupiter and Saturn, as a consequence of which he did not complete his original programme.

By initially only considering the ratio of the mean motions of the sun and the moon, Hill substantially simplified the differential equations. For, as pointed out by Poincaré [1905], by excluding the solar eccentricity and parallax, in Hill's formulation the sun can be said to describe a circle with a large radius. Hill's second insight was to choose rectangular coordinates uniformly rotating with the angular velocity of the sun, so that the time no longer appeared explicitly in the



equations<sup>24</sup>. This choice of coordinate system was in contrast to the prevailing methods which invariably involved polar coordinates.

Using this formulation, the expressions for the coordinates of the moon referred to the rotating axes can be represented by Fourier series (with undetermined coefficients) and are periodic. The solution is then obtained by substituting the Fourier series into the differential equations and determining the coefficients as functions of the parameter  $m$  which depends on the ratio of the mean motions of the sun and moon. In order to avoid the multiplication of trigonometric functions, and to enable a reduction to algebraic form, Hill took a further innovatory step and introduced complex variables. Substituting the complex variables into the differential equations gave rise to an infinite system of algebraic equations from which the coefficients could be determined in terms of  $m$ , either algebraically or numerically. One particular advantage of Hill's method was the ease with which the approximation could be extended as far as desired.

Hill, having realised that the periodic solution he had found was of interest beyond its application to the lunar problem, varied the value of  $m$ , by taking moons of 10, 9, ... , 3 lunations (the time from one new moon to another) in the periods of their primaries, and obtained a family of different periodic solutions which he then studied in detail. For moons of longer lunation, he found that the method was not practicable and resorted to mechanical quadrature. His final periodic solution involved a moon with a cusped orbit, which he called the moon of maximum lunation (FIG. 2.3. iii)<sup>25</sup>. However, this attribution was shown to be mistaken first by Adams and then by Poincaré who subsequently proved that the cusp was in fact succeeded by looped orbits (FIG. 2.3.iv)<sup>26</sup>.

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<sup>24</sup> An idea for which Hill acknowledged his debt to Euler.

<sup>25</sup> Hill [1878, 331-335].

<sup>26</sup> Hill [Collected Works I, 326] and Poincaré [MCI, 104-109].

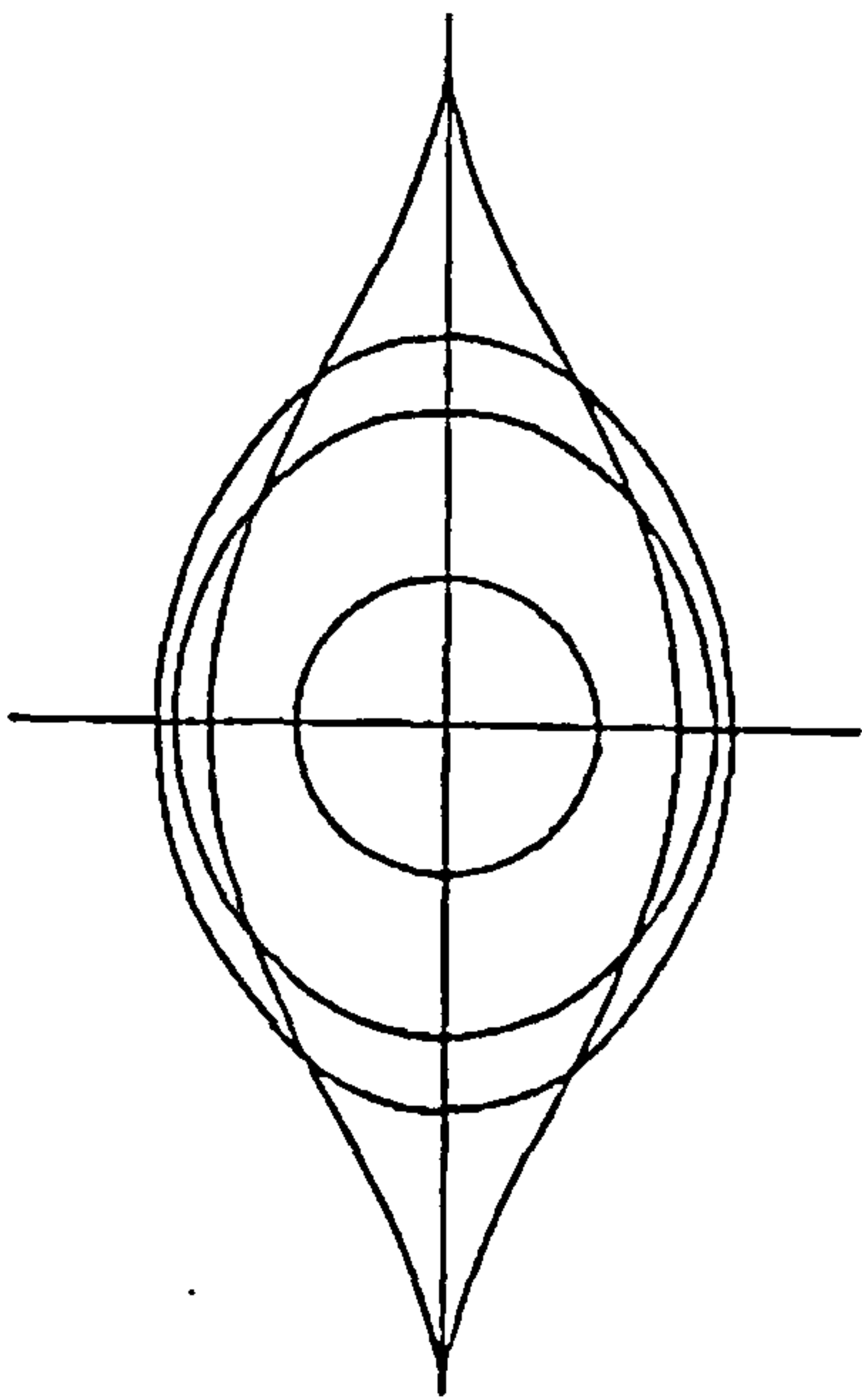


FIG. 2.3.iii.

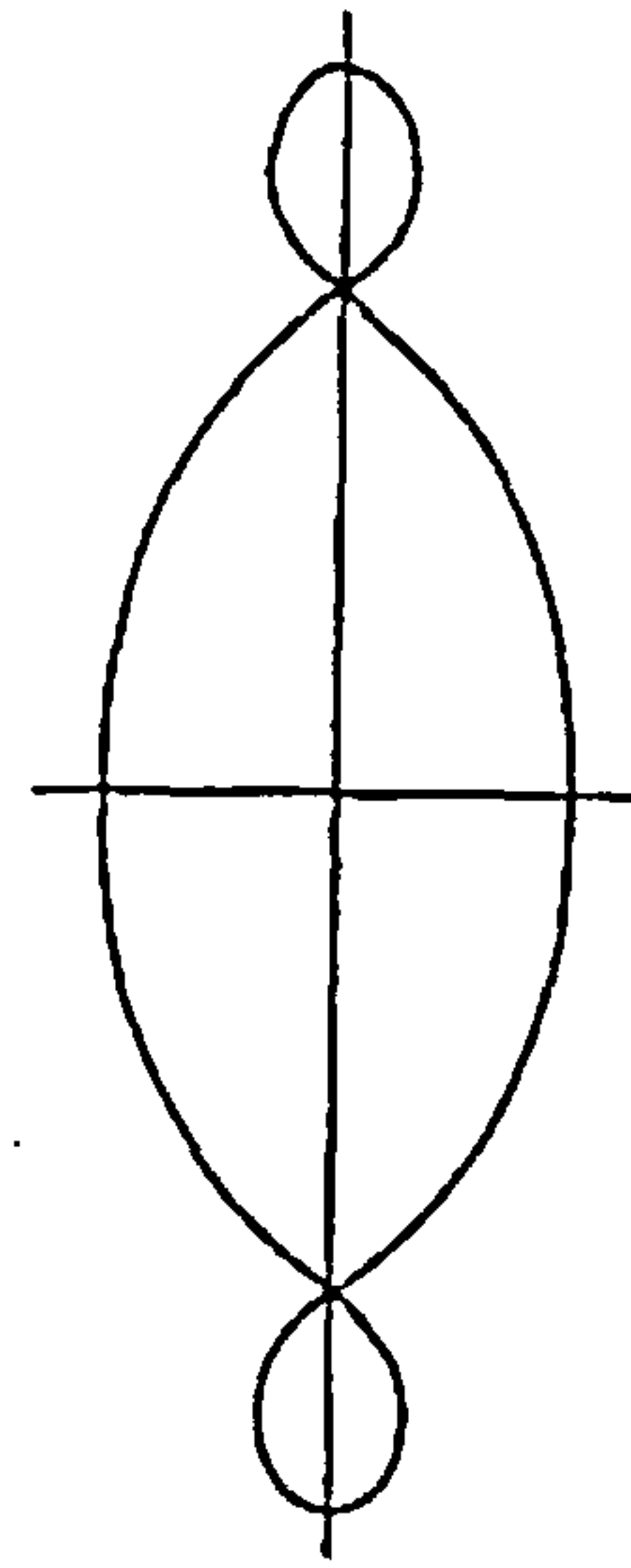


FIG. 2.3.iv.

Another fundamental idea included in the 1878 paper was that of curves of zero velocity which Hill used to show that the moon could never escape from its orbit around the earth. He derived this property by considering Jacobi's integral which gives the square of the velocity relative to the moving axes. Since this quantity is necessarily positive, equating it to zero gives the equation of a surface which separates space into parts: those in which the velocity is real and those in which the velocity is imaginary. Since the surface consists of various curves and folds, it is hard to understand in detail. However examination of it does reveal certain limitations on the motion of the moon's orbit, in particular it provides an upper limit for its radius, and as a result certain conclusions can be drawn about the stability of the motion. This idea was taken up and used to great advantage by Darwin [1897] and is discussed in 8.4.1 in relation to Darwin's work.

In [1877], having determined a periodic solution (now called the intermediate orbit or variational curve), Hill wrote the equations of variation from this periodic solution taking account of the first power of the lunar eccentricity. This led him to a fourth order system of linear differential equations with periodic coefficients. By the use of known integrals combined with elegant transformations of his own he reduced the system to a single linear differential equation of second order [1877, 246],

$$\frac{d^2 w}{dt^2} + \Theta w = 0 \quad (2.3.i)$$



where  $w$  is the normal deviation of the moon from the intermediate orbit and  $\Theta$  only depends on the relative position of the moon with reference to the sun.  $\Theta$  can, therefore, be expressed as a Fourier series

$$\begin{aligned}\Theta &= \theta_0 + \theta_1 \cos 2t + \theta_2 \cos 4t + \dots \\ &= \sum_j \theta_j \zeta^{2j}\end{aligned}$$

where  $\zeta = e^{it}$  and  $\theta_i = \theta_{-i}$ , and so equation (2.3.i) can be written

$$\frac{d^2 w}{dt^2} + w \sum_{j=-\infty}^{\infty} \theta_j \zeta^{2j} = 0. \quad (2.3.ii)$$

and this equation is now generally known as Hill's equation<sup>27</sup>.

If  $\theta_0$  is much greater than  $\theta_i$  ( $i = 1, 2, \dots$ ) then an approximate form of equation (2.3.i) is

$$\frac{d^2 w}{dt^2} + \theta_0 w = 0$$

which has the particular solution

$$w = K\zeta^c + K'\zeta^{-c},$$

where  $K$  and  $K'$  are arbitrary constants and  $c = \sqrt{\theta_0}$  is the ratio of the lunation to the anomalistic month<sup>28</sup>. When the additional terms of  $\Theta$  are considered this has the effect of modifying the value of  $c$  and adding to  $w$  extra terms of the general form  $A\zeta^{c+2i}$ , and a particular solution of equation (2.3.i) is therefore

$$w = \sum_{i=-\infty}^{\infty} b_i \zeta^{c+2i}$$

where each  $b_i$  is a constant coefficient and the value of  $c$  is to be determined. Substituting this value of  $w$  into equation (2.3.ii) and setting the coefficient of each power of  $\zeta$  equal to zero generates a doubly infinite system of algebraic equations.

These equations can then be used to determine the ratios of all the coefficients  $b_i$  to one of them  $b_0$  which can be regarded as the arbitrary constant. If all the  $b_i$  are

<sup>27</sup> Hill's equation can also be considered as a generalised form of Mathieu's equation

$$\frac{d^2 w}{dt^2} + (a + b \cos 2t)w = 0.$$

See Whittaker and Watson [1927, Chapter XIX].

<sup>28</sup> The time taken for the moon to pass from perigee to perigee.

eliminated from these equations, then this gives rise to an infinite determinant involving  $c$ , denoted  $\mathcal{D}(c)$ , which, when equated to zero, determines  $c$ .

From this determinant Hill derived an expression for  $c$  in terms of the parameter  $m$ . In the first approximation, the value of  $c$  is  $\sqrt{\theta_0}$ , while in the second  $c = \sqrt{1 + \sqrt{(\theta_0 - 1)^2 - \theta_1^2}}$ .

This is a remarkably simple expression for an approximate value of the motion of the moon's perigee and one which gives a value only about 1/60 in excess of that given by observation, the difference being mainly due to the neglect of the lunar inclination.

By performing further manipulations and transformations, Hill reduced the determinant to an infinite series in what he assumed to be a convergent form. Hill's final expression for the determinant turned out to be equivalent to starting with the equation  $\mathcal{D}(c) = 0$ , assuming  $c = \sqrt{\theta_0}$  as the first approximation, and then expanding the expression  $\sin^2\left(\frac{\pi}{2}c\right)$  in powers and products of the coefficients  $(\theta_1, \theta_2, \dots)$ .

Taking this expression up to terms of order twelve in  $m$  Hill achieved a value for  $c$  which was accurate up to the 15th decimal place. By contrast, he showed that Delaunay's method calculated up as far as terms in  $m^9$  produced a solution which was not even correct up to four significant figures, and, furthermore, he estimated that Delaunay's series would have to be prolonged up to terms in  $m^{27}$  to obtain a result of comparable accuracy to his own.

Immediately after the publication of Hill's 1877 paper, J. C. Adams, the English astronomer who had been among the first to predict the presence of the planet Neptune from unexplained perturbations in the orbit of Uranus<sup>29</sup>, communicated a brief paper to the Royal Astronomical Society [1877] in which he remarked that many years previously (1868) his own work on the lunar theory had followed a very similar course. Although his investigations had dwelt on the motion of the moon's node, and he had not used rotating rectangular coordinates and so had been unable to find the rapidly converging series which Hill had deduced, he had found the same infinite determinant. Unfortunately he had not published his work as he had

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<sup>29</sup> Adams' predictions, originally made in 1845, were contemporaneous with those of the French astronomer Le Verrier and prompted a bitter priority dispute. The two astronomers took almost no part in the controversy, the feud being largely conducted by English and French journalists.

thought it insufficiently complete, and he did admit that he considered his method to be much less elegant than that proposed by Hill.

Although the idea of an infinite determinant had occurred elsewhere prior to Hill's 1877 paper, it appears not to have been widely known<sup>30</sup>. Hill himself had not previously encountered it and the general possibility of using it does not seem to have been considered until the publication of Hill's paper. However, there was a problem with Hill's idea in that his results depended on the convergence of the determinant, a property which he did not prove. He was well aware of the defect, as he made plain in the introduction to [1878]:

*"I regret that, on account of the difficulty of the subject and the length of the investigation it seems to require, I have been obliged to pass over the important questions of the limits between which the series are convergent, and of the determination of the superior limits to the errors committed in stopping short at definite points."*

The incompleteness of Hill's result meant that the theory of infinite determinants was not immediately taken up as mathematical technique. Almost ten years later the missing convergence proof was supplied by Poincaré [1886d], and from then on the power of Hill's theory began to be recognised. Hill's position is now secure as the founder of the theory of infinite systems of linear equations, his research in this field ranking alongside his contribution to dynamical astronomy as one of his principal achievements.

Despite the results he obtained in [1877, 1878], it was some time before Hill's work received the recognition it deserved. In 1888 when Darwin began to study Hill's papers he remarked that although they seemed to be very good scarcely anybody knew about them<sup>31</sup>. This could have been partly due to Hill's nationality (America was only beginning to emerge as a mathematical force in the 1870s) as well as his own rather solitary disposition. On the other hand there was also the comparative inaccessibility of his particular style of mathematics combined with the deficiency

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<sup>30</sup> Infinite determinants first appear in the researches of Fürstenau [1860]. See Whittaker and Watson [1927, 36].

<sup>31</sup> Brown [1916a, lii].

in his theory of infinite determinants. Nevertheless, given the nature of Hill's research it is still surprising to find that his work took so long to be appreciated<sup>32</sup>.

However, from 1886 Hill's work did reach a wider audience, for in that year not only did Poincaré prove the convergence of the infinite determinant but also Hill's 1877 paper was republished in *Acta* (the first paper in English to appear in the journal). And it was not very much later that through Poincaré's theory of periodic solutions that the impact of Hill's work on the progress of dynamical astronomy and the three body problem began to be felt.

Poincaré clearly respected and admired Hill's work, and Hill's new perspective on three body problem, encapsulated by his introduction of a new class of periodic solutions, was certainly an important source of inspiration for him. Darwin [1900] even went so far as to propose that it may have been as a consequence of Hill's work that Poincaré was prompted to embark on his work in celestial mechanics. Furthermore, there is the evidence of Poincaré's continued interest in Hill's research, both astronomical and mathematical, as exemplified by his improvements and extensions to Hill's results, and it has been said that when Poincaré travelled to the United States in 1904, the one person he made a point of visiting was Hill<sup>33</sup>.

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<sup>32</sup> For example, as Wintner observed [1941, 440], the significance of Hill's periodic solutions escaped Heinrich Bruns who reviewed [1878] for the *Jahrbuch über die Fortschritte der Mathematik* 10, 782.

<sup>33</sup> Sternberg [1969, I, 129].



### 3. Poincaré's Related Work before 1889

#### 3.1 Introduction

Poincaré's 1890 memoir on the three body problem brought together a whole host of mathematical ideas and techniques which he had developed over the previous decade. Almost from the beginning of his academic career he had been concerned with the fundamental problems of celestial mechanics and many of the papers which he published during the 1880s relate to his interest in the subject. These include several of a broad theoretical nature as well as those in which he responded to explicit questions of dynamical astronomy.

Firstly, at the very backbone of [P2] is Poincaré's acclaimed memoir on curves defined by differential equations. In this memoir, published in four parts between 1881 and 1886, he initiated the qualitative theory of differential equations [1881, 1882, 1885, 1886] in the real domain. These papers are full of new ideas many of which form the basis for results in [P2]. The three body problem features prominently in these pages and Poincaré is quite clear about its motivating role in the development of his ideas.

Secondly, there are the papers in which he addressed either a particular aspect of the three body problem or a connected problem of celestial mechanics, and in some cases these involved developing the work of another mathematician or astronomer. All of these papers are comparatively short, none of them approaching the scale of



[P2], but in them can be found his initial researches into periodic solutions and his early investigations into the convergence of trigonometric series used in celestial mechanics.

Finally, there are other papers in which Poincaré developed ideas and techniques which he used in [P2] but which can be considered to have been generated in a more general context. Notable amongst these are his thesis [1879] and his paper on asymptotic series [1886a].

### 3.2 The qualitative theory of differential equations

During the 18th century, the realisation that it was not possible to integrate the majority of differential equations using known functions had increasingly led to the study of the properties of the differential equations themselves<sup>1</sup>. By the middle of the 19th century this practice had become firmly established, and by the 1860s, the success of complex function theory, meant, with a few isolated exceptions, that the emphasis was firmly placed on the investigation of the behaviour of the function in the neighbourhood of a point in the plane. Thus at the beginning of the 1880s when Poincaré began his memoir on the analysis of functions defined by differential equations, research was, in effect, centred on studying the local properties of a solution to a differential equation. Poincaré's approach was radically different. He looked beyond the confines of a local analysis and brought a global perspective to the problem undertaking a qualitative study of the function in the whole plane<sup>2</sup>.

In [1880] Poincaré stated that his objective was to provide a *geometric* study of the solution curves of a first order differential equation, and indeed it was his geometrical insight which became one of the the hallmarks of his work on differential equations. As Gilain [1991] and Gray [1992] have argued, what was new and important was Poincaré's idea of thinking of the solutions in terms of curves rather than functions and it was this which marked a departure from the work of his predecessors whose research had been dominated by power series methods.

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<sup>1</sup> See Kline [1972, Chapter 21].

<sup>2</sup> As Gilain [1991] in an excellent article has pointed out, Poincaré's qualitative approach to differential equations was not entirely without precedent. In 1836 Charles Sturm made a qualitative study of second order linear differential equations in the real domain. However, the two approaches were set in quite different contexts. For a comparison between them see Gilain [1991, 224-225]. Sturm's papers are treated in detail in Lützen [1990, 435-446].

Importantly, Poincaré's interest in differential equations was not only driven by an intrinsic interest in the equations themselves. He also had a particular interest in some of the fundamental questions of mechanics, most notably the question of the stability of the solar system, and he recognised the necessity of a qualitative theory of differential equations for furthering the understanding of this type of question. In this context therefore he saw it as important to consider the global properties of *real* as opposed to complex solutions. His attention to the real case marked another notable departure from the work of earlier investigators which had been concentrated on the complex case.

Although the memoir was published as four papers, in terms of content it divides into two parts. The first two papers, which constitute the first nine chapters, were published in consecutive issues of Liouville's Journal in 1881 and 1882 and are devoted to the study of the simplest type of differential equation. The third and fourth papers which appeared in 1885 and 1886, are concerned with equations of higher order and degree.

### 3.2.1 Papers I and II

Poincaré divided the study of a function into two parts: qualitative - the geometrical study of the curve defined by the differential equation - and quantitative - the numerical calculation of values of the function. He was quite explicit at the beginning of the first paper that his work was going to centre on the first part and that an element of his motivation for a qualitative study was his interest in the three body problem:

*"Moreover, this qualitative study has in itself an interest of the first order. Several very important questions of analysis and mechanics reduce to it. Take for example the three body problem: one can ask if one of the bodies always remains within a certain region of the sky or even if it can move away indefinitely; if the distance between two bodies will infinitely increase or diminish, or even if it will remain within certain limits? Could one not ask a thousand questions of this type which would be resolved when one can construct qualitatively the trajectories of the three bodies? And if one considers a greater number of bodies, what is the question of the invariability of the elements of the planets, if not a real question of qualitative*

*geometry, since to show that the major axis has no secular variations shows that it constantly oscillates between certain limits."*<sup>3</sup>.

This point was later succinctly summarised by Jacques Hadamard when commenting on Poincaré's work:

*"The most important of them (questions of analysis and mechanics) is well known, and its example presents itself with the whole spirit of the progress of astronomy: it is the stability of the solar system. The single fact that this question is essentially qualitative suffices to show the necessity of his point of view."* <sup>4</sup>.

Apart from identifying the question of the stability of the solar system as an essentially qualitative problem, Poincaré had another stimulus for the qualitative consideration of differential equations: the analogy provided by research into algebraic equations. The proven success of qualitative investigations into algebraic equations was to him a clear indication of the potential of this approach.

Following the analogy, he began by constructing the curves defined by the differential equations. His initial researches centered on the simplest case: the construction of the solution curves of the equation

$$\frac{dx}{X} = \frac{dy}{Y} \tag{3.2.i}$$

where  $X$  and  $Y$  are polynomials in  $x$  and  $y$ , and so  $\frac{dy}{dx}$  is given as a single-valued rational function of  $x$  and  $y$ .

Although this equation, by virtue of its simplicity, has no direct application in celestial mechanics, by using it as the foundation for his study, Poincaré provided himself with a basis from which he could extend and elaborate his results to take in more complex systems.

To circumvent the problem that was posed by the difficulty of the construction of curves with infinite branches, Poincaré first projected the plane onto a sphere<sup>5</sup>. The

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<sup>3</sup> Poincaré [1881, 4].

<sup>4</sup> Hadamard [1912, 240].

<sup>5</sup> The projection was made gnomonically, that is the centre of the sphere is the centre of projection. Thus each point on the plane is projected into two points on the sphere and the projection of a straight line is a great circle.

differential equation then associates a determined direction with each point on the sphere and no two solution curves can intercept except at singular points.

Looking for relationships between the different solution curves of the same differential equation, Poincaré began with a local analysis and examined the behaviour of these curves in the neighbourhood of a singular point. Unlike his predecessors Briot and Bouquet who had studied singular points without the constraint of distinguishing between the real and complex case, Poincaré only considered real values. He showed that there were four possible different types of singular points and classified them by the behaviour of the nearby solution curves:

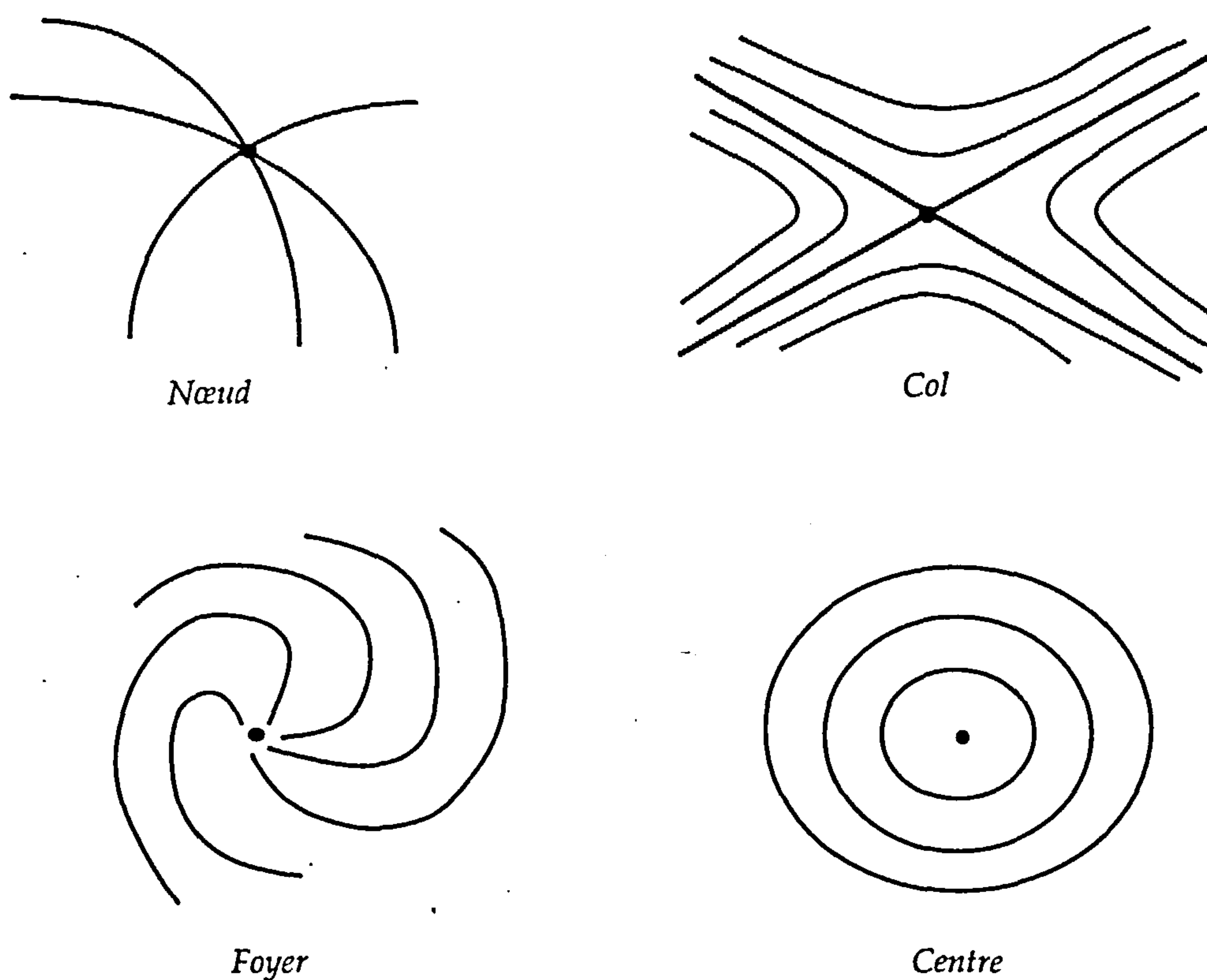


FIG 3.2.i. Singular Points

*nœuds* (nodes) through which an infinite number of solution curves pass; *cols* (saddle points) through which only two solution curves pass, these two curves acting as asymptotes for neighbouring solution curves; *foyers* (foci) which the solution curves approach in the manner of a logarithmic spiral; and *centres* (centres) around which the solution curves are closed enveloping one another.



Having used direct algebraic computation to show that these four types do necessarily exist, he studied their distribution. He found that in the general case only three types prevailed: nodes, cols and foci; centres only arising under very exceptional circumstances. This was an unexpected result since earlier studies of differential equations had shown that in the cases where elementary integration is possible the most usual singular points are nodes, cols and centres.

To describe the nature of a singular point Poincaré introduced the idea of an index which gave a measure of the direction of the flow given by the solution curves about the singular point. Using this idea in relation to a system which could be described by equations (3.2.i) (i.e. for the case which corresponds to a simply connected surface with a single direction associated with each point on it, such as the flow on a sphere) he was led to a relationship between the different types of singular points: the number of nodes  $N$  plus the number of foci  $F$  is equal to the number of cols  $C$  plus two, i.e.  $N + F = C + 2$ , which is now known as the Poincaré index theorem for a flow on a sphere.

He next looked at the behaviour of solution curves beyond the neighbourhood of singular points. Here again the results he found were unexpected.

Since the differential equation assigns a direction at each point of the solution curve, Poincaré took an algebraic curve on the surface and studied the direction of the solution curve at the points where the two curves cross. He called the points where the algebraic curve was tangent to the solution curve points of *contact*. He found that in a great number of cases there existed branches of closed curves (*cycles*) which were nowhere tangent to the solution curves and he called these *cycles without contact*. Knowledge of the presence of these cycles is important because a solution curve cannot meet such a cycle in more than one point (otherwise some solution would have a contact), and so if it crosses such a cycle, it cannot recross it.

Cycles without contact were not the only closed curves which played an important role in Poincaré's theory. Poincaré also found closed solution curves which he called *limit cycles*. These are solution curves which have no singular points and which other solution curves approach asymptotically. The solution curves spiral in towards a limit cycle but never actually reach it. His discovery of their existence originated from his idea of indefinitely following a solution curve in one direction and looking at all the possible outcomes. Engaging in this process led him to the important result that every solution curve which does not end in a node is a cycle or a

spiral. In other words, all solution curves are either closed or, with the exception of those which end in a singular point, they asymptotically approach a limit cycle.

To prove the existence of limit cycles Poincaré considered the consecutive crossing points of a given (unclosed) solution curve  $C$  with an algebraic curve which cut the solution curve in an infinite number of points. To facilitate the handling of these crossing points he introduced the idea of *consequents* (iterates), where if  $M_1$  and  $M_2$  are two such consecutive crossing points he called  $M_2$  the consequent of  $M_1$ . (This was an idea he was to expand and use to particular effect in [P2].) Then, using the fact that no two curves satisfying the equation can intersect (except at a singular point which is excluded), he showed that the successive crossing points must approach a common limiting position, say  $H$ , and since  $H$  is therefore its own consequent, the solution curve through it must be closed and so must be a limit cycle for the given curve  $C$ .

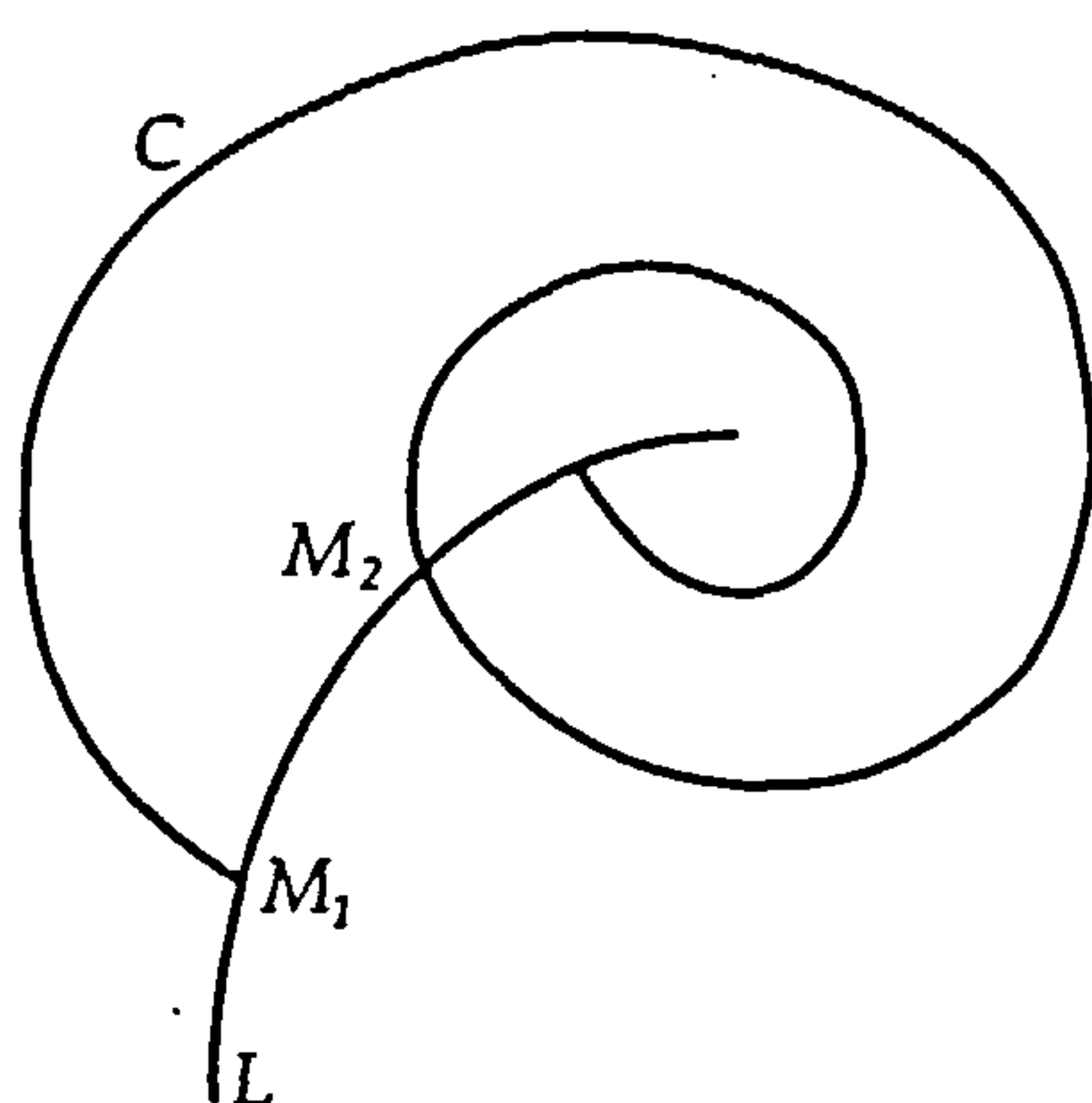


FIG. 3.2.ii. Consequents

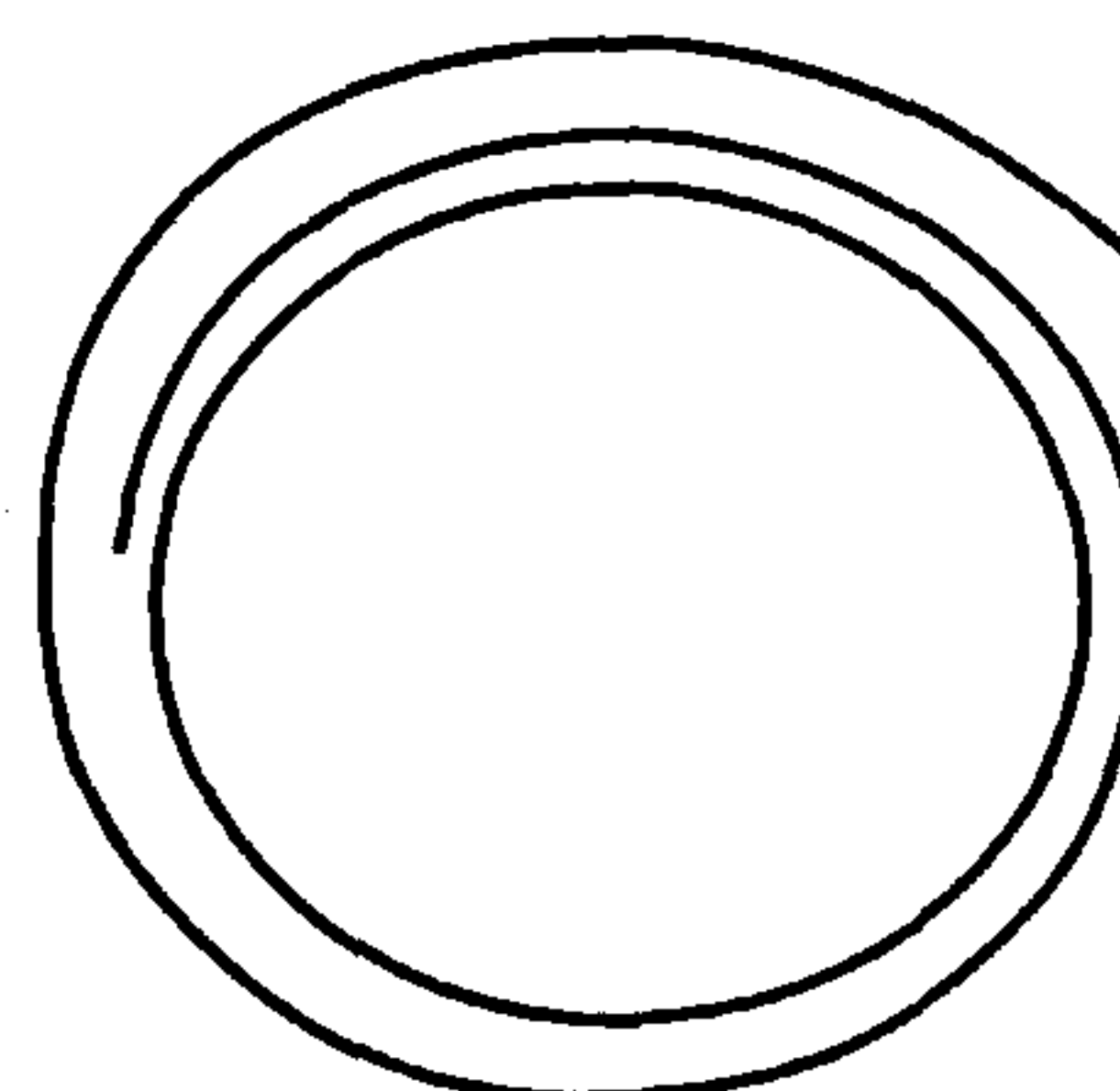


FIG. 3.2.iii. Limit cycle

By obtaining results about the distribution of limit cycles Poincaré began to generate a qualitative description of the flow described by the differential equation. He was able to determine the number of limit cycles within a given region of the sphere and also to find the particular regions in which a given number of limit cycles exist. But it was not until the next paper that he gave a dynamical interpretation of his results.

### 3.2.2 Papers III and IV

Poincaré opened the third paper with a discussion on stability which he began by reenforcing the connection with celestial mechanics:

*"One cannot read the first two parts of this memoir without being struck by the resemblance between the various questions which are treated there and the great astronomical problem of the stability of the solar system. This latter problem is, of course, much more complicated, since the differential equations of the motion of celestial bodies are of much higher order. Furthermore, one meets in this problem a new difficulty essentially different from those which we have had to overcome in the study of first order equations, and I intend to bring it out, if not in this third part, at least in the final part of this work."*<sup>6</sup> [1885, 90].

This is the first occasion on which Poincaré explicitly tackled the question of stability, and in order to do so he began to use the language of dynamics to describe the differential equations. He reformulated equation (3.2.i) as

$$\frac{dx}{dt} = X, \quad \frac{dy}{dt} = Y, \quad (3.2.i')$$

where  $x, y$  are regarded as the coordinates of a moving point, and  $t$  is the time. He said that the orbit of a point was stable if the point returned infinitely often arbitrarily close to its initial position<sup>7</sup>. Although this is essentially an impractical definition of stability in that it allows for intervening oscillations of any magnitude, nevertheless, from a theoretical point of view it does encompass a great degree of flexibility and it allowed Poincaré (and later Birkhoff) to derive some remarkable results.

Poincaré first translated his results concerning cycles without contact and limit cycles into dynamical terms before considering them in the context of stability. In both cases, he showed that their presence is sufficient to indicate instability, although in the case of limit cycles, it is the solution curves which asymptotically approach a limit cycle which are unstable while the limit cycle itself is stable.

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<sup>6</sup> Poincaré tackled the problem of higher order equations in the fourth paper, concentrating on the higher degree case in the third paper.

<sup>7</sup> A definition Poincaré later attributed to Poisson. See [P2, 313].

Poincaré's analysis revealed that in the simple case described by equations (3.2.i) it was generally possible to cover the sphere with an infinite number of closed cycles of both types, and, furthermore, that the number of limit cycles was usually finite. Consequently, he was able to conclude that for this case instability was the rule, stability the exception. The exception occurring when there were no cycles without contact and all the solution curves satisfying the differential equation were closed, mutually enveloping one another, as, for example, in the neighbourhood of a centre.

To study differential equations of first order but of higher degree, i.e. equations of the form

$$F\left(x, y, \frac{dx}{dy}\right) = 0,$$

$F$  being a polynomial, Poincaré adopted a geometric representation. He considered the surface  $S$  with equation

$$F(x, y, z) = 0$$

and investigated the motion of a point upon it, putting the equations of motion into the form

$$\frac{dx}{dt} = X, \quad \frac{dy}{dt} = Y, \quad \frac{dz}{dt} = Z$$

where  $X$ ,  $Y$ , and  $Z$  are polynomials in  $x$ ,  $y$ , and  $z$  such that

$$\frac{\partial F}{\partial x}X + \frac{\partial F}{\partial y}Y + \frac{\partial F}{\partial z}Z = 0.$$

In other words, he studied the solution curves of a vector field tangent to the surface  $S$ . In general, his discussion followed his treatment of the simpler case. He showed that the surface  $S$  was covered by an infinite number of closed curves which were either cycles without contact or limit cycles. He then examined the distribution of singular points and found that the relationship between them was given by

$$N + F - C = 2 - 2p$$

where  $p$  is the genus of the surface  $S$ , and is a fundamental invariant of the problem<sup>8</sup>. Since the sphere has genus 0, he recognised this as a generalisation of the relationship he had previously found for equations of first degree. Moreover, the relationship showed that the only surface upon which there can be a flow with no

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<sup>8</sup> If at most  $2p$  closed cycles can be drawn on a surface  $S$  without dividing the surface into two or more separate regions, then the surface is said to have genus  $p$ .



singular points is a surface with genus 1, i.e. a surface which can be identified with a torus. As Gray [1992, 511] has observed, it was this result which led Poincaré to a detailed study of non-singular flows on the torus since, apart from a flow on a surface of genus 0, these are the simplest type of flows to study.

On the torus Poincaré chose variables  $\omega$  and  $\phi$ , with the differential equations given by

$$\frac{d\omega}{dt} = \Omega, \quad \frac{d\phi}{dt} = \Phi$$

where  $\Omega$  and  $\Phi$  are given continuous periodic functions of  $\omega$  and  $\phi$  which never vanish simultaneously.

In the particular case when

$$\frac{d\omega}{dt} = a, \quad \frac{d\phi}{dt} = b$$

where  $a$  and  $b$  are positive constants, and the general solution is

$$\frac{\omega}{a} = \frac{\phi}{b} = \text{constant},$$

Poincaré found that there were no limit cycles, and hence the orbit of the point was stable.

Examination of the more general case where  $\Omega$  and  $\Phi$  are both positive and again there are no singular points on the torus led Poincaré to the development of what was to be one of the most important ideas used in [P2]: the idea of the first return map.

He considered the cycle without contact defined by the meridian  $\phi = 0$  and defined a set  $P$  to be the set of points  $M_i$  with  $-\infty < i < +\infty$ , where  $M_0$  is the point on the meridian at time  $t = 0$ ,  $M_i$  the  $i$ th consequent of  $M_0$  and  $M_{-i}$  the  $i$ th antecedent of  $M_0$ . Then, skilfully employing Cantor's innovative idea of derived sets<sup>9</sup>, he was able to draw conclusions about the stability of the orbits of the points in the set  $P$ . Denoting  $P'$  as the derived set of  $P$ , he showed that either: 1) every point of  $P$  belongs to  $P'$  and the orbit of the point is stable, or 2) no point of  $P$  belongs to  $P'$  and the orbit is unstable, and moreover both these situations could occur. In other words, in the case

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<sup>9</sup> Cantor [1872] defined the derived set of a given point set to be the set of its limit points. For a full discussion of Cantor's work on set theory see Dauben [1979].

of equations with degree greater than one, stability was not the exception, in contrast to the case of equations of first degree.

Poincaré also determined the circular order (in a fixed direction) in which the points of  $M$  were distributed on the meridian  $\phi = 0$ . Excluding the case of a limit cycle, he found that the order depended on an irrational number  $\mu$  (now known as the *rotation number* of the flow<sup>10</sup>). By defining  $\alpha_i$  as the length of the arc  $M_i M_{i+1}$ , he proved that the limit as  $n$  tends to infinity of  $\frac{\alpha_i + \dots + \alpha_{i+n}}{n}$  is independent of  $i$ , i.e. it is independent of the initial point, and is equal to  $\frac{2\pi r}{\mu}$ , where  $r$  is the radius of the meridian. Having found  $\mu$  he employed an elementary procedure to place each successive point  $M_i$  on the meridian. Then, using the fact that  $\mu$  was irrational, he showed that the derived set  $P'$  was what Cantor called a perfect set<sup>11</sup>.

There was, however, one problem which Poincaré could not resolve. Given a differential equation where the set  $P'$  was the same for all orbits, he was unable to establish whether it was possible for certain orbits to belong to the case where every point of  $P$  belongs to  $P'$ , while other orbits belonged to the case where no point of  $P$  belongs to  $P'$ . In other words, was it possible that some orbits could be stable while at the same time others were unstable? Or put in yet another way, was it possible for the perfect set  $P'$  to be disconnected? Although Poincaré was able to indicate some circumstances under which such a situation was impossible, he was unable to prove that it was always impossible.

He outlined how this problem was linked with the question of convergence of trigonometric series, in particular the method used by Lindstedt:

*"It is impossible not be struck by the analogy of this method of approximation with that of Lindstedt in celestial mechanics, and not to realise that the convergence of the process which I have shown is closely related to the convergence of series used by the learned astronomer from Dorpat. But the problem that we have treated here is evidently much simpler than the analogous questions of celestial mechanics, and if the difficulties are similar they are less numerous and without doubt easier to*

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<sup>10</sup> Birkhoff [1920a, 87].

<sup>11</sup> Cantor called a set perfect if it was unchanged by derivation. In other words, a set is perfect if every point in it is a limit point and the set is closed. See Dauben [1979].

*overcome. It is this consideration which has led me to persist with this question which has been the object of this chapter and to which I shall doubtless return as I find new results."* [1885, 157].

To study higher order differential equations Poincaré considered  $x$  as the coordinates of a point moving in  $n$ -dimensional space with  $t$  as an auxiliary variable representing time, so that the differential equations are written as

$$\frac{dx_i}{dt} = X_i \quad (i = 1, \dots, n)$$

where  $X$  are polynomials in  $x$ , and the solution curves are represented by the trajectory of the point moving in  $n$ -dimensional space.

By considering the second order case where the equations can be put in the form

$$\frac{dx}{dt} = X, \quad \frac{dy}{dt} = Y, \quad \frac{dz}{dt} = Z,$$

he established the existence of five different types of singular points together with three different types of singular lines defined by the points of intersection of the three surfaces

$$X = 0, \quad Y = 0, \quad Z = 0.$$

He then classified the solution trajectories according to their behaviour in relation to the singular points.

Poincaré also considered the question of finding solutions to the equations in terms of convergent series valid for all real values of the time from  $t = -\infty$  to  $t = +\infty$ . In one of his earlier notes [1882a] he had considered the differential equations defined by

$$\frac{dx_1}{X_1} = \frac{dx_2}{X_2} = \dots = \frac{dx_n}{X_n}$$

and observed that if an auxiliary variable  $s$  is defined by equating the common value of the above ratio to

$$\frac{ds}{X_1^2 + X_2^2 + \dots + X_n^2 + 1}$$

then it is possible to choose a positive constant  $\alpha$  such that the  $x$ 's can be expressed by series in powers of

$$\frac{e^{\alpha s} - 1}{e^{\alpha s} + 1}$$

which are convergent for all real values of  $s$ , but only under the condition that the trajectories do not meet a singular point except for infinite values of the variables  $s$  and  $t$ .

He now considered this result in the context of the three body problem. He found that for all real values of the time  $t$  the coordinates of the three bodies could be expressed in powers of

$$\frac{e^{at} - 1}{e^{at} + 1}$$

but only providing that it was known in advance that the distance between any two of the three bodies would remain greater than a given distance. In other words, the expansion is only valid providing there is never a collision between any two of the bodies. Since there is no way of foreseeing mathematically from the initial conditions whether or not such a collision will take place this is a formidable gap in the theory. As a result, Poincaré was led to admit that he could not see any possibility of being able to take advantage of this method in celestial mechanics<sup>12</sup>.

With regard to using the distribution of the singular points as a way of gaining qualitative information about the behaviour of the solutions of the differential equations, Poincaré found that the increase in the number of different types of singular points made the investigation correspondingly more difficult. However, by returning to an idea which he had used in [1884] which involved a theorem due to Kronecker, and introducing Kronecker's index for a closed surface he was able to make some progress<sup>13</sup>. In [1884] (details of which are discussed in 3.3.2) he had generalised Hill's idea of a periodic solution by applying Kronecker's theorem to the three body problem and thereby established the existence of an infinite number of periodic solutions. Now he considered the closed solution curves to the differential equations. What he found was that in the case of second order equations the closed trajectories, which represent the periodic solutions, play an analogous role to that of the singular points in the first order case. Thus in order to gain an understanding of the nature of the flow, instead of studying the solution curves close to a singular point he studied the trajectories neighbouring a closed trajectory.

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<sup>12</sup> Thirty years later Sundman utilised an analogous expansion with remarkable results. See 8.3.2.

<sup>13</sup> Kronecker [1869]. For indications of its history see Scholz [1980, 278-281].



Poincaré's idea of using periodic solutions in this way showed remarkable insight and one which was to be of central importance in [P2].

In order to investigate the behaviour of the trajectories in the neighbourhood of a periodic solution, he introduced the idea of a surface without contact (analogous to a cycle without contact), now better known as a transverse (or Poincaré) section. This is a surface  $F(x, y, z) = 0$  on which there is no real point which satisfies

$$\frac{\partial F}{\partial x} X + \frac{\partial F}{\partial y} Y + \frac{\partial F}{\partial z} Z = 0,$$

and so  $F$  is not tangent to any trajectory. Again he undertook his analysis on the simplest type of solution space available within the constraints of the system, which in this case was a torus without contact which contained no singular points in its interior.

He studied the different types of trajectory which approach a closed trajectory by examining their intersection with a transverse section. He chose axes in the transversal with an origin at the point where  $t = 0$ , and at each point  $M$  of the closed trajectory he constructed a transversal. Since each point in the neighbourhood of the closed trajectory will lie on a unique transversal it can be represented by a point  $x, y, t$  where  $x, y$  are coordinates of the point in the plane of the transversal and  $t$  represents the particular transversal.

Any trajectory starting infinitely close to the closed trajectory (say at  $m$ ) will meet the transversals as the motion proceeds and will return to the original transversal when  $t = 2\pi$ , meeting it at the point  $m_1$  (the consequent of  $m$ ). This is the idea of the first return map. The trajectory will continue and then meet the original transversal again at a third point  $m_2$ , the consequent of  $m_1$ , and so on. The problem is then reduced to investigating the distribution of the successive consequents, or, in modern terminology, the iteration of a point transformation.

Providing the point transformation is sufficiently regular, it is possible, as a first approximation, to consider only its linear terms. Poincaré showed that the behaviour of the trajectories depended on the nature of the eigenvalues of the linear transformation and, furthermore, that a striking correspondence could be made between these eigenvalues and the different types of singular points. In [P2] he further investigated the properties of these eigenvalues (which he then named *characteristic exponents*) in order to deal with the question of stability of periodic solutions.

Studying the different situations corresponding to the different kinds of eigenvalue, he identified four types of trajectory: three whose behaviour could be easily understood, which he classified as corresponding to nodes, foci and cols, and a fourth, which arose when the eigenvalues were conjugate pure imaginary, whose behaviour was more complex and which he initially thought corresponded to centres but which turned out to contain subtle and important differences. He found in this latter case that if none of the constant terms in the trigonometric series which satisfy the differential equations were equal to zero then this fourth case was equivalent to the first and there was instability.

Poincaré next looked for the conditions under which all the constant terms in the trigonometric series were equal to zero. As he observed, the case when all the constants are equal to zero is the case which, although appearing extremely unlikely to occur, is the one which is encountered in the study of the general equations of dynamics. Importantly, Poincaré showed that the conditions under which the constants vanished involved another new and important idea, and one which was later to play a fundamental role in [P2], the idea of an invariant integral.

To understand what Poincaré meant by an invariant integral, first consider a system of  $n$  first order differential equations as defining the motion of a point in an  $n$ -dimensional space. Then consider a set of such points having a certain volume of space  $V$  at time  $t$  and so at any subsequent time  $t'$  the set has a new volume  $V'$ . If the volume  $V'$  remains constant whatever the value of  $t'$ , then the volume is an invariant integral. For example, in the case of the differential equations of motion of an incompressible fluid, the volume is an invariant integral.

This was not Poincaré's first published reference to the concept. The idea first appears in [1886b], a paper published earlier in the same year. But in this earlier paper he had simply shown that it was the presence of a specific invariant integral which was the necessary condition for a particular result to hold and he had not provided any discussion of the underlying theory. Although, importantly, he had recognised that it was the presence of an invariant integral which accounted for the success of Lindstedt's method in eliminating the secular terms from the series used in celestial mechanics. It was knowledge of this property which was unknown to Lindstedt which, as he now explained, had enabled him to widen the class of equations for which Lindstedt's method was valid.

In his consideration of the question of the conditions under which the constant terms in the trigonometric series vanish, Poincaré first proved that the constant terms were equal to zero providing there was no closed surface without contact. He then showed that the existence of a certain invariant integral, which would now be called the volume in phase space, was sufficient for this latter condition to be met.

Although Poincaré did not at this stage give any indication of the relationship between invariant integrals and the equations of dynamics, implicit in his argument is one of the important results of his theory of invariant integrals. For, as he later showed in [P2], Hamiltonian systems always admit the volume in phase space as an invariant integral. In other words the existence of an invariant integral is a fundamental property of Hamiltonian systems, and hence the constant terms are bound to vanish. It was only in [P2] where the idea of an invariant integral played an essential role in his stability arguments that Poincaré gave a name to the concept and developed a coherent theory.

Having discovered a condition under which stability was possible, Poincaré still had the problem of deciding when it occurred. He realised that apart from the perennial problem of "small divisors" arising from the near commensurability of the frequencies of the interacting motions, the question also turned on whether or not the series concerned were uniformly convergent, and being able to decide under what conditions uniform convergence was assured. However, despite a long and detailed discussion of a particular example in which he examined the different situations which could arise depending on different initial conditions, he was forced to conclude that even in the general case when the constants did all vanish the series were not necessarily uniformly convergent. Hence the stability question was still unresolved.

To summarise, in looking at the behaviour of trajectories near a given periodic solution, what Poincaré had shown was that there were principally three different situations which could arise. Firstly, the moving point could either continuously recede from the periodic solution or asymptotically approach it, in which case the orbit of the point did not possess Poisson stability. Secondly, the moving point could oscillate within given limits close to the periodic solution, in which case the orbit did possess Poisson stability. Finally, the moving point could come arbitrarily close to any other point in the domain, in which case there was not only Poisson stability since the point would always return as close as desired to its initial



position, but there was also instability in the sense that the point could go arbitrarily far away from its initial position and it was impossible to assign limits to its coordinates. Since Poincaré's methods did not allow him to distinguish between the second and third situations, this presented a significant problem, not least because the cases represented by these situations are precisely those which are encountered in the general equations of dynamics.

Thus, although he had established many substantial and important results for second order differential equations, Poincaré's analysis was still less complete than that for the first order case. Discussion of the periodic solutions had proved extremely fertile but the difficulties of convergence and small divisors still remained. Poincaré ended the fourth and last paper in the series with his view of the implications of his results for the future progress of celestial mechanics:

*"From the above, one can easily understand to what extent the problems due to small divisors and the quasi-commensurability of mean motions that one meets in celestial mechanics result from the nature of things which cannot be changed. It is extremely likely that they will be encountered whatever method is used."* [1886, 222].

### 3.3 Celestial mechanics and the three body problem

#### 3.3.1 Trigonometric series

Poincaré's earliest work in celestial mechanics concerned the convergence of trigonometric series of the form

$$\sum A_n \sin \alpha_n t + \sum B_n \cos \alpha_n t$$

which were used by astronomers to integrate differential equations such as

$$\frac{d^2x}{dt^2} + n^2x = \Phi(x,t).$$

These series are quite different from Fourier series in that the coefficients of time appearing as arguments inside the trigonometric functions are not proportional to integral coefficients and may decrease or increase indefinitely. The problem with the use of these series in celestial mechanics is in establishing their convergence. Are they convergent and if so, is that convergence absolute? If they are not absolutely convergent are they what are now called asymptotic? If they are absolutely convergent are they uniformly convergent? These are all questions which



had not been addressed until Poincaré made them the subject of a detailed mathematical investigation, most notably in [1884b]<sup>14</sup>. In particular Poincaré was interested in the series derived by the Swedish astronomers Gylden and Lindstedt and, beginning in 1882, he published several papers discussing the above issues, often specifically in response to the astronomers' work.

Poincaré's initial researches dwelt on the distinction between absolute and uniform convergence. His first result showed that if the convergence was not uniform then the function could attain arbitrarily large values, either by indefinitely increasing or by the amplitude of its oscillations indefinitely increasing [1882b, 1885a]. This was an important result in that it meant, contrary to what the astronomers had previously believed, that the ordinary convergence of a trigonometric series was not a sufficient condition for stability and so could not be used as a criteria for establishing results such as the stability of the solar system.

When in 1883 Lindstedt proposed a new series solution for the three body problem, Poincaré did not take long to respond [1883a, 1884b]. Lindstedt [1883a, 1884], instead of providing a formal proof of convergence for his series, had supposed that it was possible to choose the necessary constants in such a way that convergence was assured, at least for a given interval of time. Poincaré proved that if the series was absolutely convergent for such an interval of time, however short, then it was always convergent. He also showed that there could not be two solutions to the problem, since a function can only be represented by one such absolutely convergent series. Furthermore, he pointed out that although it was true that there were particular values of the constants for which the mutual distances of the three bodies could be expanded as convergent trigonometric series, it was by no means certain, indeed it was unlikely, that the convergence would subsist for other values of the constants, even for those arbitrarily close. As a result he was led to the conjecture that Lindstedt's series were in fact asymptotic rather than absolutely convergent. That is, he thought that the series represented the mutual distances for a limited period of time only and did not do so indefinitely.

Later Poincaré was prompted by a paper of Gylden's to consider the question of convergence in a slightly different context. In his paper, Gylden had been concerned with the problem of improving the convergence for a given trigonometric series. In

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<sup>14</sup> Hadamard [1922, 160] described this paper as "... a work remarkable for its shortness and simplicity in comparison with its fundamental importance."

[1886c] Poincaré showed that, providing the function and its derivatives were finite and they satisfied certain continuity conditions, then it was possible to find an upper bound for the coefficients in the trigonometric series representing the function and hence ascertain the strength of the convergence of the series<sup>15</sup>.

Poincaré's first published proof of the divergence of Lindstedt's series was contained in [P2], where it stood out as one of the results which he considered to be of particular importance. He later returned to the topic in the second volume of the *Méthodes Nouvelles* where he gave a complete discussion of the methods used by astronomers in relation to the series used in celestial mechanics. This and Poincaré's later work in celestial mechanics are discussed in 7.2 and 7.3.

In [1886b] Poincaré addressed a particular problem in Lindstedt's perturbation method. It will be recalled that Lindstedt's method involved a symmetry restriction. Lindstedt had introduced this restriction as a way of ensuring that at each stage of the approximation only one secular term was introduced which was a necessary condition for his method to work. By a clever application of Green's theorem and, for the first time, using the idea of an invariant integral, Poincaré showed that the secular term which appeared in each approximation was necessarily unique and therefore the restriction was unnecessary. In other words, the class of equations for which Lindstedt's method was valid was more general than Lindstedt himself had supposed.

In [1889] Poincaré gave a new derivation for Lindstedt's series using Hamilton-Jacobi theory. This new derivation had the advantage of completely by-passing Lindstedt's earlier restriction, and thus rendered superfluous Poincaré's earlier idea involving Green's theorem.

### 3.3.2 Periodic solutions of the three body problem

The best known of Poincaré's early papers in celestial mechanics is his first paper on periodic solutions of the three body problem [1884a]. This is the paper which was

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<sup>15</sup> Given a trigonometric series  $\sum A_m \sin mx + \sum B_m \cos mx$  Poincaré said that the convergence of the series was of order  $p$  if

$$|m^p A_m| \leq K, \quad |m^p B_m| \leq K,$$

where  $K$  is a positive quantity independent of  $m$ , and he then measured the strength of the convergence by its order.

mentioned in [1886] and which was published in the *Bulletin Astronomique* in 1884, an abstract having appeared in the *Comptes Rendus* during the previous year [1883].

Poincaré's interest in the work of Hill and periodic solutions, a topic which dominated Poincaré's later researches in celestial mechanics, manifested itself here for the first time. By the application of a theorem due to Kronecker on solutions in systems of equations<sup>16</sup>, Poincaré proved that it was possible to choose the initial conditions for the three body problem (in the case where two of the masses were very small relative to the third) in such a way that the mutual distances of the three bodies were periodic functions of time. He thereby proved the existence of a whole continuum of periodic solutions, thus giving a generalisation of Hill's result.

Furthermore, Poincaré showed that this type of periodic solution could be distinguished into three different kinds:

1. Those in which the inclinations are zero, i.e. all the bodies move in the same plane, and the eccentricities of the orbits are very small;
2. Those in which the inclinations are zero and the eccentricities finite;
3. Those in which the inclinations are finite and the eccentricities are very small.

He also speculated on the idea of a fourth kind of periodic solution in which both the inclinations and the eccentricities were finite, although he was unable to prove its existence except for certain values of the ratio of the two smaller masses.

Although the probability of the actual occurrence of such solutions was essentially zero (since they depended on particular values of the initial elements), Poincaré's insight was to realise that their importance lay in interpreting their relationship with other nearby solutions. He saw that if the initial elements of a solution were very close to those which corresponded to a periodic solution, it was possible to relate the true positions to the positions they would have occupied in the periodic solution and, to quote Gylden, use this solution as an *intermediate orbit*. By supposing that the order of the inclinations and the eccentricities was sufficiently small so that their squares could be neglected, Poincaré showed that the differences between the true orbits and the intermediate orbits could be expressed by trigonometric series with no secular term. This greatly reduced the error which

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<sup>16</sup> Kronecker [1869].



arose through the general method which involved the secular variation of the eccentricities.

As previously mentioned, Poincaré extended his ideas on periodic solutions from this paper into an investigation into the properties of closed solution curves in his memoir on curves defined by differential equations. In turn, the theory which he developed there provided the fundamental backdrop for his later discussion on periodic solutions which formed the central part of [P2].

### 3.4 Other papers

The final category of Poincaré's earlier work to be considered concerns two papers which are both related to the theory of differential equations, and although they are of a general nature and not specifically related to the three body problem, they contain results which make an important contribution to the development of the theory in [P2].

#### 3.4.1 Thesis

Poincaré's thesis [1879] which was examined by Bouquet, Bonnet and Darboux, concerned the study of integrals of first order partial differential equations in the neighbourhood of a singular point. It was his second paper on the theory of differential equations and, as Hadamard remarked, it contained a strong pointer towards his future success with the topic and its applications to celestial mechanics:

*"Even Poincaré's thesis contained a remarkable result which was destined later to provide him with a powerful lever in his researches in celestial mechanics."* <sup>17</sup>.

Looked at in the context of the theory of differential equations already in existence at the time, Poincaré's thesis was the natural convergence of two streams of thought. On the one hand, Cauchy, and later Kovalevskaya, had applied Cauchy's method of majorants<sup>18</sup>, to obtain results about the solutions of partial differential equations in the neighbourhood of an ordinary point, while on the other, Briot and Bouquet, and later Fuchs, using similar methods, had studied the solutions of ordinary

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<sup>17</sup> Hadamard [1921, 206].

<sup>18</sup> Cauchy called the method *calcul des limites* because it establishes the lower bounds or limits within which the series in question will necessarily converge.



differential equations in the neighbourhood of a singular point. Poincaré both followed Cauchy by considering the solutions of partial differential equations, and followed Briot and Bouquet by considering these solutions in the neighbourhood of a singular point.

Poincaré's analysis divided naturally into two parts according to whether the singularities under consideration were essential. In the case where the singularities were non-essential, he found that the solutions satisfied algebraic equations with coefficients analytic with respect to the variables. Although his treatment concerned a single partial differential equation, his results were analogous to those that would have been obtained by applying the theory to a system of ordinary differential equations, and it was in this analogous form that he applied the results in [P2].

With regard to the second and more difficult case concerning the essential singularities, one of the equations he considered was of the form

$$\frac{\partial z}{\partial x_1} X_1 + \dots + \frac{\partial z}{\partial x_n} X_n = \lambda_1 z$$

where the  $X_i$  can be expanded as powers of  $x_1, \dots, x_n$ , with no constant term, and the first degree terms can be reduced to  $\lambda_1 x_1, \dots, \lambda_n x_n$ . He showed that this equation admits an analytic solution in  $x_1, \dots, x_n$  providing, firstly, there is no relation of the form  $m_2 \lambda_2 + \dots + m_n \lambda_n = \lambda_1$  where the  $m$  are positive integers, and, secondly, that in the plane for the complex variable  $\lambda$  the convex polygon containing the points  $\lambda_1, \dots, \lambda_n$  does not contain the origin. This latter condition was the result to which Hadamard later referred, and its importance is contained in the fact that it defines a space of non-existence for the solutions of the equations. In [P2] Poincaré not only used this result explicitly but also further extended it for use in connection with his celebrated *asymptotic solutions*.

As in the previous case, there is a sense in which this single partial differential equation can be thought of as being equivalent to a system of ordinary differential equations, and seen in this light Poincaré's work can be considered as a generalisation of Briot and Bouquet's researches on a single differential equation.

### 3.4.2 Asymptotic series

With the emphasis on the rigorisation of analysis in the first half of the 19th century the question of the legitimacy of divergent series became increasingly controversial. On the one hand divergent series were known to produce fallacious

results if used indiscriminately, but on the other it was known that there were some divergent series, called semiconvergent series, which, for a given number of terms, provided an increasingly better approximation to the function as the variable increased, the best known example being that of Stirling's series for the gamma function<sup>19</sup>. In addition, it had long been recognised that some divergent series provided good numerical approximations for the functions they represented. In the latter part of the 19th century the increased application of these "useful" divergent series, particularly in dynamical astronomy, meant that there was a growing need to find some way of distinguishing them from amongst other divergent series<sup>20</sup>.

In [1886a] Poincaré's tackled exactly this question and his solution provided the first formal definition of asymptotic series. He began with a divergent series of the form

$$A_0 + \frac{A_1}{x^1} + \frac{A_2}{x^2} + \dots$$

in which he defined the sum of the first  $n + 1$  terms to be  $S_n$ . He said that a series of this type asymptotically represented a function  $f(x)$  if the expression

$$x^n (f(x) - S_n)$$

went to zero as  $x$  increased indefinitely. In other words he had defined a general series which had exactly the same property as Stirling's series: the larger the value of the variable, the closer the series approximates the function. This can be put more formally, by saying that a series is an asymptotic expansion for a function  $f(x)$  if for each  $n$  and each  $x$  sufficiently large but depending on  $n$ ,

$$x^n |f(x) - S_n(x)| < \varepsilon$$

where  $\varepsilon$  is arbitrarily small. Thus the value of the function  $f(x)$  can be calculated to a high degree of accuracy for large values of  $x$  by taking the appropriate number of terms in the partial sum  $S_n$ . Since the constants of the series are defined uniquely, it follows that if a function has an asymptotic series representation it is unique,

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<sup>19</sup> The gamma function is defined by:  $\Gamma(z) = \int_0^\infty t^{z-1} e^{-t} dt$ , for  $\text{Re}(z) > 0$ ; and Stirling's series is given by:

$$\log \Gamma(z) = (z - \frac{1}{2}) \log z - z + \frac{1}{2} \log (2\pi) + \sum_{r=1}^{\infty} \frac{(-1)^{r-1} B_r}{2r(2r-1)z^{2r-1}}.$$

<sup>20</sup> The history of asymptotic solutions of differential equations in the 19th and early 20th centuries is well described by Schlissel [1976].

although one asymptotic series can represent several different functions. Importantly, Poincaré showed that asymptotic series satisfy most of the same properties as convergent series, with the exception that in general they cannot be differentiated to form another asymptotic series<sup>21</sup>.

Having formalised the distinguishing property of these series, Poincaré applied the theory to a particular class of ordinary differential equations. During the 1860s and 1870s Fuchs and Thomé had established important results concerning the solution of linear ordinary differential equations in the neighbourhood of a singular point<sup>22</sup>. Building on their results Poincaré considered the integration of equations of the form

$$P_n \frac{d^n y}{dx^n} + \dots + P_1 \frac{dy}{dx} + P_0 y = 0$$

where the  $P_i$  are polynomials in  $x$ .

If the equation has an irregular singular point at  $x = \infty$ , then Thomé had shown that in addition to  $m$  possible convergent series solutions ( $m < n$ ), there exist series of the form

$$e^Q x^a \left( A_0 + \frac{A_1}{x^1} + \frac{A_2}{x^2} + \dots \right)$$

where  $Q$  is a polynomial in  $x$ , which formally satisfy the equation but which are generally divergent, and it was these divergent series on which Poincaré focused his attention. Since the dominant characteristic of the series was the degree of the polynomial  $Q$ , Poincaré identified them by this property. Thus if the polynomial  $Q$  is of degree  $p$ , then Poincaré called the series a normal series of order  $p$ .

Firstly, Poincaré proved that the order of the differential equation's normal series solutions at  $x = \infty$  could be determined. This involved introducing the idea of the rank of the differential equation. Let  $M_i$  be the degree of the polynomial  $P_i$  and

$$N_i = \frac{M_i - M_n}{n - i}.$$

If  $h$  is the largest of the  $n$  quantities  $N_i$ , and  $k$  is the integer equal to or immediately larger than  $h$ , then Poincaré said that the equation has rank  $k$  at  $x = \infty$ , or the equation has a singularity of rank  $k$  at  $x = \infty$ . He then proved that if the

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<sup>21</sup> With regard to the differentiation of asymptotic series, consider the function  $e^{-x} \sin(e^x)$ .

<sup>22</sup> See Schlissel [1976] and Gray [1984].

differential equation has an irregular singularity of rank  $k$  at  $x = \infty$ , then all its normal series solutions at  $x = \infty$  were of order  $k$ . Next, considering an equation with first order normal series solutions, i.e. an equation of rank one, he proved that each series, although divergent, represented asymptotically one integral of the differential equation for large positive values of  $x$ . In the case of equations of higher rank, he first reduced them to rank one before attempting to find the asymptotic solutions.

Although Poincaré himself did not obtain many specific results using asymptotic series, the impact of the paper was far reaching. By creating a formal framework for these series, he provided a stimulus for investigations into asymptotic solutions of a variety of classes of ordinary differential equations. Moreover, from the point of view of his later work and looking ahead to [P2], the theory played a fundamental role in his discussion of the asymptotic solutions of the restricted three body problem.



## 4. Oscar II's 60th Birthday Competition

### 4.1 Introduction

In the autumn of 1890 Poincaré's memoir on the three body problem was published in the journal *Acta Mathematica* as the winning entry in the international prize competition sponsored by Oscar II, King of Sweden & Norway to mark his 60th birthday on January 21, 1889.

A combination of royal patronage and carefully planned public relations meant that the competition achieved the unusual distinction of gaining recognition that stretched well beyond the world of mathematics. In the numerous obituary notices and commentaries on Poincaré's œuvre, not only is the memoir singled out for particular acclaim but the point is often made that it was as a consequence of winning the Oscar prize that Poincaré's name entered the public domain. Paul Painlevé in a speech at Poincaré's funeral said:

*"In 1889, at the announcement of the result of the competition, France learnt with joy that the gold medal, the highest award of the new competition, had been awarded to a Frenchman, a young scholar aged thirty five, for a*

*marvellous study of the stability of the solar system, and the name of Henri Poincaré became known."*<sup>1</sup>.

A view which was endorsed by Gaston Darboux, the Permanent Secretary of the French Academy of Sciences, in a speech made at the Academy in honour of Poincaré:

*"From that moment on, the name of Henri Poincaré became known to the public, who then became accustomed to regarding our colleague, no longer as a mathematician of particular promise, but as a great scholar of whom France has the right to be proud."*<sup>2</sup>.

However, the paper which appeared in *Acta* differed remarkably from the version which had actually won the prize almost two years earlier. Its eventual publication drew to a close the competition which, despite appearances to the contrary, had been beset with difficulties from its inception more than six years previously.

## 4.2 Organisation of the competition

By the late 19th century mathematical prize competitions had become well established as a method for seeking solutions to specific mathematical problems. These competitions usually emanated from the national Academies, particularly those in Paris and Berlin, the questions set reflecting the interests of the Academy concerned. Although the prizes offered were generally financial in nature, they were valued much more in terms of academic prestige. Thus the existence of a mathematical competition at this time was no novelty, but the Oscar competition was somewhat unusual in that its sponsor, anxious that it should transcend national barriers, did not associate his prize with an institution but chose rather to link it to an academic journal.

Oscar was well known within mathematical circles, and in her autobiography the Russian mathematician and protégé of Weierstrass, Sonya Kovalevskaya, who spent the last years of her life in Stockholm in the position of being the only European female professor of mathematics, said of him:

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<sup>1</sup> Painlevé *Discours prononcées aux funérailles*. Archives de l'Académie des Sciences, Paris.

<sup>2</sup> Darboux [1914].

*"King Oscar is a pleasant and cultivated person. As a young man he attended lectures at the university, and still today shows an interest in science, although I cannot vouch for the profundity of his erudition. He has no official contact with the university but is extremely sympathetic to it and very amicably disposed towards its professors in general and to myself in particular."*<sup>3</sup>.

As a student at the University of Uppsala Oscar had distinguished himself in mathematics and throughout his life continued to maintain an active interest in the subject (as well as education in general) through publishing and making awards to individual mathematicians. Thus the establishment of an important prize competition in mathematics would have been seen as a natural extension of his role as a patron of the subject.

From its beginnings in 1884, the competition was organised by Gösta Mittag-Leffler, who was the professor of pure mathematics at the newly established Stockholm Högskola (later the University of Stockholm) and founder and editor-in-chief of *Acta Mathematica*<sup>4</sup>. Having obtained his doctorate from the University of Uppsala in 1872, Mittag-Leffler had studied under Hermite in Paris, Ernst Schering in Gottingen and Weierstrass in Berlin and therefore had first-hand experience of life within the premier mathematical communities in Europe. This, combined with his involvement with *Acta*, meant he was well placed to promote the idea of an international competition.

Inspired by Weierstrass, Mittag-Leffler's own mathematical interests lay almost entirely in the realms of analytic function theory. His *Habilitationsschrift* on the foundations of the theory of elliptic functions had been published in 1876, followed in 1877 by the first publication of the "Mittag-Leffler" theorem on the analytic representation of a single-valued complex function, and his later work focused on the problem of analytic continuation. However, he was not only a talented mathematician but he was also a skilled communicator. He assiduously cultivated

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<sup>3</sup> Kovalevskaya [1978, 228].

<sup>4</sup> The first issue of *Acta Mathematica* appeared at the end of 1882. For the history of its foundation see Domar [1982].

and nurtured mathematical contacts both at home and abroad, and maintained an extremely vigorous correspondence<sup>5</sup>.

Prior to the start of the competition, Mittag-Leffler had established a relationship with the King through the foundation of *Acta*<sup>6</sup> but it is not clear whether the idea of holding the competition came from Mittag-Leffler or whether it can be attributed to the King himself. Certainly Mittag-Leffler was always keen to enhance his reputation within the mathematical community and would have relished the opportunity to be involved in a major mathematical competition. It therefore seems quite likely that the project emerged as a consequence of his initiative.

What appears to be the first reference to the competition occurs in a long letter from Mittag-Leffler to Kovalevskaya written in June 1884, although the contents of the letter show that the topic was already under discussion. The letter also provides clear evidence that from the outset the competition was intended to be one of pre-eminent importance in the mathematical field. The following extract outlines the proposed form of the competition:

*"I agree with Weierstrass, if none of the answers on the set question are worthy of the prize, then the medal must be awarded to the mathematician who within recent years has made the best discoveries in higher analysis. ... we should not award our prize more frequently than every fourth year. Malmsten and the King want the prize jury to be appointed by the King and to consist of*

- 1. The main editor of Acta Mathematica*
- 2. A German or Austrian mathematician - = Weierstrass*
- 3. A French or Belgian mathematician - = Hermite*
- 4. An English or American mathematician - = Cayley? or Sylvester*
- 5. A Russian or Italian mathematician - = the first time Brioschi or Tschebychev, the second time Mrs Kovalevskaya.*

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<sup>5</sup> Mittag-Leffler's considerable correspondence which is preserved at the Mittag-Leffler Institute is described in Grattan-Guinness [1971].

<sup>6</sup> In 1882 Oscar had provided both financial and moral support to help Mittag-Leffler found *Acta*. See Domar [1982].



*After each prize giving two of the prize judges should leave the jury and new ones should be appointed by King Oscar as long as he is alive - he must be able to appoint (substitutes) for both the leaving members. After King Oscar's death, the three remaining must appoint two new members but always in such a way as to fit the categories mentioned above."*<sup>7</sup>.

In the event, Mittag-Leffler was unable to fulfil any one of Oscar's requirements exactly. The difficulties with which he was faced are well illustrated by Kovalevskaya's reply written while on holiday in Berlin:

*"In regard to the question of the prize Weierstrass has promised me that he will write you his opinion on that in more detail as soon as he receives a letter from you. I did not inform him of what you wrote me in the letter before last with regard to the choice of jury, for I was sure in advance of his complete disapproval. Indeed I believe that in this way the thing presents many practical difficulties. Just consider how one could hope that four famous mathematicians, Weierstrass, Hermite, Cayley and Tscheychev would ever agree on the merits of a memoir. I believe it is certain that each of the four would refuse to become part of the jury as soon as he learned the names of the other three. As for Weierstrass, I am so sure of this that I didn't even venture to talk to him about it. In general Weierstrass thinks that it will be quite difficult for the jury to agree when they have no opportunity to talk face to face. To do it by mail is considerably more difficult; and at bottom, why would these old gentlemen take so much trouble for us? There, I fear, is a very great difficulty! As for the honour, quite the contrary, each of the four that you named will be outraged that you chose the others along with him."*<sup>8</sup>.

Although there was a certain amount of melodramatic effect in Kovalevskaya's letter (Hermite and Weierstrass certainly had a healthy respect for each other), for the most part her presentiments proved to be well founded. The eventual outcome was a commission comprised of only three: Charles Hermite, Karl Weierstrass and Mittag-Leffler himself.

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<sup>7</sup> Letter from Mittag-Leffler to Kovalevskaya 7.6.1884, M-L I (tr. S. Norgaard). For the complete extract see Appendix 1.

<sup>8</sup> Cooke [1984, 106].

Hermite was one of the dominant figures of French analysis in the second half of the 19th century, and from 1869 a professor both at the École Polytechnique and at the Sorbonne, resigning from the former in 1876 while maintaining his position at the latter until 1897. By the time of the competition he had established an international reputation in both teaching and research, and his courses attracted an audience from all over Europe. Not only was he a leading exponent of Cauchy's complex function theory, but also he actively promoted Weierstrass' ideas in France. His career had begun in the 1840s with work on elliptic and Abelian functions, topics which continued to occupy him throughout his mathematical life. By the late 1870s, having achieved notable success with his research into a variety of other topics such as quadratic forms, invariant theory, and fifth degree equations, he returned once again to elliptic function theory. Throughout his life Hermite maintained an extensive and influential correspondence with other mathematicians, most notably with the Dutch mathematician Stieltjes<sup>9</sup>, but also, significantly, with Mittag-Leffler<sup>10</sup>.

In 1873 Mittag-Leffler had studied with Hermite in Paris and a close friendship had developed between them. From the time of Mittag-Leffler's arrival, Hermite made no secret of the high regard in which he held the work of his German counterpart, Weierstrass. As Mittag-Leffler later recalled, his earliest memory of Hermite was of being greeted by the words:

*"You have made a mistake, Monsieur, you should have taken the courses of Weierstrass in Berlin. He is the master of us all."*<sup>11</sup>.

As a result of his connection with Hermite, Mittag-Leffler was able to remain in constant touch with the mathematical life in Paris, and moreover Hermite's friendship had proved to be extremely valuable with regard to the launching of *Acta*. Not only did Hermite show his support for the idea by sending him a handsome donation towards the initial financing of the project, but, more importantly, it was with Hermite's help that Mittag-Leffler had been able to secure papers for the first issue of the journal from three extremely talented young

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<sup>9</sup> *Correspondence d'Hermite et de Stieltjes*, edited by B. Baillaud and H. Bourget, 2 volumes, Paris, 1905.

<sup>10</sup> The Hermite-Mittag-Leffler correspondence 1874-1883; 1884-1891; 1892-1900 is published in *Cahiers* 5 (1984), 49-285; 6 (1985), 79-217; 10 (1989), 1-82 respectively.

<sup>11</sup> Mittag-Leffler [1902, 131].

French mathematicians: Appell, Picard and Poincaré, all of whom were former students of Hermit. And, importantly for Mittag-Leffler, all three of them continued to contribute to the journal, as well as Hermite himself.

Weierstrass was a professor at the University in Berlin, a position he had held since 1864. He was a leading, if not the foremost, analyst in Germany, and his reputation as an expositor of new ideas drew students from across the world. Weierstrass had first come to prominence with his papers on Abelian functions published in 1854 and 1856<sup>12</sup>, having spent the previous decade establishing his theory of analytic functions on the foundation of power series. He lectured on a variety of topics including several aspects of elliptic function theory, as well as the theory of Abelian functions, and the calculus of variations. He was also interested in the application of analysis to problems in mathematical physics, and in particular the  $n$  body problem.

However, Weierstrass' compulsion for rigour meant that he found it extremely difficult to complete anything for publication, with the result that his fame rested largely on his power as a teacher, and his influence was to a great extent carried by his former students, one of whom was Mittag-Leffler. While in Berlin Mittag-Leffler had established a good relationship with Weierstrass, and when he left in 1876, continued their association through correspondence<sup>13</sup>.

Thus although Mittag-Leffler had failed in his original task of appointing a commission of five members, he had managed to engage two of the leading analysts of the day, one from each of the premier mathematical nations, and, importantly, two mathematicians with whom he had already established warm and productive friendships<sup>14</sup>.

However, despite the reduction in the number of people involved, such a choice of commission did still present certain practical difficulties. Apart from the obvious

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<sup>12</sup> Zur theorie der Abelschen Functionen *Crelle's Journal* 47 (1854), 289-306; Theorie der Abelschen Functionen *Crelle's Journal* 52 (1856), 285-380.

<sup>13</sup> Domar [1982] has noted that there was a slight lull in their correspondence at the beginning of the 1880s which coincided with the founding of *Acta* and with Weierstrass being put in charge of *Crelle's Journal*.

<sup>14</sup> Not only did the composition of the commission not accord with the King's original conception, but also the idea of making the competition a regular event was never taken any further. That the competition was held only once was probably due both to the original difficulties in organising a commission and to the considerable problems which the commission later encountered.



problems arising from the different geographical locations involved, Weierstrass in Berlin, Hermite in Paris and Mittag-Leffler in Stockholm, there was the additional complication engendered by the lack of a common first language. Although Mittag-Leffler was more than competent in both French and German and usually happy to use either, Hermite and Weierstrass, while familiar with each other's languages, preferred to correspond in their own<sup>15</sup>.

The commission being appointed, Mittag-Leffler was faced with the formidable undertaking of achieving a consensus of opinion with regard to the question to be set. Naturally, for the competition to establish a international reputation, it was essential that it should attract entries of the highest international calibre which in turn would depend on the nature of the question to be solved. However, it soon became clear that to limit the competition to one question alone was going to be counter-productive. As pointed out by Hermite, there were by this time an unprecedented number of mathematicians working in different branches of analysis<sup>16</sup>. Thus to single out a particular topic on which to pose the question would be to impose a constraint which inevitably would restrict the quality of the entries. Moreover, the imposition of such a limitation would preclude the inclusion of any work of an innovatory nature. The difficulty was compounded by the fact that the King himself was keen that the competition should address a specific question. The possibility of having a single very open question was discounted because of the fear that it might lead to a situation in which it would have been impossible to identify a winner from several entries of comparable merit, each on an entirely different topic.

After an intensive correspondence between all three members of the commission, with Hermite and Weierstrass exchanging ideas through Mittag-Leffler<sup>17</sup>, and the King becoming increasingly impatient, a format was finally agreed. The competition would consist of four questions but the possibility of submitting an entry on an alternative topic would also be included.

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<sup>15</sup> Mittag-Leffler's correspondence shows that he was extremely proficient in both French and German but he did occasionally claim otherwise, as, for example, in a letter written to Kronecker in July 1885 which he began with, "*Please excuse me for writing to you in French. However badly I write French I find it easier and make less mistakes than in German.*". Mittag-Leffler-Kronecker correspondence, M-L I.

<sup>16</sup> Hermite to Mittag-Leffler, 25.2.1885, No. 150, M-L I. *Cahiers* 6 (1985), 100.

<sup>17</sup> Mittag-Leffler used Kovalevskaya to translate Weierstrass' questions into French for Hermite. Letter from Mittag-Leffler to Hermite, 20.2.1885, No. 356, M-L I.



In mid-1885 the official announcement of the competition was published in both German and French in *Acta*<sup>18</sup>. Mittag-Leffler also sent an English translation for publication in *Nature* where it appeared in the issue for July 30, 1885<sup>19</sup>.

The announcement gave details of the prize (a gold medal together with a sum of 2,500 Crowns<sup>20</sup>), named the commission, listed the questions and stipulated the conditions of entry. The entries were to be sent to the chief editor of *Acta* before June 1, 1888, and, as was customary in such competitions, they were to be sent in anonymously, identifiable only by a motto and accompanied by a sealed envelope bearing the motto and containing the author's name and address. The entries were not to have been previously published and notice was given that the winning entry would be published in *Acta*.

Of the four questions set, the first three were proposed by Weierstrass and the fourth by Hermite. The first question addressed the well-known  $n$ -body problem, reflecting Weierstrass' longstanding interest in the problem<sup>21</sup>. The second question required a detailed analysis of Fuchs' theory of differential equations; the third question asked for further investigation into the first order non-linear differential equations studied by Briot and Bouquet; the last question concerned the study of algebraic relations connecting Poincaré's Fuchsian functions which have the same automorphism group.

### 4.3 Kronecker's criticism

The publication of the announcement had one unintentional and unwelcome consequence. It provoked an angry reaction from Leopold Kronecker, also a professor at the University in Berlin. Kronecker, apparently incensed by being left out of the commission, wrote to Mittag-Leffler with a catalogue of complaints about the

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<sup>18</sup> *Acta* 7, I-VI.

<sup>19</sup> See Appendix 2.

<sup>20</sup> For comparison: Domar [1982] cites Mittag-Leffler's annual salary in 1882 as a professor in Stockholm as 7,000 Crowns, and *Nature* (February 21, 1889, 396) in the announcement of the competition result states that it is equivalent to £160.

<sup>21</sup> 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 [Mittag-Leffler 1912, 31], and in 1880/81 he gave a seminar on the problems of perturbation theory in astronomy [Moser 1973, 6]. 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].

competition<sup>22</sup>. However, it was no secret that an intense rivalry existed between himself and Weierstrass and he was undoubtedly equally angry at the latter's inclusion on the commission. It is more than likely that this was the real motivation for the attack<sup>23</sup>.

Kronecker accused Mittag-Leffler of using the competition as a vehicle for advertising *Acta*. Why had the competition not been proposed by the Swedish Academy? It was an accusation Mittag-Leffler could easily refute: the King wished the competition to be announced in *Acta*, not only because *Acta* could claim a wider readership in the mathematical world than the transactions of the Swedish Academy, but also because of the King's personal interest in the journal. On being challenged on the choice of members for the commission, Mittag-Leffler explained to Kronecker that his instructions had been to choose a commission of three, consisting of a representative from each of the two premier mathematical nations, Germany and France, while the third member was to come from Sweden. With regard to the German representative, it had been a straight choice between him and Weierstrass, both of whom were equally suited to the task, but Weierstrass, being some 8 years older than Kronecker, had been chosen purely on the grounds of his "venerable" age. This may have mollified Kronecker but, not surprisingly, Weierstrass was not impressed by this particular line of reasoning. However, Kronecker levelled his most serious charge at Question 4, the question set by Hermite. Kronecker not only maintained that he was the best person to judge algebraic questions of this type but also that he had already proved that the results required to resolve this particular question were impossible to achieve and he threatened to tell the King as much<sup>24</sup>. As a defence, Mittag-Leffler could only plead ignorance on behalf of himself, Hermite and Weierstrass<sup>25</sup>. Mittag-Leffler concluded his reply with a barrage of flattery well calculated to appeal to Kronecker's vanity.

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<sup>22</sup> The contents of Kronecker's letter to Mittag-Leffler have been reconstructed from Mittag-Leffler's reply which was written in July 1885 and which is at the M-L I.

<sup>23</sup> Weierstrass believed that Kronecker's avowed antipathy to the work of George Cantor reflected Kronecker's opposition to his own work. See Biermann [1988, Chapter 5].

<sup>24</sup> Mittag-Leffler to Hermite, August 1885, M-L I.

<sup>25</sup> Shortly afterwards Hermite met Kronecker and told him that he accepted full responsibility for Question 4. He explained his intentions in setting the question and, moreover, admitted that he had set it specifically with Poincaré in mind. Hermite's explanation seems to have satisfied Kronecker as he did not pursue the issue. Perhaps it was sufficient that Weierstrass was not involved. *Cahiers* 6 (1985), 108-111.

Kronecker let matters rest, but not for long. In 1888 he launched another attack but this time it was directed at the contents of Question 1. On this occasion he did not write to Mittag-Leffler but instead made his complaint into a public affair by presenting a Note at a meeting of the Berlin Academy<sup>26</sup>.

Weierstrass, in composing the question, had drawn on information contained in a speech on Dirichlet given by Kummer<sup>27</sup>. This had led Weierstrass to say that Dirichlet had told a "friend" that he had discovered a method for integrating the differential equations of mechanics and through this method had succeeded in proving the stability of the solar system. However, since Kronecker was the "friend" to whom Dirichlet had communicated his results Kronecker felt he could claim to know what Dirichlet had really said and disputed the accuracy of Weierstrass' remarks. Kronecker's version of the events was that Dirichlet had first told him about the stability proof and then only later and on a separate occasion told him about the method, in other words the two events were not contingent as Weierstrass had implied.

The content of Kronecker's second offensive would not have come as a complete surprise to Mittag-Leffler since, in August 1885, he had received a long letter from Kronecker part of which centred on this question<sup>28</sup>. In addition, in October the following year, 1886, Kronecker had openly declared that he considered Dirichlet to have been misquoted in the question and he intended to publish his version of events<sup>29</sup>. Since Kronecker's complaint concerned unpublished work by Dirichlet who had died in 1859, almost 30 years before, it is not clear why he waited a further three years before going public rather than pursuing the issue at the time he first raised it with Mittag-Leffler.

However, the fact that Kronecker had seen fit to use the Berlin Academy of Sciences to air his views, combined with the fact that it was Weierstrass who had set the question, gave the commission strong grounds for thinking that this attack was yet a

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<sup>26</sup> Kronecker [1888].

<sup>27</sup> See Appendix 2.

<sup>28</sup> Kronecker also disputed that the definition of "higher analysis" could be used to describe Questions 1 and 4. In addition he also claimed to be highly competent to answer both these questions. Letter from Kronecker to Mittag-Leffler, 16.8.1885, M-L I.

<sup>29</sup> Mittag-Leffler to Hermite, 7.10.1886, Archives de l'Académie de Science, Paris. *Archive for History of Exact Sciences* 10, 1973, 162.



further manifestation of the rivalry between the two Germans rather than a direct assault on the commission as a whole. Nevertheless, since Kronecker steadfastly maintained that he did not know who had composed the question, it was difficult for the commission to know how best to respond to him. Should they do so collectively and in the name of the commission, or should Weierstrass personally take on the responsibility?

Hermite made it quite plain that he did not wish to be involved in the dispute. Not only did he consider the matter to be an entirely German affair between the "two princes of analysis", but also he considered it his patriotic duty to avoid doing anything that could be construed as having a national connection<sup>30</sup>. He was convinced that Kronecker was a committed Francophobe and in consequence felt there was nothing to be gained by his intervention.

Weierstrass had no difficulty in dealing with Kronecker's complaints but was reluctant to do so on his own - he considered it the responsibility of the commission<sup>31</sup>. He believed that his own description of the events was essentially true. For even if Dirichlet had told Kronecker about the proof and the method at different times (which probably meant at the most one or two days apart) that did not mean that they had not been connected by Dirichlet. Likewise neither was the order in which Dirichlet related his discoveries to Kronecker evidence that that was the order in which he had discovered them. The only point Weierstrass was willing to concede was that he had omitted Kronecker's name as the "friend" to whom Dirichlet had communicated his results. In any case, from Weierstrass' point of view, what was important about Dirichlet's remarks was the fact that they provided real hope that a solution to the  $n$  body problem could be found and hence a good reason for including the question in the competition.

After much deliberation the commission decided that they would be in a better position to reply to Kronecker when the judging of the competition had been completed and the winning paper(s) published. Thus they elected to leave the matter open until then. It turned out to be a wise decision. Not only did subsequent events overshadow the issue but the need to reply was obviated by Kronecker's death which occurred in 1891 shortly after the publication of the winning memoirs.

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<sup>30</sup> Hermite to Mittag-Leffler, 6.6.1888, M-L I. *Cahiers* 6 (1985), 140-2.

<sup>31</sup> Weierstrass to Mittag-Leffler, 23.5.1888, M-L I. Mittag-Leffler [1912, 47-49].



## 4.4 The entries

Despite the fact that the identity of entrants for the competition was supposed to be secret, all three members of the commission were aware before the closing date that Poincaré meant to submit an entry. As early as July 1887 Poincaré, who was at that time the professor of mathematical physics and probability at the Sorbonne, had made clear his intentions to Mittag-Leffler<sup>32</sup>, explicitly mentioning Question 1. In October of the same year Hermite told Mittag-Leffler that although he knew Poincaré was working on something for the competition, he did not know whether Poincaré would submit it and, in any case, he was not sure whether Poincaré was working on astronomy (Question 1) or Fuchsian functions (Question 4). Mittag-Leffler, still scarred from Kronecker's original attack, admitted to Poincaré that he hoped he would provide an answer to Question 4<sup>33</sup>.

In fact the selection of topics for the competition was such that it would have been possible for Poincaré to have submitted an entry on any one of them. This begs the question: had they all been chosen with Poincaré in mind? This was certainly the case with Question 4 (see *Footnote* 25), but perhaps Weierstrass too had designed his questions to appeal particularly to Poincaré. In any case Mittag-Leffler was an unquestionable champion of Poincaré's work<sup>34</sup>. In the event, Poincaré chose to devote himself to seeking a solution to the most difficult of the four, the one on the  $n$ -body problem.

By the closing date twelve entries had been received. Shortly afterwards a list of their titles was published in *Acta* where, in accordance with the rules of the competition, the authors were identified solely by their respective mottos<sup>35</sup>.

The entries were numbered in the order in which they were received and five of the entries, including that of Poincaré (number 9), attempted Question 1, one attempted question 3 (number 4), and the remaining six covered a variety of topics of their own choice.

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<sup>32</sup> Poincaré to Mittag-Leffler, 16.7.1887, M-L I. *Acta* 38 (1921), 162-3.

<sup>33</sup> Mittag-Leffler to Poincaré, 17.11.1887, M-L I.

<sup>34</sup> Mittag-Leffler secured Poincaré's support for the launch of *Acta*, publishing important papers by him in each of the first five volumes.

<sup>35</sup> See Appendix 3 and *Acta* 11, 401-402.

When Poincaré's entry arrived it was clear that his reading of the competition regulations had been somewhat perfunctory. As required he had inscribed his memoir with an epigraph, *Nunquam præsriptos transibunt sidera fines* (Nothing exceeds the limits of the stars), but, contrary to the correct procedure, he had omitted to enclose a sealed envelope containing his name, and instead had written and signed a covering letter. In addition to this official letter he had also sent a personal note to Mittag-Leffler to tell him that he had sent in an entry<sup>36</sup>. This infringement of the rules was doubtless a complete oversight on Poincaré's part, as from his point of view anonymity was an impossibility since he had already told Mittag-Leffler and Hermite that he intended to send in an entry. Added to which his entry was an explicit development of his earlier work on differential equations with which all three members of the commission were familiar, and in any case his handwriting would not have gone unrecognised.

Apart from Poincaré, it has only been possible to identify positively the authors of three of the other entries: numbers 4, 8, and 10. With regard to entry number 8, the correspondence between the members of the commission makes clear that they themselves quickly established its author. The paper had been submitted with a covering note from Paul Appell, professor of rational mechanics at the Sorbonne and a regular contributor to *Acta*, claiming that it had been written by someone "well-known to him"<sup>37</sup>. Having originally surmised that the author was a student or friend of Appell's, the commission rapidly came to the correct conclusion that it had been written by Appell himself<sup>38</sup>.

The authors of numbers 4 and 10 are identifiable as a result of the communication they had with the commission after the winner of the competition had been announced. Number 4 came from Guy de Longchamps, a professor in Paris, who, having a rather high opinion of his own work and having been passed over for the prize, saw fit to complain to Hermite (who did not share his opinion) about the manner in which the competition had been conducted<sup>39</sup>. Number 10 was the entry of Jean Escary, a professor at the Military School of La Flèche, who later became a professor at the Lycée de Constantine in Algeria. On learning of Poincaré's success he

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<sup>36</sup> Poincaré to Mittag-Leffler, 17.5.1888, Nos. 40, 41, M-L I.

<sup>37</sup> Appell to Mittag-Leffler, 13.5.1888, M-L I.

<sup>38</sup> Mittag-Leffler to Hermite, 17.10.1888, No. 1146, M-L I.

<sup>39</sup> Hermite to Mittag-Leffler, 4.2.1889. *Cahiers* 6 (1985), 160.

wrote to Mittag-Leffler enclosing some corrections to his own paper and praising Poincaré<sup>40</sup>. Earlier Mittag-Leffler, having spotted one of Escary's mistakes and unaware of the paper's authorship, had confided in Weierstrass that he thought that the paper was by Dillner, a professor in Uppsala since "... for a long time the poor man has been unable to deal with mathematics."<sup>41</sup>

Although there were officially twelve entries to the competition, the correspondence at the Mittag-Leffler Institute does reveal one further entry which was not only personally addressed to the King, but also having been written on 26th December 1888, arrived too late for consideration<sup>42</sup>. Nevertheless, had it arrived on time, it would not have added significantly to the commission's task since the entrant, Cyrus Legg from Clapham, London, claimed the prize for his proof of the trisection of the angle by ruler and compass alone!

#### 4.5 Judgement of the entries

A large part of the judging of the competition was done by correspondence. Mittag-Leffler, having received the entries in Stockholm, appointed one of the editors of *Acta*, Edvard Phragmén, the task of doing the preliminary reading prior to having copies of the most significant entries made and sent to Hermite and Weierstrass. Within a fortnight of the closing date Mittag-Leffler had written to both Hermite and Weierstrass with his opinion that there were only three entries worthy of consideration: those from Poincaré and Appell, and one which came from Heidelberg (number 5), although none of them had provided a complete solution to any of the set questions. Poincaré's entry on Question 1 was essentially concerned with the restricted three body problem (rather than the  $n$  body problem); Appell had not attempted any of the set questions but instead had provided a memoir on the expansion of Abelian functions by trigonometric series; and the entry from Heidelberg had treated Question 1 from an astronomical point of view. By the beginning of July Mittag-Leffler's confidence in the outcome was sufficient for him to

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<sup>40</sup> *Fortschritte* for 1889 and 1893 lists editions of Escary's paper as being published elsewhere but gives no further details.

<sup>41</sup> Mittag-Leffler to Weierstrass 16.11.1888, M-L I.

<sup>42</sup> Cyrus Legg to King Oscar II, 26.12.88, M.L.I.



write to Weierstrass to say that he thought Poincaré should win<sup>43</sup>. Shortly afterwards he added the further endorsement that Phragmén thought Poincaré's paper quite remarkable.

Mittag-Leffler then spent August in Germany with Weierstrass so that they could study the different memoirs together. After which he wrote to Hermite to tell him that they now considered only two of the memoirs were contenders for the prize: those of Poincaré and Appell, and since Appell had not attempted any of the proposed questions, the prize should go to Poincaré, with an honourable mention for Appell<sup>44</sup>. Meanwhile Hermite himself had been studying Poincaré's memoir and conveyed to Mittag-Leffler that he too was absolutely convinced as to the importance of the work<sup>45</sup>.

The commission had quickly reached a unanimous decision but the hard part of their work had barely begun. It was one thing to recognise the quality of Poincaré's work but quite another to understand it. Not only was Poincaré's entry extremely long but also it contained many new ideas and results which required careful study. Moreover, as Hermite freely admitted, the difficulties of comprehension were compounded by Poincaré's customary lack of detail:

*"But it must be acknowledged, in this work ([P1]) as in almost all his researches, Poincaré shows the way and gives the signs, but leaves much to be done to fill the gaps and complete his work. Often Picard has asked him for enlightenment and explanations on very important points in his articles in the Comptes Rendus, without being able to obtain anything except the statement: "it is so, it is like that", so that he seems like a seer to whom truths appear in a bright light, but mostly to him alone."*<sup>46</sup>.

The correspondence shows all three members of the commission struggling with various parts of the memoir, but it was Mittag-Leffler who, determined that the version submitted to the King should be as complete as possible, entered into correspondence with Poincaré (notwithstanding the rules of the competition whereby he should have been ignorant of the paper's authorship) appealing for

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<sup>43</sup> Mittag-Leffler to Weierstrass, 3.7.1888, M-L I.

<sup>44</sup> Mittag-Leffler to Hermite, 17.10.1888, No. 1146, M-L I.

<sup>45</sup> Hermite to Mittag-Leffler, 17.10.1888, *Cahiers* 6 (1985), 146.

<sup>46</sup> Hermite to Mittag-Leffler, 22.10.1888. *Cahiers* 6 (1985), 147.



clarification on several issues<sup>47</sup>. In answer to these appeals, Poincaré produced substantial *Notes* to be appended to the paper, the nature and extent of which can be clearly seen in the table of contents as it appeared when the paper was originally printed for publication in *Acta*<sup>48</sup>. These supplements eventually amounted to an additional 93 pages, which represented more than a fifty percent increase in the length of the memoir. Mittag-Leffler also took the opportunity to ask Poincaré for a sealed envelope containing his name and address in order to remedy Poincaré's earlier omission<sup>49</sup>.

Mittag-Leffler may have had no qualms about his contact with Poincaré but Weierstrass certainly did and he made a point of asking Mittag-Leffler not to mention the fact that he knew for certain that Poincaré had entered the competition<sup>50</sup>. He later told Mittag-Leffler that it was almost an axiom in Germany that a prize paper must be published exactly in the form in which it was submitted<sup>51</sup>. From his own point of view Weierstrass thought that the proper time for additions and corrections was when the paper was being edited for publication, but then only providing they were clearly acknowledged<sup>52</sup>.

With regard to the jury's opinion of the other papers, Weierstrass wrote to Mittag-Leffler in November with a report on five of the entries, although in reality the result of the competition had already been decided<sup>53</sup>. Of note in this report was his dismissal of number 5 as insufficiently mathematical, his recognition of the quality of Appell's paper, an opinion which had been further endorsed by Schwarz to whom he had given Appell's paper to review, and, of course, his opinion on Poincaré's paper. He reiterated his belief that Poincaré's paper deserved the prize and singled out the particular results which he thought most important, the details of which are discussed in 6.2.

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<sup>47</sup> Mittag-Leffler to Poincaré, 15.11.1888, M-L I.

<sup>48</sup> See Appendix 5a.

<sup>49</sup> Poincaré to Mittag-Leffler, 25.10.1888, No. 43, M-L I.

<sup>50</sup> Weierstrass to Mittag-Leffler, 6.7.1888, M-L I.

<sup>51</sup> Weierstrass to Mittag-Leffler, 2.4.1890, M-L I.

<sup>52</sup> Weierstrass to Mittag-Leffler, 8.3.1890, M-L I.

<sup>53</sup> Weierstrass to Mittag-Leffler, 15.11.1888, M-L I. Mittag-Leffler [1912, 50-52].

## 4.6 The prize announcement

It now only remained for the commission to fulfil their final obligations to the King. This involved producing reports on the competition, one on each of the winning memoirs and one on the competition as a whole, the intention being for the reports on the memoirs to be published alongside the memoirs themselves. It fell to Weierstrass as the originator of the question to write the report on Poincaré's paper, although Mittag-Leffler did express certain reservations about Weierstrass' capacity for the undertaking on account of the poor state of Weierstrass' health<sup>54</sup>. The question of who should write the report on Appell's paper proved rather more difficult. Although it seemed without doubt that it would put an impossible strain on Weierstrass to write both reports, Hermite felt that since Appell was both a friend and a compatriot (not to mention a relation by marriage) the objectivity of his judgement would be called into question and so he was reluctant to shoulder the responsibility<sup>55</sup>. Nevertheless, in the end Mittag-Leffler was able to persuade Hermite to take on the task. The writing of the general report, which was to be in French, was undertaken by Mittag-Leffler, Oscar having already made clear the sort of detail he required.

On the 20th January 1889, the day before the King's 60th birthday, Mittag-Leffler went to the palace and the result was officially approved. The King decreed that the general report should be translated into Swedish and printed in the newspaper *Posttidningen*<sup>56</sup>. The following day Mittag-Leffler wrote to Poincaré to tell him that he would be receiving an official copy of the report via the Swedish ambassador in Paris within the next few days<sup>57</sup>. Everything had been completed to the King's satisfaction with the sole exception of Weierstrass' report on Poincaré's memoir. Weierstrass, who had made no secret of his ill health throughout the competition, was sufficiently unwell to fulfil his obligation within the allotted time but gave assurances that it shortly would be completed.

Needless to say, Kronecker too was keenly awaiting Weierstrass' report as Mittag-Leffler confided to Poincaré:

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<sup>54</sup> Mittag-Leffler to Hermite 17.10.1888, No. 1146, M-L I.

<sup>55</sup> Hermite to Mittag-Leffler, 22.10.1888. *Cahiers* 6 (1985), 147-149.

<sup>56</sup> Mittag-Leffler to Weierstrass, 26.1.1889, M-L I.

<sup>57</sup> Mittag-Leffler to Poincaré, 21.1.1889, M-L I. For a copy of the report see Appendix 4.

*"Kronecker is dreadful and he is only waiting for the publication of the report so that he can criticise it."*<sup>58</sup>.

Mittag-Leffler also wrote to the Academy of Sciences in Paris with details of the competition results, adding that, as stated in the announcement of the competition, the winning memoirs would be published in the next volume of *Acta*, which he hoped would appear that October<sup>59</sup>. The news of Poincaré's and Appell's success was well publicised in the French press<sup>60</sup> and in recognition of their achievement, they were both made Knights of the Legion of Honour. The French triumph also proved favourable for Mittag-Leffler since he too was similarly honoured for his role in promoting French mathematics.

Nevertheless, Mittag-Leffler's troubles were far from over. The publication of the general report, which contained only a cursory indication of Poincaré's results, signalled the start of a distressing polemic between Mittag-Leffler and the astronomer Gylden who at the time was not only lecturing at the Stockholm Högskola alongside Mittag-Leffler but was also on the editorial board of *Acta*. From what Gylden could glean from the report, he believed that most of Poincaré's results were already contained in his own [1887] memoir on the convergence of series used in celestial mechanics, and said as much in the February meeting of the Swedish Academy of Sciences. Once again Mittag-Leffler was placed in an uncomfortable position. Called upon by the King to defend Poincaré's memoir at the March meeting of the Academy, he wisely wrote to Poincaré explaining his dilemma and asking for further assistance. Although he completed the defence to his own satisfaction, the issue refused to die down immediately and continued to haunt him during the ensuing months. The details of the controversy are discussed in 6.3.

However, Mittag-Leffler's dispute with Gylden paled into insignificance when compared with the problem which subsequently emerged. As already remarked, Mittag-Leffler, having allowed time for editing, had hoped to have the volume of *Acta* containing the winning memoirs published by October 1889. Apart from Weierstrass' report, for which he had continued to press although without success,

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<sup>58</sup> Mittag-Leffler to Poincaré 23.2.1889, M-L I.

<sup>59</sup> *Comptes Rendus* 108 (25.2.1889), 8.

<sup>60</sup> Hermite to Mittag-Leffler, 28.1.1889. *Cahiers* 6 (1985), 159.



the actual printing was completed by the end of November. But the volume did not appear until the end of the following year and, furthermore, it did not contain a replica of the memoir Poincaré had submitted for the prize. What had occurred in the interim?

## 4.7 Discovery of the error

The first glimmer that anything was awry occurred in July 1889. Phragmén, who was editing Poincaré's memoir for publication, alerted Mittag-Leffler to some passages in it which seemed to him a little obscure. Thus prompted, Mittag-Leffler wrote to Poincaré for yet further clarification<sup>61</sup>. However, it was not until much later that the scale of the problem became evident. Poincaré, in the course of dealing with Phragmén's queries, realised that he had made a serious error in a different part of the paper. At the beginning of December, he wrote to Mittag-Leffler and, making no attempt to conceal his distress, told him that he had written to Phragmén to tell him of the error, the consequences of which were more far-reaching than he first thought, and as a result of which he was having to make substantial changes to the memoir<sup>62</sup>.

This was most unwelcome news for Mittag-Leffler since, although the volume of *Acta* had not been published, a limited number of printed copies of the memoir had already been circulated. Once more Mittag-Leffler's carefully nurtured mathematical reputation was in jeopardy. Despite his confidence in the overall quality of the memoir, he was only too conscious of the inevitable damage he would suffer should word of the error become public. Although he knew to whom the copies had been sent, procuring their return would not be easy since several had been dispatched to destinations outside Sweden.

Mittag-Leffler wrote to Poincaré to let him know the whereabouts of some of the copies<sup>63</sup>. As far as those outside Sweden were concerned, Hermite and Weierstrass had each received one, as had the analyst Camille Jordan and the editor of *Mathematische Annalen*, Walther von Dyck. The one piece of good news was that a

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<sup>61</sup> Mittag-Leffler to Poincaré, 16.7.1889, M-L I.

<sup>62</sup> Poincaré to Mittag-Leffler, postmarked 1.12.1889. The contents of this letter are given in full at the end of 5.8.3.

<sup>63</sup> Mittag-Leffler to Poincaré, 5.12.1889, M-L I.



copy had not been sent to Kronecker! With regard to those in circulation in Stockholm, Mittag-Leffler was particularly anxious to retrieve the copies from Gylden and Lindstedt without arousing suspicion. In an attempt to minimise the scandal, he suggested to Poincaré that everything concerning the error should be kept between themselves at least until publication of the new memoir.

Several other recipients of pre-publication copies of the memoir can be identified from a note sent by Phragmén to Mittag-Leffler at the end of January<sup>64</sup>. In this Phragmén listed Gylden, Lindstedt, Lindelöf, Mohelins, Lie, Hill, Stone, Kovalevskaya and Lindquist, as well as the Royal Swedish Academy of Sciences and the Minister of Justice, as either still being in possession of a copy or having returned one to the Institute.

Mittag-Leffler appears to have been tireless in his determination to ensure that no evidence of the incorrect memoir should remain outside the offices of *Acta*. Today there are two bound copies of the original printed paper in existence at the Mittag-Leffler Institute and inside the front cover of one of them, written in Mittag-Leffler's hand, is a Swedish phrase which translated means "*whole edition destroyed*".

In order to safeguard himself still further, Mittag-Leffler gave detailed instructions to Poincaré as to how he would like the introduction to the reworked memoir to appear<sup>65</sup>. Furthermore, he also asked Poincaré to pay for the printing of the original memoir. A request to which Poincaré agreed without demur, despite the fact that the cost turned out to be just over 3,500 Crowns, some 1,000 Crowns more than the prize he had originally won.

Mittag-Leffler was also faced with the problem of telling Hermite and Weierstrass about the error. As fellow members of the commission, he knew that the news of its discovery would be most unwelcome. He adopted two quite different approaches.

In Hermite's case, Mittag-Leffler had little choice but to be open about the situation since he knew that inevitably Hermite would hear about it from Poincaré himself. Thus, as soon as Mittag-Leffler heard the news from Poincaré he immediately sent Hermite copies of the correspondence between himself and Poincaré, admitting that he believed the error to be so serious that he thought it likely that almost every

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<sup>64</sup> Phragmén to Mittag-Leffler, 26.1.1890, M-L I.

<sup>65</sup> Mittag-Leffler to Poincaré, 5.12.1889, M-L I.

page in the memoir would contain a false result<sup>66</sup>. Meanwhile, Hermite had seen Poincaré who had told him not only about the error but also that he thought things were not quite so bad as he had originally described to Mittag-Leffler. As a measure of his confidence, Poincaré had offered to prepare a summary of his results for Hermite so that Hermite could give a report to the Paris Academy of Sciences the following week<sup>67</sup>.

Unfortunately, Poincaré's original fears were realised. Exactly a week later Hermite was writing to Mittag-Leffler to tell him that he thought the situation was very serious after all<sup>68</sup>. He had heard nothing from Poincaré in the interim, the promised summary had never arrived and he had not been able to make his report to the Academy. Thereafter Hermite, not wishing to cause Poincaré undue distress, communicated with him no further on the subject, leaving the matter entirely for Mittag-Leffler to handle. Nevertheless, it is clear from the correspondence that despite Hermite's anxiety he was convinced that Poincaré would eventually resolve the problem.

With regard to Weierstrass, Mittag-Leffler took an entirely different line. He initially played down the seriousness of the problem and managed to give Weierstrass the impression that the delay in publication was simply due to the correction of some minor details. Doubtless Kronecker's presence in Berlin served to strengthen Mittag-Leffler's resolve to minimise the problem. As a result, in February 1890 when Gylden and Wolf brought to Berlin the rumours of serious errors in the paper, Weierstrass was placed in an extremely embarrassing position<sup>69</sup>. Awkward questions were asked which he was in no position to answer. He demanded an explanation.

In defence, Mittag-Leffler claimed that his decision not to reveal everything about the error had been purely motivated by his consideration for Weierstrass' delicate state of health<sup>70</sup>. He told Weierstrass that he believed Gylden was only acting in self interest and that the situation was nothing like so bad as Gylden was trying to

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<sup>66</sup> Mittag-Leffler to Hermite, 6.12.1889, No. 1372, M-L I.

<sup>67</sup> Hermite to Mittag-Leffler, 10.12.1889. *Cahiers* 6 (1985), 180-181.

<sup>68</sup> Hermite to Mittag-Leffler, 17.12.1889. *Cahiers* 6 (1985), 181-182.

<sup>69</sup> Weierstrass to Mittag-Leffler, 8.3.1890, M-L I.

<sup>70</sup> Mittag-Leffler to Weierstrass, 15.3.1890, M-L I.

make out. Moreover, he claimed that the French mathematicians, including Poincaré and Hermite, were quite relaxed about the problem, and since the critics, such as Kronecker, whom he denounced as only being able to recognise something as important if he had done it himself, and Gylden, were in a minority, there was no need for Weierstrass to worry! Weierstrass could do little except express his dissatisfaction at the way things had turned out and ask Mittag-Leffler to send him a proof of the new version as soon as possible<sup>71</sup>. He was clearly frustrated at not discovering the error himself, although he was more concerned about the inaccuracies which might be contained in the general report, the extent of which he could not ascertain until he knew the details of the error. Fortunately, his worries in this direction were groundless. The generalities the report contained still held true and the lack of mathematical detail meant that there was nothing in it that could not equally well be applied to the revised memoir.

Phragmén's role in setting Poincaré on the trail of an error which had escaped the attention of all three members of the commission was certainly worthy of recognition. However, and characteristically, Mittag-Leffler did not see it in his best interests to acknowledge Phragmén's participation publicly. Nevertheless, he did ask Poincaré for his written support to help Phragmén in his attempt to secure the chair in mechanics at the university in Stockholm<sup>72</sup>, and perhaps it may be more than coincidence that Phragmén was promoted to the editorial board of *Acta* in the following year. It is also of interest to note that in November 1889 Phragmén wrote a paper in which he showed that some of Poincaré's results could be applied to dynamical problems other than the restricted three body problem.

## 4.8 Publication of the winning entries

However, and doubtless to the relief of Mittag-Leffler, by the beginning of January 1890 Poincaré had completed his reworking of the memoir and sent a copy to Phragmén to edit for publication. Not only had he made substantial alterations to accommodate the corrections but also, where appropriate, he had incorporated the explanatory *Notes* into the paper itself. Thus the revised paper took on a significantly different appearance to that of its predecessor.

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<sup>71</sup> Weierstrass to Mittag-Leffler, 2.4.1890, M-L I.

<sup>72</sup> Mittag-Leffler to Poincaré, 4.12.1889, M-L I.



Although printing began at the end of April that year, a backlog of other work meant that it was not completed until the middle of November. When Volume 13 of *Acta* eventually appeared it contained both Poincaré's and Appell's memoirs together with Hermite's report on the latter. Weierstrass' report on Poincaré's memoir, still not finished, was promised for a future volume. As Weierstrass himself remarked to Mittag-Leffler, it was extremely fortunate that he had never completed the original report, but in the event, he never managed to complete a revised report either.

Prior to the discovery of the error Weierstrass had got as far as writing an introduction for the proposed report which he had eventually sent to Mittag-Leffler in March 1889. But this was only concerned with issues connected with the actual question and contained no mention of Poincaré's paper. So although the comments in it were not invalidated by the error, it did not provide the much-needed guide to Poincaré's paper. Nevertheless, it was certainly not without interest and Mittag-Leffler [1912] selected it to appear in his biography of Weierstrass which was published in *Acta* and which solely focused on Weierstrass' interest in the  $n$  body problem<sup>73</sup>. A discussion of this introduction is given in 6.3 where it is put into context with Weierstrass' private remarks about Poincaré's memoir, many of which Mittag-Leffler also saw fit to publish in [1912].

Given Mittag-Leffler's initial concern over obtaining Weierstrass' report, it might seem somewhat surprising that he was not able to induce him to complete it. However, after the discovery of the error, there is a marked reduction in Mittag-Leffler's concern for the report. Weierstrass had made it quite plain to Mittag-Leffler that he felt a moral obligation to make public the history of the error, but Mittag-Leffler's preoccupation with his own reputation meant that he was extremely keen to play down the error's importance and undoubtedly he wanted Weierstrass to do likewise. Weierstrass' position being contrary to that of Mittag-Leffler's, it is tempting to assume that Mittag-Leffler considered it in his own best interests for Weierstrass' report never to appear.

Thus, over a year later than Mittag-Leffler had originally planned, the climax to the competition, the publication of the winning entries in *Acta*, finally took place. More than six years had elapsed since Mittag-Leffler had written optimistically to

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<sup>73</sup> Mittag-Leffler [1912, 63-65].



Kovalevskaya with the original plans for the competition. Despite Kovalevskaya's less than enthusiastic response, Mittag-Leffler could scarcely have foreseen the turbulent course of events which were to follow. Nevertheless, in the final analysis Mittag-Leffler's considerable efforts were rewarded. Once the *Acta* volume was in circulation, the rumours of the error faded and the brilliance of Poincaré's memoir was acknowledged. Importantly for Mittag-Leffler, his hope that the competition would result in some major new mathematics had been amply fulfilled. Poincaré's memoir had ensured that King Oscar's 60th birthday celebration would not be forgotten.

## 5. Poincaré's Memoir on the Three Body Problem

### 5.1 Introduction

Poincaré's memoir is remarkable in many ways. Firstly, Poincaré's unprecedented qualitative approach to the three body problem and its intrinsic dynamics is unequivocally more powerful than any previous methodology. Starting from a reductionist view and considering the periodic solutions of the restricted three body problem, Poincaré's global qualitative perspective led to the brilliant discovery of asymptotic solutions which constitute a whole new class of solutions for the problem. The discovery of these solutions and the complex nature of their behaviour was quite unpredicted and mark a turning point in the history of dynamics. Secondly, the paper contains many new and innovative ideas which have been extended, not only within the context in which they were developed, but also in several other branches of mathematics. Thirdly, it provided the fundamental basis for what is often described as his *chef d'œuvre*, his three volume *Les Méthodes Nouvelles de la Mécanique Céleste*<sup>1</sup>. Furthermore, the paper contains what is essentially the first mathematical description of chaotic motion in a dynamical system, and it will be

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<sup>1</sup> Henceforth referred to as [MN, I-III].

shown that this latter aspect, as well as being historically important in its own right, has an added significance in the context of the history of the paper itself.

In 4.5 and 4.7 the circumstances were described which resulted in Poincaré making two major changes to the content and structure of the memoir before its publication in 1890. The first of these, the addition of substantial explanatory *Notes*, was made in response to requests for more detail from Mittag-Leffler, and the second, the extensive rewriting, was the result of the discovery of an important error. Although Poincaré touched on the subject of these changes in his introduction to the published paper [P2], he did not make clear the extent of the alterations. Unfortunately, it has not been possible to trace the paper Poincaré originally submitted for the prize but correspondence at the Mittag-Leffler Institute suggests that, excluding the *Notes*, it assumed a very similar form to the first printed version [P1], copies of which still exist at the Institute. Of especial importance amongst the latter is the one [P1a] which was personally corrected and extended by Poincaré, and to which he attached a note detailing the changes. This copy in its altered form corresponds almost exactly to the published memoir and it provides a remarkable record of the way the memoir was rewritten. Its existence has made it possible to follow the metamorphosis of the entire memoir and provide a complete picture of the exact nature of the error.

This chapter, as well as containing a detailed mathematical analysis of the memoir, also describes how [P2] relates both to the version which actually won the prize and to the *Notes*. It is shown how much of the original version was retained in [P2], how the *Notes* were integrated (or not) into [P2], and to what extent [P2] was shaped by the detection of the error. Furthermore, it is argued that the discovery and correction of the error plays a fundamental role in the perception of the memoir today.

As a prelude to the mathematics, the tables of contents of both printed versions are compared in order to give a preliminary idea of the overall structure of each version and their relationship to each other. The differences detailed here are also noted at the appropriate points in the mathematical study. This is accompanied by an comparison of the two introductions since this provides an interesting insight into how Poincaré's own perspective on his work changed.

Poincaré's chapter and section headings from [P2] are followed, and copies of the tables of contents of [P1] and [P2] are included as Appendices 5a and 5b respectively.

## 5.2 Tables of contents

In each case the memoir is prefaced with an introduction before being split into two parts: *Generalities* and *The equations of dynamics and the  $n$  body problem*. The first part is devoted to developing the theory and the second to applying it, each part being divided into chapters which are then subdivided into sections. The only difference in the format being that in [P1] the sections are numbered within each chapter, while in [P2] the section numbering runs straight through the memoir making it easier for cross-referencing. [P1] concludes with nine explanatory *Notes*, the individual topics being additionally identified by the letters *A* to *I*.

A comparison of the table of contents shows two major changes in the first part of the memoir. Chapter I in [P2] contains four sections as opposed to only one in [P1]. In addition to the identical first section on notation and definitions, [P2] includes two sections, §2 and §3, on the method of majorants, as well as a section, §4, on the integration of linear differential equations with periodic coefficients. Most of §2 and all of §3 are taken from *Note E*, and §4 is *Note D* reproduced in its entirety. The format of Chapter II is identical in both versions, although there are significant changes to the content. *Note C* is incorporated at the end of §6, and §8 contains major alterations. Chapter III contains the most important change in Part 1 with the addition in [P2] of a new concluding section, §14, on the asymptotic solutions of the dynamical equations, the contents of which, apart from including the latter half of *Note I*, do not appear in [P1]. The other sections in Chapter III carry the same headings in both versions but there are significant changes and additions to their content.

In the Part 2 the differences are more marked.

In [P1] the application of the theory to the equations of dynamics is confined to the first chapter which contains five sections, with a second chapter devoted to a general resumé of the results (positive and negative) and a final chapter consisting of a single section on Poincaré's endeavours to generalise his results to the  $n$  body problem. [P1] then concludes with the nine *Notes*.

In [P2] the first section corresponds with that in [P1] but it is now the only section in the first chapter. Significantly, the topic of asymptotic surfaces has been revised to merit a chapter in its own right. The new second chapter consists of four sections,



none of which retain an exact title from [P1]. One of the sections, §17, contains material from [P1], one, §18, contains an amended version of *Note F* together with some additions, and two, §16 and §19, are entirely new. The third chapter on miscellaneous results includes as §20 the section on periodic solutions of the second kind taken from the first chapter in Part 2 of [P1], a section on the divergence of Lindstedt's series, §21, taken both from the negative results in [P1] and *Note A*, and a section on the non-existence of uniform integrals, §22, which contains the rewritten contents of *Note G*. The last chapter in [P1] on the  $n$  body problem is transferred almost intact to become the last chapter of [P2]. The section on positive results from [P1] is omitted altogether.

Insofar as the *Notes* are concerned, with the exception of *Note B*, *New statement of results* which is entirely deleted and *Note H*, *Characteristic exponents* (rewritten and expanded to appear as part of §12), they are incorporated, either whole or in part, into the main text of [P2] as indicated above, and the exact places where they occur are indicated. *Note B* was a summary of Poincaré's main results described in more practical terminology for the benefit of astronomers and its exclusion from [P2] is discussed.

Whenever a particular piece of the memoir is not specifically ascribed to either [P1] or [P2], then it is correct to assume that it appeared in the same form in both versions.

### 5.3 Poincaré's introductions

Poincaré began the introduction to [P1] with the admission that although the memoir had been written in response to Question 1 of the four competition questions, he had not been able to achieve a complete resolution of the problem set<sup>2</sup>. He was plainly keen to emphasise that he had not found a definitive answer to the three body problem and made it clear that he had concentrated on the restricted problem which he specified as follows:

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<sup>2</sup> Question 1: "A system being given of a number whatever of particles attracting one another mutually according to Newton's law, it is proposed, on the assumption that there never takes place an impact of two particles, to expand the coordinates of each particle in a series proceeding according to some known functions of time and converging uniformly for any space of time." For the complete question see Appendix 2.

*"I consider three masses, the first very large, the second small but finite, the third infinitely small; I assume that the first two each describe a circle around their common centre of gravity and that the third moves in the plane of these circles. An example would be the case of a small planet perturbed by Jupiter, if the eccentricity of Jupiter and the inclination of the orbits are disregarded."* [P1, 8].

He gave an indication of the main mathematical techniques which he had employed in the memoir. These included the trigonometric form of the power series solutions derived by both Lindstedt and Gylden which he had used to avoid the inclusion of secular terms known to exist in the series used by Laplace and Poisson; Cauchy's method of majorants which he had applied to prove the convergence of the series; his own geometric methods (taken from his earlier memoir on differential equations) which he had used to prove the stability of the solution; and his new idea of invariant integrals, the theory of which he had developed in order to facilitate the application of his geometric methods to the equations of dynamics.

He emphasised that the central topic of the memoir would be provided by his discussion of periodic solutions, and drew attention to the fact that he had been able to develop the theory using Cauchy's methods since the periodic solutions were untroubled by the problem of small divisors.

[P1] was printed as though it was an exact replica of Poincaré's competition entry and as such retained its "anonymous" format. However, apart from the other indicators mentioned in the previous chapter, a cursory reading of the introduction would probably have been sufficient to identify the author. In outlining the background to his methods, Poincaré needed to reference his own work, and consequently give his name, no less than five times.

Poincaré opened the introduction to [P2] by revealing that it was a reworking of his competition entry. He explained that the revision had resulted from incorporating the *Notes* and some additional explanations into the main body of the paper, a task which he considered a logical necessity but which he had not had time to do earlier. Although he did mention the error, acknowledging both Phragmén's role in detecting it and his assistance in general, he adhered to Mittag-Leffler's request and gave no hint of what it might have been.

Nevertheless, he did make it clear that he had included some substantial additions to the opening chapter by the way of reformulation of established theorems. In

drawing attention to his work on the periodic solutions he mentioned both the asymptotic and the doubly asymptotic solutions, and in this connection indicated the nature of the restricted three body problem although this time without completely stating the problem. He also mentioned his proof of the recurrence theorem.

Above all, he stressed what he called his negative results. These were his proof of the non-existence of uniform integrals for the restricted three body problem, and his proof of the divergence of Lindstedt's series, although he was careful to point out that he did not consider the divergence to detract in any way from the practical usefulness of the series. As a measure of the difficulties he had encountered in trying to generalise his results, he said that he believed a complete solution to the three body problem would require analytic tools quite different and infinitely more complicated than any of those known at present.

Finally, in connection with one of the series he had discussed, he acknowledged an analogy with a paper by Karl Bohlin [1888] which, since it had been published shortly before the closing date of the competition, he had not originally referred to.

Comparing the two introductions shows that Poincaré changed his emphasis from one which concentrated on mathematical techniques to one which stressed so-called negative results. In the introduction to [P1] there is a sense of optimism and it appears to herald the memoir as a step forward in a progression which is inexorably going on towards a complete resolution of the problem. The tenor of the introduction to [P2] is quite different. The future progress of the problem has lost its air of inevitability. In what follows it will be seen that the mathematical implications of the memoir's essential revision were both far-reaching and quite unexpected, and undoubtedly account for Poincaré's change in attitude.

## **5.4 General properties of differential equations**

The first chapter of the memoir provided the definitions and background for the theory to follow. Although most of the terminology used by Poincaré would have been familiar to the contemporary mathematical community, the length of the memoir and the fact that it was directed towards an international audience meant that Poincaré would have been conscious of the need to avoid any ambiguity or misunderstanding.



With regard to the background, Poincaré's concern was the integration of the differential equations using series methods. To this end he discussed and extended Cauchy's method of majorants with regard to both ordinary and partial differential equations, and concluded with a section on the use trigonometric series based on the methods of Floquet.

#### 5.4.1 Notation and definitions

The memoir centred on the system of ordinary differential equations given by

$$\frac{dx_i}{dt} = X_i \quad (i = 1, \dots, n)$$

where the  $X_i$  are single-valued analytic functions of the  $n$  variables  $x_1, \dots, x_n$ , and which may or may not be autonomous. In the case when  $n = 3$ , then, as in the earlier papers on the qualitative theory of differential equations, Poincaré made a geometric representation of the system. The  $x_i$  are considered as the coordinates of a point  $P$  in space so that as the time varies  $P$  describes a trajectory, and the set of trajectories which pass through each point of a given curve in space form a *surface trajectory*. From this representation Poincaré was led to a definition of stability in which he defined the system as stable if all its surface trajectories were closed. In other words the system was stable if any point  $P$  remained within a bounded region of space.

#### 5.4.2 The method of majorants

The method of majorants had originated with Cauchy in [1842] in the search for proofs for the existence of solutions to differential equations. Broadly speaking, the method is used to show that a power series in the independent variable (derived by the method of undetermined coefficients) which satisfies the differential equation does have a definite domain of convergence. It had been simplified by Briot and Bouquet [1854], used by Weierstrass in [1842] although not published until 1894<sup>3</sup>, studied by Fuchs<sup>4</sup>, and as previously mentioned, Poincaré himself had already worked on it in his thesis published in 1879.

Poincaré gave Cauchy's basic principle in the following form:

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<sup>3</sup> Weierstrass' work became known to his students and colleagues in the late 1850s. See Cooke [1984, 28].

<sup>4</sup> See Gray [1984].



Given a system of differential equations

$$\frac{dy}{dx} = f_1(x, y, z), \quad \frac{dz}{dx} = f_2(x, y, z) \quad (5.4.i)$$

where  $f_1$  and  $f_2$  can be expanded in increasing powers of  $x, y$  and  $z$ , then the equations have a unique solution

$$y = \phi_1(x), \quad z = \phi_2(x)$$

where  $\phi_i$  are Taylor series in  $x$  which vanish with  $x$ .

To verify that such a solution does indeed exist, it is necessary to prove that the series are convergent. The two functions  $f_1$  and  $f_2$  are replaced by the majorant function

$$f(x, y, z) = \frac{M}{(1 - \alpha x)(1 - \beta y)(1 - \gamma z)}$$

$M, \alpha, \beta, \gamma$  being chosen in such a way that each term of  $f$  has a larger coefficient (in absolute value) than the corresponding term in  $f_1$  and  $f_2$ . Replacing  $f_1$  and  $f_2$  by  $f$  increases the coefficients of  $\phi_1$  and  $\phi_2$  and since the series for  $f$  is convergent, the two series created by the exchange must be convergent, which in turn implies convergence of the original series for  $f_1$  and  $f_2$ .

In [P1], Poincaré made extensive reference to Cauchy's results, but he confined to *Note E* both his own exposition of the method, as well as some new developments which he had derived. In [P2] he extended these developments and, in incorporating the method into the memoir, he added two new sections. In the first, §2, he was concerned with theory as applied to ordinary differential equations, while in the second, §3, he dealt with its application to partial differential equations. Almost all the results from *Note E* were transferred into these two sections, and, in addition, three new theorems, III, V and VI, were included in §2.

The importance of these theorems within the context of the memoir is due to the fact that Poincaré's theory of periodic solutions depends fundamentally upon them. By formally including them at the beginning of [P2] Poincaré put into place an essential part of the foundations of the theory. In [P1] he used the results on frequent occasions but often with little or no reference, which made it extremely difficult to validate his arguments. Although not connected with the error the addition of this section (together with the following one) represents a significant contribution towards his aim of creating a more logical structure to the memoir.

Although most of the theorems contained in §2 are now well known, they are all stated here for ease of reference later.

In his first theorem, Poincaré extended Cauchy's original result by finding an expansion for the solution in terms of a parameter  $\mu$  as well as the independent variable  $t$ .

*Theorem I.* Suppose that the functions  $f_1$  and  $f_2$  depend, not only on  $x, y$  and  $z$ , but also on an arbitrary parameter  $\mu$  and that they can be expanded as series in  $x, y, z$  and  $\mu$ . The equations (5.4.i) can be written in the form

$$\frac{dx}{dt} = f(x, y, z, \mu) = 1, \quad \frac{dy}{dt} = f_1(x, y, z, \mu), \quad \frac{dz}{dt} = f_2(x, y, z, \mu) \quad (5.4.ii)$$

and it is possible to find three series

$$x = \phi(t, \mu, x_0, y_0, z_0) = t + x_0, \quad y = \phi_1(t, \mu, x_0, y_0, z_0), \quad z = \phi_2(t, \mu, x_0, y_0, z_0)$$

which formally satisfy the equations and which reduce respectively to  $x_0, y_0$  and  $z_0$  for  $t = 0$ . Then, provided  $t, \mu, x_0, y_0$  and  $z_0$  are sufficiently small, these series are convergent.

To prove the theorem Poincaré simply replaced the functions  $f, f_1$  and  $f_2$  by the function

$$f'(x, y, z, \mu) = \frac{M}{(1 - \beta\mu)(1 - \alpha(x + y + z))}$$

and formed majorant series for  $x, y$  and  $z$  which converge for sufficiently small values of  $t, \mu, x_0, y_0$  and  $z_0$ . However, although this gives the desirable result that the series solution is an expansion in ascending powers of the parameter as well as the independent variable, it also necessarily contains a severe restriction on the value of  $t$ . As Poincaré was ultimately looking for solutions valid for all values of time, it was essential that this restriction should be relaxed. In the following theorem, which has now become a classic in the theory of differential equations depending upon a parameter, Poincaré showed how the restriction could be loosened by proving the existence of a series solution which is an expansion in powers of the parameter and not of the independent variable.

*Theorem II.* Excluding one exceptional case,  $x, y$  and  $z$  can be expanded as powers of  $\mu, x_0, y_0$  and  $z_0$  for any value of  $t$ , provided  $\mu, x_0, y_0$ , and  $z_0$  are sufficiently small.

Briefly, consider the solution to equations (5.4.ii)

$$x = \omega_1(t, \mu), \quad y = \omega_2(t, \mu), \quad z = \omega_3(t, \mu)$$

which is such that  $x = y = z = 0$  when  $t = 0$  and which converges when  $0 < t < t_1$ . Then if  $x, y$ , and  $z$  are replaced in the equations (5.4.ii) by

$$x = \xi + \omega_1, \quad y = \eta + \omega_2, \quad z = \zeta + \omega_3$$

the differential equations become

$$\frac{d\xi}{dt} = \phi(\xi, \eta, \zeta, \mu), \quad \frac{d\eta}{dt} = \phi_1(\xi, \eta, \zeta, \mu), \quad \frac{d\zeta}{dt} = \phi_2(\xi, \eta, \zeta, \mu), \quad (5.4.iii)$$

and  $\phi, \phi_1$  and  $\phi_2$  vanish when  $\xi = \eta = \zeta = \mu = 0$ . Since it has been supposed that  $f, f_1$  and  $f_2$  can be expanded in powers of  $\mu$  then the same will be true of  $\phi, \phi_1$  and  $\phi_2$ , and these expansions can also be shown to be convergent in  $0 < t < t_1$ . Thus there exists a solution of equations (5.4.iii) as series in  $\mu$ , which is such that  $\xi = \eta = \zeta = 0$  when  $t = 0$ , and which converges in  $0 < t < t_1$ .

The exceptional case occurs when the functions  $f_1$  and  $f_2$  are no longer analytic in the variables  $x, y$ , and  $z$ , i.e. when they become infinite or cease to be single-valued. For if the functions are not analytic then it is no longer possible to expand the functions in series as required. In other words if as  $t$  changes the trajectory goes through a singular point, the theorem no longer holds. In the three body problem the functions given by the equations cease to be analytic in the case of a collision. However, since Weierstrass had specifically excluded collisions in the competition question, Poincaré considered the theorem sufficient in this respect<sup>5</sup>.

Poincaré next proved explicitly that the solutions depend analytically on the initial conditions. This theorem did not appear in [P1] and it seems likely that he originally believed the result to be self-evident from Theorem II.

*Theorem III.* Let

$$x = \omega_1(t, \mu, x_0, y_0, z_0), \quad y = \omega_2(t, \mu, x_0, y_0, z_0), \quad z = \omega_3(t, \mu, x_0, y_0, z_0)$$

be the solutions of the differential equations which reduce to  $x_0, y_0, z_0$  for  $t = 0$ . Then the functions  $\omega_i(t_1 + \tau, \mu, x_0, y_0, z_0)$ , ( $i = 1, 2, 3$ ) can be expanded as powers of  $\mu, x_0, y_0, z_0$ , and  $\tau$ , provided that these quantities are sufficiently small.

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<sup>5</sup> Poincaré appears not to have considered the possibility of non-collision singularities. The impossibility of such singularities in the three body problem was proved by Painlevé in [1897]. See 8.3.

Poincaré attributed the next theorem, now more familiarly known as the implicit function theorem, to Cauchy and his method of majorants<sup>6</sup>. Although it was not a new result, he had originally included it in *Note E* because it played such a pivotal role in his own investigations. Theorems V and VI which only appear in [P2] are direct extensions of it.

*Theorem IV.* A system of  $n$  equations

$$f_i(y_1, \dots, y_n, x_1, \dots, x_p) = 0, \quad (i = 1, \dots, n)$$

where the  $f$  are analytic functions of the  $n + p$  variables  $y$  and  $x$ , and vanish with them, can be solved for  $y_1, \dots, y_n$  in increasing powers of  $x_1, \dots, x_p$ , if the Jacobian of the functions  $f$  with respect to  $y$  is not zero when  $x$  and  $y$  vanish together.

The final two theorems and the accompanying corollaries take account of the case when the Jacobian does vanish. Poincaré did not include them in [P1], although he did make use of the results. In [P2] he did not provide proofs but instead referred to his own thesis and the work of Pusieux.

*Theorem V.* Let  $y$  be a function of  $x$  defined by the equation

$$f(y, x) = 0$$

where  $f$  can be expanded in powers of  $x$  and  $y$ . Suppose that for  $x = y = 0$ ,  $f$  and  $\frac{df}{dy}, \frac{d^2f}{dy^2}, \dots, \frac{d^{m-1}f}{dy^{m-1}}$  vanish, but  $\frac{d^mf}{dy^m}$  does not vanish. There will exist  $m$  series of the following form

$$y = a_1x^{1/n} + a_2x^{2/n} + \dots$$

( $n$  a positive integer,  $a_1, a_2, \dots$  constant coefficients) which satisfy the original equation.

*Corollary I.* If the above series satisfy the equation, then so does the series

$$y = a_1\alpha x^{1/n} + a_2\alpha^2 x^{2/n} + \dots$$

where  $\alpha$  is an  $n$ th root of unity.

*Corollary II.* The number of series of the form given in Theorem V which can be expanded in powers of  $x^{1/n}$  (which cannot be expanded in powers of  $x^{1/p}$ ,  $p < n$ ) is divisible by  $n$ .

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<sup>6</sup> The history of the implicit function theorem is convoluted and worth further research. It is certainly not clear that Poincaré was right in his attribution.



*Corollary III.* If  $k_1 n_1$  is the number of the series which can be expanded as powers of  $x^{1/n_1}$ , ..., and if  $k_p n_p$  is the number of the series which can be expanded as powers of  $x^{1/n_p}$ , then

$$k_1 n_1 + \dots + k_p n_p = m$$

and if  $m$  is odd, at least one of the numbers  $n_1, \dots, n_p$  is also odd.

*Theorem VI.* Given the  $p$  equations:

$$f_i(y_1, \dots, y_p, x) = 0 \quad (i = 1, \dots, p)$$

where the left hand sides can be expanded in powers of  $x$  and  $y$  and vanish with these variables, then, providing the equations are distinct, it is always possible to eliminate  $y_2, \dots, y_p$  and arrive at a unique equation  $f(y_1, x) = 0$  of the same form as the equation in Theorem V.

*Corollary to Theorems V and VI.* Since Theorem IV holds whenever the Jacobian of  $f$  is not equal to zero, then whenever the  $x$  vanish,  $y_1 = \dots = y_n$  is a simple solution of equations  $f_1 = \dots = f_n = 0$ .

Furthermore, by Theorems V and VI and their Corollaries, Theorem IV is also true if this solution is multiple, provided the order of multiplicity is odd.

#### 5.4.3 The method of majorants applied to partial differential equations

In applying the method of majorants to partial differential equations, Poincaré began with the Cauchy-Kovalevskaya theorem<sup>7</sup>. This is an important result in the theory of partial differential equations which continues to play a major role today<sup>8</sup>. In stating the theorem, Poincaré accorded due credit to Kovalevskaya, and the acknowledgement he gave to her here is often cited:

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<sup>7</sup> See Kovalevskaya [1875].

In its modern form the simplest case of the theorem can be stated as follows:

Any equation of the form  $\frac{\partial z}{\partial x} = f(x, y, z, \frac{\partial z}{\partial y})$  where the function  $f$  is analytic in its arguments for values near the given initial conditions  $(x_0, y_0, z_0, \frac{\partial z}{\partial y})$  where  $\frac{\partial z}{\partial y}$  is evaluated at  $x = x_0, y = y_0$ , possesses one and only one solution  $z(x, y)$  which is analytic near  $(x_0, y_0)$ .

The theorem can be generalised to functions of more than two independent variables, to derivatives of higher order and to systems of equations.

<sup>8</sup> A good and thorough study of Kovalevskaya's work together with some applications of the theorem are given by Cooke [1984, 22-38].

"Mme Kovalevskaya has considerably simplified Cauchy's proof and has given the theorem its definitive form." [P2, 26].

Poincaré himself had previously extended Kovalevskaya's results in his thesis (see 3.4.1). He now generalised these results, which concerned the first order partial differential equation

$$\frac{\partial z}{\partial x_1}X_1 + \frac{\partial z}{\partial x_2}X_2 + \dots + \frac{\partial z}{\partial x_n}X_n = \lambda_1 z,$$

where the  $X_i$  are power series in  $x_1, \dots, x_n$ , to the equation

$$\frac{\partial z}{\partial t} + \frac{\partial z}{\partial x_1}X_1 + \frac{\partial z}{\partial x_2}X_2 + \dots + \frac{\partial z}{\partial x_n}X_n = \lambda_1 z,$$

and found sufficient conditions for this equation to have an integral which can be expanded in powers of  $x$  and which is periodic with respect to  $t$ .

Poincaré then considered the partial differential equation

$$\frac{\partial z}{\partial t} + \frac{\partial z}{\partial x_1}X_1 + \frac{\partial z}{\partial x_2}X_2 + \dots + \frac{\partial z}{\partial x_n}X_n = 0,$$

and showed that a general integral of this equation is given by

$$z = f(T_1 e^{-\lambda_1 t}, \dots, T_n e^{-\lambda_n t}),$$

where  $f$  is an arbitrary function, and  $T_i$  are power series in  $x$  and periodic with respect to  $t$ . Furthermore since solving this partial differential equation is equivalent to solving a system of ordinary differential equations of the form

$$dt = \frac{dx_1}{X_1} = \frac{dx_2}{X_2} = \dots = \frac{dx_n}{X_n} \quad (5.4.iv)$$

he observed that a general integral of equations (5.4.iv) is given by

$$T_1 = K_1 e^{\lambda_1 t}, \dots, T_n = K_n e^{\lambda_n t},$$

where  $K_i$  are  $n$  constants of integration<sup>9</sup>.

In order to determine the variables  $x_1, \dots, x_p$ , as functions of  $x_{p+1}, x_{p+2}, \dots, x_n$ , he considered

$$\frac{\partial x_i}{\partial t} + \frac{\partial x_i}{\partial x_{p+1}}X_{p+1} + \frac{\partial x_i}{\partial x_{p+2}}X_{p+2} + \dots + \frac{\partial x_i}{\partial x_n}X_n = X_i \quad (i = 1, \dots, p) \quad (5.4.v)$$

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<sup>9</sup> This is a standard technique for solving partial differential equations which was introduced by Lagrange and extended by Cauchy. See Kline [1972, 531-535].

and showed that these equations admit a series solution in  $x_{p+1}, x_{p+2}, \dots, x_n$ , and sines and cosines of multiples of  $t$ , provided the  $\lambda$  satisfy certain conditions. Returning to his earlier work on differential equations [1886, 172] he was further able to show that providing the initial conditions on  $\lambda$  were changed in a certain way, then the equations (5.4.v) have a particular integral of the form

$$x_i = \phi_i(x_{p+1}, x_{p+2}, \dots, x_n, t), \quad (i = 1, \dots, p)$$

where the  $\phi$  can be expanded as series in  $x_{p+1}, x_{p+2}, \dots, x_n$  and sines and cosines of multiples of  $t$ .

Given the above result, and if the equations

$$dt = \frac{dx_1}{X_1} = \frac{dx_2}{X_2} = \dots = \frac{dx_n}{X_n}$$

are in the same form as the equations (5.4.iv) except that the  $\lambda$  no longer satisfy the sufficiency conditions for equations (5.4.v) to have an analytic solution, then Poincaré found, not a general solution, but one containing  $n - p$  arbitrary constants.

#### 5.4.4 Integration using trigonometric series

In the final part of Chapter I of [P2], which exactly followed *Note D* in [P1], Poincaré considered the integration of differential equations using trigonometric series. Using a result which he had derived in [1886c] concerning the convergence of trigonometric series, he showed that the series

$$f(x) = A_0 + A_1 \cos x + \dots + A_n \cos nx + \dots + B_1 \sin x + \dots + B_n \sin nx + \dots,$$

where  $f$  is continuous and periodic of period  $2\pi$ , is absolutely and uniformly convergent.

He then considered the system of linear differential equations

$$\frac{dx_i}{dt} = \phi_{i,1}x_1 + \phi_{i,2}x_2 + \dots + \phi_{i,n}x_n \quad (i = 1, \dots, n)$$

where the  $n^2$  coefficients  $\phi_{ik}$  are periodic functions of  $t$  of period  $2\pi$ .

Since if

$$x_1 = \Psi_{i,1}(t), \dots, x_n = \Psi_{i,n}(t) \quad (i = 1, \dots, n)$$

are  $n$  linearly independent solutions of the system of equations, then

$$x_1 = \Psi_{i,1}(t + 2\pi), \dots, x_n = \Psi_{i,n}(t + 2\pi)$$

are also solutions, and linear combinations of the  $n$  solutions can be written as

$$\Psi_{i,k}(t + 2\pi) = A_{i,1}\Psi_{1,k}(t) + \dots + A_{i,n}\Psi_{n,k}(t)$$

where the  $A$  are constant coefficients. This in turn leads to the eigenvalue equation for the matrix  $A$

$$\begin{vmatrix} A_{1,1}-S & A_{1,2} & \dots & A_{1,n} \\ A_{2,1} & A_{2,2}-S & \dots & A_{2,n} \\ \dots & \dots & \dots & \dots \\ A_{n,1} & A_{n,2} & \dots & A_{n,n}-S \end{vmatrix} = 0.$$

If  $S_1$  is a root of the eigenvalue equation, then  $\theta_{1,i}(t + 2\pi) = S_1\theta_{1,i}(t)$ , where  $\theta_{1,i}(t) = \sum_{k=1}^n B_i \Psi_{k,i}$ , and  $B_i$  are constant coefficients. By putting  $S_1 = e^{2\alpha_1\pi}$  and substituting for  $S_1$ , Poincaré showed that  $e^{\alpha_1 t}\theta_{1,i}(t)$  was a periodic function (with period  $2\pi$ ) which could be expanded as an (absolutely and uniformly convergent) trigonometric series  $\lambda_{1,i}$ . Hence he could write a particular solution to the differential equations as

$$x_i = e^{\alpha_1 t}\lambda_{1,i}(t),$$

which gave a correspondence between each root of the eigenvalue equation and each particular solution of the differential equations.

Providing all of the roots of the eigenvalue equation are distinct, there will then be  $n$  linearly independent solutions to the differential equations. Thus the general solution is

$$x_i = C_1 e^{\alpha_1 t}\lambda_{1,i}(t) + \dots + C_n e^{\alpha_n t}\lambda_{n,i}(t)$$

where  $C$  and  $\alpha$  are constants.

In addition, Poincaré showed that if the eigenvalue equation has a double root then terms of the form  $e^{\alpha_1 t}t\lambda(t)$  will be introduced into the solution for the differential equations. Similarly a triple root will introduce terms of the form  $e^{\alpha_1 t}t^2\lambda(t)$  and so on.

In this analysis Poincaré was augmenting results on the theory of differential equations which had originated with Euler and Johann Bernoulli, been generalised to the complex case by Fuchs and finally connected to the Jordan canonical form by Hamburger<sup>10</sup>. Poincaré's innovation was to extend the theory to a system of differential equations with periodic coefficients.

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<sup>10</sup> See Gray [1984, 1-5].



## 5.5 Theory of invariant integrals

### 5.5.1 Introduction

In accordance with his qualitative approach to the theory of differential equations, Poincaré's investigations into the three body problem are dominated by his research into the geometry of the problem. As the first stage of this research he made a thorough analysis of the concept of *invariant integrals* which he had originally introduced in [1886].

Although Poincaré was not the first to recognise the existence and value of invariant integrals, they are earlier encountered in both Liouville [1838] and Boltzmann [1871], he was the first to formalise a theory centred on the concept. In [1886a] he had used the idea of a particular invariant integral within the context of a problem concerning the stability of the solutions of differential equations. He now considered the whole concept in a broader sense, developing a general theory which revealed that the existence of an invariant integral is a fundamental property of Hamiltonian systems of differential equations. Of particular importance is Poincaré's use of the theory in connection with the stability of the motion in the restricted three body problem.

The last part of the chapter is devoted to a series of theorems, all of which are characterised by their geometric nature and include one of Poincaré's most celebrated results: the original formulation of his recurrence theorem. These theorems provide Poincaré with the geometrical framework for his later analysis, the qualitative study giving him an insight into the global behaviour of the system. The introduction to *Note F* in [P1] (which does not appear anywhere in [P2]) includes an interesting remark which gives his own view on these theorems:

*"These theorems have been given in a geometric form which has to my eyes the advantage of making clearer the origin of my ideas ..."* <sup>11</sup>.

The chapter is also particularly important with regard to Poincaré's error. For it was at the end of this chapter that the initial stages of the error occurred. In essence, Poincaré failed to take proper account of the exact geometric nature of a

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<sup>11</sup> "Ces théorèmes ont été présentés sous une forme géométrique qui avait à mes yeux l'avantage de mieux faire comprendre la genèse de mes idées ...". [P1, 220].

particular curve. It will be seen later how the correction of this mistake led to dramatic changes in the geometric description of his later results.

### 5.5.2 Definition of invariant integrals

Poincaré considered the system of differential equations

$$\frac{dx_i}{dt} = X_i \quad (5.5.i)$$

where the  $X_i$  are given functions of  $x_1 \dots x_n$ , and the equations are regarded as defining the motion of a point with coordinates  $(x_1, \dots, x_n)$  in an  $n$ -dimensional space. Thus, if the initial positions of an infinite number of such points form an arc of a curve  $C$  in the  $n$ -dimensional space, then at time  $t$  they will have formed a displaced arc  $C'$ , its shape determined by the differential equations.

Poincaré defined an invariant integral of the system as an expression of the form  $\int \sum Y dx_i$  which maintains a constant value at all times  $t$ , where the integration is taken over the arc of a curve and  $Y$  are given functions of  $x$ . He then extended the definition to encompass double and multiple integrals, where the order of the invariant integral is defined to correspond with the dimension of the region of integration.

To give a dynamical interpretation of the idea, he used the example of the motion of an incompressible fluid, where the motion of the fluid is described by the differential equations

$$\frac{dx}{dt} = X, \quad \frac{dy}{dt} = Y, \quad \frac{dz}{dt} = Z,$$

together with the condition

$$\frac{\partial X}{\partial x} + \frac{\partial Y}{\partial y} + \frac{\partial Z}{\partial z} = 0,$$

which asserts that the fluid is incompressible. As the fluid is incompressible, the flow is volume preserving and so the volume, which is given by the triple integral  $\iiint dx dy dz$ , is an invariant integral.

More generally, if the equations (5.5.i) have the added relation

$$\sum \frac{\partial X_i}{\partial x_i} = 0,$$

then the "volume"  $\iint \dots \int dx_1 dx_2 \dots dx_n$ , is always an invariant integral. Thus the equations in Hamiltonian form

$$\frac{dx_i}{dt} = \frac{\partial F}{\partial y_i}, \quad \frac{dy_i}{dt} = -\frac{\partial F}{\partial x_i},$$

where  $F$  is a function of the double series of variables  $x_1, \dots, x_n, y_1, \dots, y_n$ , and the time  $t$ , always admit the volume in phase space,  $\int \dots \int dx_1 \dots dx_n dy_1 \dots dy_n$ , as an invariant integral, since

$$\sum \frac{\partial \left( \frac{\partial F}{\partial y_i} \right)}{\partial x_i} + \sum \frac{\partial \left( -\frac{\partial F}{\partial x_i} \right)}{\partial y_i} = 0.$$

By considering a particular solution of the variational equations, Poincaré found a second invariant of the Hamiltonian system, namely the double integral  $\iint \sum dx_i dy_i$ .

Looking specifically at the  $n$  body problem, he found that there not only existed invariant integrals which could be deduced from the ten classical integrals of the problem, but there was also a further invariant not associated with any integral of the original equations. The additional invariant was given by

$$\int \sum (2x_i dy_i + y_i dx_i) + 3(C_1 - C_0)t,$$

where  $C_0$  and  $C_1$  are the values of the energy constant at the extremities of the arc along which the integral is evaluated (in  $6n$  dimensional space).

The proof of these last two results first appeared in *Note C*.

### 5.5.3 Transformation of invariant integrals

The transformation of variables is one of the most frequently employed methods of solving differential equations in celestial mechanics and Poincaré's next consideration was the effect of such transformations on the associated invariant integrals.

Considering the system of differential equations (5.5.i) with the condition

$$\sum \frac{\partial (MX_i)}{\partial x_i} = 0,$$

such that  $J = \int M dx_1 \dots dx_n$  is a positive invariant, he found that the transformation

$$x_i = \Psi_i(z_1, \dots, z_n) \quad (i = 1, \dots, n),$$

left the invariant  $J$  positive, provided that in the domain under consideration the  $x$  are single-valued functions of the  $z$  and vice versa<sup>12</sup>.

In the case where one of the new variables is chosen to be  $z_n = C$ , where  $F(x_1, \dots, x_n) = C$  is a particular integral of the original equations, he found that the transformed equations

$$\frac{dz_1}{dt} = Z_1, \dots, \frac{dz_{n-1}}{dt} = Z_{n-1},$$

admit a positive invariant integral of order  $n - 1$ .

He further observed that the situation was significantly different if the transformation included the independent variable  $t$ . For if equations (5.5.i) have an invariant integral of order  $n$ , and if  $t_1$  is the new independent variable which is defined by  $t_1 = \theta(x_1, \dots, x_n)$ , then the new invariant integral is given by

$$\int M \left( \frac{\partial \theta}{\partial x_1} X_1 + \dots + \frac{\partial \theta}{\partial x_n} X_n \right) dx_1 \dots dx_n.$$

From this result Poincaré was led naturally to the consideration of sections transverse to the flow. For in the case where  $n = 3$ , where  $x_i$  are regarded as the coordinates of a point  $P$  in space, then a transverse section  $S$  of a surface  $\theta(x_1, x_2, x_3) = 0$ , is the part of the surface on which all the points satisfy

$$\frac{\partial \theta}{\partial x_1} X_1 + \frac{\partial \theta}{\partial x_2} X_2 + \frac{\partial \theta}{\partial x_3} X_3 \neq 0.$$

In other words, the flow defined by the differential equations goes through the surface  $S$  and is nowhere tangent to it.

To investigate the existence of invariant integrals over  $S$ , Poincaré used his idea of consequents which in [1882] he had introduced as point iterations on transverse sections, and which he now extended to include curves and areas. He considered a volume  $V$  bounded by a surface trajectory, where the surface trajectory was formed from a curve  $C$  on  $S$  bounding an area  $A$  passing to its consequent  $C'$  bounding an area  $A'$ . He then showed that if there is a positive invariant integral which extends to the volume  $V$ , there is another integral which conserves its value over the area  $A$  or any of its consequents.

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<sup>12</sup> The function  $M$  satisfying the linear partial differential equation, called the *last multiplier* of the system of differential equations, was introduced by Jacobi in [1844].



#### 5.5.4 The use of invariant integrals

To look at the role of invariant integrals in relation to the stability of the solutions of the restricted three body problem, Poincaré extended his original definition of stability to include the definition he had used in [1885] and which he now called Poisson stability. In this definition the motion of a point  $P$  is said to be stable if it returns infinitely often to positions arbitrarily close to its initial position.

Using the result, which today is more familiarly known as his recurrence theorem, Poincaré established that, given certain initial conditions, there are an infinite number of solutions of the restricted three body problem that are Poisson stable, and that those which are not Poisson stable can be considered exceptional in a sense which he made precise.

*Theorem I (recurrence theorem):* Suppose that the coordinates  $x_1, x_2, x_3$  of a point  $P$  in space remain finite, and that the invariant integral  $\iiint dx_1 dx_2 dx_3$  exists; then for any region  $r_0$  in space, however small, there will be trajectories which traverse it infinitely often. That is to say, in some future time the system will return arbitrarily close to its initial situation and will do so infinitely often.

In other words, given a system with three degrees of freedom in which the volume is preserved, there are an infinite number of solutions which are Poisson stable. Poincaré's proof of the theorem is attractively simple<sup>13</sup>.

Consider a region  $R$  with volume  $V$  within which the point  $P$  remains. Then consider a very small region  $r_0$  of  $R$  with volume  $v$  which at time  $t$  consists of an infinite number of moving points. At time  $\tau$  these points will have filled out a region  $r_1$ , at time  $2\tau$  a region  $r_2$ , etc., and at time  $n\tau$  a region  $r_n$ , where  $r_0$  and  $r_1$  have no point in common and  $r_n$  is the  $n$ th consequent of  $r_0$ . Since the volume is preserved, each region  $r_0 \dots r_n$  will have the same volume  $v$ . Thus if  $n > \frac{V}{v}$  then at least two of the regions have a part in common. Consideration of the successive consequents of this common region shows that there is a collection of points which belong simultaneously to  $r_0$  and to an infinite number of other regions, and that this collection of points itself

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<sup>13</sup> It is sometimes suggested that in order properly to rigorise Poincaré's argument it is necessary to have the concept of the "measure" of a set of points, a concept which was not available until Lebesgue presented his ideas on integration in [1902]. In 1915 Van Vleck [1915, 335] reformulated the theorem in terms of measure theory and shortly afterwards Carathéodory [1919] provided a proof. Wintner [1947, 414] believed Poincaré's proof to be correct, and according to Brush [1980] this view is endorsed by Clifford Truesdell who considers Carathéodory's reformulation to be simply "cosmetic".

forms a region  $\sigma$ . From the definition of the region  $\sigma$ , every trajectory which starts from a point within it goes through the region  $r_0$  infinitely often.

*Corollary:* It follows from the above that there exist an infinite number of trajectories which pass through the region  $r_0$  infinitely often; but there may exist others which pass through it only a finite number of times, although these latter trajectories may be regarded as exceptional.

By exceptional Poincaré meant that the probability that a trajectory starting in the region  $r_0$  does not pass through the region more than  $k$  times is zero, however large  $k$  and however small the region  $r_0$ . The corollary and its proof were additions to [P2]. In [P1] Poincaré had simply stated the claim that the stable trajectories would outnumber the unstable, in direct analogy with the irrational and rational numbers.

As Poincaré pointed out, the theorem holds in a variety of other cases, namely, when the volume is not an invariant integral but there exists a positive invariant integral  $J = \iiint M dx_1 dx_2 dx_3$ , which remains finite; when  $n > 3$  providing there exists a positive  $n$ -dimensional invariant integral and the  $n$  coordinates of the point  $P$  in the  $n$ -dimensional space remain finite; and when the positive  $n$ -dimensional invariant integral extended over the whole  $n$ -dimensional space remains finite, even if the  $n$  coordinates are not constrained to remain finite.

He also distinguished between the cases when a known integral of equations (5.5.i)

$$F(x_1, \dots, x_n) = \text{constant},$$

is the equation of a system of closed surfaces in an  $n$ -dimensional space, and when the integral is the equation of a system of unbounded surfaces in an  $n$ -dimensional space. In the former the conditions of the theorem are satisfied without any further constraints, but in the latter the theorem only holds providing a positive invariant integral exists which has a finite value when extended to all systems of values of  $x$  where  $C_1 < F < C_2$ .

In [P2] Poincaré used this last property to extend a result in Hill's lunar theory. Hill [1878] had proved the existence of an upper bound for the radius vector of the moon. Now Poincaré was able to strengthen Hill's result by proving that the moon returned infinitely often to positions as close as desired to its initial position. In other words, he proved that the moon possesses Poisson stability.

Poincaré regarded the variables in Hill's differential equations as representing the coordinates of a point in four-dimensional space so that the accompanying integral

represented a system of unbounded surfaces. He then proved that the fourth order invariant integral of the system extended to all the points contained between two of these surfaces was finite. It therefore followed that the recurrence theorem held, which implied the existence of trajectories which pass infinitely often through any region of the four-dimensional space however small the region.

With regard to the restricted three body problem, Poincaré appealed to Bohlin's [1887] generalisation of Hill's result in which Bohlin had proved the existence of an upper bound for the radius vector of the planetoid. Poincaré then showed that providing the Jacobian integral remained within certain limits, which in general is the case, the motion of the planetoid also possesses Poisson stability.

Poincaré was unable to extend the result to the general three body problem because in this case it is no longer possible to assign limits to the coordinates.

In [P1] Poincaré included very little of the above concerning Hill's theory and made no explicit statement about stability in connection with either the lunar theory or the restricted three body problem. He did not prove the result concerning the fourth order invariant integral nor did he make its significance more accessible by putting it into the context of a particular problem. It was therefore very difficult to get a full understanding of what he was trying to achieve. The problems were partly ameliorated by the addition of *Note B* in which he translated his results into the more physical language used by astronomers and gave proper references to the work of both Hill and Bohlin. Nevertheless, the lack of detail and firm statements on the behaviour of the radius vector still left Mittag-Leffler confused and he found it necessary to ask Poincaré for a summary of his definition of stability<sup>14</sup>. Poincaré's detailed response in which he carefully spelt out the differences between his results and those of Hill [1878] and Bohlin [1887] (i.e. that he had proved the existence of a lower bound for the radius vector of the planetoid as well as an upper bound) formed the basis for *An addition to Note B*<sup>15</sup>.

Poincaré's next theorem is a generalisation of the result he had applied in [1886b] when he used the idea of an invariant integral for the first time. This and the rest of the results in this chapter are concerned with the properties of the mapping associated with the flow which takes a transverse section into itself.

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<sup>14</sup> Mittag-Leffler to Poincaré, 21.12.1888, M-L I.

<sup>15</sup> Poincaré to Mittag-Leffler, 15.1.1889, No. 45a, M-L I.



*Theorem II:* If  $x_1, x_2, x_3$ , represent the coordinates of a point in space, and there exists a positive invariant integral, then there is no *closed* transverse section.

For  $n > 3$ , the theorem can be given in analytic form.

Many dynamical problems, particularly those of celestial mechanics, involve very small parameters, and these can often be used to form power series expansions of the solutions to the differential equations. In the case of the restricted three body problem, the natural parameter which arises is that of the mass of the smaller of the two primaries, generally designated by  $\mu$ . The beauty of using  $\mu$  as the parameter in this problem is that it is possible to change the nature of the problem by changing the value of  $\mu$ . For if  $\mu = 0$ , the problem reduces to two two body problems and can therefore be solved. This leads to the idea of starting with a particular solution for which  $\mu = 0$ , and then seeing if it is possible to find solutions for values of  $\mu$  which are close to but not equal to zero, and this is exactly what Poincaré did.

In order to apply his theoretical results to the restricted three body problem, Poincaré now started to consider the differential equations

$$\frac{dx_1}{dt} = X_1, \quad \frac{dx_2}{dt} = X_2, \quad \frac{dx_3}{dt} = X_3,$$

as functions of the  $x_i$  and  $\mu$ , the solutions of which could be expanded in terms of the parameter.

*Lemma I:* Consider part of a transverse section  $S$ , passing through the point  $a_0, b_0, c_0$ ; if  $x_0, y_0, z_0$  are the coordinates of a point of  $S$ , and if  $x_1, y_1, z_1$  are the coordinates of its consequent, then  $x_1, y_1, z_1$  can be expanded in powers of  $x_0 - a_0, y_0 - b_0, z_0 - c_0$  and  $\mu$ , providing these quantities are sufficiently small.

Lemma I originally appeared in *Note E*, its proof referring to what is now Theorem II in the section on the method of majorants. In [P2] Poincaré adjusted the proof by referring to Theorem III from the same section. In addition, rewriting the memoir gave Poincaré the opportunity to move the Lemma into a more logical position. It made sense to insert it where it now came because the reason for its inclusion was its specific role in the proof of the following Lemma which had appeared in the main text of [P1].



*Lemma II:* If the distance between two points  $A_0$  and  $B_0$  belonging to part of a transverse section  $S$  is very small of  $n$ th order, then it will be the same for their consequents  $A_1$  and  $B_1$ .

Poincaré also changed what he meant by the expression "small quantity of  $n$ th order". In [P1] he had defined a function of  $x_1, x_2, x_3$  and  $\mu$ , as being a small quantity of  $n$ th order if it could be expanded in powers of  $\mu$  with the first term in the expansion being a term in  $\mu^n$ . In [P2] he defined a function of  $\mu$ , which need not have a power series expansion in  $\mu$ , as a small quantity of  $n$ th order if it tended to zero with  $\mu$  in such a way that the ratio of the function to  $\mu^n$  tended towards a finite limit. The change was necessary in order to accommodate the alterations which he subsequently made to Theorem III.

So far Poincaré's revisions have in general been what might loosely be described as cosmetic. Theorems have been added, proofs enhanced but no fundamental alterations have been made to any of the results in [P1], and overall the effect has been to give this early part of the memoir an altogether more coherent structure. The rest of this chapter tells a rather different story. The changes that Poincaré made from now on were no longer simply improvements but instead were necessitated by the discovery of a mistake in the Corollary to Theorem III [P1], as a result of which Poincaré had to alter the Theorem substantially, a change which involved removing the Corollary altogether. In order to better describe the mistake, the published conclusion to the chapter will be given first, followed by its erroneous counterpart.

Theorem III involved the use of what Poincaré called an *invariant curve*. He defined an  $n$ th order invariant curve as a curve on  $S$  which coincides with its  $n$ th consequent.

*Theorem III [P2]:* Let  $A_1AMB_1B$  be an invariant curve, such that  $A_1$  and  $B_1$  are the consequents of  $A$  and  $B$ . Suppose that the arcs  $AA_1$  and  $BB_1$  are very small (i.e. they tend to zero with  $\mu$ ) but that their curvature is finite.

Suppose that the invariant curve and the position of the points  $A$  and  $B$  depend upon  $\mu$  according to some rule, and that there exists a positive invariant integral. If the distance  $AB$  is very small of the  $n$ th order and the distance  $AA_1$  is not very small of the  $n$ th order, then the arc  $AA_1$  intersects the arc  $BB_1$ .

*Proof:* The points  $A$  and  $B$  can always be joined by an arc of the curve  $AB$  wholly situated on the part of the transverse section  $S$  and whose total length is of the same order of magnitude as the distance  $AB$ , i.e. a very small quantity of  $n$ th order. Let  $A_1B_1$  be the arc of the curve which is the consequent of  $AB$ . It will also be very small of  $n$ th order by Lemma II. The possible hypotheses are:

1. The two arcs  $AA_1$  and  $BB_1$  intersect each other (as in FIG.5.5.i<sup>16</sup>).
2. The curvilinear quadrilateral  $AA_1B_1B$  is such that the four arcs which comprise its sides do not have a point in common except for the four corners  $A, A_1, B, B_1$  (as in FIG.5.5.ii<sup>17</sup>).
3. The two arcs  $AB$  and  $A_1B_1$  intersect each other at a point  $D$  (as in FIG. 5.5.iii<sup>18</sup>).
4. One of the arcs  $AB$  or  $A_1B_1$  intersects one of the arcs  $AA_1$  or  $BB_1$ ; but the arcs  $AA_1$  and  $BB_1$  do not intersect each other, neither do the arcs  $AB$  and  $A_1B_1$ .

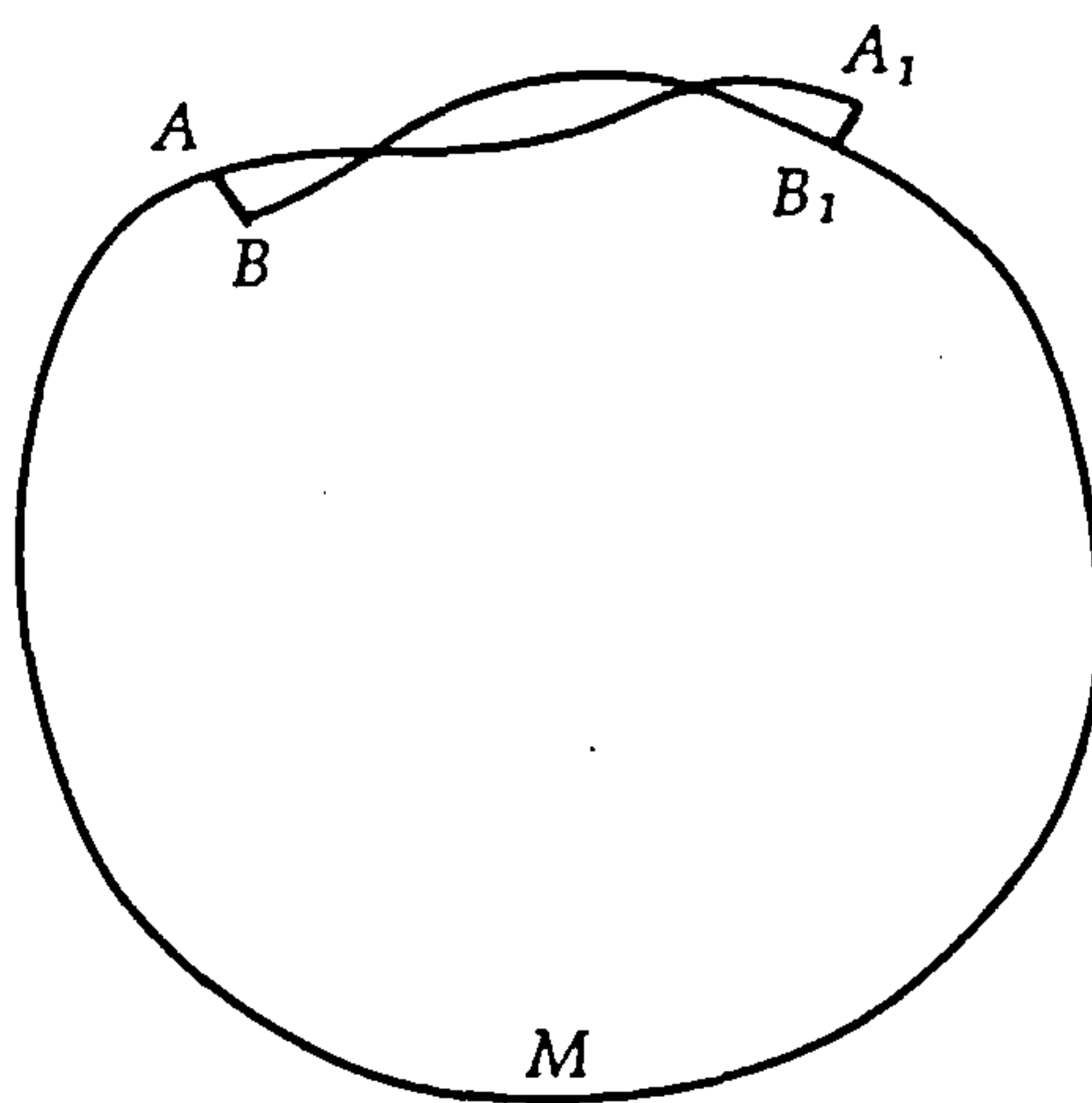


FIG. 5.5.i.

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<sup>16</sup> Poincaré did not include a diagram of this possibility.

<sup>17</sup> Poincaré [P2, 329]

<sup>18</sup> Poincaré [P2, 329]

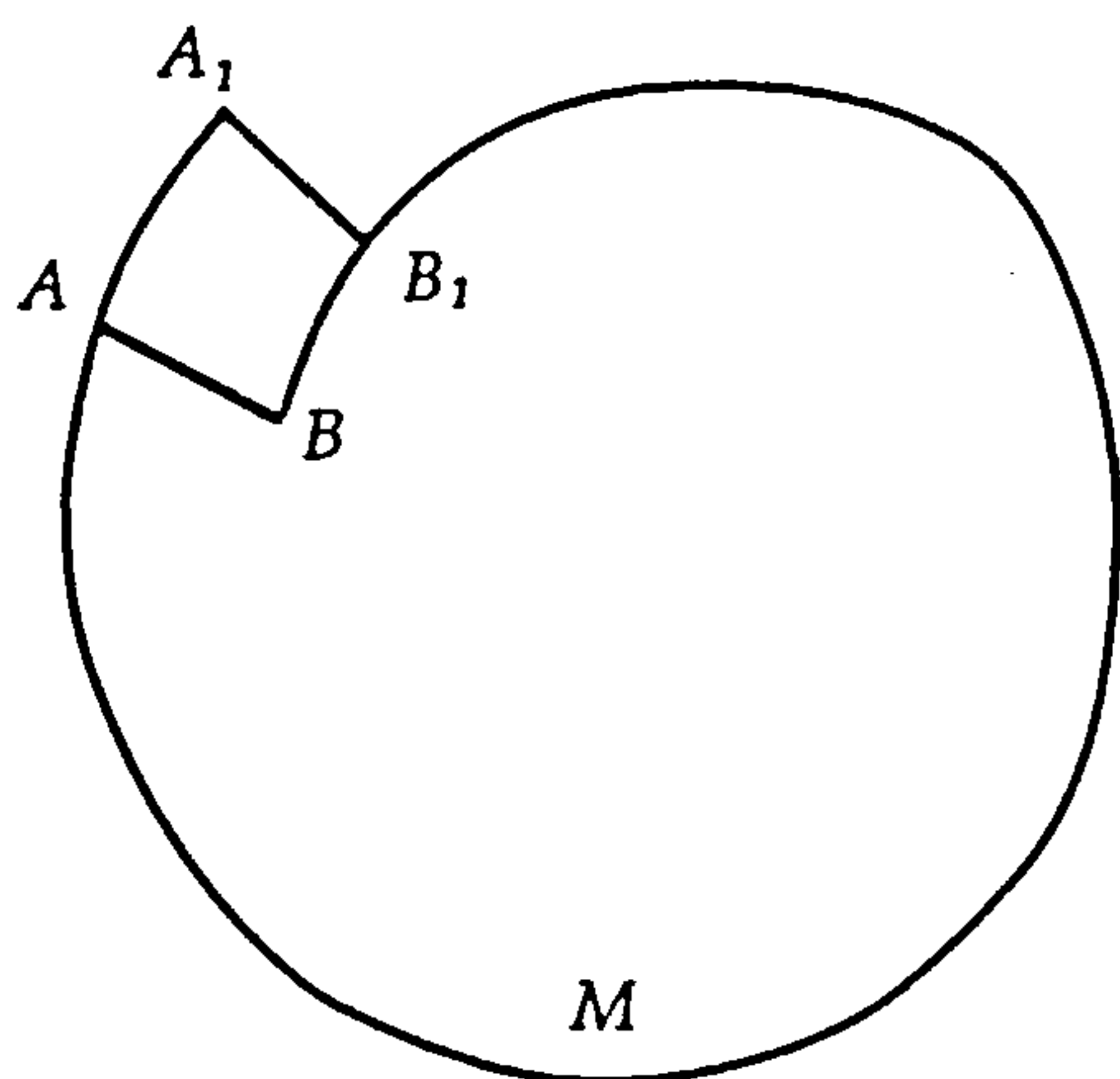


FIG. 5.5.ii.

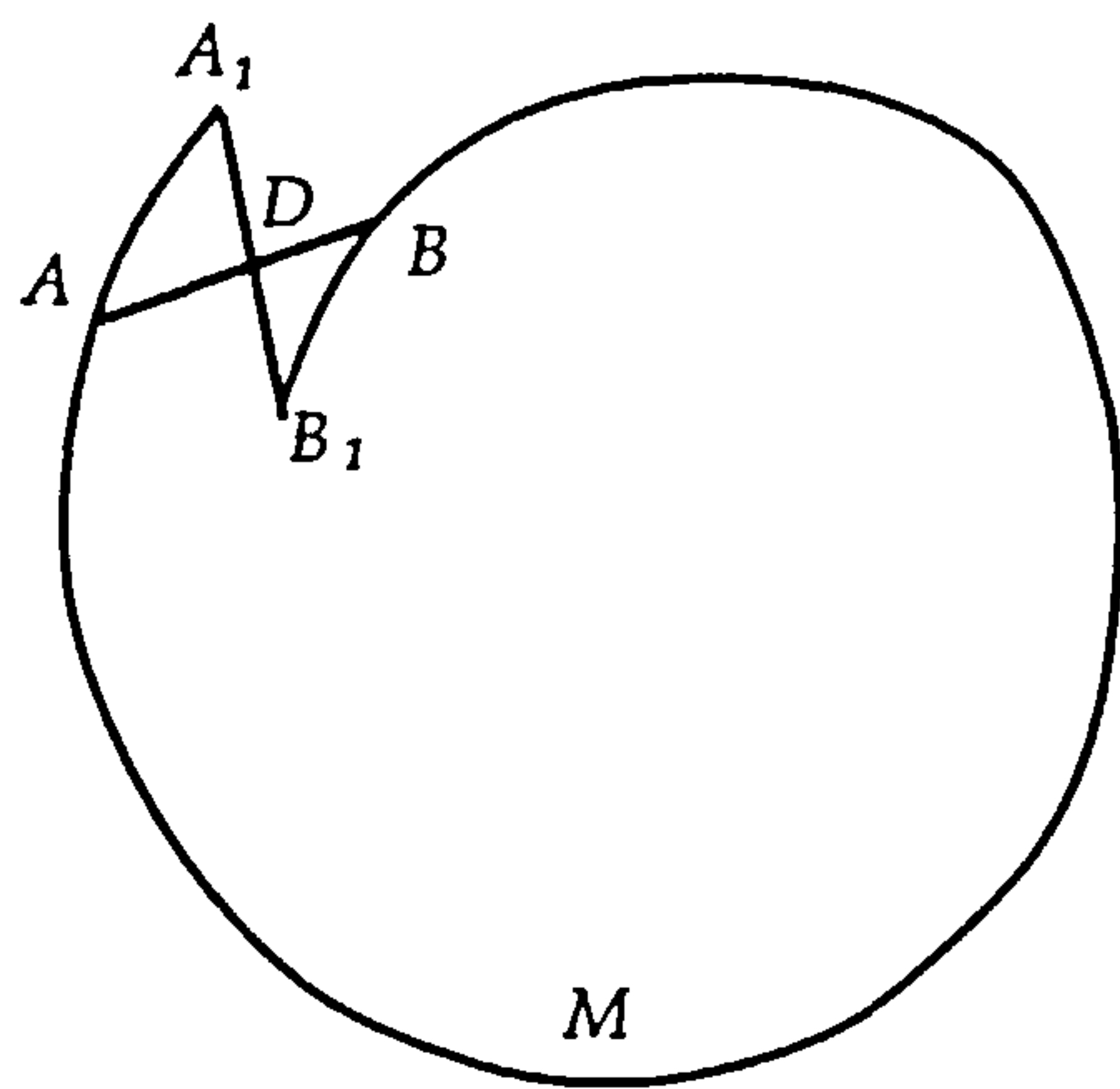


FIG. 5.5.iii.

Poincaré swiftly established that the first hypothesis was the only acceptable one. In other words, he established that the curve  $A_1AMB_1B$  was self-intersecting.

In Theorem III [P1] Poincaré introduced a new term, *quasi-closed*, which he did not use in [P2] but which is important with regard to the error. He defined an  $n$ th order curve  $C$  (which in general depends on  $\mu$  and is contained on part of a trans-versal section  $S$ ) to be quasi-closed if it has two points  $A$  and  $B$  on it which are separated by a finite arc, and whose distance apart is very small of  $p$ th order,  $A$  and  $B$  being the two points of closure. Unfortunately, it is not clear from this definition what exactly Poincaré meant by the term, although he did attempt to clarify it by including a particular example in which the distance  $AB$  was shown to be of order  $n - 1$ , providing  $\mu$  was sufficiently small. He also used the example to show that if a curve is quasi-closed and it depends on  $\mu$ , then it will be closed for  $\mu = 0$ .

*Theorem III [P1]:* If an invariant curve  $C$  is quasi-closed such that the distance between the points of closure  $A$  and  $B$  is very small of  $n$ th order, and there exists a positive invariant integral, the distance from the point  $A$  to its consequent  $A_1$  and that of  $B$  to its consequent  $B_1$  are very small of  $n$ th order.

Poincaré's proof first showed that FIG. 5.5.iii and not FIG. 5.5.ii provided the correct description of the curve  $C$ . (He used the same diagrams in both [P1] and [P2]). He then achieved the desired result by an application of the triangle inequality.

The relationship between the two theorems is in Poincaré's consideration of the distance between the point  $A$  and its consequent  $A_1$ . The purpose of Theorem III [P1] was to show under what conditions the distance  $AA_1$  would be very small of  $n$ th

order, whereas in Theorem III [P2] Poincaré was considering the situation in which the distance  $AA_1$  was particularly specified *not* to be very small of  $n$ th order. The third hypothesis in Theorem III [P2] gives the correct description of the curve defined by Theorem III [P1].

Poincaré introduced the Corollary to Theorem III [P1], somewhat presciently as it turned out, by saying he believed it would reveal the importance of the theorem.

*Corollary [P1]:* If it has been proved that invariant curve  $C$  is quasi-closed so that the distance between the points of closure  $A$  and  $B$  is very small of  $n$ th order at least, if moreover it is known that the distance of the point  $A$  to its consequent is a finite quantity or a small quantity of  $n - 1$ th order at most, and finally if there is a positive invariant integral, then the curve  $C$  is *closed*. If it was only quasi-closed, then the distance from the point  $A$  to its consequent would have to be of  $n$ th order.

This is where the mistake occurred. Poincaré thought that he had proved that the curve  $C$  was closed, whereas, as he later showed in Theorem III [P2], the curve was in fact self-intersecting. It will be seen later that when Poincaré invoked the Corollary in [P1], his contingent results made essential use of the fact that the curve was supposed to be closed. He did not consider the possibility that the curve might be self-intersecting. What will become clear is that the distinction between closed and self-intersecting curves lies at the heart of the profound alterations to Poincaré's final conclusions. Thus with regard to fully comprehending the error, it would have been helpful to have had a clearer definition of the term quasi-closed.

Why did Poincaré make this mistake? One possibility is that it was quite simply an oversight. As remarked in 4.5, it was well known that Poincaré often paid scant attention to detail, and certainly the deadline for the competition would not have encouraged him to do otherwise. Perhaps a more convincing argument might be that he originally had a preconceived idea about how he thought the curve would behave. So if he thought he had found what he was expecting, he might not have felt the necessity to closely scrutinise his results, particularly if he was short of time. As is shown later, the behaviour of the self-intersecting curve is extremely complex and quite unlike anything Poincaré (or anybody else) had previously encountered. Indeed, it is evident from the correspondence that when he did discover the mistake, it came as a significant shock (see 5.8.3).

In [P1] Poincaré also included two extensions to Theorem III, as well as a further theorem, Theorem IV. None of these appear anywhere in [P2], either because the



discovery of the error made them redundant or because they had no special relevance to any other part of [P2]. They are stated here for the sake of completeness.

*First Extension to Theorem III:* Suppose that  $A_n B_n$  coincides partly with  $A_0 B_0$  and partly with the extension of  $A_0 B_0$  such that  $A_0 B_0$  is an  $n$ th order invariant curve. Suppose also that the distance between  $A_0$  and  $B_p$  is a small quantity of  $q$ th order, where  $p$  is prime to  $n$ , then the distance from  $A_0$  to its  $n$ th consequent  $A_n$  is a very small quantity of  $q$ th order.

This is the case described by FIG. 5.5.iv where  $n$  is taken to be 5.

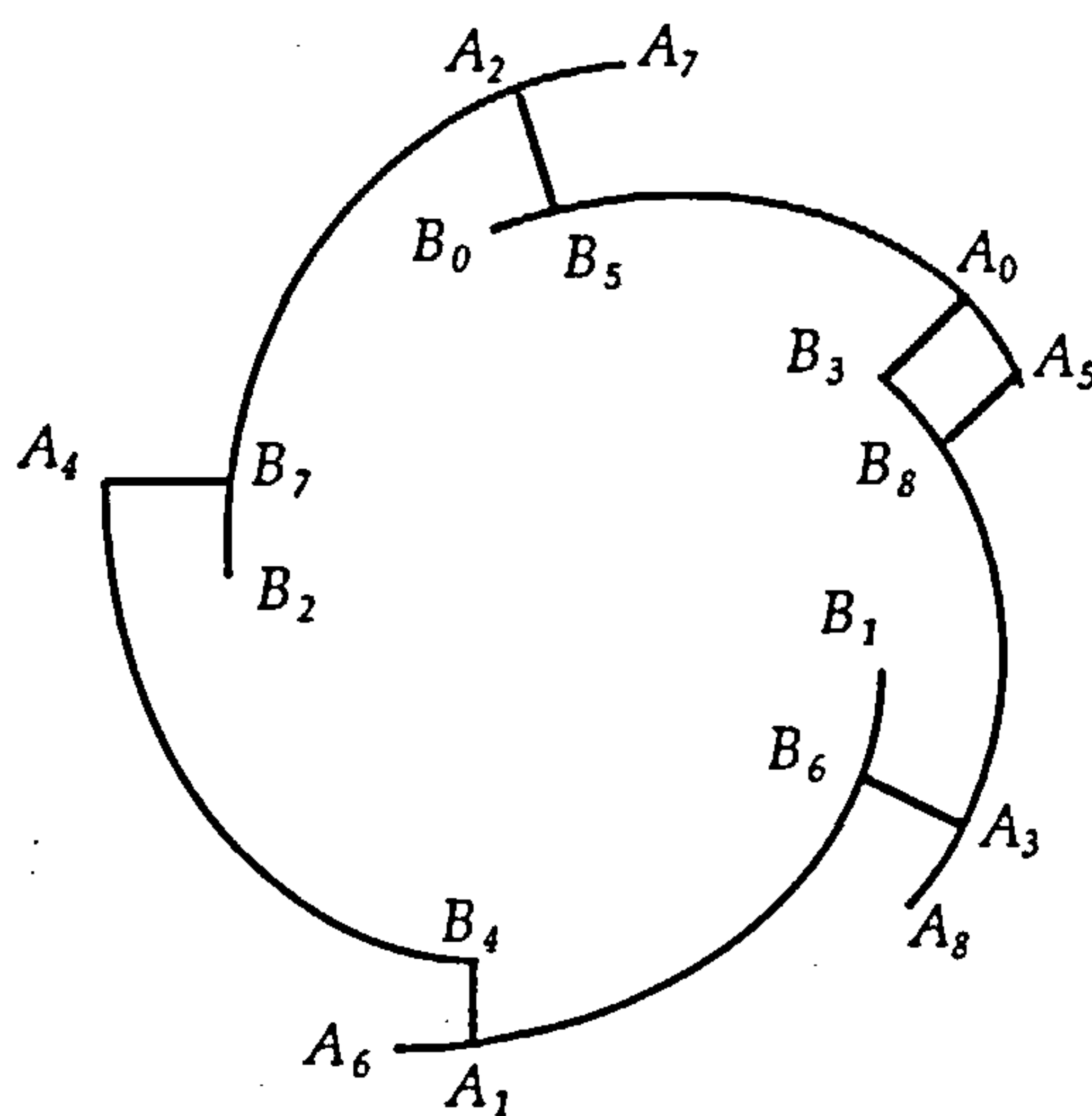


FIG. 5.5.iv.

Combining this generalisation with the Corollary, Poincaré deduced a further generalisation in which he gave the conditions under which he claimed that the set of curves formed by  $A_0 B_0$  and its successive consequents would form a "closed" invariant curve of first order. By invoking the Corollary he again reiterated the error he had made earlier.

Poincaré prefaced the second extension to Theorem III [P1] by saying that he did not expect to use it in what followed, although he later cited it twice, both references becoming redundant in the revision.

*Second Extension to Theorem III:* A curve without being rigorously invariant may be invariant up to a very small quantity of  $p$ th order. If the distance between a curve  $C$ , which is not rigorously invariant, and an arbitrary point of its  $n$ th consequent is a

very small quantity of  $p$ th order, then such a curve is called  $n$ th order *semi-invariant* up to very small quantities of  $p$ th order. If a semi-invariant curve is quasi-closed such that the distance between the points of closure  $A$  and  $B$  is very small of  $q$ th order, the distance from the point  $A$  to its  $n$ th consequent  $A_n$  will be very small of order at least  $q$  providing  $2q < p$ , and of order  $p - q$  providing  $2q > p > q$ .

*Theorem IV:* Consider a transverse section  $S$  which is simply connected. Let a point on  $S$  be determined by a particular system of coordinates (to be defined) which is analogous to polar coordinates. Let  $O$  be an arbitrary point on  $S$  at which infinitely many branches of a curve meet, in the same way that radius vectors meet at the pole in polar coordinates. Suppose that  $O$  is the only common point of any two branches of the curve and that an arbitrary branch is defined by the angle  $\theta$  between its tangent at  $O$  and a fixed line passing through  $O$ .

Consider a second system of closed concentric curves containing the point  $O$ . Furthermore, suppose that any curve of the second system has one and only one point in common with any curve of the first system. Consider a fixed branch of the first system  $B_0$  and let  $P$  be the point where it cuts a moving curve of the second system. Let  $\rho$  be the length of the arc of the curve  $B_0$  between  $O$  and  $P$ . The moving curve can then be defined by  $\rho$ . Finally suppose that through an arbitrary point  $P$  of  $S$  there passes one and only one branch of the first system. The coordinates  $\rho$  and  $\theta$  can then be used to define the position of  $P$  on  $S$ .

Let  $\alpha$  be a simply connected area of  $S$  limited by a closed curve  $k$ . Let  $\alpha_n$  be the  $n$ th consequent limited by the closed curve  $k_n$ . If the two areas  $\alpha$  and  $\alpha_n$  have a part in common and  $O$  belongs to this communal part, if the points of  $k$  have same coordinate  $\theta$  as their  $n$  consequents, if the curve  $k$  meets each of the branches of the first system at one point (such that when one crosses the closed curve  $k$ ,  $\theta$  varies between 0 and  $2\pi$ ), if there is a positive invariant integral, then two at least of the points  $k$  coincide with their  $n$ th consequents.

Poincaré also gave a second more succinct statement of the theorem which did not involve the coordinate system defined above. Let  $k$  be a closed curve on a simply connected transverse section  $S$  with  $n$ th consequent  $k_n$ . If each of the points of  $k$  can be joined to its  $n$ th consequent by arcs of curves on  $S$  in such a way that no two of these arcs have a point in common, and, moreover there is a positive invariant integral, two at least of the points of  $k$  will coincide with their consequents.

There is no direct reason why Poincaré included this Theorem in [P1] as he made no use of it there. His only reference to it was an expression of regret that he did not have the opportunity to show how it could be applied in the study of the spatial distribution of closed trajectories. It could have been that because he was writing to a deadline it was just easier to keep it in, or maybe because he was not good at organising his material. On the other hand, he might have decided that having established the result it made sense to publish it so that it was available should he need it at some later date.

## 5.6 Theory of periodic solutions

### 5.6.1 Introduction

Poincaré's discussion of periodic solutions forms the central topic of the memoir. In it he brings together principles and techniques both from the previous chapters and from his earlier papers on differential equations and the three body problem. The chapter is dominated by two important ideas connected with the stability of the periodic solutions. The first of these concerns certain constants which arise in the solutions and which he originally introduced in [1886]. These are now identified as *characteristic exponents* and an investigation into their behaviour reveals information about the stability of the solutions. Secondly, there is his remarkable discovery of an entirely new class of solutions which asymptotically approach an unstable periodic solution and which he called *asymptotic solutions*.

Poincaré's rewriting of the memoir resulted in several additions and alterations to the chapter, only the section on characteristic exponents surviving the transition intact. The most radical change concerned the analytical description of the asymptotic solutions which underwent a major revision, culminating in the addition, at the end of the chapter, of a completely new section concerned with the asymptotic solutions of the autonomous Hamiltonian equations.

### 5.6.2 Existence of periodic solutions

Poincaré began with the equations

$$\frac{dx_i}{dt} = X_i \quad (i = 1, \dots, n) \quad (5.6.i)$$

where the  $X_i$  are functions of  $x, t$  and the mass parameter  $\mu$ , but now he assumed the functions  $X_i$  to be periodic of period  $2\pi$  with respect to  $t$ . If for  $\mu = 0$  there exists a

periodic solution  $x_i = \phi_i(t)$ , where  $\phi_i$  is a periodic function of  $t$  with period  $2\pi$ , then the question Poincaré asked was whether this periodic solution could be analytically continued for small values of the disturbing parameter  $\mu$ .

Poincaré began by looking for series in powers of  $\mu$  with periodic coefficients which would satisfy the differential equations. If, having proved the existence of such series, he could also prove their convergence, then he would have proved the existence of the required periodic solutions. However, having got as far as proving the existence of the series, rather than prove their convergence, he decided instead to prove the existence of the periodic solutions, which would then imply the convergence of the series. It is not clear why Poincaré changed his approach, especially as he said he thought that the convergence argument could be made directly although he gave no indication as to how this could be done. Perhaps, as Ian Stewart suggests, he could foresee complications or maybe he was not absolutely sure how to go about it<sup>19</sup>.

He considered a particular solution close to the original periodic solution

$$x_i(0) = \phi_i(0) + \beta_i, \quad x_i(2\pi) = \phi_i(0) + \beta_i + \Psi_i,$$

where  $\Psi_i$  are analytic functions of  $\mu$  and  $\beta$  which vanish with these variables, and then sought  $\Psi_i$  such that they satisfy the equations

$$\Psi_1 = \dots = \Psi_n = 0. \quad (5.6.ii)$$

His analysis showed that providing the Jacobian  $\Delta$  of  $\Psi$  with respect to  $\beta$  was not zero these equations could be resolved and hence equations (5.6.i) do have periodic solutions for small values of  $\mu$ .

In considering the case when  $\Delta = 0$ , he used the same method in both [P1] and [P2] but its dependence on the method of majorants meant that his improvements to Chapter I in [P2] were particularly beneficial with respect to clarifying his procedure.

If equations (5.6.ii) are distinct, then  $\beta_1, \dots, \beta_{n-1}$  can be eliminated to give a unique equation  $\Phi = 0$ . If  $\mu$  and  $\beta_n$  are then regarded as coordinates of a point in a plane, this equation can be regarded as representing a curve passing through the origin with each of its points corresponding to a periodic solution. By constructing the part of the

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<sup>19</sup> Stewart [1989, 67]



curve close to the origin, Poincaré was then able to study the behaviour of periodic solutions which correspond to small values of  $\mu$  and  $\beta$ .

If  $\Delta = 0$ , then (for  $\mu = \beta_n = 0$ )  $\frac{d\Phi}{d\beta_n} = 0$ . The curve  $\Phi = 0$  is then tangent to the line  $\mu = 0$  at the origin, and, moreover, when  $\mu = 0$ , the equation  $\Phi = 0$  will be an equation in  $\beta_n$  which admits zero as a multiple root. If the order of multiplicity of the root is  $m$ , then, by Theorem V of Chapter I, there exist  $m$  series in positive fractions of  $\mu$ , which vanish with  $\mu$  and, when substituted for  $\beta_n$ , satisfy  $\Phi = 0$ . Using these series, Poincaré considered the intersection of the part of  $\Phi = 0$  which is close to the origin with the two lines  $\mu = \varepsilon$ ,  $\mu = -\varepsilon$  which are very close to the line  $\mu = 0$ . If  $m_1$  ( $m_2$ ) are the number of points of intersection of  $\Phi = 0$  with  $\mu = \varepsilon$  ( $\mu = -\varepsilon$ ) which are real and close to the origin then Poincaré claimed that  $m$ ,  $m_1$  and  $m_2$  all have the same parity<sup>20</sup>. Thus if  $m$  is odd, then  $m_1$  and  $m_2$  are at least equal to one and there exist periodic solutions for small values of  $\mu$ . The result holds for both positive and negative values of  $\mu$ , although clearly in the context of the restricted three body problem no physical meaning can be attached to the latter.

The above analysis also led Poincaré to the important result that as  $\mu$  varies the periodic solutions disappear in pairs in the same way as real roots of algebraic equations. For if  $m_1 \neq m_2$ , then, since they have the same parity, their difference is an even integer and so as  $\mu$  increases continuously, the number of periodic solutions which disappear as  $\mu$  changes sign will be even. In other words, a periodic solution can only disappear when it becomes identical with another periodic solution.

Poincaré looked at the case when for  $\mu = 0$  the differential equations admit an infinite number of periodic solutions of the form

$$x_1 = \phi_1(t, h), \dots, x_n = \phi_n(t, h),$$

where  $h$  is an arbitrary constant. The equations (5.6.ii) are then no longer distinct for  $\mu = 0$  and  $\Phi$  contains  $\mu$  as a factor, i.e.  $\Phi = \mu\Phi_1$ . In this case Poincaré showed that the equations still have periodic solutions for small values of  $\mu$  but only providing that when  $\mu = 0$ , the equation  $\Phi_1 = 0$  admits  $\beta_n = 0$  as a root of odd order.

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<sup>20</sup> The justification for this claim is not immediately obvious and Poincaré later gave an explanation in [MN I, 70-71].

In [P2] he made the additional point that in the case where the equations admit a single-valued integral  $F(x_1, \dots, x_n) = \text{constant}$ , equations (5.6.ii) will not be distinct unless further conditions are imposed.

Poincaré next considered the existence of periodic solutions when the functions  $X_i$  are autonomous and periodic solutions can be of any period. In which case, if the equations have one periodic solution, they will have an infinite number. For if  $x_i = \phi_i(t)$  is a periodic solution, the same will be true of  $x_i = \phi_i(t + h)$ , whatever the value of the constant  $h$ .

If for  $\mu = 0$  the equations have a periodic solution  $x_i = \phi_i(t)$  of period  $T$ , and if for small values of  $\mu$

$$x_i(0) = \phi_i(0) + \beta_i, \quad x_i(T + \tau) = \phi_i(0) + \beta_i + \Psi_i$$

where  $\Psi_i$  are analytic functions of  $\mu, \beta_1, \dots, \beta_n, \tau$ , then periodic solutions will exist for small values of  $\mu$  providing it is possible to resolve the  $n$  equations

$$\Psi_1 = \Psi_2 = \dots = \Psi_n = 0$$

with respect to the  $n + 1$  unknowns  $\beta_1, \dots, \beta_n, \tau$ .

Poincaré showed that having chosen any one of the  $\beta_i = 0$ , then a sufficient condition for the existence of periodic solutions for small values of  $\mu$  is that not all the determinants in the matrix

$$\begin{bmatrix} \frac{\partial \Psi_1}{\partial \beta_1} & \frac{\partial \Psi_1}{\partial \beta_2} & \dots & \frac{\partial \Psi_1}{\partial \beta_n} & \frac{\partial \Psi_1}{\partial \tau} \\ \frac{\partial \Psi_2}{\partial \beta_1} & \frac{\partial \Psi_2}{\partial \beta_2} & \dots & \frac{\partial \Psi_2}{\partial \beta_n} & \frac{\partial \Psi_2}{\partial \tau} \\ \dots & \dots & \dots & \dots & \dots \\ \frac{\partial \Psi_n}{\partial \beta_1} & \frac{\partial \Psi_n}{\partial \beta_2} & \dots & \frac{\partial \Psi_n}{\partial \beta_n} & \frac{\partial \Psi_n}{\partial \tau} \end{bmatrix}$$

are simultaneously zero for  $\mu = \beta_i = \tau = 0$ , although in this case the periodic solutions have period  $T + \tau$  as opposed to period  $T$ .

### 5.6.3 Characteristic exponents

Having established the existence of periodic solutions, Poincaré now turned his attention to the question of their stability. Assuming a periodic solution  $\phi(t)$  of equations (5.6.i) had been found he formed the variational equations in order to study the behaviour of nearby solutions.

Since the variational equations are linear differential equations with periodic coefficients, the results from the end of Chapter I give  $n$  particular solutions

$$\xi_{1k} = e^{\alpha t} S_{1k}, \quad \dots, \quad \xi_{nk} = e^{\alpha t} S_{nk}, \quad (k = 1, \dots, n),$$

where the  $\alpha$  are constants and the  $S_{ik}$  are periodic functions of  $t$  with the same period as  $\phi(t)$ .

The constants  $\alpha$  are what Poincaré called the *characteristic exponents* of the periodic solution, and his insight was to realise that they were the key to the stability problem. For if  $\alpha$  is purely imaginary then the  $\xi$  remain finite and the solution can be said to be stable and, conversely, if  $\alpha$  is not purely imaginary then the solution can be said to be unstable. In other words, investigating the stability of the periodic solutions is equivalent to investigating the properties of their characteristic exponents. As already mentioned, the idea was not entirely new to Poincaré at this time, it had first appeared in [1886], but, as with the case of invariant integrals, he now engaged in a more detailed study.

Drawing further from his results from Chapter I, he proceeded to show that if two characteristic exponents are equal then terms of the type  $te^{\alpha t} S_{jk}$  appear in the solution, and, similarly, if three characteristic exponents are equal then terms which include  $t^2$  outside the exponential and trigonometric functions appear, and so on. He also showed that if the system is autonomous or it has a single-valued integral, then in either case one of the characteristic exponents vanishes.

With regard to Hamiltonian systems, he found that the characteristic exponents can always be arranged in pairs of equal magnitude but opposite sign. Thus if the Hamiltonian system is autonomous then two of the characteristic exponents are zero. He called the  $n$  distinct quantities  $\alpha^2$  the *coefficients of stability* of the periodic solution.

By considering a particular solution of the variational equations in which  $\xi_i = \beta_i$  for  $t = 0$ , and  $\xi_i = \beta_i + \Psi_i$  for  $t = 2\pi$ , he derived the eigenvalue equation

$$\begin{vmatrix} \frac{\partial \Psi_1}{\partial \beta_1} + 1 - e^{2\alpha\pi} & \frac{\partial \Psi_1}{\partial \beta_2} & \dots & \frac{\partial \Psi_1}{\partial \beta_n} \\ \frac{\partial \Psi_2}{\partial \beta_1} & \frac{\partial \Psi_2}{\partial \beta_2} + 1 - e^{2\alpha\pi} & \dots & \frac{\partial \Psi_2}{\partial \beta_n} \\ \dots & \dots & \dots & \dots \\ \frac{\partial \Psi_n}{\partial \beta_1} & \frac{\partial \Psi_n}{\partial \beta_2} & \dots & \frac{\partial \Psi_n}{\partial \beta_n} + 1 - e^{2\alpha\pi} \end{vmatrix} = 0$$

from which it can be seen that if  $\alpha = 0$ , then the equation is equivalent to  $\Delta = 0$ <sup>21</sup>. Conversely, it also implies that if  $\Delta$  is zero then one of the characteristic exponents must vanish. Consequently, Poincaré could re-express his result concerning the existence of periodic solutions by saying that if equations (5.6.i) have a periodic solution for  $\mu = 0$  for which none of the characteristic exponents vanish, they will have also have a periodic solution for small values of  $\mu$ .

#### 5.6.4 Periodic solutions of the equations of dynamics

Poincaré next considered the existence of periodic solutions in the autonomous Hamiltonian system

$$\frac{dx_i}{dt} = \frac{\partial F}{\partial y_i}, \quad \frac{dy_i}{dt} = -\frac{\partial F}{\partial x_i}, \quad (i = 1, 2, 3) \quad (5.6.iii)$$

$$F = F_0 + \mu F_1 + \mu^2 F_2 + \dots$$

where  $F_0$  is a function of the  $x$  only (since in the general problem of dynamics the force function is dependent only on the distance) and  $F_1, F_2, \dots$  are functions of all variables  $x, y$  and periodic of period  $2\pi$  with respect to each  $y$ .

Thus when  $\mu = 0$ ,  $x_i$  are constants and  $y_i = n_i t + \varpi_i$ , where  $n_i = -\frac{\partial F_0}{\partial x_i}$ , and  $\varpi_i$  are constants of integration. So for a solution of the differential equations to be periodic when  $\mu = 0$ , it is necessary and sufficient for the  $n_i$  to be commensurable, and, providing the  $\frac{\partial F_0}{\partial x_i}$  are independent of each other, the  $x_i$  can always be chosen so that this condition is fulfilled, and the period  $T$  will then be the lowest common multiple of the  $\frac{2\pi}{n_i}$ . In other words, when  $\mu = 0$  there are an infinite number of choices for the constants  $x_i$  which will lead to periodic solutions.

The question then arises of whether these periodic solutions can be analytically continued for small values of  $\mu$ . What Poincaré found was that such analytic continuation was possible providing the periodic solutions correspond (in the simplest case) to pairs of Kepler circles with rational frequency ratio and a certain phase relation determined by the critical points of a function  $\Psi$ , where  $\Psi$  is the mean value of  $F_1$  considered as a periodic function of  $t$ .

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<sup>21</sup> If the solution being considered differs only slightly from the periodic solution, so that the squares and higher powers of  $\xi$  can be neglected, then the squares and higher powers of  $\beta_i$  can be neglected likewise.



Although he approached the question of analytic continuation using the same methods in both [P1] and [P2], the two presentations appear rather different. The results are essentially similar but in [P2] they are expressed in a more logical order with additional explanations. In [P1] he began by expanding the coordinates as a series in  $\mu$  and then proving the existence of the periodic solutions, whereas in [P2] he first proved the existence of the periodic solutions before expanding the coordinates and determining the coefficients of the series. Furthermore, [P2] contains a discussion of the application of the theory to the restricted three body problem which is not found in [P1].

He started in [P2] by supposing that for  $\mu \neq 0$  a particular solution at  $t = 0$  is given by

$$x_i = a_i + \delta a_i, \quad y_i = \varpi_i + \delta \varpi_i$$

and that for  $t = T$  the solution has the values

$$x_i = a_i + \delta a_i + \Delta a_i, \quad y_i = \varpi_i + n_i T + \delta \varpi_i + \Delta \varpi_i.$$

Thus the solution will be periodic if

$$\Delta a_1 = \Delta a_2 = \Delta a_3 = \Delta \varpi_1 = \Delta \varpi_2 = \Delta \varpi_3 = 0.$$

However, since  $F = \text{constant}$  is an integral of equations (5.6.iii) and  $F$  is periodic with respect to  $y$ , these equations are not independent. Hence it is only necessary to satisfy five of them. Furthermore, choosing  $t = 0$  when  $y_1 = 0$ , gives  $\varpi_1 = \delta \varpi_1 = 0$ .

Poincaré showed that the five equations could be satisfied provided both that  $\varpi_2$  and  $\varpi_3$ , were chosen in such a way that

$$\frac{\partial \Psi}{\partial \varpi_2} = \frac{\partial \Psi}{\partial \varpi_3} = 0, \quad (5.6.iv)$$

and that neither the Hessian of  $\Psi$  with respect to  $\varpi_2$  and  $\varpi_3$ , nor the Hessian of  $F_0$  with respect to  $x_i^0$  were equal to zero<sup>22</sup>.

Since  $\Psi$  is finite and periodic in  $\varpi_2$  and  $\varpi_3$ , equation (5.6.iv) is always satisfied, and so providing  $\mu$  is sufficiently small and neither of the two Hessians vanish, there exists a periodic solution of period  $T$ , where  $T$  is determined by the choice of the numbers  $n_i$ .

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<sup>22</sup> The Hessian of a function is the determinant of the matrix of which the entries are given by the second partial derivatives of the function. It is named after the German geometer Ludwig Otto Hesse (1811-1874).

Furthermore, if  $n_i' = n_i (1 + \varepsilon)$  then, providing  $\varepsilon$  is small, there exists a periodic solution for small values of  $\mu$ ,

$$\phi_i(t, \mu, \varepsilon), \quad \phi_i'(t, \mu, \varepsilon)$$

with period  $T' = \frac{T}{1 + \varepsilon}$  which is nearly equal to  $T$ .

In the case of the restricted three body problem, where there are only two degrees of freedom, the function  $\Psi$  depends only on  $\varpi_2$  and so the relations (5.6.iv) reduce to

$$\frac{\partial \Psi}{\partial \varpi_2} = 0 \quad (5.6.v)$$

and the Hessian of  $\Psi$  reduces to  $\frac{\partial^2 \Psi}{\partial \varpi_2^2}$ . Hence, corresponding to each of the simple roots of equation (5.6.v) there is a periodic solution for all sufficiently small values of  $\mu$ , and, as established in 5.6.2, the same is true for each of the roots of odd order.

Returning to the case where the periodic solutions have period  $T$ , and having shown that they could be expressed in the form of convergent series in powers of  $\mu$ ,

$$x_i = x_i^0 + \mu x_i^1 + \mu^2 x_i^2 + \dots \quad (i = 1, 2, 3)$$

$$y_i = y_i^0 + \mu y_i^1 + \mu^2 y_i^2 + \dots$$

Poincaré's next step was to determine the coefficients of the series.

Considering the unperturbed motion gives values for  $x_i^0$  and  $y_i^0$  but calculating the remaining coefficients requires a careful analysis. Poincaré's procedure, although somewhat lengthy, did not, however, impose any further restrictions on the periodic solutions since the only constraint on its validity was that the Hessian of  $F_0$  with respect to  $x_i^0$  did not vanish.

Applying the theory to specific problems, Poincaré began with the system described by the differential equation

$$\frac{d^2 \rho}{dt^2} + n^2 \rho + m \rho^3 = \mu R(\rho, t).$$

This equation, now generally known as Duffing's equation, is often encountered in celestial mechanics, where it occurs in the theory of libration<sup>23</sup>. It also arises in solid mechanics where it can be modelled by a pendulum under the action of an imposed periodic force.

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<sup>23</sup> Duffing [1918] made an extensive study of this equation in the context of solid mechanics.

To prove the existence of periodic solutions, Poincaré simply applied a series of transformations to put the equation into Hamiltonian form, from which it was straight-forward to see that the requisite conditions were fulfilled. Although, as he observed, when the non-linearity is absent, the Hessian with respect to  $F_0$  is zero, and the theory can no longer be applied.

Turning to the three body problem Poincaré encountered the same difficulty with the vanishing Hessian although, as he described, in the case of the restricted problem it can be easily overcome. In this particular case the small number of variables means that it is possible to find a function of  $F$  which can be used legitimately to replace  $F$  in the Hamiltonian equations and for which the Hessian of  $F_0$  does not vanish. Unfortunately, the same method does not work in the general problem and an alternative method of establishing the existence of periodic solutions needs to be found<sup>24</sup>.

### 5.6.5 Characteristic exponents of the equations of dynamics

To calculate the characteristic exponents of the autonomous Hamiltonian system, Poincaré began by supposing that a periodic solution of the equations was given by  $x_i = \phi_i(t)$ ,  $y_i = \Psi_i(t)$  with a nearby solution given by  $x_i = \phi_i(t) + \xi_i$ ,  $y_i = \Psi_i(t) + \eta_i$ . This leads to the equations of variation

$$\begin{aligned} \frac{d\xi_i}{dt} &= \sum \frac{\partial F}{\partial y_i \partial x_k} \xi_k + \sum \frac{\partial F}{\partial y_i \partial y_k} \eta_k, \\ \frac{d\eta_i}{dt} &= - \sum \frac{\partial F}{\partial x_i \partial x_k} \xi_k - \sum \frac{\partial F}{\partial x_i \partial y_k} \eta_k, \end{aligned} \quad (i, k = 1, 2, 3)$$

with solutions in the form

$$\xi_i = e^{\alpha} S_i, \quad \eta_i = e^{\alpha} T_i, \quad (5.6.vi)$$

$S_i$  and  $T_i$  being periodic functions of  $t$ . Since it is an autonomous Hamiltonian system, two of the characteristic exponents are zero and so there are only four particular solutions.

When  $\mu = 0$ ,  $F$  is reduced to  $F_0$ , and the variational equations are reduced to

$$\frac{d\xi_i}{dt} = 0, \quad \frac{d\eta_i}{dt} = - \sum \frac{\partial F_0}{\partial x_i^0 \partial x_k^0} \xi_k,$$

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<sup>24</sup> Poincaré resolved this difficulty in [MN I, 133].

where the coefficients of the second equation are constants. In this case, the most general solution is given by  $\xi_i = 0$ , and  $\eta_i = \eta_i^0$ , where  $\eta_i^0$  are constants of integration, and so all six characteristic exponents are zero.

To find values for the functions  $\alpha$ ,  $S_i$  and  $T_i$  which satisfy equations (5.6.vi) for small values of  $\mu$ , Poincaré sought series expansions in powers of the parameter. The difficulty is that since all the characteristic exponents are zero when  $\mu = 0$ ,  $\alpha$  cannot be expanded in integer powers of  $\mu$  since the conditions necessary for the implicit function theorem to be valid are no longer fulfilled. This leads to the question of whether  $\alpha$  can be expanded in fractional powers of  $\mu$ .

What Poincaré found was that  $\alpha$ , as well as  $S_i$  and  $T_i$ , could be expanded in powers of  $\sqrt{\mu}$ , and so could be written<sup>25</sup>

$$\alpha = \alpha_1 \sqrt{\mu} + \alpha_2 \mu + \dots \quad (\alpha_0 = 0, \text{ since } \mu = 0 \Rightarrow \alpha = 0)$$

$$S_i = S_i^0 + S_i^1 \sqrt{\mu} + S_i^2 \mu + \dots$$

$$T_i = T_i^0 + T_i^1 \sqrt{\mu} + T_i^2 \mu + \dots$$

To calculate the coefficients in these series, he proceeded by first substituting these series in equations (5.6.vi), and differentiating with respect to  $t$ . Next, having expanded the second derivatives of  $F$  as series in integer powers of  $\mu$ , he made the appropriate substitutions in the variational equations, and then determined the coefficients by equating powers of  $\sqrt{\mu}$ . By this process he was able to calculate the coefficients as far as  $\alpha_i$ ,  $S_i^m$  and  $T_i^m$ .

In [P1] Poincaré went straight into the calculation of the coefficients without first proving that such series do indeed exist, and, moreover, giving no mathematical explanation as to why they should be series in powers of  $\sqrt{\mu}$  rather than  $\mu$ , or indeed rather than any other fractional power of  $\mu$ . He went some of the way towards rectifying this omission in *Note H* although his proof for the existence of the series invoked theorems concerning the method of majorants which only appeared in [P2]. [P2] contained a much more detailed existence proof for the series for  $\alpha$ , including showing that the expansion for  $\alpha$  only contains odd powers of  $\sqrt{\mu}$ ,

<sup>25</sup> Poincaré was not the first to form series in powers of the square root of the parameter. As he acknowledged in his introduction to [P2], series of this type occur in Bohlin's [1888] paper where they are used to overcome the problem of small divisors in planetary perturbation theory. Poincaré later made a careful examination of Bohlin's series in [MN II]. See 7.2.3.



and it also contained a proof for the existence of the other two series, neither of which had been included in [P1]<sup>26</sup>.

In his determination of the coefficients in the series for  $\alpha$ , Poincaré found that the sign of  $\alpha_1^2$  depended on the sign of  $\frac{\partial^2 \Psi}{\partial \omega^2}$ , in other words it depended on the derivative of equation (5.6.v), the roots of which correspond to periodic solutions. Since the stability of the periodic solutions depends on the sign of  $\alpha^2$ , if  $\mu$  is sufficiently small, this translates into the stability being dependent on the sign of  $\alpha_1^2$ . Poincaré was therefore interested in the behaviour of equation (5.6.v). He considered the general case when the equation only has simple roots, i.e. the roots correspond to maxima and minima of the function  $\Psi$ . Since  $\Psi$  is a periodic function, there is at least one maximum and one minimum within each period, and exactly the same number of each. Consequently there are precisely as many roots for which the derivative and  $\alpha_1^2$  are positive, as roots for which the derivative and  $\alpha_1^2$  are negative. This means that, corresponding to each system of values of  $n_1$  and  $n_2$  there is at least one stable and one unstable periodic solution and, providing  $\mu$  is sufficiently small, there are exactly the same number of each.

In [P2] Poincaré also showed how it was possible to continue the calculation of the coefficients for the series for  $S_i$  and  $T_i$  beyond the terms  $S_i^m$  and  $T_i^m$  already calculated.

Many of the changes Poincaré made to this section can be directly attributable to intervention from Phragmén. In the introduction to [P2] Poincaré specifically mentions Phragmén's help with regard to the calculation of the coefficients for the series for  $S_i$  and  $T_i$ . Furthermore, according to Mittag-Leffler, Poincaré's additions of the existence proofs were also prompted by queries from Phragmén<sup>27</sup>.

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<sup>26</sup> Although Poincaré had shown algebraically why the expansions had to be in powers of  $\sqrt{\mu}$ , he gave no dynamical explanation of the result. In essence, the square root arises because near a periodic solution a perturbation changes the nature of some of the phase curves so straightforward perturbation theory cannot be used. However, a local transformation can be found in which the unperturbed Hamiltonian is similar to that of a vertical pendulum for which the separatrix (the special phase curve which separates the phase curves with different properties) width is of the order of  $\sqrt{\mu}$ , and this automatically introduces a  $\sqrt{\mu}$  into the new perturbation.

<sup>27</sup> Mittag-Leffler to Poincaré 16.7.1889, M-L I.

### 5.6.6 Asymptotic solutions

Poincaré now turned his attention to the unstable periodic solutions and the behaviour of other solutions in their immediate neighbourhood.

Starting with equations (5.6.i), he supposed that

$$x_1 = x_1^0, \dots, x_n = x_n^0$$

was a periodic solution with a neighbouring solution  $x_i = x_i^0 + \xi_i$ . He then derived a system of equations to determine  $\xi_i$

$$\frac{d\xi_i}{dt} = \Xi_i \quad (5.6.vii)$$

where the  $\Xi$  are functions which can be expanded in powers of  $\xi$ , are periodic with respect to  $t$ , and have no terms independent of  $\xi$ . Neglecting powers of  $\xi$ , equations (5.6.vii) reduce to the linear equations of variation with general solution  $\xi_i = \sum A_k e^{\alpha_k t} \phi_{ki}$ , where  $A$  are constants of integration,  $\alpha$  characteristic exponents, and  $\phi$  periodic functions of  $t$ .

To solve the equations when they include powers of  $\xi$ , Poincaré made the linear transformation

$$\xi = \sum \eta_k \phi_{ki}$$

so that (5.6.vii) become

$$\frac{d\eta_i}{dt} = H_i = H_i^1 + \dots + H_i^p + \dots \quad (5.6.vii')$$

where the  $H_i$  are functions of  $t$  and  $\eta$  of the same form as  $\Xi$ , and  $H_i^p$  represent the collection of terms of  $H_i$  of degree  $p$  with respect to  $\eta$ . He then looked for general solutions to equations (5.6.vii) and (5.6.vii').

By writing

$$\eta_i = \eta_i^1 + \dots + \eta_i^p + \dots$$

where  $\eta_i^p$  represent the terms of  $\eta_i$  of degree  $p$  with respect to  $A$ , replacing  $\eta_i$  in  $H_i^k$ , and calculating  $\eta_i^q$  by recurrence, he found

$$\frac{d\eta_i^q}{dt} - \alpha_i \eta_i^q = \sum C A^q e^{\Omega t}$$

where  $A^q = A_1^{\beta_1} \dots A_n^{\beta_n}$ ,  $\Omega = \gamma \sqrt{-1} + \sum \alpha \beta$ ,  $\gamma$  is a positive or negative integer and  $\sum \alpha \beta = \alpha_1 \beta_1 + \dots + \alpha_n \beta_n$ .

This equation is satisfied by

$$\eta_i^q = \sum \frac{CA^q e^{\Omega t}}{\Omega - \alpha_i}$$

where  $C$  is generally imaginary,  $\beta$  are positive integers with sum  $q$ , excluding the exceptional case when  $\Omega - \alpha_i = 0$ , when terms in  $t$  are introduced.

He then proved that the series

$$\eta_i = \sum N \frac{A_1^{\beta_1} A_2^{\beta_2} \dots A_n^{\beta_n}}{\Pi} e^{\Omega t},$$

where  $\Pi$ , which represents the product of the divisors  $\Omega - \alpha_i$ , is convergent, providing that  $\Omega - \alpha_i$  does not become less than any given quantity  $\varepsilon$  for positive integer values of  $\beta$  and positive or negative integers  $\gamma$ , i.e. if neither of the two convex polygons containing  $\alpha \pm \sqrt{-1}$  contain the origin or if the real part of the quantities  $\alpha$  are the same sign and not equal to zero. Although he observed that the convergence followed immediately from his results on the method of majorants applied to partial differential equations (see 5.4.3), he also provided a direct proof.

With the restricted three body problem in mind, Poincaré next considered the particular system represented by the differential equations

$$\frac{dx_1}{dt} = X_1, \quad \frac{dx_2}{dt} = X_2,$$

with the added condition

$$\frac{\partial X_1}{\partial x_1} + \frac{\partial X_2}{\partial x_2} = 0,$$

which implies that the "volume" is an invariant integral.

As Poincaré remarked, since a state of the system only depends on the variables,  $x_1$ ,  $x_2$ , and  $t$ , it can be represented by the position of a point in space with coordinates  $e^{i\omega t} \cos t, e^{i\omega t} \sin t, x_2$ . A periodic solution can then be represented by a closed curve, and if the periodic solution is unstable the coefficient of stability  $\alpha^2$  will be real and positive. In this case the  $\eta_i$  can be expanded as a series in  $Ae^{\alpha t}$  and  $Be^{-\alpha t}$ . Thus if  $A = 0$ , and  $t \rightarrow +\infty$ , then  $\eta_1$  and  $\eta_2 \rightarrow 0$  and the corresponding solution asymptotically approaches the periodic solution. Similarly, if  $B = 0$ , and  $t \rightarrow -\infty$ , then again  $\eta_1$  and  $\eta_2 \rightarrow 0$  and the solution again asymptotically approaches the periodic solution. These two series of solutions, the first corresponding to  $t = +\infty$ , and the second corresponding to  $t = -\infty$ , are what Poincaré called *asymptotic solutions*. Moreover, since each of these series corresponds to a sequence of curves which asymptotically

approaches a closed curve  $C$ , Poincaré called the surface formed by the set of these curves an *asymptotic surface*. Thus, there are two asymptotic surfaces, one corresponding to  $t = +\infty$ , and the other corresponding to  $t = -\infty$ , and both of these surfaces pass through the closed curve  $C$ .

In [P1] Poincaré went through a similar analysis to show that in the case of equations (5.6.i) the series for  $\eta$  could be expanded in a convergent series in  $Ae^{\alpha t}$ . But at the end of the analysis he added the claim that if the differential equations depend on the parameter  $\mu$  then the series could also be expanded in powers of  $\mu$  or  $\sqrt{\mu}$ , according to the circumstances. Nowhere did he prove that such expansions were actually possible. Furthermore, implicit in his claim was that the series in each case was convergent. Particularly significant is the fact that he made no attempt to distinguish between the autonomous and nonautonomous cases. As he later discovered, neglecting to make this distinction was a serious oversight.

In [P2] the ending of the section was quite different. Poincaré proved that the  $\eta$  could be represented by a series in  $\mu$ , and, moreover, that these series were convergent providing, firstly that the differential equations depend on the parameter  $\mu$  and the functions  $X_i$  can be expanded in powers of the parameter; secondly that for  $\mu = 0$ , all the characteristic exponents  $\alpha$  are distinct and can be expanded in integer powers of  $\mu$ ; and thirdly it is possible to remove all the constants  $A$  which correspond to an  $\alpha$  whose real part  $\leq 0$ .

The significant condition to observe is the second one which concerns the characteristic exponents. For this condition means that if the system under consideration is an autonomous Hamiltonian system, then the series are not convergent, and in particular the series are not convergent in the case of the restricted three body problem. This point is central with regard to Poincaré's error. For in [P1] Poincaré had not appreciated that in describing the behaviour of asymptotic solutions there was a critical difference between autonomous and nonautonomous systems, a difference which initially manifests itself in the values of the characteristic exponents. In [P2] the distinction between the two cases is clearly made, the nonautonomous case having been dealt with here and the autonomous case being the subject of the next section.

### 5.6.7 Asymptotic solutions of the equations of dynamics

Poincaré had already proved that there were circumstances under which the autonomous Hamiltonian system would have periodic solutions, and so to establish



the existence of asymptotic solutions he only had to make certain that one of the corresponding characteristic exponents  $\alpha$  was real. That being so, it only remained to ascertain the form of the asymptotic solutions.

In the case discussed in the previous section the functions  $X_i$  were expanded in powers of  $\mu$  and the characteristic exponents were distinct for  $\mu = 0$ . In the case of the autonomous Hamiltonian equations, the right hand side of the equations can again be expanded as powers of  $\mu$ , but now all the characteristic exponents vanish when  $\mu = 0$ .

This results in several important differences. Firstly, as Poincaré had already described, the expansions for the characteristic exponents are in powers of  $\sqrt{\mu}$  rather than  $\mu$ . Similarly, the expansions of the functions  $\phi_{i,k}$  which appear in the general solution to the variational equations and which, in this case, are the expansions of the functions  $S_i$  and  $T_i$ , are also in powers of  $\sqrt{\mu}$  rather than  $\mu$ . Furthermore, this implies that the expansions of the functions  $H_i$ , are in powers of  $\eta$ ,  $\exp(t\sqrt{-1})$ ,  $\exp(-t\sqrt{-1})$ , and  $\sqrt{\mu}$  (and not of  $\mu$ ). Although  $\eta_i$  can be derived as before

$$\eta_i = \sum N \frac{A_1^{\beta_1} A_2^{\beta_2} \dots A_n^{\beta_n}}{\prod} e^{\Omega_i},$$

the expansions of  $N$  and  $\prod$  are now also in powers of  $\sqrt{\mu}$ .

These differences led Poincaré to ask the following questions:

1. Since  $N$  and  $\prod$  can be expanded as powers of  $\sqrt{\mu}$ , can the quotient  $\frac{N}{\prod}$  also be expanded in powers of  $\sqrt{\mu}$ ?
2. If the answer to Question 1 is yes, then this implies the existence of series in  $\sqrt{\mu}$ ,  $A_i e^{a_i t}$ ,  $\exp(t\sqrt{-1})$ , and  $\exp(-t\sqrt{-1})$ , which *formally* satisfy the equations; are these series convergent?
3. If the series are not convergent, can they be used to approximate the asymptotic solutions?

With regard to Question 1, since both  $N$  and  $\prod$  can be expanded in powers of  $\sqrt{\mu}$ , Poincaré realised that the only problem which could arise with the expansion of their quotient is the appearance of negative powers of  $\sqrt{\mu}$ . For if this should occur then the asymptotic solutions would cease to exist for  $\mu = 0$ . His answer therefore consisted in proving that these negative powers never arise. He had previously

recognised the existence of this particular problem and an earlier version of the proof is included in *Note I*.

Poincaré had therefore proved the existence of series which formally satisfied the equations but were these series convergent? Importantly, Poincaré showed that they were not. However, when he first discovered the divergence of these series it was entirely unexpected. His analysis in [P1] had led him to believe that the series were actually convergent. Put in the context of the whole memoir, his original failure to appreciate the divergence of these series is essentially the analytical analogue of the geometrical mistake which he made at the end of his discussion on invariant integrals in [P1].

In [P2] Poincaré proved that, rather than being convergent, the series belonged to the class of divergent series which he had defined in [1886a] as asymptotic series. [P1a] reveals that he was slightly concerned about the status of this particular proof despite describing it in [P2] as "*rigoureuse*". In [P1a] the word "*rigoureuse*" was originally preceded by the word "*plus*", which was then crossed out and replaced by the word "*absolument*", which was then also crossed out.

He began with the expression  $(\Omega - \alpha_i)^{-1}$ . If  $\gamma$  is not equal to zero, then this expression can be expanded in powers of  $\sqrt{\mu}$  but the radius of convergence of the series will tend to zero as  $\frac{\gamma}{\sum \beta}$  tends to zero. Thus if the expression  $\frac{1}{\prod}$  is expanded in powers of  $\sqrt{\mu}$ , there will always be an infinite number of such expressions for which the radius of convergence of the expansion is arbitrarily small. If the same is true for  $\frac{N}{\prod}$ , then this implies that the series are divergent.

Rather than considering the series for  $\eta_i$ , Poincaré began with the simpler series

$$F(w, \mu) = \sum \frac{w^n}{1 + n\mu}$$

where  $w = Ae^{\alpha t}$ . This series in  $w$  is uniformly convergent when  $\mu > 0$  and  $|w| < w_0 < 1$ , and if differentiated the resulting series is also uniformly convergent.

On the other hand, if the function  $F(w, \mu)$  is expanded as a series in  $\mu$

$$F(w, \mu) = \sum_{n,p} w^n (-n)^p \mu^p \quad (5.6.viii)$$

then as Poincaré knew from his theory developed in [1886a], the series is not convergent but is an asymptotic expansion.

Poincaré then claimed that the series (5.6.viii) was completely analogous both to the series which represent the functions  $\eta_i$ ,

$$\sum \frac{N}{\prod} w_1^{\beta_1} \dots w_k^{\beta_k} e^{\gamma \sqrt{-1}} = F(\sqrt{\mu}, w_1, \dots, w_k, t), \quad (w_i = A_i e^{\alpha_i t})$$

and to the series

$$\sum w_1^{\beta_1} \dots w_k^{\beta_k} e^{\gamma \sqrt{-1}} \frac{d^p \left( \frac{N}{\prod} \right)}{(d\sqrt{\mu})^p} = \frac{d^p F}{(d\sqrt{\mu})^p}.$$

These two series are uniformly convergent when expanded in powers of  $w$  provided  $|w| < w_0 < 1$  and  $\sqrt{\mu}$  is real, but if  $\frac{N}{\prod}$  is expanded in powers of  $\sqrt{\mu}$ , then they are divergent. Thus if they are analogous to the series (5.6.viii) they must be asymptotic expansions.

Poincaré first defined  $\Phi_p(\sqrt{\mu}, w_1, \dots, w_k, t)$  to be a polynomial of degree  $p$  in  $\sqrt{\mu}$  which can be expanded in powers of  $w$ , and  $\exp(\pm t\sqrt{-1})$ . The series for  $\frac{F - \Phi_p}{\sqrt{\mu}^p}$  is then given

by

$$\sum \frac{1}{\sqrt{\mu}^p} \left( \frac{N}{\prod} - H_p \right) w_1^{\beta_1} \dots w_k^{\beta_k} e^{\gamma \sqrt{-1}}$$

where  $H_p$  is the group of terms in the expansion of  $\frac{N}{\prod}$  in which the exponent of  $\sqrt{\mu}$  is at most equal to  $p$ . To prove that the series for  $\eta_i$  is an asymptotic expansion, this series must be shown to be uniformly convergent with its terms tending to zero as  $\mu$  tends to zero. This convergence proof turned out to require a long and delicate analysis and Poincaré's attempt in [P2] included some unproven assertions which doubtless accounts for his concern about the rigour<sup>28</sup>.

Nevertheless, he correctly concluded that the series for the asymptotic solutions

$$x_i = x_i^0 + \sqrt{\mu} x_i^1 + \mu x_i^2 + \dots \quad y_i = n_i t + y_i^0 + \sqrt{\mu} y_i^1 + \mu y_i^2 + \dots$$

were asymptotic expansions and, in addition, that if they were differentiated they would also give asymptotic expansions.

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<sup>28</sup> Later, in the first volume of the *Méthodes Nouvelles*, Poincaré gave a fuller version of the proof in which he supplied the missing details [MN I, 353-382].

## 5.7 Study of the case with two degrees of freedom

In [P1] the opening chapter of the second part of the memoir consisted of five sections and constituted the major part of this half of the memoir. In [P2] Poincaré changed the structure so that only the first of these sections, which concerned the geometric representation of systems of differential equations, was contained in the first chapter of the second part of [P2].

In this part of the memoir Poincaré was primarily concerned with applying the theory from the previous part to the restricted three body problem, and consequently he focused on the Hamiltonian form of the differential equations with two degrees of freedom

$$\frac{dx_i}{dt} = \frac{\partial F}{\partial y_i}, \quad \frac{dy_i}{dt} = -\frac{\partial F}{\partial x_i} \quad (i = 1, 2) \quad (5.7.i)$$

where  $x_i$  are linear variables,  $y_i$  are angular variables, and  $F$  is an autonomous function of  $x_i$  and  $y_i$ , periodic with period  $2\pi$  with respect to  $y_i$ .

His strategy was to begin by showing in general how such a system can be given a geometric representation which uniquely identifies its each and every state, the chosen representation depending on the given constraints of the particular problem under consideration.

In the first instance, since the four variables are linked by the Jacobian integral,

$$F = (x_1, x_2, y_1, y_2) = C,$$

Poincaré could represent each state of the system by a point in space. Adding further conditions he developed representations in which each state of the system was either represented by a point contained between two tori, or represented by an interior point of a torus. He then showed how this kind of representation could be used in the restricted three body problem.

With the restricted three body problem he started by defining the position of the planetoid using the osculating elements, i.e. the variables defined by means of the instantaneous ellipse described by the planetoid round the centre of gravity of the system. He adopted Tisserand's [1887] notation (derived from Delaunay), to write the equations of motion in canonical form

$$\frac{dL}{dt} = \frac{\partial R}{\partial l}, \quad \frac{dl}{dt} = -\frac{\partial R}{\partial L} \quad (5.7.ii)$$



$$\frac{dG}{dt} = \frac{\partial R}{\partial g}, \quad \frac{dg}{dt} = -\frac{\partial R}{\partial G},$$

where  $l$  is the mean anomaly of the planetoid,  $g$  is the longitude of its perihelion,  $n$  is the mean motion of the planetoid,  $L = \sqrt{a}$ , where  $a$  is the semi-major axis of the instantaneous ellipse, and  $G = \sqrt{a(1 - e^2)}$ , where  $e$  is the eccentricity of the ellipse. In order to preserve the canonical form, the standard perturbation function is increased by the addition of the term  $\frac{1}{2a} = \frac{1}{2L^2}$  to give the Hamiltonian  $R$ .

Poincaré chose his units so that the masses of the two primaries were  $1 - \mu$  and  $\mu$ , the gravitational constant was equal to one, the mean motion of the smaller of the two primaries was equal to one and its longitude was equal to  $t$ . Under these conditions the angle from which the distance between the two smaller masses is seen from the larger differs from  $l + g - t$  by a periodic function of  $l$  of period  $2\pi$ .

Since the distance between the primaries is constant and the distance between the larger of the two primaries and the planetoid is only dependent on  $L, G$  and  $l$ , the function  $R$  is therefore only dependent on  $L, G, l$  and  $l + g - t$ . Moreover, since  $R$  is periodic with period  $2\pi$  with respect to  $l$ , and with respect to  $l + g - t$

$$\frac{\partial R}{\partial t} + \frac{\partial R}{\partial g} = 0,$$

and equations (5.7.ii) admit the integral  $R + G = \text{constant}$ .

However  $R$  has an explicit dependence on  $t$ , and so equations (5.7.ii) are not in the required form of equations (5.7.i). To remedy this Poincaré made the transformation

$$x_1 = G, \quad x_2 = L, \quad y_1 = g - t, \quad y_2 = l$$

$$F(x_1, x_2, y_1, y_2) = R + G.$$

The function  $F$  is dependent on the mass parameter  $\mu$ , and so can be written

$$F = F_0 + \mu F_1$$

which if  $\mu = 0$  reduces to

$$F = F_0 = \frac{1}{2a} + G = x_1 + \frac{1}{2x_2^2},$$

which is a function of only the linear variables.

By definition  $L^2 \geq G^2$ , which implies that  $x_2 \geq x_1 \geq -x_2$ . If  $x_1 = +x_2$ , then the eccentricity is zero, and the perturbation function and the state of the system only

depend on the difference of the longitude of the two smaller masses, i.e. they only depend on

$$l + g - t = y_1 + y_2.$$

Consequently

$$\frac{\partial F}{\partial y_1} = \frac{\partial F}{\partial y_2},$$

which implies

$$\frac{d(x_1 - x_2)}{dt} = 0,$$

and since  $x_2^2 \geq x_1^2$ , the maximum value of  $x_1 - x_2$  is 0. ( $x_1$  is not identically equal to  $x_2$  since in equations (5.7.i)  $x_1 = x_2$  only if there is a singularity.) If  $x_2 = -x_1$ , again the eccentricity is zero but the motion is then retrograde, which always occurs when  $x_1$  and  $x_2$  are of different sign.

To create a geometric representation for the restricted three body problem, Poincaré needed to represent the system using only three variables. He therefore sought to express  $x_1$  and  $x_2$  as single-valued functions of  $y_1, y_2$  and a new variable  $\xi$ .

He began with  $\mu = 0$  and considered the plane in which the coordinates of a point are defined by

$$X = x_1 + C, \quad Y = x_2$$

which, from the definition of  $F$  and the constraint on  $x_1$ , implies

$$X + \frac{1}{2Y^2} = 0, \quad Y > X + C > -Y.$$

The construction of the curve  $X + \frac{1}{2Y^2} = 0$  together with the lines  $X + C = \pm Y$ , takes two different forms depending on the value of the constant  $C$  as shown in *FIGS. 5.7.i* and *5.7.ii*<sup>29</sup>, the transition point which occurs when the line  $CD$  becomes tangent to the curve takes place when  $X = 1/2$ ,  $Y = 1$ , and  $C = 3/2$ . Although the inequalities are satisfied by the curves  $BC$  and  $DE$ , in *FIG. 5.7.i* and  $BC$  in *FIG. 5.7.ii*, the part of the curve which is of interest with respect to the problem is the part which is bounded, that is  $BC$  in *FIG. 5.7.i*.

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<sup>29</sup> Since the figures are symmetric with respect to the  $X$  axis, Poincaré only included diagrams of the top half of the  $X, Y$  plane [P2, 402].

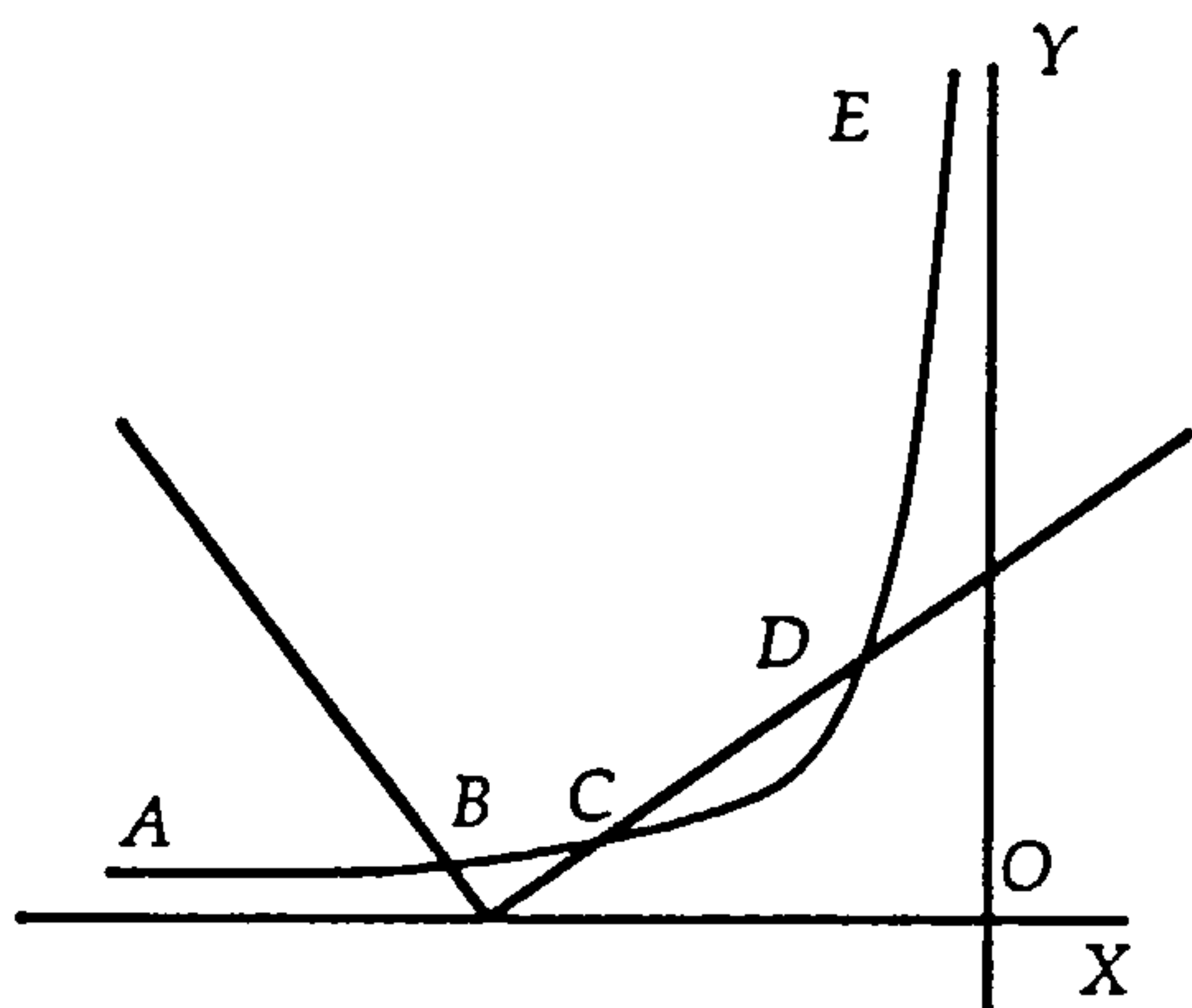


FIG. 5.7.i.

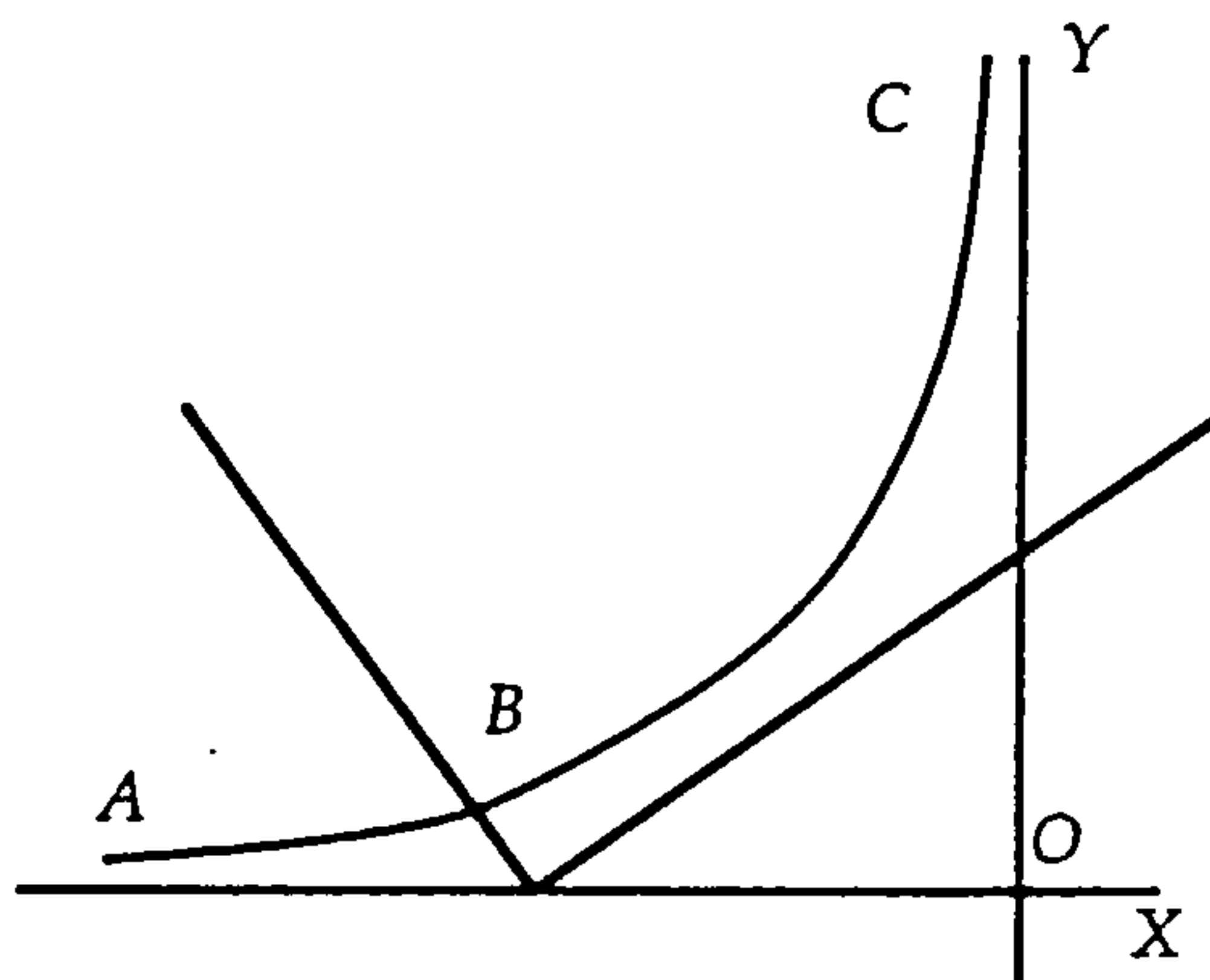


FIG. 5.7.ii.

For  $\mu = 0$ , choosing  $\xi = \frac{x_2 - x_1}{x_2 + x_1}$ , fulfils the required conditions since along the arc CB,  $\xi$  will increase constantly from 0 to  $\infty$ .

For small values of  $\mu$ ,  $\xi$  can be chosen in the same way, but only if  $x_2 > 0$ , and the Jacobian  $\frac{\partial(\xi, F)}{\partial(x_1, x_2)}$  is not equal to zero. Providing the value of C is not close to  $3/2$  these conditions are satisfied for small values of  $\mu$ , and  $\xi$  can be taken as the independent variable.

Finally, in order to make the most convenient representation, Poincaré made a further transformation to another set of canonical variables

$$\begin{aligned} x_1' &= x_1 + x_2, & x_2' &= x_1 - x_2, \\ y_1' &= \frac{1}{2}(y_1 + y_2), & y_2' &= \frac{1}{2}(y_1 - y_2). \end{aligned}$$

In this form  $y_i'$  are angular variables which if increased by  $2\pi$  generate an identical increase in  $y_i$  and so the system remains unchanged. The system also remains unchanged if simultaneously  $y_1'$  and  $y_2'$  are each increased by  $\pi$ . A state of the system can then be represented by a point in space with rectangular coordinates

$$X = \cos y_1' \exp(\xi \cos y_2') \quad Y = \sin y_1' \exp(\xi \cos y_2') \quad Z = \xi \sin y_2'.$$

In this representation each point in space corresponds to a single state of the system, while the two systems of values  $(x_1', x_2', y_1', y_2')$  and  $(x_1', x_2', y_1' + \pi, y_2' + \pi)$ , which correspond to two different points of space, correspond to only one state of the system.

In addition, applying the transformation has the effect of reducing the fourth order invariant integral of the Hamiltonian equations to a third order positive invariant.

When  $\mu = 0$ , equations (5.7.ii) integrate to give

$$L = \text{constant}, \quad G = \text{constant}, \quad g = \text{constant}, \quad l = nt + \text{constant}.$$

These solutions can be represented by trajectories which are closed whenever the mean motion  $n$  is a rational number. They lie on the surface trajectories which are defined by the general equation  $\xi = \text{constant}$  and, consequently, generate closed surfaces of revolution analogous to tori.

In his next chapter, Poincaré showed the effects on these results when the system no longer remains unperturbed and  $\mu$  takes on small values.

Meanwhile he concluded the current chapter with the consideration of two more dynamical problems. For the first he returned to the system described by Duffing's equation, and for the second he considered a heavy point mass moving on a frictionless surface in the neighbourhood of a stable equilibrium. In each case he generated a similar representation to the one he had derived for the restricted three body problem.

It is noticeable that Poincaré accorded this section on geometrical representations a higher degree of prominence in [P2] than in [P1]. In the latter it is included as the opening section of the first chapter of the second part of the memoir, while in the former it warrants an entire chapter to itself. What prompted this change of emphasis?

It is clear that for Poincaré framing dynamical problems geometrically came naturally (as exemplified by his remarks on the theorems concerning invariant integrals). However, his kind of geometric approach to celestial mechanics represented something quite new in mathematics, and its sheer novelty would have been sufficient to make the contents of the memoir almost inaccessible to those of a more practical persuasion. This was certainly the view adopted by Mittag-Leffler who, while studying the original memoir, expressed the concern that Poincaré's resolution of the restricted three body problem was given in a form which would be difficult to understand by anyone except those very familiar with his work, and that astronomers in particular would not understand it all<sup>30</sup>. Mittag-Leffler identified as the main source of potential difficulty the fact that Poincaré was

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<sup>30</sup> Mittag-Leffler to Poincaré 15.11.88, M-L I.



working in a three-dimensional *multiplicité* (= manifold) which was not the Euclidean three-dimensional space in which the bodies actually moved.

Poincaré responded to Mittag-Leffler's remarks by translating his most important results into a more traditional format known to be familiar to astronomers, and which he added as *Note B*. However, the discovery of the mistake invalidated a large part of the *Note*, and he completely excised all trace of it in the revision.

So it could be argued that he chose to use the structure of the memoir to stress the geometrical representation rather than include a revised version of *Note B*. Given the nature of the new results, the rewriting would have been a delicate undertaking and not one he would have relished in the time he had available. In any event, he would have been primarily concerned with the response from mathematicians rather than astronomers, and so it is perhaps not surprising that he chose not to develop this side of the problem any further at this stage. However, he did not entirely forget the astronomers, for in the following year he wrote a summary of his results from [P2]:

"... for the readers of the *Bulletin astronomique* who do not have time to read 'in extenso' the original memoir which is very voluminous." [1891, 480],

which, although materially similar to *Note B*, was in fact a new paper which had a quite different structure.

## 5.8 Study of asymptotic surfaces

Poincaré now set out to find the exact equations for the asymptotic surfaces (the geometric representations of the asymptotic solutions of the differential equations), and thereby derive an understanding of the behaviour of the asymptotic solutions. Although he approached the problem in a similar way in both [P1] and [P2], in [P2] he added an entirely new section purely for the purpose of stating the problem and outlining a strategy for dealing with it, giving a clear indication of the importance he now attached to the topic.

His method was to calculate the coefficients of an increasing number of terms in the asymptotic series

$$x_1 = s_1(y_1, y_2, \sqrt{\mu}) \quad x_2 = s_2(y_1, y_2, \sqrt{\mu}) \quad (5.8.i)$$

and then use the series to make better approximations to the asymptotic surfaces

$$x_1 = f_1(y_1, y_2), \quad x_2 = f_2(y_1, y_2), \quad (5.8.ii)$$

where the  $x_i$  satisfy

$$\frac{\partial F \partial x_i}{\partial x_1 \partial y_1} + \frac{\partial F \partial x_i}{\partial x_2 \partial y_2} + \frac{\partial F}{\partial y_i} = 0. \quad (5.8.iii)$$

The first approximation would only involve the first two terms in the series (5.8.i), i.e. the error would be of the order of  $\mu$ . The second approximation would then involve a larger number of terms so that the error would be of the order  $\mu^p$  for any fixed  $p$ , no matter how large, and the final analysis would concern the exact equations (5.8.ii). Poincaré's initial problem, therefore, was to form the series  $s_i$  which, when substituted for  $x_i$  would formally satisfy equations (5.8.iii).

He had already shown that the generating periodic solution could be represented geometrically by a closed curve through which passed two asymptotic surfaces, and that it was possible to move from one surface to the other by changing  $\sqrt{\mu}$  to  $-\sqrt{\mu}$ . As a result, it was clear that changing  $\sqrt{\mu}$  to  $-\sqrt{\mu}$  in equations (5.8.ii) would give rise to a second asymptotic surface which would cut the first. Moreover, considering these two asymptotic surfaces as two sides of the same surface, then this surface would have the notable feature of a double curve.

If  $s_1^p$  and  $s_2^p$  are the sums of the first  $p$  terms of the series  $s_1$  and  $s_2$ , then the equations

$$x_i = s_i^p(y_1, y_2, \sqrt{\mu}) \quad x_i = s_i^p(y_1, y_2, -\sqrt{\mu}) \quad (i = 1, 2)$$

represent two surfaces differing only slightly from the two sides of the asymptotic surface and so they too must cut each other. Thus if these two surfaces are considered as two sides of a single surface, then this single surface will also display a double curve. In what follows Poincaré proved that this condition was sufficient to distinguish  $s_1$  and  $s_2$  from all the series of the same form which satisfy equations (5.8.iii).

### 5.8.1 First approximation

Most of this section in [P2] came from two consecutive sections from Chapter I in [P1]. It began with *The Equation of Asymptotic Surfaces* and ended with the first half of *The Construction of Asymptotic Surfaces (first approximation)*.

Poincaré had already shown that, providing  $\mu$  is sufficiently small, the differential equations have periodic solutions of which exactly half are stable and half unstable. He had also shown that through each closed curve representing an unstable periodic solution pass two asymptotic surface trajectories, and that these

surface trajectories have a series expansion in terms of  $\sqrt{\mu}$ . His aim now was to determine the coefficients of the first two terms in this series, that is the first two terms in the series (5.8.i).

Since he was concerned with the restricted three body problem, he began with the Hamiltonian equations (5.7.i) assuming that  $F$  could be expanded in powers of the mass parameter  $\mu$

$$F = F_0 + \mu F_1 + \mu^2 F_2 + \dots,$$

with  $F_0$  independent of  $y$ . In order to ensure the existence of a generating periodic solution, he supposed that for certain values of  $x_i$ , say  $x_i^0$ ,  $\frac{\partial F_0}{\partial x_i} (= n_i)$  were commensurable.

The general form of the equation of a surface trajectory is then given by

$$x_i = \Phi_i(y_1, y_2), \quad (i = 1, 2)$$

providing the functions  $\Phi_i$  are chosen such that  $F(\Phi_1, \Phi_2, y_1, y_2) = C$ , and they satisfy equations (5.8.iii).

To integrate equations (5.8.iii) Poincaré supposed

$$x_i = x_i^0 + \sqrt{\mu} x_i^1 + \mu x_i^2 + \dots \quad (5.8.iv)$$

where  $x_i$  are very close to  $x_i^0$ , the latter having been chosen such that the ratio  $n_1:n_2$  is commensurable. It then remained to determine the coefficients  $x_i^k$  such that when the series (5.8.iv) are substituted into the equations (5.8.iii) the equations are formally satisfied.

To generate a sequence of equations from which he could determine  $x_i^k$ , Poincaré substituted the series for  $x_i$  in the series for  $F$  then equated powers of  $\mu$ .

In his determination of the coefficients  $x_i^k$  Poincaré first showed that they were periodic functions of  $y_1$  and as such could be expanded as trigonometric series in sines and cosines of multiples of  $y_1$ . He then found that

$$x_1^1 = 0, \quad x_2^1 = \sqrt{\frac{2}{N}([F_1] + C_1)} \quad (5.8.v)$$

where the notation  $[F_1]$  represents the average value of the function  $F_1$  considered as a periodic function of  $y_1$ ,  $N = -\frac{\partial^2 F_0}{(\partial x_2^0)^2}$ , and  $C_1$  is an integration constant. Thus he could write the series to be used in the first approximation as

$$x_1 = x_1^0, \quad x_2 = x_2^0 + \sqrt{\frac{2\mu}{N}([F_1] + C_1)}.$$

At this point in [P2] Poincaré stopped following the section on the *Equation of Asymptotic Surfaces* from [P1] and continued instead with material taken from the section on *Construction of Asymptotic Surfaces (first approximation)*.

The remaining pages from the *Equation of Asymptotic Surfaces*, which did not appear in [P2], contained an outline of Poincaré's method for determining the remaining coefficients  $x_i^k$ , which was clearly not required for the first approximation and which, for each coefficient, involved the choice of  $C_k$ , an arbitrary constant of integration. The section concluded with three points raised for discussion:

1. When are the series thus obtained convergent?
2. How should the arbitrary constants  $C_1, C_2, \dots, C_{k-1}, \dots$  be determined?
3. What are the properties of the functions defined by the series?

all of which he addressed in later sections (see 5.8.3).

In [P2] Poincaré considered his results in the context of the geometric representation of the restricted three body problem. To simplify the notation, he suppressed the primes and called the variables  $x_i$  and  $y_i$  (not to be confused with the original  $x_i$  and  $y_i$ , i.e.  $G, L, g - t$ , and  $l$ ). The new  $y_i$  are linear functions of

$$y_1' = \frac{1}{2} (g - t + l) \quad y_2' = \frac{1}{2} (g - t - l)$$

and the ratio  $\frac{x_2}{x_1}$  is a linear function of  $\xi$ . Poincaré was now able to define completely the position of a point  $P$  in the space so that every relation between  $y_1, y_2$ , and the ratio  $\frac{x_2}{x_1}$  was the equation of a surface, and both  $y_1$  and  $y_2$  could be increased by a multiple of  $2\pi$  without changing the position of  $P$ .

The coefficients from (5.8.v) then gave the first approximation for the equation of the surface trajectories

$$\frac{x_2}{x_1} = \frac{x_2^0 + x_2^1 \sqrt{\mu}}{x_1^0 + x_1^1 \sqrt{\mu}} = \frac{x_2^0}{x_1^0} + \frac{\sqrt{\mu}}{x_1^0} \sqrt{\frac{2}{N}([F_1] + C_1)}. \quad (5.8.vi)$$

Poincaré's next problem was to identify the particular surfaces which, as he had earlier described, displayed a double curve. This led him to consider the



intersections of the surfaces defined by equations (5.8.vi) with the transverse section  $S$  defined by the surface  $y_1 = 0$ .

The position of a point  $P$  on the surface  $S$  is defined by the two coordinates  $\frac{x_2}{x_1}$  and  $y_2$ , which, since they are analogous to polar coordinates, means that the curves  $\frac{x_2}{x_1} = \text{constant}$  are closed concentric curves on the surface  $S$  and the position of a point  $P$  on  $S$  is unchanged when  $y_2$  is increased by  $2\pi$ . Since  $\sqrt{\mu}$  is very small, the intersections of the surfaces defined by equation (5.8.vi) with the transverse section defined by  $y_1 = 0$  differ very little from the curves  $\frac{x_2}{x_1} = \text{constant}$ .

In order to investigate the curves formed by the intersections, Poincaré needed to understand the nature of the function  $[F_1]$ . He found that it was a finite periodic function of  $y_2$ , in other words, it was similar to the function  $\Psi$  he had found previously. To look at a general function of this type he supposed that as  $y_2$  varied from 0 to  $2\pi$ ,  $[F_1]$  varied as in FIG. 5.8.i<sup>31</sup>, where  $\phi_1 > \phi_3 > \phi_2 > \phi_4$ .

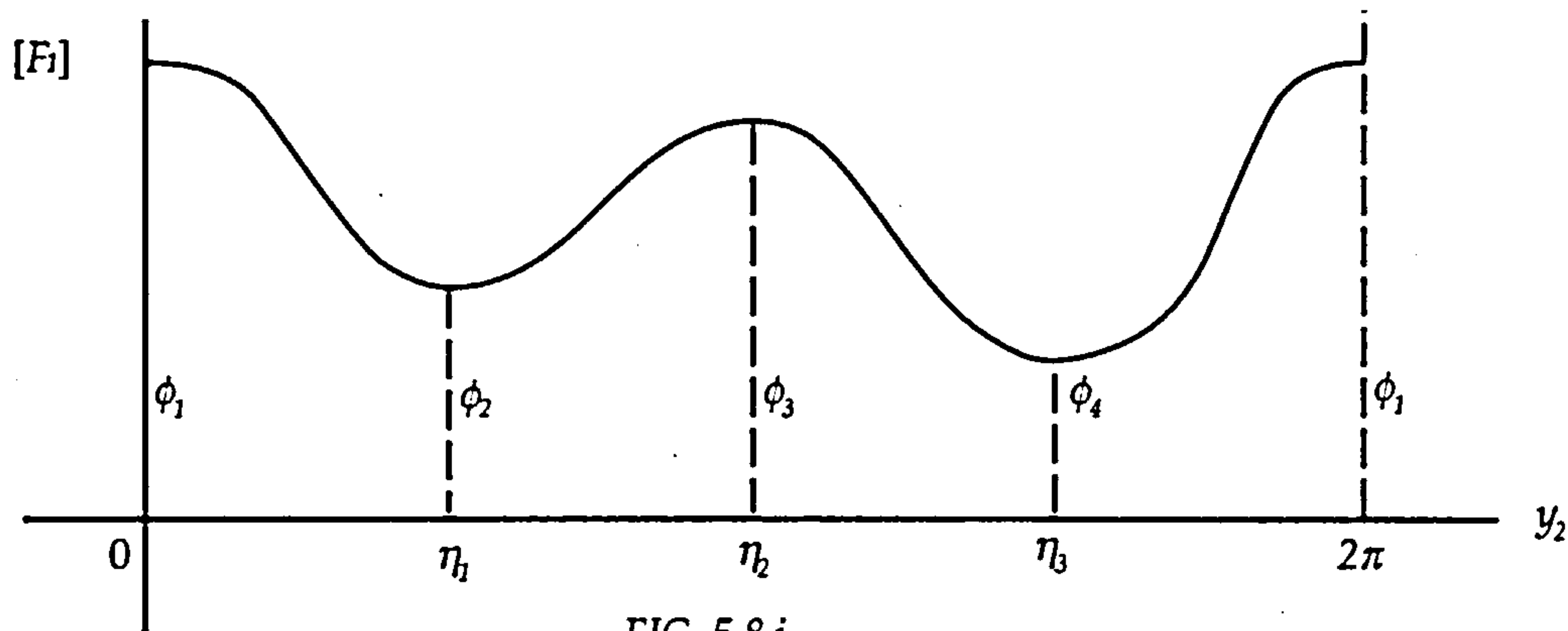


FIG. 5.8.i.

He then constructed a set of curves defined by

$$y_1 = 0, \quad \frac{x_2}{x_1} = \frac{x_2^0}{x_1^0} + \frac{\sqrt{\mu}}{x_1^0} \sqrt{\frac{2}{N}([F_1] + C_1)},$$

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<sup>31</sup> Poincaré [P2, 418].

the shape of each curve depending on the value of the constant  $C_1$  as shown in FIG. 5.8.ii<sup>32</sup>. Each one of these curves lies in the plane  $y_1 = 0$ , and so if  $y_1$  is now varied from 0 through to  $2\pi$ , the curves will each sweep out a surface. More precisely, if through each point on an arbitrary one of these curves is drawn one of the lines defined by the equations  $y_2 = \text{constant}$ ,  $\frac{x_2}{x_1} = \text{constant}$ , then the set of all these lines constitutes a closed surface which is exactly one of the surfaces defined by equation (5.8.vi).

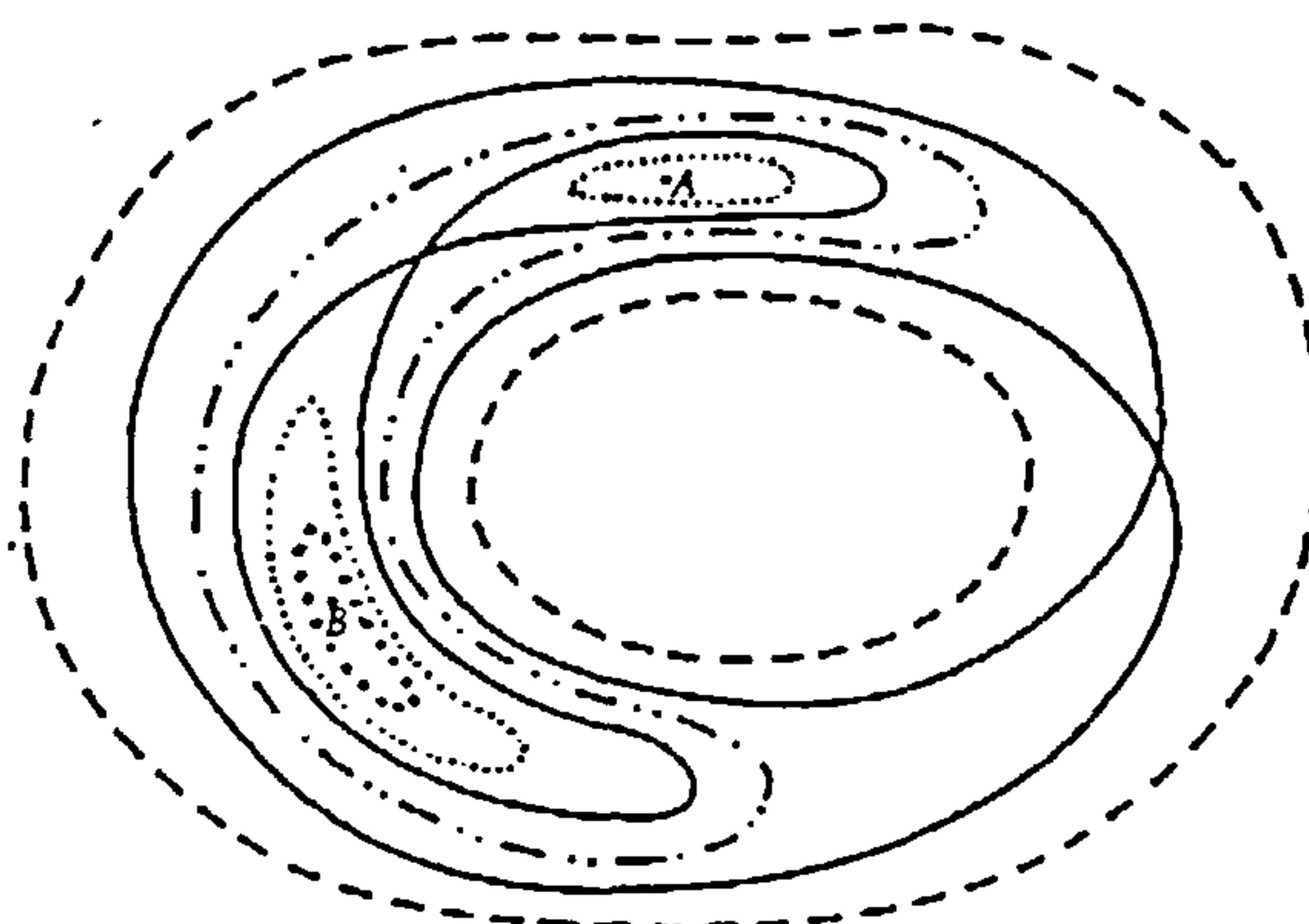


FIG. 5.8.ii.

Since each of the roots of the equation  $\frac{[dF_1]}{dy_2} = 0$  corresponds to a periodic solution (cf equation 5.6.v), the periodic solutions correspond to the extremum points of  $[F_1]$ . In this case (due to the choice of  $[F_1]$ ) there are four extremum points, which represent four periodic solutions, two stable and two unstable. The two stable periodic solutions correspond to the two isolated closed curves of the surfaces  $C_1 = -\phi_3$  and  $C_1 = -\phi_1$  (points A and B), and the two unstable periodic solutions correspond to the double curves of the surfaces  $C_1 = -\phi_2$  and  $C_1 = -\phi_4$ . By the criteria established earlier, the latter two are the ones in which Poincaré was interested and which represent his first approximation.

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<sup>32</sup> Poincaré [P2, 419].

In [P1], Poincaré arrived at the same result but, due to his earlier analysis and his (erroneous) belief that the asymptotic surfaces could be represented by convergent series in  $\sqrt{\mu}$ , he also included the following conclusions:

1. At the first approximation the asymptotic surfaces are closed surfaces, and this result is confirmed by following approximations.
2. Since every asymptotic surface is a surface trajectory, its intersection with the transverse section  $S$  will be an invariant curve  $C$ . Consider a curve  $C'$ :

$$y_1 = 0, \quad \frac{x_2}{x_1} = \frac{x_2^0}{x_1^0} + \frac{\sqrt{\mu}}{x_1^0} \sqrt{\frac{2}{N}([F_1] - \phi_4)}.$$

This will differ very little from the invariant curve  $C$  (up to the order of  $\mu$ ). Its consequent will also differ very little from the consequent of  $C$ , i.e.  $C$  itself. Thus the curve  $C'$  will differ very little from its own consequent (up to the order of  $\mu$ ).

3. The curve  $C'$  is a closed curve; the curve  $C$  from which it differs only slightly will thus be a quasi-closed curve such that the distance between the points of closure will be of the order of  $\mu$ . Thus the asymptotic surface cuts the surface  $y_1 = 0$  as a quasi-closed curve.

The distance of an arbitrary point  $P$  on the surface  $C_1 = -\Phi_4$  to its consequent  $P'$  will be of order  $\sqrt{\mu}$ . Likewise the distance of an arbitrary point on the curve  $C'$  to its consequent will also be of order  $\sqrt{\mu}$ .

Later it is shown how Poincaré used these results in [P1], and how they were invalidated by the discovery of the error.

### 5.8.2 Second approximation

The purpose of the second approximation, which only appeared in [P2], was to determine some arbitrary number of coefficients of the series (5.8.iv). Since Poincaré had originally believed the series to be convergent rather than asymptotic, there was no equivalent section in [P1].

Nevertheless, most of the section is in fact taken from *Note F* which Poincaré had added to [P1] because he wanted to include an analytic description of the asymptotic surfaces to complement his geometric one. *Note F*, therefore, contained what Poincaré then believed to be a description of the entire series. It is therefore not surprising to find that in editing *Note F* for inclusion as the second approximation

Poincaré made what he described in the note attached to [P1a] as an "important correction".

Poincaré began the second approximation in the same fashion as the first, but he then transformed the problem using Hamilton-Jacobi theory<sup>33</sup>.

Since the system of differential equations is an autonomous Hamiltonian system, the expression  $x_1 dy_1 + x_2 dy_2$  is an exact differential and so can be written

$$dS = x_1 dy_1 + x_2 dy_2$$

where  $S(y_1, y_2)$  is a solution of the Hamilton-Jacobi partial differential equation

$$F\left(\frac{\partial S}{\partial y_1}, \frac{\partial S}{\partial y_2}, y_1, y_2\right) = C,$$

which can be expanded as a series in  $\sqrt{\mu}$

$$S = S_0 + S_1 \sqrt{\mu} + S_2 \mu + S_3 \mu \sqrt{\mu} + \dots$$

where the coefficients  $S_i$  are functions of  $y_1$  and  $y_2$ . Moreover since

$$\frac{\partial S_k}{\partial y_1} = x_1^k, \quad \frac{\partial S_k}{\partial y_2} = x_2^k.$$

the problem of determining the coefficients of the asymptotic series amounts to determining partial derivatives of the coefficients in the series for  $S$  and hence to determining the coefficients in the series for  $S$ .

When  $C_1 > -\phi_4$  Poincaré proved that  $\frac{\partial S}{\partial y_1}$  and  $\frac{\partial S}{\partial y_2}$  could be determined as (divergent) trigonometric series in sines and cosines of multiples of  $y_1$  and  $y_2$ . However, his main concern was with the case when  $C_1 = -\phi_4$ , and the series represent the asymptotic solutions. When this occurs the expression  $[F_1] + C_1$  is never negative, and it only reaches zero when  $y_2 = \eta_3$ .

Choosing  $\eta_3$  as the origin for  $y_2$ , he put the expression into the form of a trigonometric series

$$[F_1] + C_1 = \sum A_m \cos my_2 + \sum B_m \sin my_2.$$

For  $y_2 = 0$  this function and its derivative vanish. Since the function is always positive, zero is, therefore, a minimum. As a result, the function

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<sup>33</sup> See Jacobi [1866].



$$\frac{[F_1] + C_1}{\sin^2 \frac{y_2}{2}}$$

can also be expanded in sines and cosines of multiples of  $y_2$ , and since it is a periodic function of  $y_2$  which neither vanishes nor becomes infinite, it is possible to write

$$\frac{\sin^2 \frac{y_2}{2}}{\sqrt{[F_1] + C_1}} = \sum A_m \cos m y_2 + \sum B_m \sin m y_2.$$

From this expression Poincaré showed that  $\frac{\partial S_p}{\partial y_1}$  and  $\frac{\partial S_p}{\partial y_2}$  are periodic functions of  $y_1$  and  $y_2$  where the period is  $2\pi$  with respect to  $y_1$  and  $4\pi$  with respect to  $y_2$ . Furthermore, after a detailed analysis he showed that it was also possible to ensure that the functions remained finite and so could be expanded as sines and cosines of multiples of  $y_1$  and  $\frac{y_2}{2}$ , where if  $p$  is even they will contain only the even multiples of  $\frac{y_2}{2}$  and if  $p$  is odd they will contain only the odd multiples of  $\frac{y_2}{2}$ .

He was then able to write the approximate equations for the asymptotic surfaces as the asymptotic series

$$x_1 = \sum_{p=0}^{p=n} \mu^{p/2} \frac{\partial S_p}{\partial y_1}, \quad x_2 = \sum_{p=0}^{p=n} \mu^{p/2} \frac{\partial S_p}{\partial y_2}.$$

These series, as he had previously proved, are divergent, but since they are asymptotic, if they are stopped at the  $n$ th term then the error is very small, providing, of course,  $\mu$  is very small.

The "important correction" that Poincaré made to *Note F* concerned these series. His earlier analysis in Chapters II and III of Part 1 in [P1], had led him to believe that the asymptotic surfaces could be represented by series in  $\sqrt{\mu}$ , which were convergent for arbitrary values of  $y_1$  and  $y_2$ , providing  $\mu$  was sufficiently small. As a result he thought that his calculations in *Note F* had shown that the asymptotic surfaces could be represented by the infinite series

$$x_1 = \sum_{p=0}^{p=\infty} \mu^{p/2} \frac{\partial S_p}{\partial y_1}, \quad x_2 = \sum_{p=0}^{p=\infty} \mu^{p/2} \frac{\partial S_p}{\partial y_2},$$

which he believed to be convergent.

### 5.8.3 Third approximation

In the final approximation Poincaré constructed the asymptotic surfaces exactly, or rather their intersection with the transverse section  $y_1 = 0$ . Here the differences between [P1] and [P2] are quite dramatic.

In [P1] Poincaré's objective was to determine the coefficients of the series defining the asymptotic surfaces. He began by quickly disposing of the two cases where the series were clearly divergent. In the first case, when  $C_1 > -\phi_4$ , he likened the series to those derived by Lindstedt: divergent but nonetheless useful since the divergence derives from large multipliers rather than small divisors and so it is relatively slow. In the second, when  $C_1 < -\phi_4$ , he gave an analysis which became the introduction to *Periodic solutions of the second class* in [P2] and which is discussed later.

He then moved on to the case where  $C_1 = -\phi_4$ , which he believed gave rise to convergent series defining the asymptotic surfaces. He therefore set about determining the coefficients of the series given the properties he thought he had previously established, namely that they were periodic with respect to  $y_1$ , that they were real and finite, and that they were convergent for sufficiently small values of  $\mu$ . This involved showing it was possible to choose the series of constants derived in the section on the *Equation of Asymptotic Surfaces* so that the series were convergent. That being done, he returned to the geometry in order to give an actual description of the asymptotic surfaces.

To clarify the description, he used FIG. 5.8.iii<sup>34</sup>. The plain lines which identify the two curves  $AO'B'$  and  $A'O'B$  represent the two asymptotic surfaces which cut the surface  $y_1 = 0$ , and the dashed line represents the curve  $y_1 = y_2 = 0$ . The dotted and dashed line, which is a closed curve with a double point at  $O$ , represents the curves with equation

$$y_1 = 0, \quad \frac{x_2}{x_1} = \frac{x_2^0}{x_1^0} + \frac{\sqrt{\mu}}{x_1^0} \sqrt{\frac{2}{N}([F_1] - \phi_4)}.$$

which arise when the surfaces which differ very little from the asymptotic surfaces cut the surface  $y_1 = 0$ . The generating (unstable) periodic solution is represented by a

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<sup>34</sup> Poincaré [P2, 438].

closed trajectory cutting the surface  $y_1 = 0$  at the point  $O'$ , and the distance  $OO'$  is of order  $\mu$ .

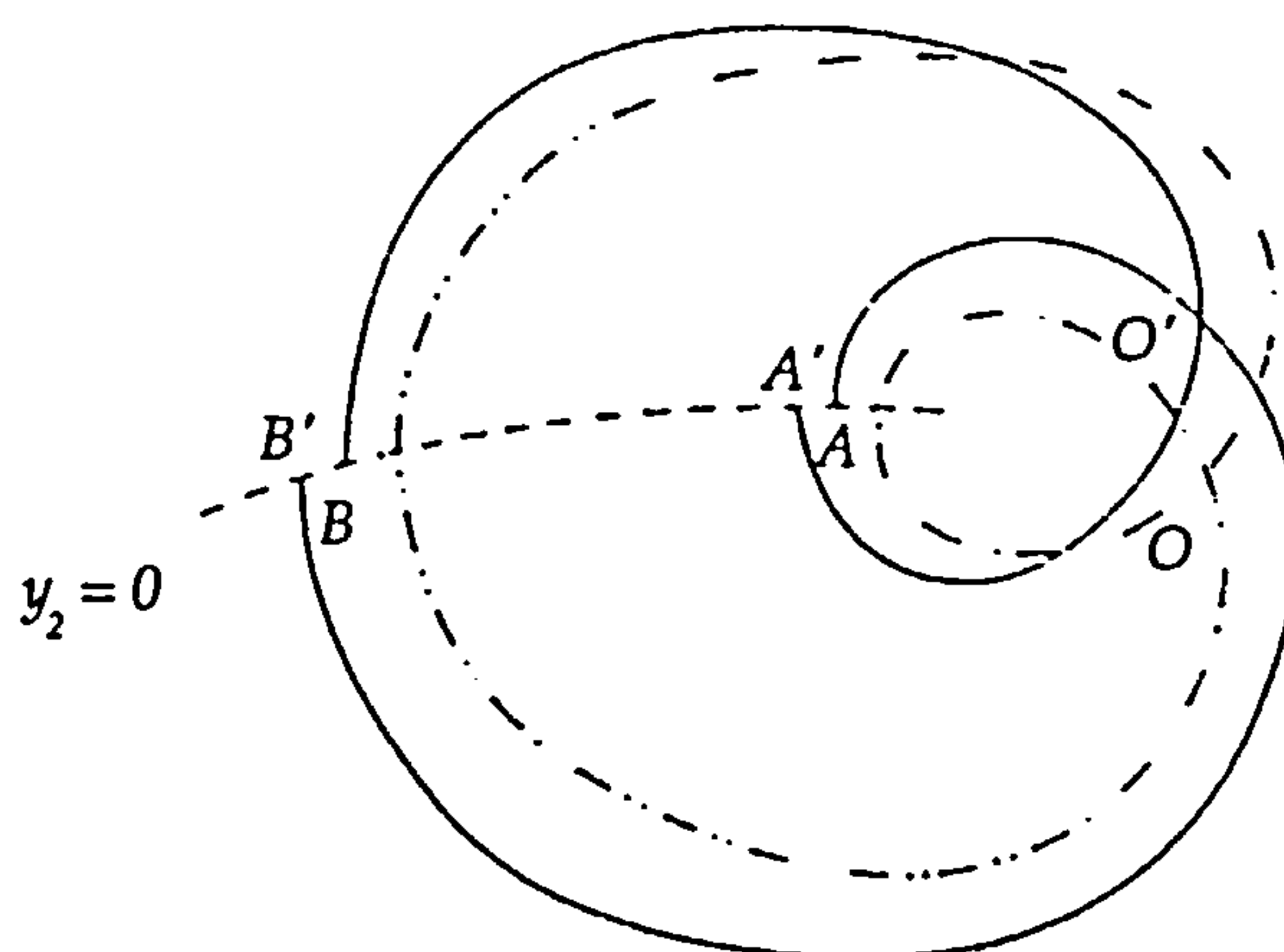


FIG. 5.8.iii

Poincaré used his results from the end of the first approximation to infer, firstly, that the curve  $BO'B'$  is quasi-closed, the distance between the points of closure being infinitely small of order  $\mu$ , and, secondly, that the distance of the point  $B$  to its consequent is of the order of  $\sqrt{\mu}$ . Appealing to the (invalid) Corollary to Theorem III he concluded (erroneously) that the curve  $BO'B'$  was rigorously closed, i.e. that the points  $B$  and  $B'$  were coincident, and consequently that the asymptotic surfaces were closed. Furthermore, inherent in this conclusion was the implication of stability.

He ended by adding that a similar argument could be used to establish that the asymptotic surfaces corresponding to the unstable periodic solution  $C_1 = -\Phi_2$  were also closed.

Thus Poincaré believed he had proved, given certain initial conditions, i.e. sufficiently small values of  $\mu$ , that relative to a given unstable periodic solution, there was a set of asymptotic solutions which could be considered stable in the sense that they remained confined to a given region of space, and, moreover, that this set of solutions was well behaved and their behaviour could be completely understood. His analysis in [P2] led to a very different conclusion.

In [P2] Poincaré used the same diagram as in [P1] (FIG 5.8.iii), with the same labelling, except that in [P2] the dotted and dashed line represented the curves with equation

$$y_1 = 0, \quad x_1 = s_1^p(0, y_2), \quad x_2 = s_2^p(0, y_2).$$

As before,  $s_i^p$  were the sums of the first  $p$  terms of the series  $s_i(y_1, y_2)$ , and were periodic functions of period  $2\pi$  with respect to  $y_1$  and  $4\pi$  with respect to  $y_2$ .

The first question Poincaré considered was whether the curves  $AO'B'$  and  $A'O'B$  were closed. It was clear that they would be if the series  $s_i$  were convergent. For in this case, the plain curves would differ as little as required from the dotted and dashed curves, since the distance from a point on the former to a point on the latter would tend to zero as  $p$  increased indefinitely. But he had already proved that the series were divergent. Nevertheless, the question still remained. Was it possible for the curves  $AO'B'$  and  $A'O'B$  to be closed even though the series were divergent?

Poincaré tackled the question by looking at the specific example of a simple pendulum weakly coupled to a linear oscillator. In this case the Hamiltonian is given by

$$-F = p + q^2 - 2\mu \sin^2 \frac{y}{2} - \mu \varepsilon \cos x \phi(y)$$

where  $\mu$  and  $\varepsilon$  are two very small parameters, and  $\phi(y)$  is a periodic function of  $y$  of period  $2\pi$ . The Hamiltonian equations are

$$\begin{aligned} \frac{dp}{dt} &= \frac{\partial F}{\partial x} = -\varepsilon \sin x \phi(y), & \frac{dq}{dt} &= \frac{\partial F}{\partial y} = -\mu \sin y + \mu \varepsilon \cos x \phi'(y), \\ \frac{dx}{dt} &= -\frac{\partial F}{\partial p} = 1, & \frac{dy}{dt} &= -\frac{\partial F}{\partial q} = 2q, \end{aligned}$$

where the variables  $p, q, x$  and  $y$  correspond to the variables  $x_1, x_2, y_1$  and  $y_2$  respectively in equations (5.7.i).

If  $\varepsilon = 0$ , then the equations have a periodic solution

$$x = t, \quad p = 0, \quad q = 0, \quad y = 0,$$

the two non-zero characteristic exponents are equal to  $\pm \sqrt{2\mu}$ , and the equations of the two asymptotic surfaces are

$$p = \frac{\partial S_0}{\partial x}, \quad q = \frac{\partial S_0}{\partial y}, \quad S_0 = \pm 2\sqrt{2\mu} \cos \frac{y}{2};$$

from which

$$p = 0, \quad q = \pm \sqrt{2\mu} \sin \frac{y}{2},$$



and hence the surfaces enclose a region which has a width which is of the order of  $\sqrt{\mu}$ .

Since there are non-zero characteristic exponents for  $\varepsilon = 0$ , there are also periodic solutions for small values of  $\varepsilon$ . The equations of the corresponding asymptotic surfaces are given by

$$p = \frac{\partial S}{\partial x}, \quad q = \frac{\partial S}{\partial y},$$

where  $S$  is a function of  $x$  and  $y$ , satisfying the equation

$$\frac{\partial S}{\partial x} + \left(\frac{\partial S}{\partial y}\right)^2 = 2\mu \sin^2 \frac{y}{2} + \mu \varepsilon \cos x \phi(y).$$

Moreover, the existence of non-zero characteristic exponents for  $\varepsilon = 0$  implies that  $p$  and  $q$  and, therefore,  $S$ , can be expanded as series in  $\varepsilon$ . So  $S$  can be put in the form

$$S = S_0 + \varepsilon S_1 + \varepsilon^2 S_2 + \dots$$

$S_0$  has already been found and equating powers of  $\varepsilon$  shows that  $S_1$  must satisfy the equation<sup>35</sup>

$$\frac{\partial S_1}{\partial x} + 2\sqrt{2\mu} \sin \frac{y}{2} \frac{\partial S_1}{\partial y} = \mu \cos x \phi(y).$$

To determine  $S_1$ , Poincaré defined a new function  $\Sigma$  to be the function which satisfies

$$\frac{\partial \Sigma}{\partial x} + 2\sqrt{2\mu} \sin \frac{y}{2} \frac{\partial \Sigma}{\partial y} = \mu e^{ix} \phi(y), \quad i = \sqrt{-1}.$$

so that  $S_1$  is the real part of  $\Sigma$ . This equation can then be satisfied by  $\Sigma = e^{ix} \Psi(y)$  which gives a linear equation in  $\Psi$

$$i\Psi + 2\sqrt{2\mu} \sin \frac{y}{2} \frac{d\Psi}{dy} = \mu \phi(y).$$

If  $\phi(y) = 0$ , then<sup>36</sup>

$$\Psi = \left(\tan \frac{y}{4}\right)^\alpha, \quad \alpha = -i \frac{1}{\sqrt{2\mu}}$$

and if  $\phi(y)$  is arbitrary, the integral can be written

<sup>35</sup> Poincaré omitted the factor 2 in the second term of this equation, although he included it in [P1a] and crossed it out.

<sup>36</sup> Poincaré put  $\alpha = -i \sqrt{\frac{2}{\mu}}$ .

$$\Psi = \left( \tan \frac{y}{4} \right)^\alpha \int \sqrt{\frac{\mu}{8}} \phi(y) \left( \sin \frac{y}{2} \right)^{-1} \left( \tan \frac{y}{4} \right)^{-\alpha} dy,$$

where  $\Psi$  can be expanded in integer powers of  $y$  for small values of  $y$ . If  $\phi(0) = 0$ , then the integral is also equal to zero and hence its limits are 0 and  $y$ .

If the curves  $AO'B'$  and  $A'O'B$  are closed<sup>37</sup>, then the function  $S$  and its derivatives will be finite for all values of  $y$  as well as being periodic of period  $4\pi$  with respect to  $y$  (cf the functions  $s_1^p$  and  $s_2^p$ ). Since this must be true for any given value of  $\varepsilon$ , it must also be true for  $S_1$ , and hence for  $\Psi$ .

Thus, for values of  $y$  close to  $2\pi$ ,  $\Psi$  should be expansible in integer powers of  $y - 2\pi$ . But since  $\left( \tan \frac{y}{4} \right)^\alpha$  cannot be expanded in this way, the condition can only hold if the integral

$$J = \int_0^{2\pi} \sqrt{\frac{\mu}{8}} \phi(y) \left( \sin \frac{y}{2} \right)^{-1} \left( \tan \frac{y}{4} \right)^{-\alpha} dy$$

is zero. However, evaluating  $J$ , using  $\phi(y) = \sin y$ , gives

$$J = -2\pi i \operatorname{sech} \left( \frac{\pi}{2\sqrt{2\mu}} \right)$$

which is clearly not equal to zero and so the curves  $AO'B'$  and  $A'O'B$  cannot be closed.

However, the lack of closure still left open the possibility that the extended curves  $O'B$  and  $O'B'$  could intersect. For if this should occur, any trajectory which passes through the point of intersection would simultaneously belong to both sides of the asymptotic surface and therefore would be a *doubly asymptotic* trajectory.

In other words, if  $C$  is the closed trajectory which passes through the point  $O'$  and represents the periodic solution, then if the trajectory is doubly asymptotic, it would begin by being very close to  $C$  when  $t$  is very large and negative, it would then asymptotically move away to deviate greatly from  $C$ , before asymptotically reapproaching  $C$  when  $t$  is very large and positive.

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<sup>37</sup> Poincaré wrote  $BO'B'$  and  $AO'A'$ .

To prove the existence of doubly asymptotic trajectories Poincaré needed to show that the system fulfilled the conditions of Theorem III in Chapter III of Part 1.

To do this he established that none of the curves  $O'B$ ,  $O'B'$ ,  $O'A$  and  $O'A'$ , were self-intersecting, i.e. that none of them have a double point; that the curvature of the curves  $O'B$  and  $O'B'$  was finite, i.e. that it does not increase indefinitely as  $\mu$  tends to zero; and that the distances  $BB'$ ,  $B_1B_1'$  together with the ratios  $\frac{BB'}{BB_1}$  and  $\frac{BB'}{B'B_1'}$  tend to zero as  $\mu$  tends to zero, where  $B_1$  is the consequent of  $B$  and  $B_1'$  is the consequent of  $B'$ . Furthermore, since the system is in Hamiltonian form, it also possesses a positive invariant integral and hence all the conditions of Theorem III are satisfied.

Therefore the arcs  $BB_1$  and  $B'B_1'$  intersect each other, i.e. the extended curve  $O'B'$  intersects the extended curve  $O'B$ , and through the point of intersection passes a doubly asymptotic trajectory <sup>38</sup>.

Poincaré had constructed the figure so that the points  $B$  and  $B'$  lie on the curve

$$y_1 = y_2 = 0,$$

and since the origin of  $y_2$  is arbitrary he supposed that at the intersection of the curves  $O'B$  and  $O'B'$ ,  $y_2 = 0$ . In this case the points  $B$  and  $B'$  are coincident and so are their consequents  $B_1$  and  $B_1'$ . Thus the two arcs  $BB_1$  and  $B'B_1'$  have the same end points. But by Theorem III (in which the area limited by the two arcs is not convex), the two arcs must intersect again at a different point  $N$ , and thus he proved that there are at least two doubly asymptotic trajectories, one passing through the point  $B$  and one passing through the point  $N$ .

He then gave the following proof that there are in fact an infinite number of doubly asymptotic trajectories.

If the points  $B$  and  $B'$  are always coincident and  $BMN$  is the part of the curve  $O'B$  between  $B$  and  $N$ ; and  $BPN$  is the part of the curve  $O'B'$  between the point  $B = B'$  and the point  $N$ , then these two arcs will limit a certain area  $\alpha$ . Furthermore, if the system is one in which the conditions of the recurrence theorem are satisfied, such as the restricted three body problem, then there are trajectories which cross this area

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<sup>38</sup> In [MN III, 384] Poincaré called this type of solution *homoclinic*. See 7.2.3.

$\alpha$  infinitely often. Hence among the consequents of the area  $\alpha$ , there will be an infinite number which will have a part in common with  $\alpha$ .

The closed curve  $BMNPB$  which limits the area  $\alpha$  has an infinite number of consequents. The arc  $BMN$  cannot intersect any of its own consequents; for the arc  $BMN$  and its consequents belong to the curve  $O'B$  and the curve  $O'B$  is not self-intersecting. Similarly, the arc  $BPN$  does not intersect any of its own consequents. Therefore either the arc  $BMN$  intersects with one of the consequents of  $BPN$ , or the arc  $BPN$  intersects with one of the consequents of  $BMN$  (as in the case under consideration). In either case the curve  $O'B$  or its extension will intersect the curve  $O'B'$  or its extension.

Thus these two curves intersect each other at an infinite number of points, and an infinite number of these points of intersection will be found either on the arc  $BMN$  or on the arc  $BPN$ . These points of intersection are all points of intersection of the curve  $O'B'$  or its extension with the curve  $O'B$  or its extension, and, since through each of these points of intersection passes a doubly asymptotic trajectory, there are an infinite number of doubly asymptotic trajectories. Similarly, the asymptotic surface which cuts the surface  $y_1 = 0$  along the curve  $O'A$  also contains an infinite number of doubly asymptotic trajectories.

This is arguably the first mathematical description of chaotic motion within a dynamical system. Significantly, Poincaré made no attempt to draw a diagram of the behaviour he had discovered, but, rather surprisingly, neither did he in any way emphasise the complexity of the situation. Nevertheless, there is no doubt that he was profoundly disturbed by his discovery as he revealed in a letter to Mittag-Leffler:

*"I have written this morning to M. Phragmén to tell him of an error I have made and doubtless he has shown you my letter. But the consequences of this error are more serious than I first thought. It is not true that the asymptotic surfaces are closed, at least in the sense which I originally intended. What is true is that if both sides of this surface are considered (which I still believe are connected to each other) they intersect along an infinite number of asymptotic trajectories.\**

*I had thought that all these asymptotic curves having moved away from a closed curve representing a periodic solution, would then asymptotically*



*approach the same closed curve. What is true, is that there are an infinity which enjoy this property.*

*I will not conceal from you the distress this discovery has caused me. In the first place, I do not know if you will still think that the results which remain, namely the existence of periodic solutions, the asymptotic solutions, the theory of characteristic exponents, the non-existence of uniform integrals, and the divergence of Lindstedt's series, deserve the great reward you have given them.*

*On the other hand, many changes have become necessary and I do not know if you can begin to print the memoir; I have telegraphed Phragmén.*

*In any case, I can do no more than to confess my confusion to a friend as loyal as you. I will write to you at length when I can see things more clearly. ..."*

*\*and moreover that their distance becomes infinitely small of order higher than  $\mu^p$  however great the order of  $p$ ."<sup>39</sup>.*

Perhaps a further indication of Poincaré's concern and confusion at his discovery of the strange behaviour of these solutions can be detected in the introduction to [P2]. Of all the results in the memoir this was clearly the most extraordinary, and yet it is not amongst those he singled out for special mention. Possibly this was because he was complying with Mittag-Leffler's request not to give details of the error which undoubtedly he would have felt obliged to do had he drawn attention to the complexity of the doubly asymptotic solutions. Or perhaps it was simply because he had had so little time in which to assess the further implications of his discovery that he felt it wiser not to emphasise it.

## 5.9 Further results

The penultimate chapter of [P2] was devoted to three separate topics: periodic solutions of the second class, the divergence of Lindstedt's series and the non-existence of any new integrals for the restricted three body problem.

Most of the chapter is derived from [P1] enhanced by some additions. The first section on periodic solutions of the second class opens with part of *The Exact*

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<sup>39</sup> Poincaré to Mittag-Leffler, postmarked 1.12.1889, No. 54a, M-L I.

*Construction of Asymptotic Surfaces*, continues with most of the section with the same name from [P1] and concludes with some new material. The section on the divergence of Lindstedt's series is essentially *Note A*, while the final section on the non-existence of uniform integrals is derived from *Note G*.

The last two sections contained the results to which Poincaré had drawn particular attention in the introduction to [P2] and which quickly came to be amongst the best known in the memoir. It is therefore quite surprising to find that originally he did not consider it necessary to include the full proofs for these results, and that they only appeared in the *Notes* as a result of prompting from Mittag-Leffler.

### 5.9.1 Periodic solutions of the second class

In Poincaré's investigation of asymptotic solutions (5.8.1) he began by showing how the periodic solutions could be represented by curves on the transverse section defined by the surface  $y_1 = 0$ , the nature of the curves depending on the value of a particular constant  $C_1$ , and his discussion then centred on the unstable periodic solution corresponding to  $C_1 = \phi_4$ . In what follows he considered the situation when the value of this constant was less than  $-\phi_4$ , i.e. when the coefficient  $x_2^1 = \sqrt{\frac{2}{N}([F_1] + C_1)}$  in the series for  $x_2$  is not always real.

Poincaré found that in this case regions of motion did exist and, furthermore, that these regions contained periodic solutions which made more than one revolution around the origin before closing up. In other words these periodic solutions were of a different type to those which he had previously found and to distinguish them he called them periodic solutions of the second class. They can be described more formally by saying that if a system has for small values of  $\mu$  a periodic solution of period  $T$ , then the periodic solutions of the second class are those periodic solutions which are close to the original periodic solution but whose periods are integral multiples of  $T$ .

Since  $[F_1]$  is a function of  $y_2$ , the behaviour of the system for different values of the constant  $C_1$  will depend on  $y_2$ . If for a chosen value of  $C_1$ ,  $x_2^1$  is real as the value of  $y_2$  varies between, say  $\eta_5$  and  $\eta_6$ , then since  $x_i^k$  are determined by a recurrence relation which is dependent on  $x_2^1$ ,  $x_i^k$  can be determined for all values  $\eta_j$  of  $y_2$  in this range. The existence of the square root means that  $x_i^k$  has two sets of values equal in magnitude but opposite in sign. If  $x_{0,i}^k$  are the functions of  $y_2$  when the square root is

positive and  $x_{1,i}^k$  are the functions of  $y_2$  when it is negative, then the latter will be the analytic continuation of the former.

What Poincaré now needed to establish was how the behaviour of the system was affected by a change in the constant  $C_1$ . Were there regions in which the value of  $y_2$  would remain finite? Since his method of using a transverse section to understand the evolution of the system reduced the dimension of the system by one, it was possible that a small change in the constant could induce some strange behaviour in  $y_2$  which manifest itself in the other dimension and which would not be captured on the transverse section for individual choices for the constant.

He therefore looked at the change in values of  $y_2$  for a very small change in the constant  $C_1$ . He replaced  $C_1$  by a new constant  $C_1'$  very close to  $C_1$ , so that  $\sqrt{\frac{2}{N}([F_1] + C_1')}$  was real whenever  $y_2$  was between  $\eta_7$  and a certain value  $\eta_8$  very close to  $\eta_6$ . Again he could determine the functions  $x_i^k$  for all values of  $y_2$  in this range, where  $x_{2,i}^k$  are the functions of  $y_2$  when the square root is positive and  $x_{3,i}^k$  the functions of  $y_2$  when it is negative.

He then constructed the four branches of the curve:

$$y_1 = 0, \quad x_1 = \phi_{k,1}(y_2), \quad x_2 = \phi_{k,2}(y_2), \quad (k = 0, 1: \eta_5 \leq y_2 \leq \eta_7; \quad k = 2, 3: \eta_7 \leq y_2 \leq \eta_8)$$

where

$$\phi_{p,q}(y_2) = x_{p,q}^0 + x_{p,q}^1 \sqrt{\mu} + \dots + x_{p,q}^k \mu^{k/2},$$

the first and second branches of the curve corresponding to the constant  $C_1$ , and meeting at a tangent to the curve  $y_2 = \eta_7$ , and the third and fourth branches of the curve corresponding to the constant  $C_1'$  and meeting at a tangent to the curve  $y_2 = \eta_8$ , as shown in FIG. 5.9.<sup>40</sup>

Poincaré had begun by regarding  $C_1$  as given and  $C_1'$  as close in value to  $C_1$  but nevertheless arbitrary. He now determined  $C_1'$  by imposing the condition that the first and third curves meet, i.e. the points  $B$  and  $B'$  are coincident. Then appealing to an earlier theorem (Theorem III, 5.5.4), he derived the result that the distance  $DD'$  between the second and the fourth curves was infinitely small of the order  $\mu^{(k+1)/2}$ . Hence he could conclude that for a limited period of time there do exist regions in which the values of  $y_2$  remain finite. These are the regions known as the

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<sup>40</sup> Poincaré [P2, 447].

regions of libration. The time constraint resulted from the fact that the series involved were asymptotic rather than convergent.

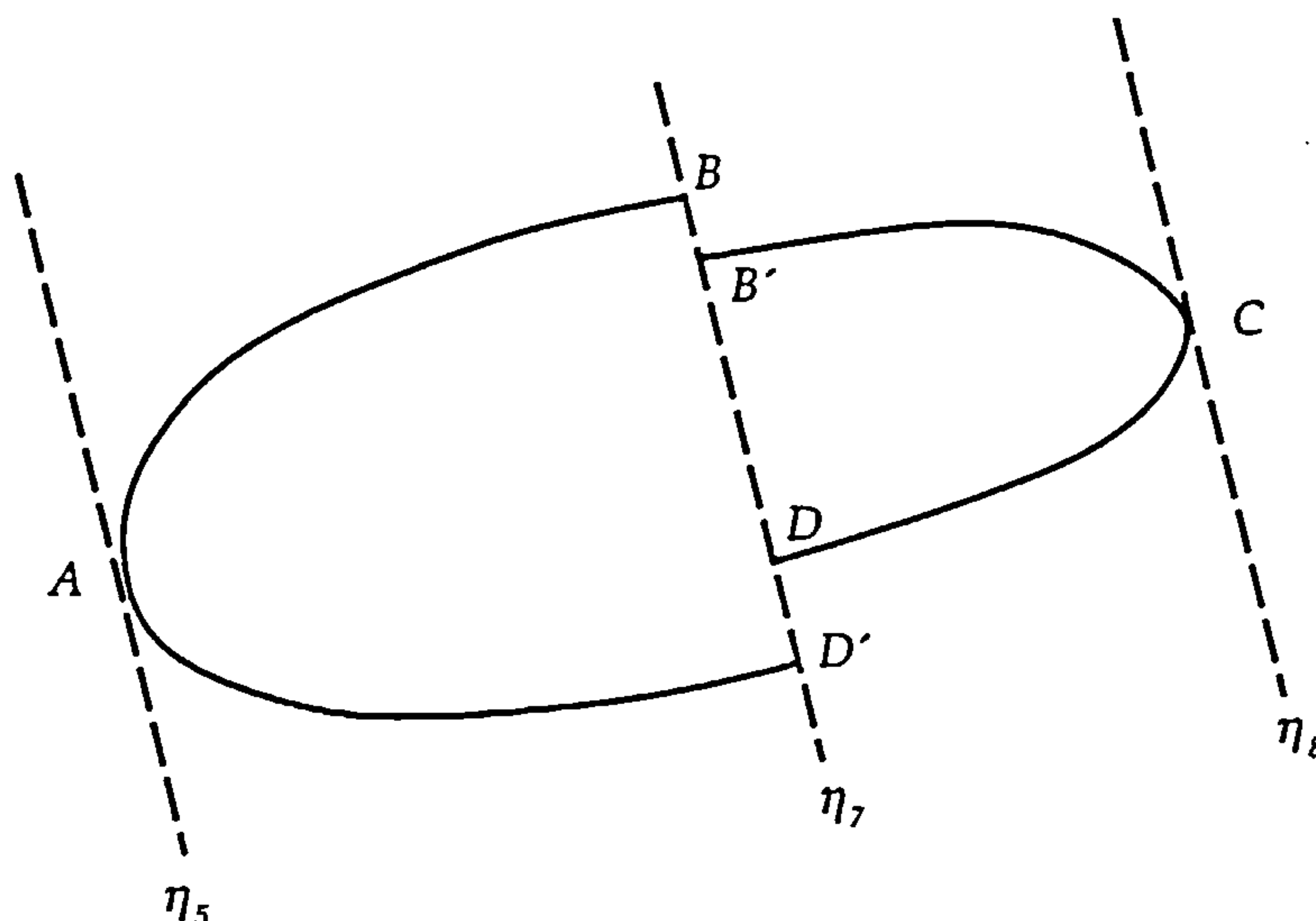


FIG. 5.9.i.

Poincaré next made a precise formulation of these regions of libration in order to ascertain whether they contained periodic solutions and, if so, what form these periodic solutions would take.

The equations

$$x_1 = x_1^0 + \mu x_1^2, \quad x_2 = x_2^0 + \sqrt{\mu} \sqrt{\frac{2}{N}([F_1] + C_1)} + \mu u_2^2 \quad (5.9.i)$$

define, up to the order of  $\mu$ , the surfaces just constructed (see FIG. 5.9.i) and so approximately satisfy equations (5.8.iii), where  $u_2^2$  is a finite function of  $y_1$  and  $y_2$  which only differs from  $x_2^2$  by a function of  $y_2$ , so that

$$\frac{\partial(u_2^2)}{\partial y_1} = \frac{\partial(x_2^2)}{\partial y_1}.$$

Poincaré then modified the form of  $F$  in the Hamiltonian equations so that the equations (5.9.i) *exactly* satisfied equations (5.8.iii). The new form of the equations can then be integrated exactly and following the same argument as given previously shows that there exist an infinite number of closed surface trajectories defined by the equation



$$\frac{x_2}{x_1} = \frac{x_2^0 + \sqrt{\mu} \sqrt{\frac{2}{N}([F_1] + C_1) + \mu u_2^2}}{x_1^0 + \mu x_1^2} \quad (5.9.ii)$$

which is of the same form as equation (5.8.vi).

Thus the same hypotheses can be made about the function  $[F_1]$  in equation (5.9.ii) as about  $[F_1]$  in equation (5.8.vi), and the two surfaces of (5.9.ii) which correspond to the values  $-\phi_2$  and  $-\phi_4$  of  $C_1$  are therefore closed with a double curve. The space can then be divided into three regions: interior, exterior and between the two sheets of the surface, the last being the region of libration.

Since the closed surface corresponding to a given value of  $C_1$  (which must be  $< -\phi_4$ ) has the same connectivity as a torus, the existence of periodic solutions depends on the behaviour of the two angular variables defining the surface. By investigating the behaviour of these angular variables Poincaré showed that there were an infinite number of values for  $C_1$  for which periodic solutions exist.

Then by deriving an equation in which  $C_1$  was defined as a continuous function of  $\mu$ , he showed that it was possible to make  $\mu$  so small that the equation no longer had a root. This means that although there exist an infinite number of closed trajectories which represent periodic solutions, as  $\mu$  decreases these solutions will disappear one after the other. In other words, if along a closed trajectory,  $\mu$  decreases continuously, then the trajectory deforms continuously and eventually disappears at a certain value of  $\mu$ . As a result, when  $\mu = 0$  all the periodic solutions in the region of libration will have disappeared. This is in contrast to the behaviour of the periodic solutions of the first class (those which only have one revolution around the primary) which continue to exist for  $\mu = 0$ .

Since he had proved that in the neighbourhood of a periodic solution (stable or unstable) there existed an infinite number of other periodic solutions, he conjectured the possibility that every region of the space, however small, was crossed by an infinite number of periodic solutions, in other words that the periodic solutions were everywhere dense. Although his work was insufficient to prove it, he believed it to be extremely likely, his belief having been strengthened by Cantor's recent work on set theory which had shown that it was possible for a set to be perfect without being connected<sup>41</sup>.

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<sup>41</sup> This point is taken up again in [MN I, 82]. See 7.2.2.

All the above is contained in both [P1] and [P2]. There is, however, an important addition to the section which only appears in [P2]. In both versions in his proof of the existence of periodic solutions of the second class, Poincaré had shown that periodic solutions exist for small values of a certain parameter  $\varepsilon$ , but in [P1] he had thought that if  $\varepsilon = \mu\sqrt{\mu}$  it automatically *followed* that periodic solutions also existed for small values of the mass parameter  $\mu$ . In the revision he realised that this result needed to be rigorously established.

He therefore returned to the Hamiltonian equations (5.7.i) and considered a *stable* periodic solution

$$x_1 = \phi_1(y_1), \quad y_2 = \phi_2(y_1),$$

of period  $2\pi$ .

This periodic solution can be approximately represented on FIG. 5.8.ii by the isolated closed curve of the surface  $C_1 = -\phi_3$ , and has two characteristic exponents  $\pm \alpha$ , the square of which is real and negative. If

$$x_1 = \phi_1(y_1) + \xi_1, \quad y_2 = \phi_2(y_1) + \xi_2$$

is a nearby solution and  $\beta_1$  and  $\beta_2$  are the initial values of  $\xi_1$  and  $\xi_2$  for  $y_1 = 0$ , and  $\beta_1 + \Psi_1$  and  $\beta_2 + \Psi_2$  are the values of  $\xi_1$  and  $\xi_2$  for  $y_1 = 2k\pi$  ( $k$  an integer), then the solution will be periodic of period  $2\pi$  if

$$\Psi_1 = \Psi_2 = 0, \tag{5.9.iii}$$

where  $\Psi_1$  and  $\Psi_2$  can be expanded in powers of  $\beta_1$  and  $\beta_2$  which depend on  $\mu$ .

If  $\beta_1$ ,  $\beta_2$  and  $\mu$  are regarded as the coordinates of a point in space, then the equations (5.9.iii) represent a curve, each point of which corresponds to a periodic solution. If  $\xi_1 = \xi_2 = 0$ , then  $\beta_1 = \beta_2 = 0$ , which implies  $\Psi_1 = \Psi_2 = 0$  and a periodic solution of period  $2\pi$  is obtained which can also be regarded as being periodic with period  $2k\pi$ .

Thus the curve (5.9.iii) consists of the entire  $\mu$  axis. Poincaré proved that if  $k\alpha$  is a multiple of  $2i\pi$  when  $\mu = \mu_0$ , then there exists another branch of the curve (5.9.iii) which passes through the point

$$\mu = \mu_0, \quad \beta_1 = 0, \quad \beta_2 = 0,$$

and so, from the previous theory, for values of  $\mu$  close to  $\mu_0$ , there exist periodic solutions other than  $\xi_1 = \xi_2 = 0$ .

The proof, which depended on the theory of invariant integrals, involved expanding  $\Psi_1$  and  $\Psi_2$  in terms of  $\beta_1$ ,  $\beta_2$  and  $(\mu - \mu_0)$ , and then showing that  $\beta_1$  and  $\beta_2$  could themselves be expanded in positive fractional powers of  $(\mu - \mu_0)$ . Thus there exists a series in  $(\mu - \mu_0)$  which is not identically zero and which satisfies equations (5.9.iii). Consequently there exists a system of periodic solutions in which the expressions for the coordinates can be expanded in positive fractional powers of  $(\mu - \mu_0)$ , and the period of which is a multiple of the generating periodic solution. When  $\mu = \mu_0$  the solution is simply the original periodic solution.

### 5.9.2 Divergence of Lindstedt's series

In [P1] Poincaré had included a section entitled *Negative Results* in which he had incorporated the result that no analytic single-valued integral of the restricted three body problem exists apart from the Jacobian integral. He claimed that a consequence of this result was that the series generally used in celestial mechanics, and in particular the series derived by Lindstedt (see 2.3.9), were, contrary to what had been previously thought, divergent. But he gave no evidence of why this assertion should be true. That it was not immediately obvious is plainly expressed by Mittag-Leffler who told Poincaré that he had spent a month with Weierstrass trying to work it out but without success<sup>42</sup>. Poincaré responded with *Note A* in which he gave two forms of a proof, but, according to Mittag-Leffler, Weierstrass was still not satisfied. Poincaré had proved that there were circumstances under which Lindstedt's series were not convergent but he had ignored the wider question of whether convergent trigonometric series solutions could ever be found. Furthermore, since Dirichlet's original remarks had led Weierstrass to believe that the such solutions did exist, he was particularly anxious to have this important point clarified<sup>43</sup>. Mittag-Leffler again asked Poincaré for a proof<sup>44</sup>. This time Poincaré replied that he thought that he had covered the point in *Note A* although he admitted that he could not be sure as he had mislaid his own copy of it<sup>45</sup>! Mittag-Leffler did not pursue the issue, in spite of further requests from Weierstrass to do so, and, significantly, Poincaré left Weierstrass' question unresolved. In [P2] Poincaré only gave the first form of the proof since the second depended on the proof

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<sup>42</sup> Mittag-Leffler to Poincaré, 15.11.1888, M-L I.

<sup>43</sup> See Appendix 2, Question 1.

<sup>44</sup> Mittag-Leffler to Poincaré, 23.2.1889, M-L I.

<sup>45</sup> Poincaré to Mittag-Leffler, 1.3.1889, No. 49, M-L I.

of the non-existence of any new integrals which he had placed in the following section.

Poincaré first showed how Lindstedt's method for approximately integrating differential equations of the form

$$\frac{d^2x}{dt^2} + n^2x = \alpha\Phi(x, t)$$

by deriving a formal trigonometric series solution without any secular terms, could be adapted to accommodate the system of Hamiltonian equations (5.7.i). He considered the system with Hamiltonian

$$F = F_0 + \mu F_1$$

where  $F_0$  is independent of  $y_1$  and  $y_2$ , and  $F_1$  is a trigonometric series of sines and cosines of multiples of  $y_1$  and  $y_2$  with coefficients which are analytic functions of  $x_1$  and  $x_2$ . The  $x_i$  and  $y_i$  are then regarded as functions of two variables  $w_i = \lambda_i t + \varpi_i$ , (as opposed to simply functions of the time) where the frequencies  $\lambda_i$  are to be determined,  $\varpi_i$  are constants of integration and

$$x_i = x_i^0 + \mu x_i^1 + \mu^2 x_i^2 + \dots \quad (i = 1, 2) \quad (5.9.iv)$$

$$y_i = w_i + \mu y_i^1 + \mu^2 y_i^2 + \dots$$

$$\lambda_i = \lambda_i^0 + \mu \lambda_i^1 + \mu^2 \lambda_i^2 + \dots$$

The coefficients  $\lambda_i^k$  are constants, and the coefficients  $x_i^k$  and  $y_i^k$  are trigonometric series in sines and cosines of multiples of  $w_1$  and  $w_2$ . Poincaré then sketched a method which, in line with Lindstedt's result, demonstrated that it was possible to determine the  $2q + 2$  constants  $\lambda_i^0, \dots, \lambda_i^q$ , so that the  $4q$  trigonometric series  $x_i^0, \dots, x_i^q, y_i^0, \dots, y_i^q$ , however large  $q$ , satisfy the Hamiltonian equations up to the order of  $\mu^{q+1}$ .

The frequencies  $\lambda_1$  and  $\lambda_2$  can be expanded in powers of  $\mu$ ,  $\omega_1$  and  $\omega_2$ , and the solutions corresponding to the values of  $\omega_1$  and  $\omega_2$  for which the frequency ratio is commensurable are therefore periodic. Corresponding to each of these periodic solutions are characteristic exponents each of which can be calculated if the general solution of the equations is known. Thus if the series are uniformly convergent, and consequently give the general solution of the equations, then the characteristic exponents of the periodic solutions can be calculated.

When Poincaré calculated the characteristic exponents under the assumption that analytic solutions do exist, he found that they were all zero. But when he put this



result into the eigenvalue equation which determines the characteristic exponents in the restricted three body problem, he arrived at a contradiction. He therefore concluded that his original assumption (that there is a general solution given by uniformly convergent series) must be false, and hence:

"... in the restricted three body problem and consequently in the general case, Lindstedt's series are not uniformly convergent for all the values of the arbitrary constants of integration which they contain." [P2, 470].

But, as Weierstrass had observed, Poincaré's discussion was incomplete. He gave no consideration to the circumstances under which convergence could occur, with the result that he gave no indication of what proportion of the series were divergent. However, Poincaré did not abandon the question. In the second volume of the *Méthodes Nouvelles* he reworked it in greater depth and generality and his conclusions are described in 7.2.3.

### 5.9.3 Non-existence of single-valued integrals

The final section in the chapter contained what soon became one of the best known results in the memoir: the proof of the non-existence of any new transcendental integral for the restricted three body problem. Only two years earlier Heinrich Bruns, a former student of Weierstrass, had proved the non-existence of any new algebraic integral for the general three body problem [1887]<sup>46</sup>, and so Poincaré's result was an important complement to that of Bruns'.

Poincaré had given an outline of a proof of this result in the *Negative Results* in [P1], and in response to yet another of Mittag-Leffler's requests for details, he had also produced an extension to the proof which he had intended to appear as *Note G*<sup>47</sup>. In [P2], having reshaped his original thoughts, he completely rewrote the proof.

More specifically, Poincaré proved that if the differential equations (5.7.i) possess the solution  $F = \text{constant}$ , where  $F$  is a single-valued analytic function of  $x_i, y_i$  and  $\mu$ , which can be expanded in powers of  $\mu$  and is periodic of period  $2\pi$  with respect to  $y_i$ , then the equations possess no other solution of the same form.

Suppose  $\Phi = \text{constant}$  is another such solution. If  $x_1 = \phi_1, x_2 = \phi_2, y_1 = \phi_3, y_2 = \phi_4$ , is a periodic solution of the differential equations such that  $x_1 = \phi_1 + \beta_1, \dots, y_2 = \phi_4 + \beta_4$

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<sup>46</sup> For a clear exposition of Bruns' result see Whittaker [1937, 358].

<sup>47</sup> Mittag-Leffler to Poincaré 21.12.1888, M-L I.

when  $t = 0$  and  $x_1 = \phi_1 + \beta_1 + \Psi_1, \dots, y_2 = \phi_4 + \beta_4 + \Psi_4$ , when  $t = T$ , then  $\Psi$  can be expanded as a power series in  $\beta$  and the eigenvalue equation in  $S$

$$\begin{vmatrix} \frac{\partial \Psi_1}{\partial \beta_1} - S & \frac{\partial \Psi_1}{\partial \beta_2} & \frac{\partial \Psi_1}{\partial \beta_3} & \frac{\partial \Psi_1}{\partial \beta_4} \\ \frac{\partial \Psi_2}{\partial \beta_1} & \frac{\partial \Psi_2}{\partial \beta_2} - S & \frac{\partial \Psi_2}{\partial \beta_3} & \frac{\partial \Psi_2}{\partial \beta_4} \\ \frac{\partial \Psi_3}{\partial \beta_1} & \frac{\partial \Psi_3}{\partial \beta_2} & \frac{\partial \Psi_3}{\partial \beta_3} - S & \frac{\partial \Psi_3}{\partial \beta_4} \\ \frac{\partial \Psi_4}{\partial \beta_1} & \frac{\partial \Psi_4}{\partial \beta_2} & \frac{\partial \Psi_4}{\partial \beta_3} & \frac{\partial \Psi_4}{\partial \beta_4} - S \end{vmatrix} = 0$$

can be formed.

The roots of this equation are  $e^{\alpha T} - 1$ ,  $\alpha$  being the characteristic exponents. Since it is the restricted three body problem which is being considered, two of the roots are zero and two are non-zero.

Furthermore,

$$\frac{\partial F}{\partial x_1} \frac{\partial \Psi_1}{\partial \beta_i} + \frac{\partial F}{\partial x_2} \frac{\partial \Psi_2}{\partial \beta_i} + \frac{\partial F}{\partial y_1} \frac{\partial \Psi_3}{\partial \beta_i} + \frac{\partial F}{\partial y_2} \frac{\partial \Psi_4}{\partial \beta_i} = 0 \quad (i = 1, \dots, 4)$$

$$\frac{\partial \Phi}{\partial x_1} \frac{\partial \Psi_1}{\partial \beta_i} + \frac{\partial \Phi}{\partial x_2} \frac{\partial \Psi_2}{\partial \beta_i} + \frac{\partial \Phi}{\partial y_1} \frac{\partial \Psi_3}{\partial \beta_i} + \frac{\partial \Phi}{\partial y_2} \frac{\partial \Psi_4}{\partial \beta_i} = 0$$

where, in the derivatives of  $F$  and  $\Phi$ ,  $x_i$  and  $y_i$  are replaced by  $\phi_i(T)$  ( $i = 1, \dots, 4$ ).

Hence either

$$\frac{\frac{\partial F}{\partial x_1}}{\frac{\partial \Phi}{\partial x_1}} = \frac{\frac{\partial F}{\partial x_2}}{\frac{\partial \Phi}{\partial x_2}} = \frac{\frac{\partial F}{\partial y_1}}{\frac{\partial \Phi}{\partial y_1}} = \frac{\frac{\partial F}{\partial y_2}}{\frac{\partial \Phi}{\partial y_2}} \quad (5.9.x)$$

or the Jacobian of  $\Psi$  with respect to  $\beta$  is zero, together with all the minors of first order.

On the other hand, if  $\phi'(t)$  is the derivative of  $\phi(t)$  then

$$\sum \frac{\partial \Psi_i}{\partial \beta_j} \phi'_j(0) = 0 \quad (i = 1, \dots, 4)$$

$$\frac{\partial F}{\partial x_1} \phi'_1(0) + \frac{\partial F}{\partial x_2} \phi'_2(0) + \frac{\partial F}{\partial y_1} \phi'_3(0) + \frac{\partial F}{\partial y_2} \phi'_4(0) = 0$$

$$\frac{\partial \Phi}{\partial x_1} \phi'_1(0) + \frac{\partial \Phi}{\partial x_2} \phi'_2(0) + \frac{\partial \Phi}{\partial y_1} \phi'_3(0) + \frac{\partial \Phi}{\partial y_2} \phi'_4(0) = 0$$

and if it can be shown that if equations (5.9.x) are not satisfied, then either

$$\phi'_1(0) = \phi'_2(0) = \phi'_3(0) = \phi'_4(0) = 0 \quad (5.9.xi)$$

or the equation in  $S$  has three zero roots. But since  $S$  only has two zero roots and equations (5.9.xi) are only satisfied for certain particular periodic solutions where the planetoid has a circular orbit<sup>48</sup>, the equations (5.9.x) must be satisfied for  $x_1 = \phi_1(T), \dots, y_2 = \phi_4(T)$ . Furthermore, since the origin of the time is arbitrary, they must also be satisfied for  $x_1 = \phi_1(t), \dots, y_2 = \phi_4(t)$ , that is for all points of the periodic solutions.

For the final part of the proof Poincaré showed that equations (5.9.x) were in fact satisfied identically which proved that  $\Phi$  is a function of  $F$ . Hence the two solutions  $F$  and  $\Phi$  cannot be distinct. Thus the equations (5.7.i) do not admit any new single-valued transcendental integral, providing the value of  $\mu$  is sufficiently small.

#### 5.9.4 Positive and negative results

In [P1] Poincaré did not have a chapter dealing specifically with the three topics treated in 5.9.1-5.9.3, although, as has been described, much of the material did appear in [P1] but in a less organised fashion. However, [P1] did include a chapter in which he gave a general resumé of his results. The chapter was fairly brief, amounting to only five pages, and was divided into two parts, positive and negative results, of which very little was reproduced in [P2].

With regard to the positive results Poincaré's main conclusion was that the trajectories in the restricted three body problem could be classified into three types: closed trajectories corresponding to periodic solutions; asymptotic trajectories; and the general trajectories which did not fit into either of the above categories. He commented that the difficult and unexpected result which he had established was that the trajectories which asymptotically approach an unstable closed trajectory were the same trajectories as those which asymptotically move away from the same unstable closed trajectory, and that the set of these asymptotic trajectories formed a closed asymptotic surface. This of course was the error which caused him so much trouble.

Of the results he described as negative, although he mentioned the divergence of Lindstedt's series, the one he considered the most important was the one concerning the non-existence of any new integrals for the equations of dynamics. He gave only a

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<sup>48</sup> See Laplace [1789-1825, X, Chapter VI].

brief outline of the proof of this result in which he related it to the restricted three body problem, although, as previously mentioned, he added a more detailed proof in *Note G*.

## 5.10 Attempts at generalisation

### 5.10.1 The $n$ body problem

In the last chapter of the memoir Poincaré returned to the competition question: the  $n$  body problem. He had originally thought that the main obstacle to generalising his results from the restricted three body problem to the  $n$  body problem would be difficulties associated with the increase in the number of variables and the resulting impossibility of creating a geometric representation. Unfortunately, this did not turn out to be the case.

Generalising the first part of the memoir presented Poincaré with little problem, since in a system with  $p$  degrees of freedom, a state can be represented by the position of a point in a space of  $2p - 1$  dimensions. As a result he was in a straightforward way able to extend to the generalised case many of his conclusions concerning periodic and asymptotic solutions. For example, as he observed, it can be shown that in the  $n$  body problem there are an infinite number of periodic solutions, stable and unstable, as well as an infinite number of asymptotic solutions.

It was in attempting to generalise the second part of the memoir that Poincaré found himself beset with difficulties. For example, he cited the example where the autonomous Hamiltonian equations have three degrees of freedom, and the problem is then to find three functions,  $x_i = \Phi_i(y_1, y_2, y_3)$ , satisfying

$$\frac{\partial x_i}{\partial y_1} \frac{\partial F}{\partial x_1} + \frac{\partial x_i}{\partial y_2} \frac{\partial F}{\partial x_2} + \frac{\partial x_i}{\partial y_3} \frac{\partial F}{\partial x_3} + \frac{\partial F}{\partial y_i} = 0. \quad (i = 1, 2, 3)$$

He found that even this relatively simple case led to the consideration of three different situations, of which two led to the problem of small divisors, while the third led to inscrutable integrals.

A second difficulty which Poincaré encountered concerned the motion of the perihelions. For in the unperturbed case when the system is in a state of Keplerian motion, since the Hessian of  $F_0$  with respect to the linear variables  $x_i$  is zero, as well as the Hessian of any arbitrary function of  $F_0$ , the perihelions remain fixed. As he observed, this difficulty does not arise in the restricted three body problem because



it is not necessary to use the longitude of the perihelion,  $g$ , as a variable, the variable  $(g - t)$  can be chosen instead.

Poincaré also drew attention to the fact that he had not made a full investigation of the periodic solutions of the unperturbed motion in the three body problem, that is when the orbits of the two smaller bodies or planets reduce to Keplerian ellipses. He had so far only considered the obvious case of periodic solutions which arise when the two mean motions are commensurable, and he had not considered the possibility of any others.

Thinking about this particular question led him to the idea of periodic motion resulting from two planets passing infinitely close to each other without actually colliding. He conjectured that if the planets did move in such a way then this would give rise to a change in their orbits which would give the appearance of a collision, and it might be possible for the initial conditions to be chosen in such a way that these "collisions" occurred periodically. If this were the case, then discontinuous solutions would be obtained which would be proper periodic solutions of the Keplerian motion. Shortage of time precluded him from pursuing the idea further at this stage but he did discuss it some ten years later in the final volume of the *Méthodes Nouvelles* where he called these solutions *periodic solutions of the second sort*<sup>49</sup>.

Poincaré thus attributed several reasons for his inability to generalise his results as he had originally hoped. Although it was evident that some of the difficulties would be overcome in the fulness of time, there were some which appeared to be beyond the scope of available techniques. His work had made it plain that the  $n$  body problem required much more research before mathematicians would be in a position to claim a full understanding of it.

With characteristic modesty, Poincaré concluded the memoir by saying that he regarded his work as only a preliminary survey from which he hoped future progress would result.

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<sup>49</sup> Poincaré [MN III, Chapter XXXI]. See 7.2.4.

## 6. Reception of Poincaré's Memoir

### 6.1 Introduction

The discovery of the error in Poincaré's original memoir and the accompanying delay in the publication meant that almost two and a half years elapsed between the submission of the manuscript to the competition and the corrected memoir's publication in *Acta*. As a result the contemporary reception of the memoir can be divided into two phases. In the first phase there was the reaction to [P1] which consisted of the opinions of those who had had unrestricted access to the memoir, namely the prize commission, together with the opinions of those whose knowledge of [P1] had been derived solely from Mittag-Leffler's report. The second phase contained the reaction to [P2].

Since at the time the prize was awarded the only publicly available information about the mathematical content of the prize-winning memoir was Mittag-Leffler's brief report, no mathematical commentaries were included in the press coverage of Poincaré's triumph. Furthermore, the prepublication copies of [P1] which made a brief appearance at the end of 1889 were in circulation for such a short period that none of the recipients would have had time to master the contents in order to make a judgement. Added to which Mittag-Leffler's campaign of secrecy after the discovery of the error, plus the additional delay caused by the backlog of publishing at *Acta*, meant that when [P2] was published, those members of the mathematical community who had heard any rumours about the error would have had plenty of

time to forget the details. The result was that by the time [P2] finally appeared, any remaining concerns about the error, if indeed they existed, seem to have disappeared, despite Poincaré's allusion to it in the introduction.

## 6.2 The views of the prize commission

As described in 4.5, the three members of the prize commission were quick to come to a unanimous decision concerning the overall merit of Poincaré's entry. However, in their correspondence to one another during the adjudication period, the only one who ventured anything more than a general opinion on the mathematics was Weierstrass.

Mittag-Leffler, although he openly indicated to Poincaré the particular points in the memoir which he felt needed further elaboration, fought shy of discussing the relative merits of any of the results with either Poincaré or his fellow members of the commission. His prime concern seems to have been in fulfilling his role as a mediator between Hermite and Weierstrass, communicating information from one to the other.

Of a more public nature was Mittag-Leffler's responsibility for the commission's general report. But since the purpose of the report was to present the opinion of the whole commission on the results of the competition, and was written with help from Hermite and Weierstrass, it gave no insight into Mittag-Leffler's personal views. In any case, the task of providing a mathematical analysis of Poincaré's memoir had been left to Weierstrass and so the report contained no details of the winning entry beyond giving a general indication of the nature of the results and emphasising the power of analytic methods in treating questions of celestial mechanics<sup>1</sup>.

In addition to writing the general report, the King had also asked Mittag-Leffler to give a résumé of Poincaré's results at the February meeting of the Swedish Academy of Sciences. In the event Mittag-Leffler's talk was not given until March, and then in rather less than favourable circumstances, the background of which is described in the following section.

Hermite, by virtue of being in Paris, was in the unique position of being able to speak directly to Poincaré about the memoir. Although he was unequivocal about his view

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<sup>1</sup> See Appendix 4.

of the memoir, as he told Mittag-Leffler, he too had sought help from Poincaré over the details:

*"Poincaré's memoir is of such rare depth and power of invention, it will certainly open a new scientific era from the point of view of analysis and its consequences for astronomy. But greatly extended explanations will be necessary and at the moment I am asking the distinguished author to enlighten me on several important points."*<sup>2</sup>.

It is not clear exactly which parts of [P1] Hermite felt needed explaining, nor is there any indication of those results which he considered the most important. The implication that Hermite felt uneasy about his ability to fully comprehend the mathematics is confirmed by his reaction to the suggestion that he might have to write the official report on the memoir. Although it was more or less understood that Weierstrass as proposer of the question should be the author of the report, Mittag-Leffler, as a result of Weierstrass' declining health, did express concern to Hermite about Weierstrass' fitness for the undertaking<sup>3</sup>. Hermite's response left no doubt as to his own feelings on the matter:

*"The task of writing the report falls by right to Weierstrass who proposed the question, the famous mathematician can with authority express reservations which would put me into an indescribably difficult position should I have to make them. Indeed what would be my position vis-à-vis Poincaré to whom I would necessarily have to appeal for explanations in order to understand the most important points of his memoir; I would no longer be in the role of judge, and I must tell you in all frankness, if I have to make the report, it would be the echo of what I had heard from listening to the author, with the intention of justifying my admiration for his genius. ... Besides, to satisfy the demand of public opinion, and taking into account the importance and seriousness of the announcement of the prize, do you think that it is advisable that the prize awarded to a Frenchman should rest on the report of a Frenchman who is his colleague and friend?"*<sup>4</sup>.

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<sup>2</sup> Hermite to Mittag-Leffler, 17.10.1888. *Cahiers* 6 (1985), 146.

<sup>3</sup> Mittag-Leffler to Hermite, 17.10.1888, M-L I.

<sup>4</sup> Hermite to Mittag-Leffler, 22.10.1888, *Cahiers* 6 (1985), 147-148.



Mittag-Leffler needed no further convincing and the responsibility for the report remained with Weierstrass who, as it will be seen, also displayed doubts about Hermite's ability to deal with the mathematics unaided.

At the 1889 public meeting of the Paris Academy of Sciences, Hermite in his official speech as Vice President, prompted by Mittag-Leffler's letter to the Secretary, used the occasion to comment on the results of the competition and commend the contents of Poincaré's memoir. In particular he drew attention to Poincaré's discovery of the asymptotic character of the series used in celestial mechanics. Rather ironically, he chose to describe this result in terms of Poincaré having discovered an error:

*"The error having been recognised, it opens a new avenue in the study of the three body problem, and this is where Poincaré's talent is displayed with brilliance."*<sup>5</sup>.

He could have had no idea how prescient those words would turn out to be.

Weierstrass' opinion of [P1] is mostly revealed in three letters to Mittag-Leffler, parts of which were published in *Acta* in Mittag-Leffler's [1912] biography of Weierstrass. The most significant of these is the first in which he gave his judgement of the competition entries. Although he commented on only five of them, he devoted more than four times the space to Poincaré's work than the other four put together<sup>6</sup>.

Weierstrass considered the most important results to be what Poincaré had described as the *negative results*, that is the divergence of Lindstedt's series and the theorem on the non-existence of single-valued integrals. Although Weierstrass was convinced that these results showed that an entirely new approach would be needed if the problem was to be solved, he was still of the opinion that a solution existed. On the positive side, he singled out Poincaré's discoveries in the various realms of stability, invariant integrals, periodic solutions and asymptotic solutions as being especially notable, and was altogether enthusiastic about the treatment of the analytic solutions of algebraic differential equations.

Nevertheless, despite Weierstrass' extensive praise for Poincaré's work, his comments were not entirely without criticism. Alongside his compliments he also

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<sup>5</sup> Hermite [1889].

<sup>6</sup> Weierstrass to Mittag-Leffler, 15.11.1888, M-L I. Mittag-Leffler [1912, 50-52].

admitted that he had found the memoir extremely difficult to read and he expressed concern about its general lack of rigour.

In the second letter written some seven weeks later, he stressed that despite having spent the intervening period working hard on the memoir he still had not completely mastered it<sup>7</sup>. Nevertheless, he was even more enthusiastic about it than before. He now believed that the results on periodic solutions and the discovery of asymptotic motion were achievements of the highest importance, even to the extent of describing them as epoch-making. On a critical note he was rather concerned about Poincaré's treatment of the stability question in the restricted three body problem. He queried the physical validity of Poincaré's definition which appeared only to put an upper bound on the distance between two points without considering what would happen if two points became infinitely close. For, as he pointed out, should this occur it would inevitably affect the form of motion and so a distinction should be drawn which would take this into account.

The manuscript of the letter also reveals a careful piece of editing by Mittag-Leffler. Tactfully omitted from the published version is Weierstrass' remark in which he confided to Mittag-Leffler that he thought Hermite must have had somebody to explain the memoir to him.

In the third letter Weierstrass explained why, contrary to what he had said in his previous letter, he was now satisfied that Poincaré's analysis does ensure that the planetoid cannot come infinitely close to the other two bodies<sup>8</sup>. He had simply overlooked that Poincaré had incorporated the condition that the value of the constant  $C$  in the equation:

$$\frac{1}{2a} + G + \mu F_1 = C$$

had to be essentially greater than  $3/2$ . Again his letter was not entirely free of criticism. As mentioned in 5.9.2, he questioned Poincaré's claim that the non-existence of any new single-valued solution necessarily implied the non-existence of convergent trigonometric solutions.

Finally, he told Mittag-Leffler that although he would definitely have the report finished by the end of the week, he was having difficulty with the introduction.

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<sup>7</sup> Weierstrass to Mittag-Leffler, 8.1.1889, M-L I. Mittag-Leffler [1912, 53-55].

<sup>8</sup> Weierstrass to Mittag-Leffler, 2.2.1889, M-L I. Mittag-Leffler [1912, 55-58].

This was because he believed the report should begin with a justification of the question in order to counter the adverse criticism which it had been lodged against it. The criticism was on two fronts: there were those who claimed that the question as it stood was completely insoluble, while others censured the limitation induced by the assumption that a collision between two points can never take place. Weierstrass indicated to Mittag-Leffler his intended response to these accusations, but his real concern was how to condense into a few lines something which he felt warranted a long discussion. Clearly much of the criticism had arisen as a result of Kronecker's [1888] publication, and given the brittle nature of the relationship between Weierstrass and Kronecker, Weierstrass was understandably keen for his defence to be carefully drafted.

Despite his intentions, Weierstrass never finished the report on [P1] (or indeed on [P2]), however, he did manage to complete the introduction, sending a copy to Mittag-Leffler in March 1889. It threw no further light on Weierstrass' judgement of the memoir, but it did give the reasons for his particular formulation of the  $n$  body problem in the competition question, as well as the criteria he had used in judging the entries which had attempted to provide a solution<sup>9</sup>.

In the question Weierstrass had asked for an expansion of the coordinates as infinite series of known functions of time which were uniformly convergent for *unbegrenzter Dauer* (=unlimited time), the implication being that such a series did actually exist. It was the phrase *unbegrenzter Dauer* which had been the main cause of the misunderstanding. For it had been interpreted as meaning that Weierstrass required series which were uniformly convergent for infinite time, i.e. for  $0 \leq t \leq \infty$ , rather than, as he had intended, series which were uniformly convergent for a fixed value of time, however large, i.e. for  $0 \leq t \leq a$  ( $a < \infty$ ). The distinction was critical because Weierstrass believed that he had managed to prove that such a series did indeed exist in the latter case, whereas he had no such proof for the former.

For the existence to be proven, it has to be shown that the distance between any two points can never become infinitely small or infinitely large as time approaches a finite limit.

The first case amounts to dealing with the possibility of a collision, and not only does the possibility of a binary collision have to be considered but also the possibility of

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<sup>9</sup> Mittag-Leffler [1912, 63-65].



multiple collisions. Weierstrass' difficulty was that when he had originally set the question he had constructed a proof in which he believed he had overcome the problem of collisions but in the interim he had forgotten it. In his letter to Mittag-Leffler he only managed to give an outline of a proof which dealt with binary collisions and to state a conjecture (with no indication of proof) about triple collisions. He gave no consideration to collisions of four or more particles.

To deal with a binary collision he assumed that the time  $t$  was sufficiently close to the moment of collision  $t_0$  so that the coordinates of all the points could be expanded in positive powers of

$$(t_0 - t)^{1/3}$$

and the expressions would then contain not  $6n$  but  $6n - 2$  arbitrary constants<sup>10</sup>. Then for arbitrary initial conditions, the probability of a collision between any two of the points would be infinitely small and so could properly be ignored. He did, however, admit that he was concerned by the fact that this method did leave open the possibility that after an infinitely long period of time two points could approach each other infinitely closely without actually colliding.

As far as triple collisions were concerned, Weierstrass claimed that it was easy to show that all three points can only collide when the three constants of angular momentum are simultaneously zero. With regard to the three body problem this was clearly an important result but unfortunately Weierstrass did not give a proof and Mittag-Leffler did not press him for one; and it was not until the beginning of the next century that Sundman, unaware of Weierstrass' conjecture, provided a proof of this result<sup>11</sup>.

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<sup>10</sup> Saari [1990] gives a clear account of the derivation of this expansion. Briefly, the equations of motion for two colliding points in the collinear central force problem are given by  $\frac{d^2x}{dt^2} = -\frac{1}{x^2}$  with solution  $x(t) \sim A(t - t_0)^{2/3}$  as  $t \rightarrow t_0$ , where  $A$  is a positive constant and  $t_0$  is the time of collision, and it can be shown for the  $n$  body problem that the same rate of approach holds for collisions of any kind taking place at  $t = t_0$ . Assuming that  $t_0 = 0$  and substituting  $X(t)t^{2/3} = x(t)$  into the equations of motion gives

$$t^2 \frac{d^2X}{dt^2} + (4/3)t \frac{dX}{dt} - (2/9)X = -\frac{1}{X^2}.$$

Making the change of variable  $s = t^{1/3}$  leads to

$$s^2 \frac{d^2X}{ds^2} + 2s \frac{dX}{ds} - 2X = -9\frac{1}{X^2},$$

which has an analytic solution in  $s$ .

<sup>11</sup> Sundman's work is discussed in 8.3.2.



Rather curiously Weierstrass does not appear to have considered the possibility of the mutual distances becoming infinitely large, and it was not until 1895 that Paul Painlevé formally proved that such a situation cannot arise in the case of the three body problem<sup>12</sup>.

Weierstrass ended his letter to Mittag-Leffler by saying that he felt that he had provided sufficient results to validate the claim that in general the coordinates of the points in the three body problem could be developed in series of the form he had specified in the question. However, since he had neither provided a proof of the impossibility of triple collision, nor eliminated the possibility that the mutual distances cannot become infinitely large, his claim was somewhat tenuous.

Nevertheless, since Weierstrass did consider it legitimate to suppose that, given an unlimited time interval, the coordinates in the  $n$  body problem were single-valued continuous functions of time and as such could be represented by a series as specified in the question, he did believe that a solution was possible, and so his question was then whether such a solution was actually feasible. That was why he had asked for the description of a method which would calculate successive terms of the series rather than asking for a complete expansion. In other words, he believed it was possible to give an approximate expression for the functions such that the difference between the expression and the function did not exceed a specified arbitrarily small limit within a time interval of arbitrary length. If this could be done then the function would be represented by an absolutely and uniformly convergent series and the problem would be solved as required.

In setting the question Weierstrass had hoped to achieve a better understanding of the true nature of the motion of celestial bodies as well as obtaining a reliable result concerning the stability of the solar system, and he had little doubt that the latter could be achieved, even without a solution which was valid for infinite time.

Earlier attempts to obtain a solution had resulted in the coordinates of the planets or variable orbital elements being represented by series of the form

$$\sum_{v_1, v_2, \dots} \{C_{v_1 v_2 \dots} \sin(c_0 + v_1 c_1 + v_2 c_2 + \dots) t\},$$

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<sup>12</sup> Painlevé's contribution and the question of non-collision singularities in the  $n$  body problem is discussed in 8.3.3.

where  $v_i$  are integers,  $t$  is the time and  $(C_{v_1}, v_2, \dots, c_1, \dots)$  are independent of  $t$ . It had been shown that, under certain assumptions, such series do formally satisfy the differential equations but what had not been resolved was whether such series were convergent and thus true expressions of the quantities to be represented. Since this problem was clearly one of the fundamental issues raised by the competition question, its treatment provided Weierstrass with a criteria on which to base his judgement.

Unfortunately, Weierstrass completed no more of his report. Nevertheless, it seems very probable that his analysis of [P1] would have been largely based on the letters described above. What is clear from the later correspondence is that his continuing delay in producing a report on [P1] was due to his difficulties over the parts of the memoir which he considered insufficiently explained and which he felt necessary to master.

He did however make one further comment concerning [P1] and that was to criticise Mittag-Leffler for the letter he had sent to the Secretary of the Paris Academy announcing the results of the competition<sup>13</sup>. The total French triumph had apparently proved rather hard for the German mathematicians to bear and, in particular, exception had been taken to Mittag-Leffler's description of Poincaré's memoir as being one of

*"... the most important pieces of mathematics of the century ...".*

### 6.3 Gylden's reaction

Undoubtedly one of the first people outside the commission to know about Poincaré's memoir was Gylden. As a member of the editorial board of *Acta*, as well as being a lecturer in astronomy at the Stockholm Högskola, he was in close touch with Mittag-Leffler and well placed to hear about the result of the competition. However, unfortunately for Mittag-Leffler, when Gylden learnt of the results in Poincaré's memoir, his reaction mirrored that of Kronecker's to the competition announcement of more than three years earlier. Gylden, having seen the general report with its remarks about the discovery of asymptotic solutions, believed that he had already discovered similar results he had had published in *Acta* [1887].

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<sup>13</sup> *Comptes Rendus* 108 (8) 25.2.1889, 387.

Gylden must have made his displeasure immediately obvious because at the end of January Mittag-Leffler wrote to Poincaré to ask him (in strictest confidence) for his opinion on Gylden's [1887] paper, and in particular whether he thought the series proposed by Gylden were convergent. This resulted in several replies from Poincaré which, some 30 years later, Mittag-Leffler published as part of the *Acta* volume dedicated to Poincaré<sup>14</sup>. However, the correspondence had only just got under way when Gylden brought the issue into the public arena.

As already mentioned, in February 1889, Mittag-Leffler had been due, at the King's request, to give a report on the principal results in Poincaré's paper at the monthly meeting of the Swedish Academy of Sciences. However, illness prevented him from attending, although given that he had already expressed a certain reluctance to make a public speech about Poincaré's work without the support of Weierstrass' report, he might well have felt somewhat relieved that the task was at least postponed. Nevertheless, those attending the February meeting of the Academy were not left in complete ignorance of Poincaré's work. For Gylden chose to make it the occasion for declaring his views on the results in Poincaré's memoir, claiming that all those of importance were already contained in his own 1887 paper<sup>15</sup>.

Not surprisingly, after Gylden's announcement, the King was more anxious than ever for Mittag-Leffler to make his speech at the Academy. He made it plain to Mittag-Leffler that he wanted it to be heard at the March meeting and since Mittag-Leffler knew that he could not be certain of being in possession of Weierstrass' report, it became a matter of urgency for him to have Poincaré's views on Gylden's results.

Prior to Gylden's public declaration, Poincaré had already begun his side of the correspondence by admitting that he had found Gylden's style very hard to read and he had also told Mittag-Leffler that to give a definitive answer to the convergence question, he would have to read each and every line of the memoir which he was reluctant to do<sup>16</sup>. From what he had seen, he was unable to say whether Gylden's method led to a proof of either convergence or divergence, although he thought divergence the more likely. In addition he expressed dissatisfaction at a particular aspect of Gylden's method. From his understanding, the successive terms in the

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<sup>14</sup> *Acta* 38 (1921), 163-173.

<sup>15</sup> Mittag-Leffler to Weierstrass, 22.2.1889, M-L I.

<sup>16</sup> Poincaré to Mittag-Leffler, 5.2.1889, No. 48, M-L I. *Acta* 38, 163-164.



expansion were not deduced recurrently but rather at each stage of the calculation choices were made which incorporated an element of chance into the process.

On hearing from Mittag-Leffler about Gylden's communication in the Swedish Academy<sup>17</sup>, Poincaré responded again and at length<sup>18</sup>. He made the point that the dispute brought into sharp focus the difference between the mathematician and astronomer with regard to their interpretation of convergence. He reasoned in detail against the rigour of Gylden's arguments, reiterating that he believed Gylden's method to rely heavily on questions of judgement.

Nevertheless, despite the critical appearance of this correspondence Poincaré maintained a high regard for Gylden's work, appreciating the flexibility and practical advantages of his methods. He had not intended to demolish Gylden but rather he had wanted to show how words such as *proof* and *convergence* take on different meanings depending on the perspective of the user, i.e. depending on whether the user is a mathematician or an astronomer. Moreover, he was sensitive to the fact that Gylden's approach was coloured by a practical interest in the problem which he himself did not share.

In the final letter of this correspondence he showed quite clearly why he believed that following Gylden's argument exactly led to divergent series<sup>19</sup>. Briefly, he started with the equation

$$\frac{d^2V}{dt^2} + n^2 s A \sin V \cos V = n^2(X) \quad \text{where } (X) = \sum s_i A_i \sin(\lambda_i n t + m V + h)$$

where  $h$  is a constant and  $\lambda_i$  and  $m$  are integers, and following Gylden he put

$$V = V_0 + V_1, \quad V_0 = -2 \arctan e^{-\xi} + \pi/2, \quad \xi = \alpha n t + c.$$

Gylden's method then involved integrating by successive approximations and at each stage of the approximation choosing suitable values for the two constants of integration and the coefficient  $\alpha$ .

Poincaré's argument hinged on the fact that he did not consider it legitimate for these choices to be arbitrary. With regard to the constants he believed that Gylden's method meant that there was in fact only one particular value of the

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<sup>17</sup> Mittag-Leffler to Poincaré, 23.2.1889, M-L I.

<sup>18</sup> Poincaré to Mittag-Leffler, 1.3.1889, No. 49, M-L I. *Acta* 38, 164-169.

<sup>19</sup> Poincaré to Mittag-Leffler, 5.3.1889, No. 50, M-L I. *Acta* 38, 169-173.



constants, from an infinite number of choices, which would lead to a convergent series and hence to a proof of the existence of asymptotic solutions. Moreover, from what he could see, Gylden's method gave no way of recognising which of the series was convergent. As far as  $\alpha$  was concerned, he emphasised that its value was completely determined and could not, as Gylden proposed, be changed with each new approximation, adding that  $\alpha n$  was equivalent to what in his own memoir he had called the characteristic exponent.

Not surprisingly Hermite and Weierstrass were also drawn into the polemic. Hermite, who had first heard about the dispute from Kovalevskaya, thought Gylden's series, like Lindstedt's, were asymptotic. However, he carefully avoided drawing any direct comparisons between the two memoirs. He had himself received a letter from Gylden but since it was written in Swedish he had been unable to read it, although given the formulæ it contained he had deduced that it must be about the convergence question.

Meanwhile Mittag-Leffler having given his talk at the Academy, had written in jubilation to Weierstrass and Poincaré, convinced that those who had heard him were agreed that Poincaré deserved to win the prize<sup>20</sup>. Although Gylden had raised objections, insisting that his series were convergent for all time, he did admit that in the neighbourhood of any set of the constants  $c_1, \dots, c_n$ , there were other values for which the series did not converge.

However Mittag-Leffler's triumph was short lived. The academic community in Stockholm decided to weigh in on the side of Gylden, and, despite the fact that Poincaré's memoir was not available to be seen, adopted the view that Gylden had indeed published proofs of everything Poincaré had done<sup>21</sup>. The consensus was that Mittag-Leffler's denial of Gylden's results had been motivated purely by jealousy, and the mathematician Backlund reinforced the idea by drawing attention to the fact that Gylden's memoir had recently been awarded the St. Petersburg prize. Meanwhile Gylden himself steadfastly maintained that the values of the constants  $c_1, \dots, c_n$  for which his series diverged formed only a countable set and so it was infinitely unlikely that the series was actually divergent. Mittag-Leffler continued to argue against him since, with Poincaré, he believed that the series were

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<sup>20</sup> Mittag-Leffler to Weierstrass, 24.3.1889, and Mittag-Leffler to Poincaré, 28.3.1889, M-L I.

<sup>21</sup> Mittag-Leffler to Weierstrass, 15.4.1889, M-L I.

divergent not just for a countable set but for a perfect set in the neighbourhood of the constants  $c_1, \dots, c_n$ . And moreover, he told Weierstrass that he thought Gylden not enough of a mathematician to understand.

Although not directly relevant to the disputes over Poincaré's memoir, it is of interest to record that in May that year Gylden met with Kronecker in Berlin, a meeting which, within the context of competition, Mittag-Leffler would surely have viewed with some misgivings. In any case, the occasion prompted Mittag-Leffler to remark to Weierstrass that although he had been led to believe that his two adversaries had understood each other perfectly, he suspected that Gylden really understood as little of Kronecker as Kronecker understood of Gylden!<sup>22</sup>

With the publication of the memoir not scheduled for several months, the controversy with Gylden died down, that is until the rumours about the error began to emerge. As mentioned above, Gylden was instrumental in bringing the rumours to Berlin and having had temporary possession of a copy of [P1], he was certainly as well qualified as anyone outside the commission to speak about it. Later when [P2] was finally published, Gylden attempted to reopen the controversy by writing directly to Hermite to protest against Poincaré's results<sup>23</sup>. He had probably hoped that he could count on Hermite's support for his own work since Hermite was known to be interested in the applications of elliptic function theory in celestial mechanics<sup>24</sup>.

Nevertheless, Hermite was not to be drawn. His response was to stand by the judgement of the commission declaring his loyalty to Mittag-Leffler and Weierstrass. In addition, Gylden's actions prompted Hermite to reassure Mittag-Leffler by telling him how well Poincaré's memoir had been received in Paris. Shortly afterwards Gylden sent Hermite part of his [1891] *Acta* paper for comment. This time Hermite managed to avoid the issue completely by replying with the claim that the paper was beyond his own mathematical domain<sup>25</sup>. However, as indicated in a subsequent letter to Mittag-Leffler, it appears that Hermite was not impressed by Gylden's grasp of analysis; in fact he described Gylden as a ghost from

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<sup>22</sup> Mittag-Leffler to Weierstrass, 12.5.1889, M-L I.

<sup>23</sup> Hermite to Mittag-Leffler, 10.1.1891. *Cahiers* 6 (1985), 188-189.

<sup>24</sup> Picard [1902] specifically mentions Hermite's sympathy for the work of Gylden in this respect. See Hermite [1877].

<sup>25</sup> Hermite to Mittag-Leffler, 1.3.1891. *Cahiers* 6 (1985), 193.

a bygone age, who had been left behind as the world of analysis transformed about him<sup>26</sup>!

## 6.4 Minkowski

One of the first documented comments about [P2] came from the young Hermann Minkowski, then a lecturer at the University in Bonn. In a letter dated 22nd December 1890 to David Hilbert, he revealed that he had so far studied the first third of the memoir and that what he had seen had reminded him of Dirichlet<sup>27</sup>.

Minkowski was also the author of the report on [P2] which appeared in the *Jahrbuch über die Fortschritte der Mathematik* for 1890 which was published in 1893, by which time Minkowski had become an associate professor at the University. This report, which appears to be the first mathematical commentary on [P2], was of quite a remarkable length<sup>28</sup>. Most reports in the *Jahrbuch* merited at most a single page, Minkowski's report on [P2] ran to seven.

Since the function of the *Jahrbuch* was to provide information about the current state of mathematical research, Minkowski's priority would have been to provide a factual rather than a critical account of the memoir. Nevertheless, it is clear from the report that he had a good grasp of Poincaré's ideas. He skilfully picked out the salient features, emphasising their relative importance, and presented them in an accessible way.

Various aspects of Minkowski's report invite special comment. These include his clear and concise description of the theory of invariant integrals in which he drew attention to the recurrence theorem; his discussion of Poincaré's use of the method of analytic continuation in the theory of periodic solutions; and the clarity with which he distinguished between Poincaré's use of the parameter  $\mu$  and its square

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<sup>26</sup> Hermite to Mittag-Leffler, 17.3.1891. *Cahiers* 6 (1985), 195-196.

<sup>27</sup> L. Rüdénberg and H. Zassenhaus *Hermann Minkowski Briefe an David Hilbert*, Springer-Verlag, 1973; 40. It seems likely that Hilbert had asked Minkowski for his opinion on the memoir since he had already told Klein that he had organised for a report on the memoir to be made at his Königsberg seminar. See G. Frei *Der Briefwechsel David Hilbert - Felix Klein (1886-1918)* Vandenhoeck & Ruprecht, Göttingen 1985, 72.

<sup>28</sup> *Jahrbuch über die Fortschritte der Mathematik* 22, 907-914.

The *Jahrbuch* report was certainly not the first report of some of the ideas in [P2], although it does appear to be the first full report of the memoir. The first volume of Poincaré's *Mécanique Céleste* which was derived from parts of [P2] was published in 1892 and a review appeared in the *Bulletin of the New York Mathematical Society* later the same year. See 7.2.5.



root. Especially notable is the fact that Minkowski freely acknowledged the difficulties associated with Poincaré's doubly asymptotic solutions. Rather paradoxically, this probably indicates that Minkowski had a better understanding of the concept than some of his contemporaries who refrained from passing any comment at all on these solutions.

## 6.5 Hill

The first person to openly question some of Poincaré's results in [P2] and to do so in an entirely formal setting was Hill. On December 27, 1895 Hill delivered the presidential address to the American Mathematical Society (AMS). His speech, which was reported in the *AMS Bulletin*, had as its subject the progress in celestial mechanics since the middle of the century. Although meant for a general mathematical audience, the speech was far from a model exposition of clarity and the threads of Hill's arguments are often difficult to unravel.

Having begun with a description of Delaunay's contribution, Hill continued:

*"Perhaps the most conspicuous labours in our subject, during the period of time we consider, are those of Professor Gylden and M. Poincaré."*<sup>29</sup>.

With regard to Poincaré's work, he cited both [P2] and the first two volumes of the *Méthodes Nouvelles* which contained many of the results from [P2] reworked in an extended and clearer form. Although his discussion was essentially centred on the *Méthodes Nouvelles*, most of his comments apply equally well to both works.

It is perhaps rather surprising that Hill took the opportunity presented by the occasion to query some of Poincaré's results. His actions can be partly explained by his belief that his own concerns were shared by other astronomers whom he thought would feel reassured by his criticism, especially those with less mathematical insight than himself. He may also have believed that a straightforward presentation of the results in a form accessible to astronomers would have been somewhat superfluous since Poincaré himself had done that in [1891], and so another account might have been considered at best repetitious or at worst confusing.

In particular Hill was concerned by Poincaré's proof of the divergence of Lindstedt's series, a result which was clearly of great practical importance to astronomers.

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<sup>29</sup> Hill [1896]. Hill's view of Gylden's work is described in 2.2.7.



However, before discussing Hill's criticisms voiced in the speech, it is relevant to note that earlier in the same volume of the *AMS Bulletin* there is another article by Hill on the convergence of Lindstedt's series<sup>30</sup>. This article was a direct response to Poincaré's proof of the divergence of the series which had appeared in [MN II] and which focused on the case where the mean motions are incommensurable<sup>31</sup>. In this paper Hill demonstrated the existence of a class of cases where convergence can be shown, although he made no attempt to disprove Poincaré's argument.

Returning to the speech, Hill, having noted the periodic solutions, drew attention to the asymptotic solutions and the role of the associated characteristic exponents. His first objection concerned the actual use of asymptotic solutions. He reasoned that since most of practical astronomy is concerned with systems which describe almost circular motion, a first approximation can be given by a periodic solution. This being the case, the coefficients of stability are then all real and negative which implies a situation which is of no interest to the working astronomer.

He next dwelt on Poincaré's different methods of proof of the divergence of Lindstedt's series. This discussion was largely aimed at re-enforcing his earlier article and although both articles quoted results from [MN II] rather than [P2], it was the essential principle of the divergence of the series which was at issue.

Finally, he questioned Poincaré's assertion that the convergence of Lindstedt's series would imply the non-existence of asymptotic solutions, arguing that this was an irrelevant observation since the domain of the two things were quite distinct, i.e. where Lindstedt's series were applicable there were no asymptotic solutions and vice versa.

Since these objections concerned what Poincaré considered to be one of his most important results, and, moreover, since their author was someone whose academic integrity Poincaré respected, his defence was swift.

In [1896] which appeared in the *Comptes Rendus* for March 2, he replied to Hill's first article. He made it clear that there was in fact no contradiction between his result and that of Hill's: they had both, and moreover in a similar way, proved the

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<sup>30</sup> Hill [1896].

<sup>31</sup> Poincaré [MN II, 277-280].

existence of cases where the series converged. However, he did emphasise the point that it was possible for the convergence not to be uniform.

In [1896a] Poincaré countered most of the claims made in Hill's speech. Essentially Hill believed the series to converge provided the variables remained within a certain domain. What Poincaré showed was that the series could not converge in any part of an arbitrary domain if in that domain there existed a periodic solution, and, furthermore, he showed that there were periodic solutions in every domain, however small. Thus if the series were convergent at all, they could only be convergent for certain discrete values of the variables and could not be convergent for values between given limits, however small those limits.

Finally Poincaré dealt with Hill's objection concerning the existence of asymptotic solutions and the divergence of Lindstedt's series. With regard to Hill's remarks about asymptotic solutions and the imaginary nature of the characteristic exponents in all cases of practical interest, he thought that the objection had arisen because Hill believed that asymptotic solutions could only exist when the variables satisfied certain inequalities. He pointed out that he had proved the existence of asymptotic solutions for the restricted three body problem in any domain, however small, for sufficiently small values of the perturbing mass. He attributed Hill's error to the fact that Hill had only considered periodic solutions of the first kind.

## 6.6 Whittaker

In 1898 Edmund Whittaker, who was then a fellow at Trinity College, Cambridge, was asked by the British Association for the Advancement of Science to draw up a report on the current state of planetary theory<sup>32</sup>. He responded with a substantial

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<sup>32</sup> Whittaker became one of the most influential mathematicians of his generation. He was appointed Astronomer Royal for Ireland in 1906, and was elected to the chair of mathematics at Edinburgh in 1912, a post which he held until his retirement in 1946. His great treatise *Modern Analysis* [1927] which was first published in 1902 was followed only two years later by the first edition of his comprehensive *Analytical Dynamics* [1937]. The latter, which remains a standard work on the subject, includes a thorough introduction to the three body problem, and contains much of his own research related to topics in dynamical astronomy.

Several of Whittaker's early papers are of interest in relation to the work of Poincaré. Of note are [1901] in which he gave a new method for expressing the solution of a dynamical problem in terms of trigonometric series; [1902] and [1902a] in which he established a new criterion for finding periodic solutions of the differential equations of dynamics and for the restricted three body problem; and in particular [1917] which concerns his discovery of the adelphic ("brotherly") integral of a dynamical system. The adelphic integral is associated with infinitesimal contact transformations between adjacent periodic solutions and Whittaker showed that when such an integral can be constructed the equations can be integrated and the convergence difficulties indicated by Poincaré [P2, 470] overcome. Whittaker also gained

review of recent work on the three body problem [1899]. Whittaker's report, which was essentially an exhaustive account of the development of dynamical astronomy from 1868 to 1898 (the dates being chosen to coincide with the publication of the last volume of Delaunay's *Lunar Theory* and the third and last volume of Poincaré's *Méthodes Nouvelles*), naturally included a detailed account of [P2] and this account provides the first commentary on the memoir to be published in English.

As befitted the nature of the report in which it appeared, Whittaker's review of [P2] was an objective summary rather than a subjective discussion. Nevertheless, echoing Minkowski's treatment of the memoir in the *Jahrbuch*, Whittaker afforded it greater attention than any of the other works he included in his review. In contrast to Hill, his treatment of [P2] was both complimentary and easy to follow. He began:

*"A new impetus was given to Dynamical Astronomy in 1890 by the publication of a memoir by Poincaré."* <sup>33</sup>,

and then gave a clear and concise description of many of the ideas discussed in the memoir: invariant integrals, stability, periodic solutions, characteristic exponents, asymptotic solutions, doubly asymptotic solutions and periodic solutions of the second class, explaining Poincaré's terminology and emphasising important results such as the recurrence theorem, and the theorem concerning the non-existence of any new single-valued integrals. His concluding remark about the final section of the memoir (in which Poincaré indicated the problems involved in generalising his results) was somewhat ambiguous in that he did not make it clear that Poincaré was raising questions concerning the general  $n$  body problem rather than solving them.

Rather curiously, given the extent of his report, Whittaker made no attempt to relate [P2] to Poincaré's earlier papers on differential equations, beyond a single reference to his result concerning the conditions for stability. Nor did he attempt to describe Poincaré's geometric representation and the innovative technique of using a transverse section in order to make the problem more tractable. Perhaps this was because he thought that the conceptual difficulty of the ideas would distract from

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international recognition through an article on perturbation theory and orbits which he contributed to Klein's "Encyklopädie" [1912]. For a sensitive and informative biography of Whittaker, see McCrea [1957].

<sup>33</sup> Whittaker [1899, 144].



the actual results, although that had not prevented him from revealing some of the rather complicated details of Gylden's method earlier in the report.

Furthermore, with regard to the doubly asymptotic solutions, he simply described them as being:

"... approximately periodic when  $t = -\infty$  and  $t = +\infty$ , but (they) are not periodic in the meantime." <sup>34</sup>.

While this is certainly true it hardly gave an indication of the complex nature of the behaviour of these solutions. Admittedly Poincaré himself had not stressed this point in [P2] but he certainly did in [MN III]. But Whittaker did not mention the complexity aspect in his review of [MN III] either. It is possible that he thought it inappropriate to emphasise them since the probability of their appearance in reality was negligible, but this seems unlikely since the same is also true of all Poincaré's periodic solutions. Nevertheless, since he again passed over the point in his treatise on analytical dynamics [1937], perhaps it was because he felt his own understanding was not sufficiently adequate to provide a discussion.

## 6.7 Other commentators

Following Whittaker's report, the continuing interest in the three body problem was described by Edgar Lovett [1912] who charted the further developments of the  $n$  body problem between 1898 and 1908. Apart from the burgeoning literature on the problem by way of journal articles, the period was especially notable for the publication of Hill's *Collected Works*, Moulton's *Introduction to Celestial Mechanics* and Whittaker's *Analytical Dynamics*. Furthermore, by the end of the decade the publication of Poincaré's lectures on celestial mechanics [LMC] was almost complete, and that of the *Collected Papers* of the applied mathematician George Darwin who pioneered the quantitative study of periodic orbits was about to begin.

Significantly, part of the structure of Lovett's essay leads straight back to Poincaré. Of the five headings he used, he included one on the qualitative resolution of the problem and one on periodic solutions. More specifically, he made several references to Poincaré's methods identifying certain areas in which the influence of Poincaré's

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<sup>34</sup> Whittaker [1899, 149].



methods had been clearly felt. In particular he noted how Poincaré's preference for the canonical form of the differential equations had led to the adoption of this formulation in other investigations. Lovett also referred to developments in the theory of invariant integrals, as well as the importance of Poincaré's theorem on the non-existence of any new single-valued integrals for the problem.

With regard to particular results in [P2], some interesting observations were made by the mathematical physicist Lord Kelvin [1891] who, having had the memoir brought to his attention by Arthur Cayley, was especially struck by the relationship between some of Poincaré's results and some conclusions of his own which he had published the previous year.

In particular, he drew attention to the similarity between Poincaré's conjecture concerning the denseness of the periodic solutions [P2, 454] and a proposition of Maxwell's concerning the distribution of energy. Maxwell had proposed:

*"... that the system if left to itself in its actual state of motion, will, sooner or later, pass through every phase which is consistent with the equation of energy."*<sup>35</sup>

which, as Kelvin pointed out, was essentially equivalent to saying that every region of space would be traversed in every direction by every trajectory. If this proposition was true, which Kelvin believed to be highly likely, then he concluded it was a necessary consequence that every motion would be infinitely close to a periodic motion. In addition, he also commented on the agreement between Poincaré's results and his own results on the instability of periodic motion observing that:

*" Poincaré's investigation and mine are as different as two investigations of the same subject could well be, and it is very satisfactory to find perfect agreement in conclusions."*<sup>36</sup>.

As Brush [1966] and Gray [1992] have described, one of the first of Poincaré's ideas from [P2] to emerge in a different context was that of his recurrence theorem. This was because the theorem appeared to demonstrate the futility of contemporary efforts to deduce the second law of thermodynamics from classical mechanics. In

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<sup>35</sup> Quoted in Thomson [1891, 512].

<sup>36</sup> Thomson [1891, 512].

1896 a debate took place in *Annalen der Physik* between Ernst Zermelo, who believed that Poincaré's theorem disproved the absolute validity of the second law of thermodynamics, and Ludwig Boltzmann, who believed in the correctness of Poincaré's theorem but disputed Zermelo's application of it<sup>37</sup>. According to Zermelo, Poincaré's theorem implied that there were no "irreversible" processes at work and hence the concept of a system with continuously increasing entropy was invalid. Boltzmann's defence was that the theorem was evidence of sudden brief moments of decreasing entropy but that the statistical nature of his kinetic theory predicted that these moments would be so far apart that they would never actually be observed and so entropy would in general increase. Although Zermelo and Boltzmann's personal debate came to an end within a year, the controversy continued to arouse interest and eventually became one of the sources for the foundation of modern ergodic theory<sup>38</sup>.

Further attention was drawn to Poincaré's work on the three body problem by his compatriot and predecessor in the chair of celestial mechanics at the Sorbonne, Félix Tisserand. In the fourth and final volume of his acclaimed *Mécanique Céleste* [1896] which was published in the year of his death, Tisserand included a chapter which consisted of Poincaré's own summary [1891] of [P2] together with some further explanations about Poincaré's periodic solutions.

Various aspects of [P2] and its underlying role in the *Méthodes Nouvelles* were naturally mentioned in Poincaré's numerous obituaries<sup>39</sup>. In addition to which, volume 38 of *Acta*, which was dedicated to Poincaré, included two long articles describing his work: one on his mathematics by Hadamard and the other on his celestial mechanics and astronomy by von Zeipel, both of which placed a firm emphasis on the significance of the memoir<sup>40</sup>. Hadamard [1921] concentrated on the relationship of [P2] to Poincaré's earlier memoirs on differential equations, while von Zeipel [1921] considered its results in conjunction with the *Méthodes Nouvelles*. Of particular note is the fact that both authors quoted the passage from [MN III] where Poincaré described the complexity of the doubly asymptotic solutions. There

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<sup>37</sup> Translations of the papers by Zermelo and Boltzmann are contained in Brush [1966].

<sup>38</sup> See 9.3.4.

<sup>39</sup> See for example Baker [1914] and Darboux [1914].

<sup>40</sup> Mittag-Leffler had begun preparing *Acta* 38 soon after Poincaré's death but the outbreak of the First World War meant that publication was delayed until 1921.

is no doubt that the importance of these solutions had by this date been recognised, even if little further had been discovered about them. The fact that [P2] featured so strongly in both of these important tributes to Poincaré's career is a fitting compliment to the breadth of vision it embraced.

## 7. Poincaré's Related Work after 1889

### 7.1 Introduction

After the revision of the memoir, Poincaré channelled much of his energy into amplifying the results it contained. Within two years of its publication the first volume of his celebrated *Les Méthodes Nouvelles de la Mécanique Céleste* was published. Its appearance heralded the start of an enterprise which occupied him in part for almost twenty years. The second volume was published in 1893, and the third (and final) volume was completed in 1899. The *Méthodes* was followed by its didactical counterpart, the *Leçons de Mécanique Céleste*. The *Leçons* were based on Poincaré's lectures given at the Sorbonne in his role as professor of mathematical astronomy and celestial mechanics, a position to which he had succeeded on the death of Tisserand in 1896. They contained a treatment of perturbation theory, the lunar theory, and the theory of tides, and were published in three volumes between 1905 and 1910, although according to Sarton [1913, 10] the project was unfinished.

As well as producing these major works, Poincaré also published several more papers on different topics in celestial mechanics, some of which were connected to ideas which had appeared in [P2] and the *Méthodes*, and some of which were in response to the work of other mathematicians. There were, for example, several papers on the expansion of the perturbation function, two notes connecting the principle of least action with the theory of periodic solutions, as well as papers on the form of the



equations in the three body problem. Other related papers included a correction to Bruns' theorem on the integrals of the three body problem, discussions of Gylden's *horistic* methods, as well as some general articles.

There were also two important papers in which Poincaré continued his research into the periodic solutions of the three body problem outside the specific context of celestial mechanics. The first of these, which he originally presented to the American Mathematical Society at the St Louis Congress in 1904, was an investigation into the geodesics on a convex surface. In this paper the discussion centred on the closed geodesics since they enjoy an analogous role to the periodic solutions in the three body problem. The second was the paper in which he announced what is today called his *Last Geometric Theorem*. This paper became well known not only because of the importance of the theorem it contained, but also because, despite strenuous efforts and having treated a variety of special cases, Poincaré had been unable to provide a general proof.

## 7.2 "Les Méthodes Nouvelles de la Mécanique Céleste"

### 7.2.1 Introduction

When George Darwin was describing the *Méthodes Nouvelles* on the occasion of presenting the medal of the Royal Astronomical Society to Poincaré in 1900, he said:

*"It is probable that for half a century to come it will be the mine from which humbler investigators will excavate their materials."*<sup>1</sup>.

With the benefit of hindsight it is now possible to say that had Darwin omitted the word "half" his prediction would still have been fulfilled. Since its publication almost a hundred years ago, Poincaré's *Méthodes Nouvelles* has continued to attract and delight mathematicians, providing a rich and varied source for researchers in celestial mechanics and dynamical systems<sup>2</sup>. Moreover, it is largely through the *Méthodes Nouvelles* that the contents of [P2] have become so widely known, for it contains the principal ideas from [P2] but in a more fully explained and developed form. A greater number of applications of the theory are included, as well as a

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<sup>1</sup> Darwin [1900, 412].

<sup>2</sup> See the Foreword by J. Kovalevsky to the Blanchard edition of [MN I], 1987.

substantial amount of new material. Perhaps most notably, the focus of attention is as much on the general three body problem as on the restricted problem.

Volume I, which was published in early 1892, essentially covered the analytical part of the theory. Of the topics previously discussed in [P2], it contains an amplified treatment of the periodic solutions, characteristic exponents, asymptotic solutions and the non-existence of new single-valued integrals. In addition, there is a long chapter on the expansion of the perturbation function.

The second volume which appeared in the following year was devoted to the methods of contemporary astronomers, namely Newcomb, Gylden, Lindstedt and Bohlin. Most of the material was completely new. There is an overlap with [P2] in the discussion of the divergence of Lindstedt's series, and the reference to Bohlin's series which Poincaré made in the *Introduction* to [P2] is clarified.

The final volume, which is characterised by Poincaré's geometrical ideas, was published in 1899. Here Poincaré returned to the subjects of invariant integrals, stability, periodic solutions of the second class and doubly asymptotic solutions, and he also included a discussion of what he now called periodic solutions of the second species, the existence of which he had conjectured at the end of [P2].

### 7.2.2 Volume I

In the opening chapter of Volume I Poincaré provided a fuller introduction to both the general and the restricted three body problems than he had done in [P2]. This included placing a greater emphasis on the role of the Hamiltonian form of the equations

$$\frac{dx_i}{dt} = \frac{\partial F}{\partial y_i}, \quad \frac{dy_i}{dt} = -\frac{\partial F}{\partial x_i}, \quad (7.2.i)$$

$$F = F_0 + \mu F_1 + \mu^2 F_2 + \dots$$

by giving an outline of Hamilton-Jacobi theory and showing how the number of independent variables are reduced through the use of the classical integrals.

The earlier treatment of periodic solutions is enhanced by the inclusion of several new applications of the theory, and Poincaré's conviction as to the importance of these solutions is encapsulated in his now renowned description in which they are described as:

*"... the only breach by which we can penetrate a fortress hitherto considered inaccessible."* [MN I, 82].

Furthermore, the conjecture concerning the denseness of the periodic solutions is more strongly affirmed. Poincaré now proposed that given any particular solution of equations (7.2.i) it should be possible to find a periodic solution (which may have an extremely long period) such that the difference between these two solutions is as small as desired for any given length of time [MN I, 82]<sup>3</sup>.

The classification of the three different kinds of periodic solutions first described in [1884a] are reintroduced and the conditions under which they exist are carefully described. The first kind, in which the inclinations are zero and the eccentricities very small, are the analytic continuation of the solutions of the circular two body problem; the second kind in which the inclinations are zero and the eccentricities finite, are generated from the solutions of the elliptic two body problem; and the third kind, in which the inclinations are finite and the eccentricities very small, are generated from an elliptic solution not in the same plane as the motion of the primaries. In other words, all Poincaré's periodic solutions are solutions which are the analytic continuation of solutions of the two body problem and hence are only valid for small values of the mass parameter.

In [P2] Poincaré had proved the existence of periodic solutions in the restricted problem which was equivalent to proving the existence of periodic solutions of the first kind. However, his proof depended on the non-vanishing of a particular Hessian, a condition which did not hold for the general three body problem. Thus in order to establish the existence of periodic solutions of the second and third kinds he first had to establish the conditions under which periodic solutions would exist when the Hessian was equal to zero<sup>4</sup>.

One issue which Poincaré did not raise in [P2] was the actual use of his periodic solutions. Since his analysis had shown that the probability of the occurrence of such periodic solutions was negligible, what practical purpose could they serve? He now made it clear that their practical value lay in the fact that they could be used as a starting point for approximating other solutions rather than as actual solutions themselves. In the case of solutions of the first kind, apart from Hill's solution, he

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<sup>3</sup> Schwarzschild [1898] in his endeavour to prove the conjecture gave a phase space interpretation in which he proposed that arbitrarily close to any point in phase space there is a point representing a periodic solution. A rigorous proof was eventually provided by Hopf [1930].

<sup>4</sup> Szebehely [1967, 437] notes that Poincaré's proof of the existence of periodic solutions of the second kind contains an error which was pointed out by Wintner [1931]. See also Sternberg [1969, II, 275], and Siegel and Moser [1971, 182].



identified as particular examples both Laplace's theory of the satellites of Jupiter, and Tisserand's study of the motion of Hyperion, a satellite of Saturn<sup>5</sup>.

With regard to the theory of asymptotic solutions, Poincaré followed the same reasoning as in [P2] but provided a more complete theory elaborating on certain points which had previously been left unexplained.

In his discussion of the question of the non-existence of any new single-valued integral, Poincaré made significant changes to his argument in [P2] in order to accommodate the general three body problem. As in [P2], he began by assuming the existence of another independent integral  $\Phi$ , but this time he introduced Poisson brackets. As he observed, the existence of  $\Phi$  implies the condition  $[F, \Phi] = 0$ , and this Poisson bracket can be expanded to give

$$[F_0, \Phi_0] + \mu([F_1, \Phi_0] + [F_0, \Phi_1]) + \dots + \dots = 0.$$

If this expression is true, then each of the Poisson brackets is equal to zero. Poincaré's strategy was to prove the invalidity of this expression. He first proved that  $\Phi_0$  is not a function of  $F_0$ , and moreover that it is independent of  $y$ . Thus since  $F_0$  is also independent of  $y$

$$-\sum \frac{\partial \Phi_0}{\partial x_i} \frac{\partial F_1}{\partial y_i} + \sum \frac{\partial F_0}{\partial x_i} \frac{\partial \Phi_1}{\partial y_i} = 0,$$

and since  $F_1$  and  $\Phi_1$  are both periodic with respect to  $y$  they can be expanded as exponentials in the form  $\exp[\sqrt{-1}(m_1 y_1 + \dots + m_n y_n)]$  where  $m_i$  are positive or negative integers. Considering  $F_1$  and  $\Phi_1$  in their exponential form leads to the equation

$$B \sum m_i \frac{\partial \Phi_0}{\partial x_i} = C \sum m_i \frac{\partial F_0}{\partial x_i}$$

where  $B$  and  $C$  depend only on  $x$ . This equation shows that the Jacobian of  $F_0$  and of  $\Phi_0$  with respect to any two  $x_i$  must vanish, providing  $B$  does not vanish, and that this occurs for all values of  $x$  such that  $\frac{\partial F_0}{\partial x_i}$  are commensurable. Thus in any domain,

however small, there is an infinite system of values of  $x$  for which the Jacobian vanishes, and since the Jacobian is a continuous function, it must vanish identically. But the vanishing of the Jacobian implies that  $\Phi_0$  is a function of  $F_0$  which is contrary to the original assumption, and so the equations cannot admit any other

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<sup>5</sup> Although Poincaré gave no practical illustrations of periodic solutions of the second and third kinds, these are found in nature as, for example, in the motion of comets.



single-valued integral. To complete the proof Poincaré also dealt with the case where one or more of the coefficients  $B$  vanish, as well as the case where the function  $F_0$  does not depend on all the variables  $x_i$ .

Finally, he applied these results to the three body problem, dealing first with the restricted problem, then the planar three body problem and finally the general problem. In the latter two cases, he found necessary but not sufficient conditions for the existence of another integral of the equations. He then proved that these conditions, which were in the form of relations between the coefficients in the expansion of the perturbation function  $F_1$ , did not exist, and hence that there are no new transcendental or algebraic integrals for the three body problem, providing  $\mu$  is sufficiently small.

In the case of the general problem, proving that these conditions did not exist required a more detailed analysis and in order to complete it Poincaré was led into a discussion of the perturbation function. It was not a topic which had arisen in [P2], although he had drawn attention to its role in a note in the *Comptes Rendus* [1891b]. Briefly, when the mean motions are incommensurable then, due to the presence of small divisors, certain terms in the perturbation function, independent of their order, may acquire relative importance. It is not generally necessary to calculate these terms exactly since what is important is to recognise whether they are negligible or not, and for this purpose an approximate value will suffice. Poincaré was therefore looking for an approximate expansion of the function and to this end his analysis was concerned with what he defined as the principal part of the function. He proceeded by recalling Darboux's method for finding the coefficients of high order terms in a Fourier or Taylor series for functions of a single variable which can be applied when the analytic properties of the functions represented by the series are known. He next extended the method to accommodate functions of two variables in order to apply it to the perturbation function and thereby derive an approximate value for the principal part of the function.

As Poincaré himself observed there was a sense in which his result concerning the non-existence of integrals was more general than the one given earlier by Bruns [1887]. For while Bruns had proved that the ten classical integrals were the only independent algebraic integrals of the three body problem, Poincaré had not only proved the non-existence of any new transcendental integral but he had also shown that the integral could not remain single-valued in a restricted domain. Nevertheless, Poincaré did also admit that there was a sense in which Bruns' result

was more general than his, since Bruns had shown the non-existence of an integral for any value of the masses, whereas his method was only valid for sufficiently small values of the masses<sup>6</sup>.

### 7.2.3 Volume II

Throughout his researches Poincaré had become increasingly aware of the differences which had evolved between the perceptions of mathematicians and astronomers as to what constituted a rigorous solution to a problem in celestial mechanics. As indicated in 6.3, this difference largely manifested itself through their respective understanding of the concept of convergence and, as illustrated by the controversy between Gylden and Poincaré, this difference sometimes led to what appeared to be inconsistent results, which were followed by disagreements between the different proponents. Poincaré now tackled this problem by making a close analysis of the principal methods for solving the equations of motion which were currently in use by astronomers, and carefully explaining the reasons for the discrepancies between the results derived in each of the disciplines. The outcome was an account of the astronomers' methods presented to facilitate comparison between them rather than to make them amenable to numerical calculation.

Each of the methods which Poincaré had selected to discuss represented an attempt to expand the coordinates in the planetary theory as series in which all the terms are periodic, the secular terms having been eliminated. He recognised that common to all the methods there was one question which required very careful scrutiny: the question of the convergence of the series derived.

For a mathematician before a series can be described as convergent it has to be rigorously proved to be so, whereas for the practical purposes of an astronomer, a series may be considered convergent if the first, say, 50 terms decrease sufficiently rapidly, with no account being taken of any later terms. To quote Poincaré's example, given the two series

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<sup>6</sup> Painlevé [1897a, 1898] gave an extension to Bruns' theorem which showed that apart from the classical integrals the  $n$  body problem has no other integrals which are algebraic functions of the velocities. In [1900] he made the corresponding extension to Poincaré's theorem. Bruns' and Poincaré's theorems were more restrictive in that they only allowed for integrals which are functions of both the coordinates and the velocities.

Cherry [1924] proved that Poincaré's theorem no longer holds if the restriction that the integral must be expanded in powers of  $\mu$  is relaxed.

Wintner pointed out [1941, 97, 241] that since Bruns' and Poincaré's results are only valid for unspecified values of the masses they are void of actual dynamical interpretation.

$$\sum \frac{(1000)^n}{n!} \quad \text{and} \quad \sum \frac{n!}{(1000)^n}$$

a mathematician would consider the first to be convergent and the second divergent, whereas an astronomer would label them conversely [MN II, 1]. Nevertheless, both approaches have a validity in their respective domains: the first in theoretical research and the second in numerical application, the point is that it is essential to know which domain is being considered before a decision is made on the approach.

As Poincaré had made clear in his introduction, from a practical point of view, the question of whether the series was actually convergent or not was not the important issue. An asymptotic series, although divergent, can provide a very good approximation to a function and can be of great practical value. What is important is to have an idea of the upper limit of the error involved in using such series, and to appreciate that these series cannot be used to establish theoretical results such as the stability of the solar system. Thus Poincaré's objective in making this distinction was not in any way aimed at devaluing the work of astronomers but rather his hope was to clarify an area of possible misunderstanding. Indeed, he emphasised the legitimacy of asymptotic expansions in practical work, quoting results from his earlier paper on the topic [1886a].

Poincaré began with Lindstedt's method, having acknowledged its equivalence with the method put forward by Newcomb in [1874]<sup>7</sup>. Using Hamilton-Jacobi theory he generalised the method by developing a full canonical analogue which included results from his earlier paper [1889]. He also identified and resolved two particular difficulties with the method. The first, which he had previously described in the *Comptes Rendus* [1892], concerned the condition that for the method to be valid there should be no linear relationship between the mean motions. But, as he observed, in the three body problem the mean motions are not only those of the two planets but also those of the perihelions and nodes, and in the first approximation, that is in Keplerian motion, the perihelion and node are fixed and the mean motions are therefore zero and the condition is therefore not fulfilled. He showed how this property could be taken into account by making an appropriate change of variable. The second problem was related to the magnitude of the eccentricities, the squares of which enter into the denominators of the expansions and so cause problems when

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<sup>7</sup> As a result of Poincaré's treatment of Lindstedt's method in the *Méthodes Nouvelles*, the method has now become a well-established perturbation method in applied mathematics. See Arnold [1988, 175-179].



they are very small. Poincaré countered this difficulty by taking a periodic solution as a starting point rather than a Keplerian ellipse.

With regard to the divergence of Lindstedt's series, Poincaré, perhaps with Weierstrass' comments in mind, went into the question in a more detailed way than he had in [P2]. In his discussion of the method itself he had shown that the Hamiltonian equations (7.2.i) can be satisfied by series of the form

$$x_i = x_i^0 + \mu x_i^1 + \mu^2 x_i^2 + \dots \quad (i = 1, \dots, n) \quad (7.2.ii)$$

$$y_i = w_i + \mu y_i^1 + \mu^2 y_i^2 + \dots$$

where the coefficients  $x_i^k$  (or  $y_i^k$ ) are periodic functions of  $w_i = n_i t + \varpi_i$ , and the frequencies are given by the series

$$n_i = n_i^0 + \mu n_i^1 + \mu^2 n_i^2 + \dots,$$

and, in the case of two degrees of freedom, the coefficients  $x_i^k$  can be represented by series of the form

$$x_i^k = A_0 + \sum B_{m_1 m_2} \frac{\sin(m_1 w_1 + m_2 w_2 + h_{m_1 m_2})}{m_1 n_1^0 + m_2 n_2^0} \quad (7.2.iii)$$

To prove that the series do provide a valid solution to the differential equations, it is a question of proving the convergence of both the series (7.2.ii) and (7.2.iii). If the series (7.2.iii) are uniformly convergent, then (as Poincaré had shown in [1884b]) the absolute value of the coefficients

$$\frac{B_{m_1 m_2}}{m_1 n_1^0 + m_2 n_2^0} \quad (7.2.iv)$$

must be bounded, in which case  $n_1^0$  and  $n_2^0$  must be incommensurable unless of course  $B = 0$ . Nevertheless, even if the frequencies are incommensurable the series can still be divergent since it is always possible to find a  $B$  such that the denominator is as small as desired and the absolute value of the coefficients (7.2.iv) is therefore not bounded. But, on the other hand, it is also possible to choose the values of  $n_1^0$  and  $n_2^0$  so that the series are convergent, and Poincaré gave the example of the case where the ratio of the frequencies is incommensurable but the square is commensurable. Furthermore, since the frequencies are determined by observation, they can only be given to within a certain approximation, and therefore it is always possible to choose the frequencies so that the series are convergent and at the same time remain within the limits of the approximation. The next question is then whether the series are convergent for all values of the constants of integration  $x_i^0$  within a certain



interval (since the  $n_i^0$  depend on the  $x_i^0$ ). It turns out that although in general this is not the case, in practice it is always possible to restrict sufficiently the calculation so that the series (7.2.iii) are only composed of a finite number of terms and the series (7.2.ii) can then be formed.

It therefore remains to discuss the convergence of series (7.2.ii). In this case, there are two questions to consider. First, are the series uniformly convergent for all values of  $\mu$  and  $x_i^0$  within a certain interval? And second, are the series uniformly convergent for all sufficiently small values of  $\mu$  for suitably chosen values of  $x_i^0$ ? Poincaré showed that the answer to the first question was no, i.e. that the series were divergent. With regard to the second he distinguished between the case when the frequencies depend on the parameter  $\mu$  and the case when the frequencies are independent of  $\mu$  as above. In the first case, he observed that for sufficiently small values of  $\mu$  it was always possible to find values of  $\mu$  such that the frequencies were rationally related (since the ratio is a continuous function of  $\mu$ ). The series then represent a periodic solution of the Hamiltonian equations for all values of the two constants of integration  $\omega_i$ . Hence if the series are convergent then corresponding to this ratio there are a double infinity of periodic solutions. But as Poincaré had previously shown both in [P2] and in [MN I], this only occurs in very exceptional cases. As a result he concluded that the series (7.2.ii) were not convergent but with the important caveat that:

*"The preceding argument does not suffice to establish this point with complete rigour."* [MN II, 103]<sup>8</sup>.

When the frequencies are independent of the parameter, the question is whether values of  $x_i^0$  can be chosen so that the series are convergent. In this situation the choice of the values of  $x_i^0$  can be made by imposing some condition on the ratio of the frequencies such as, for example, its square being rational. But in this case Poincaré was even more non-committal. All he would say was that:

*"The arguments presented in this Chapter do not allow me to affirm that this (i.e. that the series are convergent) cannot happen. They only allow me to say it is very unlikely."* [MN II, 105].

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<sup>8</sup> Moser [1973, 9] mistakenly ascribes this quote to the case where the frequencies are independent of the parameter.

Thus although Poincaré had reached fundamentally the same conclusion with regard to the divergence of the series as he had in [P2], his approach was altogether more thorough and, importantly, he had cast a shadow of doubt over his results in the cases where the frequencies can be chosen in advance. Nevertheless, despite Poincaré's note of caution it was generally accepted that he had proved the divergence of the series with the result that it looked as if Weierstrass must have been wrong. From what Poincaré had shown, there did not appear to be a set of conditions under which the series were in general convergent. But, almost seventy years later, it was shown that Poincaré's reservations were indeed prescient, for, contrary to expectations, the question was finally resolved in Weierstrass' favour by Kolmogorov, Arnol'd and Moser whose contribution is outlined in the Epilogue.

With regard to the work of Gylden, Poincaré centred on the integration of the particular form of Hill's equation given by

$$\frac{d^2x}{dt^2} = x(-q^2 + q_1 \cos 2t)$$

which Tisserand [1894, Chapter I] had called the Gylden-Lindstedt equation.

Poincaré described not only Gylden's method of integration but also the methods of Bruns, Hill and Lindstedt. He also discussed the more difficult non-linear equation of evection<sup>9</sup>

$$\frac{d^2x}{dt^2} + x(q^2 - q_1 \cos 2t) = \alpha \phi(x, t)$$

which had also been extensively analysed by Gylden.

In the final part of the volume Poincaré considered the problem of small divisors and turned to a method devised by Bohlin. Bohlin's method was essentially an improved version of Delaunay's in that it involved the same basic ideas but without the inconvenience of numerous changes of variable. However, although the method did successfully eliminate small divisors it had the disadvantage of generating the reciprocal problem of large multipliers. The analogy between Bohlin's series and Poincaré's series for the asymptotic solutions of the restricted problem referred to in the introduction to [P2] is here clear to see<sup>10</sup>. Moreover, to prove the divergence of Bohlin's series Poincaré used the same example as the one he had used to prove the

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<sup>9</sup> The evection is the largest lunar perturbation and is caused by the periodic variation in the eccentricity of the lunar orbit.

<sup>10</sup> See 5.3.2 and 5.6.7.

divergence of his own series. In the final chapter Poincaré extended Bohlin's method in order to eliminate some of the difficulties which arise when the basic method is applied to the three body problem.

In this second volume Poincaré forcefully demonstrated the importance of understanding the nature of the convergence of the different series used in the expressions for the coordinates of the planets. He identified the respective advantages and disadvantages of each of the different methods for obtaining these series, while at the same time making improvements and corrections. He recognised Newcomb's and Lindstedt's methods to be the simplest, and in particular recommended them to be used when there was not a problem with small divisors. His verdict on Gylden's methods was that although they were too complex to be of any real practical help, they were extremely valuable both in terms of the insight they lent to particular problems and in terms of their use in overcoming specific difficulties. In the case where the mean motions give rise to the problem of small divisors, then Poincaré's analysis had shown that it was necessary to use Delaunay's or Bohlin's methods. In particular, in the case of the three body problem Poincaré preferred Bohlin's methods, which were similar to his own. Most important of all, in contrast to what their authors' had assumed, he had shown that most of these series were not convergent but were instead asymptotic expansions<sup>11</sup>. The methods of Newcomb, Lindstedt and Gylden resulting in series in  $\mu$ , and Bohlin's method resulting in series in  $\sqrt{\mu}$ .

#### 7.2.4 Volume III

In the final volume of the *Méthodes Nouvelles* Poincaré concentrated on the geometric aspects of his investigations. The first third of the volume is devoted to the theory of invariant integrals where the topic is given a much improved and more logical structure than in [P2]. Poincaré applied the theory to the general three body problem and concluded with a table detailing the number of invariants in the different formulations of the problem. He also included a long discussion on invariant integrals and asymptotic solutions in which he proved that in the case of

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<sup>11</sup> Baker [1915] proved that the formal series solution to the equation

$$\frac{d^2x}{dt^2} + x(1 + \Psi) = 0, \quad \Psi = A \cos \alpha t + B \cos \beta t,$$

where  $A$  and  $B$  are small,  $\alpha$  and  $\beta$  are incommensurable, and  $A$  and  $B$  have a small common factor  $\mu$ , was not, as Poincaré had claimed [MN II, 277], divergent.



the Hamiltonian equations it is extremely unlikely that there exist any other algebraic or quadratic invariant integrals other than those he had already found, showing how this fundamental property is related to the non-existence of any new integrals for the equations. With regard to the geometric theorems which had been established at the end of the chapter on invariant integrals in [P2], he included these in a separate chapter on the theory of consequents where he gave them a more comprehensive treatment than in [P2].

On the question of stability in the three body problem, Poincaré stated three sufficient conditions:

1. The bodies can never get infinitely distant from each other;
2. The mutual distances between the bodies is never less than a given limit;
3. The system returns infinitely often arbitrarily close to its initial position;

As he observed, Hill had already proved that the first condition was satisfied in the case of the restricted problem. He then left on one side the second condition, and went on to establish, via his recurrence theorem, as he had done in [P2], that the third condition is generally satisfied for the restricted problem, although the result cannot be extended to the general problem.

He gave periodic solutions of the second class, those periodic solutions which make more than one orbit around the primary, a more extensive analysis than in [P2]. In particular he developed the connection between these solutions and the principle of least action, a topic which he had introduced in two notes in the *Comptes Rendus* [1896c, 1897]. In the first of these notes he had shown that this principle could be used to infer the existence of different kinds of periodic solutions, where the law of attraction is some inverse power of the distance higher than the square. In the second note he used the principle to distinguish between two different types of unstable periodic solutions, showing that if the constants of motion are varied continuously it is impossible to move directly between the two types of unstable periodic solution without passing through a stable periodic solution.

Poincaré also related his study of the periodic solutions of the second class to the periodic solutions discovered by Darwin through numerical analysis. He found that almost all of Darwin's results were in accordance with his own theory. The one discrepancy concerned the stability of a certain family of Darwin's periodic



solutions where Poincaré identified an error in Darwin's theory. This is explained in 8.4.1 in the discussion of Darwin's work.

At the end of [P2] Poincaré had postulated the existence of a system in which two very small bodies are describing orbits around one large body and the orbits are such that collisions almost take place at definite intervals. These orbits would then be ellipses with elements which remain almost constant except near each "collision" point where they suddenly change dramatically. In other words, Poincaré considered the possibility that all the elements in the system could vary in such a way that the motion was periodic. He now investigated this idea further.

He considered equations (7.2.i) with  $p$  degrees of freedom, with periodic solutions of period  $T$  such that when  $t$  is increased by  $T$ , the variables  $y_1, \dots, y_p$ , increase by  $2k_1\pi, \dots, 2k_p\pi$  respectively, where  $k_1, \dots, k_p$  are any integers. In the case of the three body problem  $y_1, \dots, y_6$ , represent the mean longitudes, the perihelions and the nodes of the planets, and  $F_0$  depends only on the variables  $x_1$  and  $x_2$  which are proportional to the square roots of the major axes. A solution will then be periodic if the differences between the  $y$  increase by multiples of  $2\pi$  as  $t$  increases by a period  $T$ , and in this case  $F$  only depends on these differences. If  $2k_1\pi, \dots, 2k_5\pi$  are the increases in

$$y_1 - y_6, \quad y_2 - y_6, \quad y_3 - y_6, \quad y_4 - y_6, \quad y_5 - y_6,$$

as  $t$  increases by a period, then, as Poincaré had previously established in Volume I, there are periodic solutions for arbitrary values of  $k_1, k_2$  providing  $k_3, k_4$  and  $k_5$  are zero. Poincaré now considered the idea of solutions in which the five integers  $k$  take arbitrary values.

From considerations of continuity and the fact that very little modification is required to the function  $F$  in order to regain the original Hamiltonian equations it seems likely that such solutions do exist. But when the mass parameter  $\mu$  is zero the two planets follow Keplerian orbits and it appears then that  $k_2, k_4$  and  $k_5$  must be zero. In order to counter this difficulty Poincaré assumed that the two planets describe almost Keplerian orbits except at a certain moment when their mutual distance becomes small enough to produce a strong perturbation, as a result of which their perihelia and nodes change by large amounts. For such orbits the perihelia and nodes are certainly not fixed and so for  $\mu = 0$ ,  $k_2, k_4$  and  $k_5$  are therefore not zero. Although Poincaré then claimed that his procedure was sufficient to prove the

existence of such orbits which he called periodic orbits of the second species, it is now not clear that his claim is justified<sup>12</sup>.

The final chapter of Volume III was devoted to a discussion of doubly asymptotic solutions. This contained essentially the same analysis as in [P2] but with one important addition. In [P2] Poincaré had shown that corresponding to each unstable periodic solution there is a system of asymptotic solutions, the sets of which form asymptotic surfaces, and the intersection of these asymptotic surfaces with a transverse section forming an asymptotic curve. He had distinguished two families of asymptotic solutions, one family which approached the periodic solution as  $t \rightarrow -\infty$  and one family which approached it as  $t \rightarrow +\infty$ . He had then proved that that two asymptotic curves can only intersect if they come from different families and if such an intersection should occur then this defined a doubly asymptotic solution.

However, in [P2] Poincaré had only considered the possibility of doubly asymptotic solutions arising from different families of asymptotic solutions associated with the same unstable periodic solution. Now he proposed the idea of doubly asymptotic solutions arising from asymptotic solutions associated with two different unstable periodic solutions. To distinguish between the two different types he called the former *homoclinic* solutions and the latter *heteroclinic* solutions.

He established the existence of homoclinic solutions in the restricted three body problem, as he had done in [P2], but this time he added an unequivocal statement about their bewildering complexity:

*"When one tries to depict the figure formed by these two curves and their infinity of intersections, each of which corresponds to a doubly asymptotic solution, these intersections form a kind of net, web, or infinitely tight mesh; neither of the two curves can ever intersect itself, but must fold back on itself in a very complex way in order to intersect all the links of the mesh infinitely often.*

*One is struck by the complexity of this figure that I am not even attempting to draw. Nothing can give us a better idea of the complexity of the three-*

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<sup>12</sup> Levy [1952] questions the sufficiency of Poincaré's proof and observes that P. Semirot in his thesis (1943) gives some counterexamples.

*body problem and in general all the problems of dynamics where there is no single-valued integral and Bohlin's series diverge."* [MN III, 389].

Poincaré concluded with a discussion of the heteroclinic solutions in which he proved, as he had done in the homoclinic case, that the existence of one solution of this kind is sufficient to prove the existence of an infinite number.

The difficulties that Poincaré had encountered in trying to understand doubly asymptotic solutions is evident from the fact that almost ten years had passed since he had first introduced the idea in [P2], and despite the time interval he had added relatively little to his original discussion bar the inclusion of heteroclinic solutions. It seems likely that the reason that he did not consider the possibility of the latter in [P2] was because he had discovered the homoclinic solutions as a result of realising that the asymptotic surfaces arising from one unstable periodic solution were not closed but intersecting. Thus it would have been quite natural for him only to consider the implications of this particular intersection. In any case since he had found this result sufficiently shocking in itself, it is perhaps not surprising that he did not consider the possibility of any even more complex solutions. For a homoclinic solution only involves one curve folding back on itself without self-intersecting while simultaneously cutting another curve infinitely often, but a heteroclinic solution involves two curves folding back on themselves and is as a result correspondingly more complicated. It is therefore no wonder to find that contemporary reviewers of the *Méthodes Nouvelles* had nothing to say about Poincaré's analysis of these solutions, except to reiterate his words about their complexity!

FIG. 7.2.i, which is a modern representation of Poincaré's homoclinic and heteroclinic solutions, has been included to give an indication of the complex behaviour of these solutions. They represent the intersections of the asymptotic surfaces with a transverse section showing the solutions in the early stages of their development.  $S$  and  $S'$  are the unstable periodic solutions,  $C$  and  $C'$  are two asymptotic curves corresponding to  $S$ ,  $D$  and  $D'$  are two asymptotic curves corresponding to  $S'$ , and  $P$  and  $Q$  are what today are known as a homoclinic and a heteroclinic point respectively.

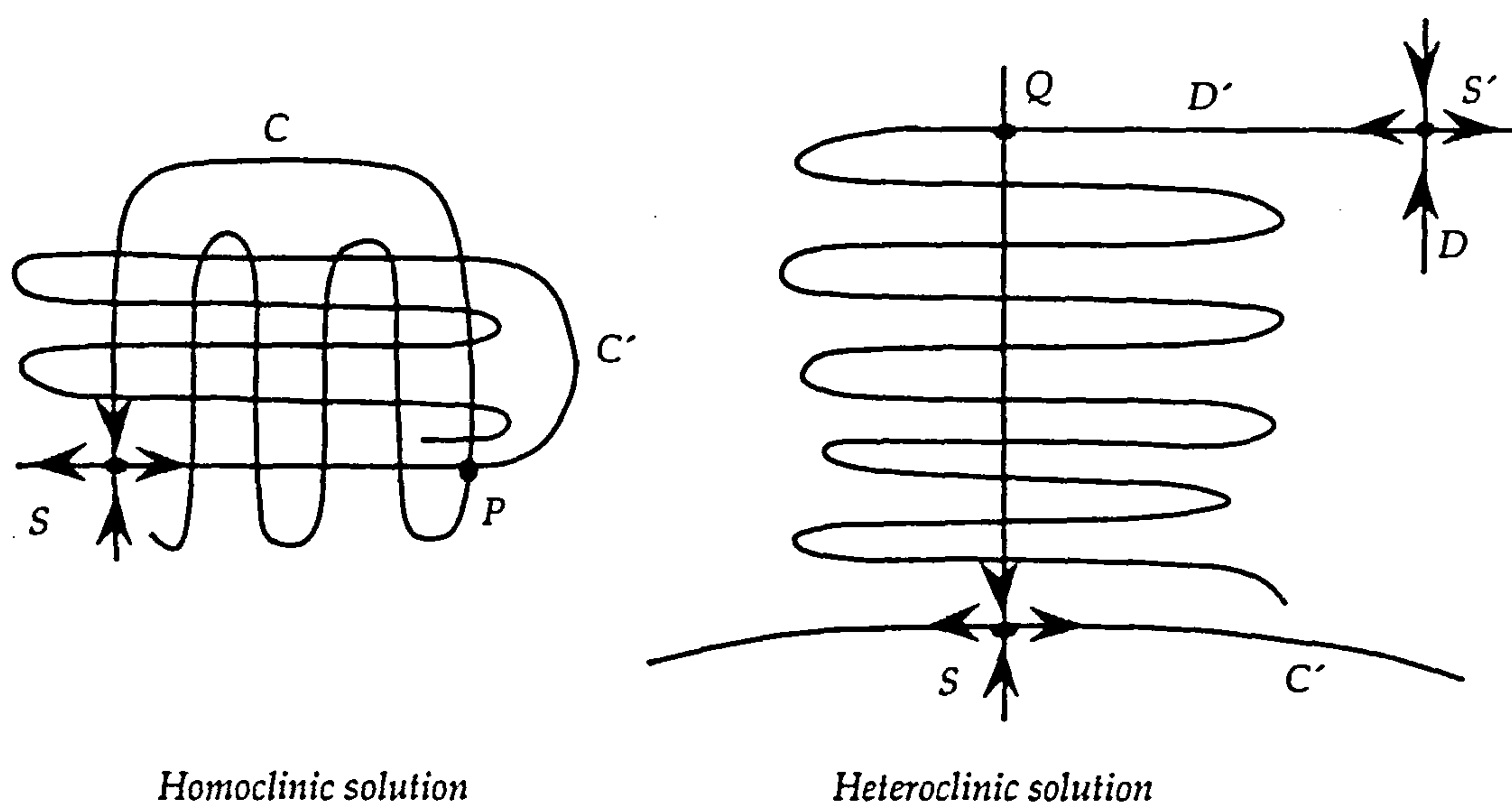


FIG. 7.2.i.

### 7.2.5 Reviews

An enthusiastic review of [MN I] was given by Brown [1892] in the first volume of the *Bulletin of the New York Mathematical Society*, in which he welcomed the appearance of a mathematical treatise on the problems of celestial mechanics while at the same time recognising the wider applications of Poincaré's methods. In particular, he was impressed by Poincaré's "*penetrative genius*" for dealing with convergence arguments, and Poincaré's treatment of periodic solutions with its application for the lunar theory. Brown successfully managed to communicate in a concise style unencumbered by mathematical detail the essence of most of Poincaré's ideas, although, whether due to a time constraint (the review was in print by the middle of 1892) or whether because he was unfamiliar with the mathematics, he glossed over the last two chapters, the one on the perturbation function and the one on the theory of asymptotic solutions, only signalling the titles and giving no indication of the contents.

Whittaker's [1899] report contains a brief review of all three volumes of the *Méthodes Nouvelles* but due to similarities with [P2], his observations are mostly confined to a brief outline of the new results in the second volume. As with his commentary on [P2] there is nothing in the way of subjective discussion, but



nevertheless it is surprising to find that there is no real indication of the stature of the work.

An extremely detailed synopsis of [MN I] was given by Perchot [1899], presumably prompted by the appearance of [MN III], who noted Poincaré's emphasis on the importance of questions of convergence. Although Perchot went into considerable mathematical detail, he essentially reproduced Poincaré's own arguments without providing any additional explanations. Since he made almost no comment on either Poincaré's methods or his results, his review provides no new insights into any of the material or how it was received. Maybe he felt it was not his position to pass judgement on Poincaré or maybe he simply did not have enough confidence in his own ability to deal with the mathematics.

Maurice Hamy [1892, 1896, 1900], an astronomer at the Observatory in Paris, provided coherent and concise reviews of all three volumes of [MN]. Clearly meant for a general scientific audience, these reviews conveyed the spirit of Poincaré's ingenuity without getting lost in the mathematical detail.

### 7.3 The three body problem and celestial mechanics

Of Poincaré's many other papers on the three body problem and celestial mechanics which he produced after [P2], several heralded results which later appeared in a volume of [MN] and several were refinements to results which had already been published. Included amongst these were several papers on the expansion of the perturbation function, and others on the calculation and convergence of the series used to integrate the differential equations of the three body problem. These will not be discussed here but they can be found in Volumes VII and VIII of the Poincaré *Œuvres*.

#### 7.3.1 Bruns' theorem

In [1896b] Poincaré added further to his work on the question of the non-existence of new integrals of the three body problem by making a correction to Bruns [1887] theorem in which Bruns had proved the non-existence of any new single-valued algebraic integral for the general three body problem for all values of the mass parameter.

The mistake in Bruns' paper related to an expression which Bruns had believed to be an exact differential, a condition which had to hold for his theorem to be true.

Poincaré's correction involved giving a simple counter example which demonstrated that the condition did not hold in the generality which Bruns had described. However, Poincaré's discovery did not in fact invalidate Bruns' result, for he went on to prove that although it was possible for functions to exist which did not satisfy the exact differential condition but which satisfied all the other necessary conditions for Bruns theorem to be true, these particular functions could not arise from the three body problem. Thus, although Bruns' argument had been technically incorrect, his conclusion regarding the integrals of the three body problem was in fact valid.

Characteristically, Poincaré did not elaborate on the details of his method and only sketched his proof in the broadest outline. It was clearly a delicate analysis, for not only was a detailed proof supplied by Forsyth found by Whittaker to contain an error, but Whittaker's own proof, although rectifying Forsyth's error, also included an error. The latter was pointed out by MacMillan [1913] in his discussion of Poincaré's correction.

### 7.3.2 Gylden's horistic methods

Poincaré's discussion of Gylden's horistic methods can be traced through a series of notes in the *Comptes Rendus* culminating in a memoir in *Acta* [1905a]. Beginning in [1901] he raised objections to Gylden's first horistic method, and then in [1904] turned his attention to the second method. In the latter his investigations showed that Gylden's method could not, as Gylden had claimed, be used to determine the general solution to the differential equations, although with certain modifications it could be used to determine a particular periodic solution. In addition he proved the falsity of Gylden's conclusion that high order terms in the perturbation function could never cause libration. Shortly afterwards, Bäcklund, who after Gylden's death in 1896 had been given the responsibility for editing Gylden's manuscripts<sup>13</sup>, called into question Poincaré's results, which in turn elicited a response from Poincaré [1904a]. Poincaré brought together his ideas on Gylden's theory in a more comprehensive form in [1905a].

Although Poincaré began [1905a] by pointing out the great service Gylden had done to celestial mechanics by the creation of new methods which had been used to great effect in, for example, the theory of small planets, he further observed that these

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<sup>13</sup> See Whittaker [1899, 144].

had now been largely superceded by more convenient methods such as those of Hill and Brown, and he was unable to speak so kindly of his later work. The paper is essentially a criticism of the method which Gylden had developed in order to overcome the problem of small divisors and which Gylden had described in a long paper on the convergence of series used in celestial mechanics [1893].

Since the usual methods used to solve problems in celestial mechanics result in solutions with certain terms which have coefficients of the form  $b/p^2$  which become infinite when  $p$  vanishes, it is clearly desirable to try to improve the methods so that they do not result in terms of this type. What Gylden tried to prove was that if when using the traditional methods a more exact calculation is made then these terms would not arise and instead there would be what he called horistic terms, these being terms with coefficients of the form  $b/(v^2 + p^2)$ ,  $v$  being a very small but non-vanishing quantity. The point being that if Gylden was right and these terms did exist then they could be used to prove the convergence of the series. In addition, Gylden had argued that the horistic method led to an important result concerning the conditions for libration. As Poincaré was to show, both these conclusions were false.

Originally Poincaré had believed that the mistakes in Gylden's paper derived from Gylden's misunderstanding of what was meant by a mathematical proof of convergence. Moreover, since Gylden's method was so obscure, Poincaré felt very reluctant to try to unravel the errors, especially as he thought, like Hill, that this obscurity would deter anyone else from using the method and so the errors would not get perpetuated. As a result, he had been tempted to let matters rest. But on a more careful examination of Gylden's paper he found, apart from the problems relating to the definition of convergence, that there were also errors contained in Gylden's first approximations which occurred right at the beginning of the analysis. In addition, other astronomers and mathematicians, notably Bäcklund, had been tempted into applying the method to practical problems and had run into difficulties. Thus Poincaré eventually felt compelled to try to provide an explanation for the mistakes.

In addition to trying to identify the faults in Gylden's analysis, Poincaré was also anxious to put the record straight regarding Gylden's result about libration. In the final part of his paper, Gylden had applied the principles of his method to the three body problem with the result that he thought he had proved that higher



order terms in the perturbation function were not responsible for libration. This deduction was based on the belief that in these higher order terms the horistic terms, which oppose libration, dominate. Poincaré, referring to his own results in [MN I] showed why this conclusion was false. His argument was based on the fact (which he had already proved) that each term of the perturbation function (however high the order) was equivalent to a system of periodic solutions. Since close to each stable periodic solution (there being as many stable as unstable periodic solutions) there are solutions which oscillate and cause libration, this means that any term in the perturbation function can cause libration, providing not all the characteristic exponents vanish, which for terms of sufficiently high order does not occur. Thus Gylden's conclusion is false.

Although Poincaré had been able to uncover some of the errors in Gylden's paper and prove the falsity of his conclusions, he was still unable to grasp fully the intricacies of Gylden's procedures. His final comments were somewhat reminiscent of his remarks about Gylden's [1887] paper discussed in 6.3:

*"Several of his (Gylden's) results are clearly correct, but they could have been reached by a much quicker method; a great number are clearly false; most of them are given in a way which is too obscure to decide whether they are true or false."* [1905a, 618].

### 7.3.3 General papers

Apart from the papers dealing with specific questions arising in celestial mechanics, Poincaré also wrote three papers of a more general nature, two on the three body problem and one on the stability of the solar system. These embraced a greater practical perspective than the other papers and were a response to the need for a more popular exposition of his ideas.

Mention has already been made of Poincaré's synopsis of [P2] which appeared in the *Bulletin Astronomique* [1891]<sup>14</sup>. This was essentially an outline of the main ideas and results from [P2] framed in such a way so as to be accessible to those, such as astronomers, whose interest in the three body problem was motivated by practical considerations. Thus in this paper he concentrated on his use of the more familiar methods of infinite series rather than on his innovative geometric insights. He again referred to the practical value of his periodic solutions, pointing out that,

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<sup>14</sup> This paper was also reproduced in its entirety by Tisserand [1896, Chapter 27].



although they were clearly an artificial construction since the probability of the initial conditions being such as to generate them was theoretically zero, they could be used to provide a very good first approximation for the intermediate orbit.

During the same period, Poincaré also provided a second review of [P2] where the exposition of his results regarding the restricted three body problem was almost completely descriptive [1891a]. He illustrated the concepts using examples rather than theoretical mathematics and even managed to avoid including a single formula either for a differential equation or an infinite series. Again he made a reference to the practical value of his theoretical solutions, and in addition, he also touched on the relationship between some of his mathematical results and the physical question of the stability of the solar system.

The stability of the solar system was also the subject of an article [1898] which appeared not only in two different publications in France in 1898 but also in *Nature* (in translation) in the same year. The article explained the basis of earlier stability proofs, such as those supplied by Lagrange and Poisson, which were founded on methods of successive approximations which showed that the variations in the elements were reduced to oscillations of a small amplitude about a mean value, and that this mean value itself was subject to oscillations. It also contained a discussion of the limitations afforded by the theoretical representation of the solar system as a system of material points subject to the exclusive action of their mutual attractions.

Acknowledging that real bodies are subject to forces other than gravitational attraction, Poincaré enquired into the nature and magnitude of what he termed these *complementary* forces. For, as he observed, if it could be shown that the effect of these forces was actually greater than the effect of the terms neglected by the approximations made in a theoretical proof of the stability, then the degree of accuracy lost through making the approximations could be legitimately ignored. The question Poincaré posed, therefore, was whether the stability was more easily destroyed by the complementary forces or by gravitational attraction.

With regard to the nature of these complementary forces, there was of course the recognised problem of the inconsistency of Newton's law with regard to the motion of the perihelion of Mercury. But, as Poincaré pointed out, providing any replacement law was sufficiently close to the inverse square law, it could be considered as equivalent from the stability point of view and thus would not effect the final

outcome. However, he identified another more compelling reason which argued against stability: the second law of thermodynamics, according to which there is a continuous dissipation of the energy generated by transforming work into heat. He suggested that this manifested itself in the motion of celestial bodies, both in the continuous action of tides, which, since the bodies are not perfectly elastic occurs even when the bodies are solid, and in the forces created by the magnetic fields of the bodies.

Having examined these forces he concluded that although the dissipation of energy resulting from their effect was extremely slow, it was still fast enough to be greater than the effect which would be imposed by those terms which were neglected by approximation in a theoretical proof of stability. In other words, from a practical point of view, Poincaré believed that the accuracy of the theoretical proof had reached its useful limit, although that did not in any way detract from the interest or value in continuing research into the purely theoretical problem.

## **7.4 General dynamics and "The Last Geometric Theorem"**

### **7.4.1 Geodesics on a convex surface**

As Poincaré had stressed in [P2] and [MN I], the study of periodic solutions was of the utmost importance in analysing the motion in the three body problem. However, after his initial assault on the topic, more than ten years elapsed before he presented another paper on the subject, and when he did it was within quite a different framework. His discovery of the complexity of the periodic and asymptotic solutions of the restricted three body problem had made him realise that to gain a greater understanding of the underlying dynamics it was first of all necessary to study a simpler dynamical problem than the one he had treated. The paper [1905b] he presented at the St Louis Congress investigated just such a dynamical problem: the question of the existence of geodesics on a convex surface. On the one hand it is a problem with two degrees of freedom and so in that sense analogous to the restricted three body problem, while on the other, there is the inherent simplification from the lack of singular points which implies a constant velocity which can be regarded as given. Furthermore, it has a direct application to the three body problem since the trajectories of the three body problem are comparable to geodesics on such a surface, and the closed geodesics therefore

represent periodic solutions. Nevertheless, despite the simplification, it is still an extremely difficult problem.

The force of the paper lay in Poincaré's use of variational calculus and the method of analytic continuation. Having shown how the property of a minimum used to characterise geodesics can be used to establish the existence of closed geodesics on an ellipsoid only slightly different from a sphere, and moreover that the total number of closed geodesics must be odd, Poincaré then considered a continuous family of analytic convex surfaces depending analytically on a parameter  $t$ , connecting a sphere where  $t = 0$  to a given surface where  $t = 1$ . He found that by continuously varying the parameter the closed geodesics appeared and disappeared in pairs. Furthermore, using topological considerations he was able to distinguish between the different types of closed geodesics by the number and arrangement of their double points. He was then able to extend his earlier result to show that on an arbitrary convex surface there is at least one closed geodesic without a double point, and, furthermore, since on an ellipsoid there are three, by continuity there are always an odd number of them. Thus Poincaré's results pointed towards the idea that there were in fact at least three closed geodesics on such a surface, although he did not attempt to prove such a conjecture. Later Birkhoff established the existence of three closed geodesics on an arbitrary convex surface, subject to certain limitations [1927, 180], and a complete solution was finally provided by Lusternik and Schnirrelmann [1930].

Poincaré also addressed the question of the stability of the closed geodesics. Here again he used his idea of characteristic exponents, and he also drew specific analogies with his results from [MN III] concerning periodic solutions and the principle of least action. Considering all the closed geodesics without a double point on an arbitrary convex surface, he found that the excess of the number which were stable over the number which were unstable remained constant. Furthermore, since on an ellipsoid this excess is equal to one (the largest and smallest of the principal ellipses on an ellipsoid are stable closed geodesics while the third is unstable), by continuity there is always at least one stable closed geodesic on an arbitrary convex surface.

Although extensive, Poincaré's account was by no means exhaustive. He did not, for example, consider higher dimensional ellipsoids, and with regard to his result that closed geodesics appear and disappear in pairs, he did not take into account the fact that infinite families of closed geodesics of the same length can appear on a



particular surface, such as occurs in the case of a sphere. Furthermore, although he had shown that for values of  $t$  sufficiently close to 0, it was possible to use analytic continuation to obtain from the principal ellipses on the ellipsoid an odd number of closed geodesics on an arbitrary convex surface, he gave no criteria for how far this method could be carried out<sup>15</sup>.

#### 7.4.2 "The Last Geometric Theorem"

Poincaré's last attack on the three body problem [1912] was also connected with the question of periodic solutions but again the form was quite different to his original investigations. This time his arguments were based on considerations of algebraic topology. In the paper, which was published only a few weeks before his death, Poincaré announced a theorem which if shown to be true would confirm the existence of an infinite number of periodic solutions for the restricted three body problem for all values of the mass parameter  $\mu$ . Furthermore, he also believed that the theorem would eventually be instrumental in establishing the denseness of the periodic solutions. However, although he had been working on the theorem for two years he had not been successful in finding a complete proof. Nevertheless, as he explained in the introduction, he felt it important to publish it despite the fact that it was in an unfinished state:

*"It seems that in these circumstances, I should refrain from any publication for as long as I am unable to resolve the question; indeed after the useless efforts that I have made for so many months, it appeared to me that it would be wisest to leave the problem to mature, while resting for several years; that would be all very well if I was certain to be able to return to it one day; but at my age I cannot be sure. On the other hand, the subject is so important (and I will search further to understand it) and the set of results already obtained so considerable, that I am resigning myself to leave them incomplete. I hope that the mathematicians who will interest themselves in this problem and who without doubt will be more successful than me, will be able to take advantage of them and use them to find the way in which they should go."* [1912, 500]<sup>16</sup>.

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<sup>15</sup> For a discussion of the difficulties and limitations of Poincaré's paper see Morse [1934, 305-358].

<sup>16</sup> Painlevé [1912], writing on the day of Poincaré's death, described the introduction as a simple but noble testament to a life completely dedicated to the search for truth.



As is well known, shortly after Poincaré's death, the young American mathematician George Birkhoff [1913] was indeed successful, and supplied a brilliantly elegant proof, creating one of the mathematical sensations of the decade<sup>17</sup>.

Poincaré's theorem can be given in the following form:

*Theorem:* Suppose that a continuous one to one area-preserving transformation  $T$  takes the ring  $R$ , formed by the concentric circles of radii  $x = a$ , and  $x = b$  ( $a > b > 0$ ), into itself in such a way so as to advance the points on  $x = a$  in a positive sense and the points on  $x = b$  in a negative sense, then there are at least two points of the ring invariant under  $T$ .

In fact, as Poincaré observed, to prove the theorem it is sufficient to prove the existence of just one invariant point, since topological considerations, show that if there is one invariant point then there must be a second<sup>18</sup>.

In considering the application of the theorem to the restricted three body problem, Poincaré began with the customary formulation of the problem in a rotating coordinate system with the Jacobian integral,

$$J = \frac{1}{2} (x'^2 + y'^2) + H(x, y) = C$$

where  $x'$  and  $y'$  are the components of the velocity. The motion then takes place in the plane region  $\beta$  defined by  $H < C$  which is bounded by a closed curve  $\alpha$ . The velocity is given in magnitude but not in direction and at each point of  $\alpha$  the velocity is zero. Therefore to each point of  $\beta$  there correspond an infinite number of *elements* (defined by a particular geodesic together with a point on that geodesic) comprising both speed and direction, and to each point of the boundary  $\alpha$  there corresponds only one element.

Poincaré first made a topological mapping of the region  $\beta$  into the interior of a circle  $\beta'$  so that the boundary  $\alpha$  is mapped into the circumference  $\alpha'$ . To examine the motion in the region  $\beta'$ , he considered a circle  $\gamma$  whose plane is perpendicular to the plane of  $\beta'$  with diameter  $MM'$ , where  $M$  is a point either in  $\beta'$  or on  $\alpha'$ , and  $M'$  is its

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<sup>17</sup> Some measure of the effort it required on Birkhoff's part to provide the proof can be estimated from the fact that several years later he apparently admitted to losing 30 lbs in weight while working on it. See Parikh [1991, 40].

<sup>18</sup> Poincaré modestly attributed this result to Kronecker, although it essentially derives from his own index theorem for the case of the sphere [1885, 125].

inverse with respect to the circle  $\alpha'$ . Then to each element through  $M$  he made a correspondence with a point on  $\gamma$ , the correspondence being determined by the direction of the element through  $M$ . Thus if  $M$  is in  $\beta'$  there are an infinite number of related points, one for each element, and if  $M$  is on  $\alpha'$  there is exactly one related point. Therefore each element corresponds to one and only one point in space and conversely.

The trajectories are therefore represented by the members  $C$  of a family of twisted curves, where the closed curves represent the periodic solutions, and one, and only one, curve passes through each point in the space. Poincaré then considered a closed curve  $C_0$  which represents a periodic solution  $G_0$ , and an area  $A$  bounded by this curve and which lies on a curved surface  $S$ . If  $A$  is simply connected and without contact, that is no curve  $C$  other than  $C_0$  is tangent to  $S$  at a point of  $A$ , and if  $P$  is a point of  $A$  with consequent  $P'$ , then the transformation  $T$  which transforms  $P$  to  $P'$  is a point transformation of  $A$  onto itself, and as Poincaré showed, the transformation is continuous. He further observed that the transformation  $T$  admits a positive invariant integral and is therefore area-preserving<sup>19</sup>.

If  $P$  is a point of  $A$  close to the boundary curve  $C_0$ , then the curve  $C_1$  through  $P$ , represents a trajectory  $G_1$  close to the periodic solution  $G_0$ . Poincaré showed that when  $P$  is very close to  $C_0$ , a function which he called the *reduced argument* of  $P$  and that of its consequent  $P'$  differ by  $\frac{2\pi}{\alpha + m}$ , where  $\pm i\alpha$  are the non-zero characteristic exponents for the stable periodic solution  $G_0$ ,  $m$  is an integer, and the reduced argument has the property that it varies steadily from 0 to  $2\pi$  around  $C_0$ .

He next considered a topological mapping of the area  $A$  onto the interior of a circle so that, using polar coordinates  $(x, y)$ ,  $C_0$  becomes the circle  $x = a$ , and on this circle  $y$  is equal to the reduced argument. The transformation  $T$  then maps the circle into itself and each point on the circle is advanced through the angle  $\frac{2\pi}{\alpha + m}$ . By his index theorem such a mapping has an odd number of fixed points in the interior of  $A$ , each of which corresponds to a periodic solution, and at least one of which is stable. If  $P_0$  is the fixed point corresponding to the stable periodic solution, and the coordinates are chosen such that  $P_0$  is the centre of the circle,  $x = 0$ , then  $T$  leaves the

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<sup>19</sup> Poincaré did not prove the proposition concerning the existence of an invariant integral but instead made reference to the appropriate parts of the *Méthodes Nouvelles*.

centre of the circle unchanged and maps the circumference onto itself in such a way that all the points are moved in the same direction through the same angle.

If  $C_0'$  represents the stable periodic solution through  $P_0$ , and  $P$  is now considered to be a point close to  $P_0$ , then Poincaré showed that in this case the increase in the value of  $y$  in passing from  $P$  to its consequent  $P'$ , was  $2\pi(\beta + n)$  where  $\pm i\beta$  were the two non-zero characteristic exponents of the periodic solution passing through  $P_0$ , and  $n$  is a fixed integer.

Finally, he considered the iterated transformation  $T^p$ , where  $p$  is a positive integer.  $T^p$  therefore conserves both  $x = a$ , and  $x = 0$ , and is area preserving. If  $T(x, y) = (X, Y)$ , then the iterated transformation will give, on  $x = a$ ,

$$Y - y = 2\pi \left( \frac{p}{\alpha + m} \right),$$

and on  $x = 0$ ,

$$Y - y = 2\pi p(\beta + n).$$

The transformation is unaltered if  $Y$  is increased to  $Y + 2\pi q$ , where  $q$  is an integer, thus on  $x = a$ ,

$$Y - y = 2\pi \left( \frac{p}{\alpha + m} + q \right),$$

and on  $x = 0$ ,

$$Y - y = 2\pi[p(\beta + n) + q].$$

If  $n$  is chosen such that  $(\beta + n)(\alpha + m) \neq 1$ , then an infinite number of pairs of values of  $p$  and  $q$  can be found such that

$$\left( \frac{p}{\alpha + m} + q \right) [p(\beta + n) + q] < 0$$

and hence either,

$$\frac{1}{\alpha + m} > -\frac{q}{p} > \beta + n \quad \text{or} \quad \frac{1}{\alpha + m} < -\frac{q}{p} < \beta + n.$$

Since the transformation fulfils the conditions for the theorem, if the theorem is true there will be at least two points which remain invariant under the transformation. Since  $p$  and  $q$  can take an infinite number of values, if there are two invariant points, there will be an infinite number and hence an infinite number of

periodic solutions. Furthermore, the existence of these periodic solutions does not depend on the value of  $\mu$ .

Poincaré also pointed out that the periodic solution corresponding to a particular pair of values of  $p$  and  $q$  can only disappear if it coincides with either  $C_0$  or  $C'_0$ , in other words if

$$-\frac{q}{p} = \frac{1}{\alpha + m} \text{ or } \beta + n.$$

Birkhoff established the proof of Poincaré's theorem through a *reductio ad absurdum*. His strategy was to show that the assumption that an invariant point did not exist for the transformation led to a contradiction. As Poincaré himself had remarked, if there exists one invariant point, then there necessarily exists a second, hence the theorem fails if it can be proved that there is no invariant point.

If  $T(x, y) = (X, Y)$  represents the transformation, then the condition that it has no invariant point is described by

$$(X - x)^2 + (Y - y)^2 > d^2 > 0$$

for all points  $(x, y)$  of the ring.

Birkhoff used the coordinate system  $x = \theta, y = r^2$ , where  $\theta$  is the angle which a line from the centre to  $(x, y)$  makes with a fixed line through the centre, and  $r$  is the distance of the point  $(x, y)$  from the centre of the ring. The transformation  $T$  is then given by

$$X = \phi(x, y), \quad Y = \varphi(x, y)$$

where  $X - x$  and  $Y - y$  are both single-valued and continuous in the ring  $R$ .

He next considered the transformation  $T_\epsilon$  defined by

$$X = x, \quad Y = y - \epsilon, \quad (0 < \epsilon < b^2)$$

which takes the circles  $C_a: y = a^2$ , and  $C_b: y = b^2$ , into the circles  $C'_a: y = a^2 - \epsilon$ , and  $C'_b: y = b^2 - \epsilon$ , respectively. It is then possible to form the auxiliary transformation  $TT_\epsilon$  and providing  $\epsilon < d$  this auxiliary transformation  $TT_\epsilon$  will also have no invariant point.

If now  $(x, y)$  are taken to be the rectangular coordinates of a point in the strip  $S$

$$-\infty < x < +\infty, \quad b^2 \leq y \leq a^2,$$



corresponding to the ring  $R$ , then the transformation  $TT_\varepsilon$  carries the upper edge of the strip  $a^2 \leq y \leq a^2 + \varepsilon$  into the lower edge, and the strip is carried into a second strip lying below the first but with a common boundary. By a repetition of the transformation a series of strips is obtained with eventually the bottom edge of one of the strips, say the  $k$ th, overlapping the edge  $y = b^2$ .

Birkhoff next constructed a curve  $PQ$ , where  $P$  is a point on  $C_a$ , with image  $P'$  under  $TT_\varepsilon$ , and  $Q = P^{(k)}$  is the point derived from  $P$  by a  $k$ -fold repetition of  $TT_\varepsilon$  which is the first inter-section of the succession of arcs  $PP', \dots, P^{(k-1)}P^{(k)}$  with the lower side of  $C_b$ , i.e.  $Q$  lies at most  $\varepsilon$  below  $C_b$  (see FIG. 7.4.i). The curve  $PQ$  is then invariant under  $TT_\varepsilon$ .

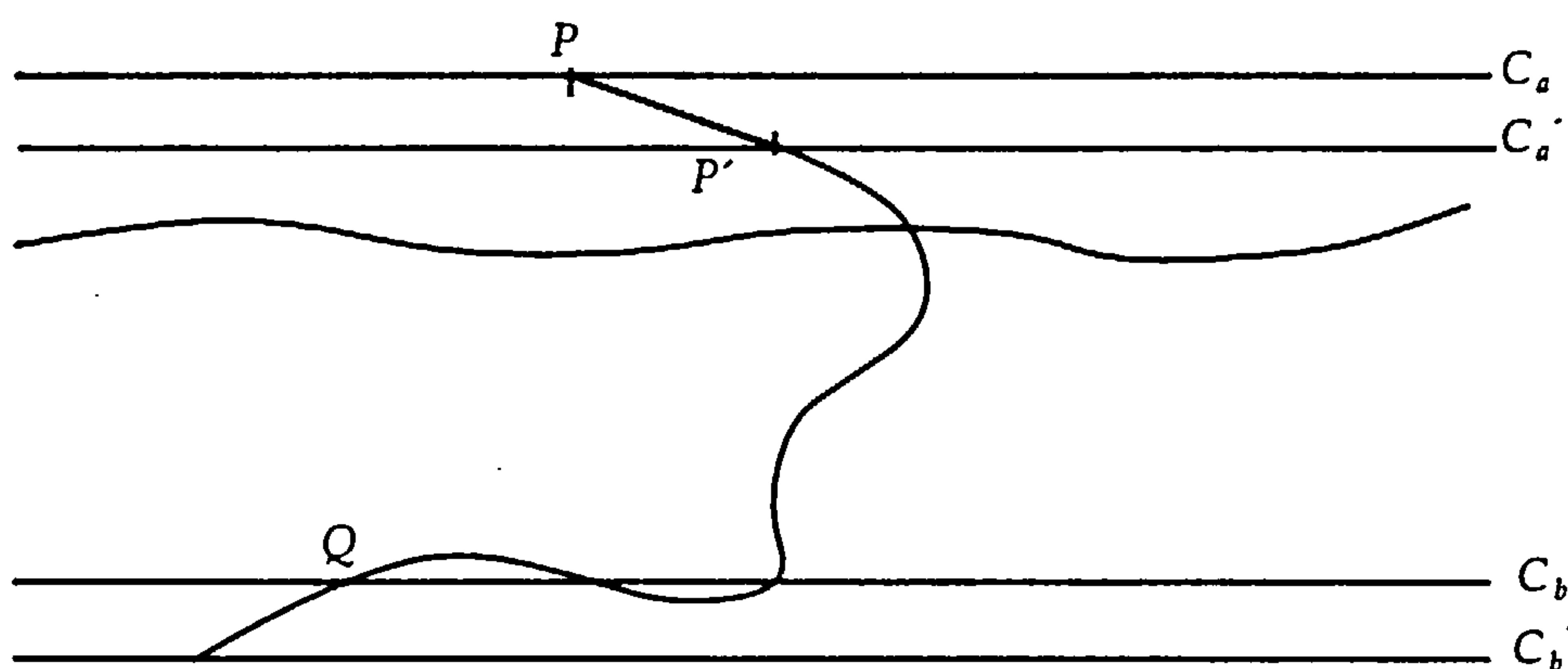


FIG. 7.4.i.

If a point  $B$  moves along  $PQ$ , then its image  $B'$  under  $TT_\varepsilon$  will move along the same curve never coinciding with it (since there are no invariant points under the transformation). The angle which the vector  $BB'$  makes with the positive direction of the  $x$  axis can be taken to be a positive acute angle, and when  $B$  has varied to its final position, the same angle lies in the second or third quadrant since  $P^{(k)}$  lies to the left of  $P^{(k-1)}$  by the hypothesis of the theorem. A rotation of the vector  $BB'$  is then the least positive angle from the first direction to the second. Furthermore, if  $B$  moves in any manner from a point on  $C_a$  to a point on  $C_b$ , then the corresponding vector  $BB'$  along the new curve will undergo exactly the same rotation as along  $PQ$ .

Birkhoff considered the inverse transformation  $T^{(-1)}$  which is similar to  $T$  except that points on  $C_a$  and  $C_b$  are moved in the reverse direction. Arguing as above, if the vector  $BB^{(-1)}$  with end point  $B^{(-1)} = T^{(-1)}(B)$  has its initial point  $B$  varied from a point of  $C_a$  to a point of  $C_b$ , then the angle of rotation will be the least negative angle

consistent with its initial and final positions. But the total rotation of  $BB^{(-1)}$  is the same as the rotation of the vector  $B^{(-1)}B$  which joins a point  $B^{(-1)}$  to its image under  $T$ . Thus by the earlier result, the rotation must also be the least positive angle, which is a contradiction. Hence there must be at least one invariant point. To show that there are at least two invariant points it is sufficient to observe that the total rotation of the vector  $BB'$  around the rectangle  $0 \leq x \leq 2\pi, b^2 \leq y \leq a^2$  is zero, but around a simple invariant point it is  $\pm 2\pi$ . There must be therefore at least two invariant points inside the rectangle.

Birkhoff continued to work on the ideas involved in Poincaré's theorem, and in particular its applications, devoting a chapter to the topic in his general account of dynamical systems [1927, 150-188]. In [1925] he extended the theorem to a non-metric form by removing the condition that the outer boundaries  $a$  and  $T(a)$  of the ring  $R$  and the transformed ring  $T(R)$  must coincide, and replacing it instead with the condition that  $a$  and  $T(a)$  are met only once by a radial line  $\theta = \text{constant}$ . In its revised form he proved that the theorem held for ring-shaped regions with arbitrary boundary curves, and that there are always two distinct invariant points. Since the extension does not involve an invariant area integral it is essentially a topological result. Its importance lies in the fact that it can be used to establish the existence of infinitely many periodic motions near a stable periodic motion in a dynamical system with two degrees of freedom, from which the existence of quasi-periodic, i.e. motions which are not periodic but limits of periodic motions, follows.

In [1928] Birkhoff explored the relationship between the dynamical system and the area-preserving transformation used in the theorem. Having shown that corresponding to such a dynamical problem there exists an area-preserving transformation  $T$  in which the important properties of the system for motions near periodic motions correspond to properties of the transformation  $T$ , he then showed that a converse form of this correspondence also exists. In other words given a particular type of area preserving transformation there exists a corresponding dynamical system.

Later Birkhoff [1931] gave a generalisation of the theorem to higher dimensions.

A further discussion of some of Birkhoff's early work on dynamical systems is given in 9.3.

## 8. Associated Mathematical Activity

### 8.1 Introduction

During the period when Poincaré was working on the three body problem and theoretical problems of celestial mechanics there were of course other mathematicians and astronomers independently pursuing related topics of research. Mention has already been made of Gylden and the somewhat unhappy consequences of his priority claim over the discovery of asymptotic solutions, but another mathematician whose work (eventually) enjoyed a far happier fate was the Russian Alexander Liapunov<sup>1</sup>. While Poincaré had been working on the three body problem, Liapunov had been engaged in a qualitative investigation into the theory of the stability of motion, and in 1892, some two years after its completion, his memoir was finally published. However, since the memoir appeared in Russian, the penetration of Liapunov's ideas into the mathematical circles of Western Europe was initially rather slow, and for the most part they were only known through a series of short notes in the *Jahrbuch über die Fortschritte der Mathematik* which appeared in 1893 and an extract published in the *Journal de Mathématiques*<sup>2</sup>. But with the publication of a French translation by Davaux in 1907 (reviewed and corrected by Liapunov), the memoir gradually began to reach a wider audience and

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<sup>1</sup> For a biography of Liapunov see Smirnov [1992].

<sup>2</sup> *Journal de Mathématiques* (5) 3, 1897, 8.

Liapunov's study of the stability question began to be recognised as forming an important complement to the work undertaken by Poincaré. The first part of the chapter is concerned with Liapunov's work on the development of stability theory and its relationship to the ideas of Poincaré.

The subject of stability was also taken up in a slightly different way by the Italian mathematician Tullio Levi-Civita. Through his combined interest in geometry, the three body problem and analytical mechanics, Levi-Civita had been led to the study of the qualitative theory of differential equations and associated questions of stability. At the turn of the century he produced a long paper on stability theory [1901] in which he took account of both Poincaré's and Liapunov's ideas and paid particular attention to the restricted three body problem. A discussion of Levi-Civita's paper concludes the first part of the chapter.

With regard to the early responses to Poincaré's researches, these can be broadly divided into two categories. On the one hand there was the work of those who were engaged in pursuing a solution to the three body problem and for whom Poincaré's memoir was a source of ideas and inspiration, and on the other there was the work of those who focused more generally on the qualitative theory in Poincaré's memoir. The remainder of the chapter will be devoted to two topics which fall into the first category and concern the progress of the solution of the three body problem: regularisation and numerical investigations. The second category, which involves the work of Jacques Hadamard and George Birkhoff which formed the basis for the emerging theory of dynamical systems is the subject of the next chapter.

As indicated by Lovett [1912], Poincaré's contribution to knowledge on the three body problem served to generate interest in the problem in several ways, but in the years immediately following the publication of [P2] and [MN I-III], there was one issue which dominated: that of the regularisation of the equations of motion. It will be recalled that Weierstrass' description of the problem included the assumption that no collisions between the bodies would take place and Poincaré had based his analysis accordingly. But if a complete solution to the problem was to be found then collisions had to be taken into account. Since collisions are described by singularities in the differential equations, this raised the question of regularisation. Could the equations be regularised at the points of singularity? This in turn raised a further question about the nature of the singularities. Were collision singularities the only type of singularity or was it possible that noncollision singularities could exist? If



these problems could be resolved then, as Poincaré had indicated [1882a, 1886], it was theoretically possible that a complete solution to the problem could be found. Several distinguished mathematicians applied themselves to these issues and in the second part of this chapter consideration is given to the contributions made by Paul Painlevé, Tullio Levi-Civita, Giulio Bisconcini, Karl Sundman and Hugo von Zeipel.

Another aspect of the three body problem which has so far eluded discussion is the question of numerical solutions. Although Poincaré's interest had been in the theoretical side of the problem, to what extent did his results affect the pursuits of those seeking a practical solution? One area in which Poincaré's influence is unquestionably present is the numerical construction of periodic solutions. Work in this field was pioneered by Sir George Darwin who devoted several years to its study with notable success. Darwin's work is of particular interest not only because he directly inherited ideas from Poincaré but also because he laid strong foundations for a field of activity which is more than ever flourishing today. The availability of powerful electronic computers has meant that numerical integration is now often a relatively quick and efficient way of gaining an insight into a complex dynamical problem, in complete contrast to the painstaking efforts required by numerical analysts at the end of the 19th century, of which Darwin's work was a model of tenacity. A discussion of Darwin's research on periodic solutions concludes the chapter.

In addition, as a further indication of the extent of Poincaré's sphere of influence, mention should be made of some other mathematicians who were motivated by considerations not necessarily confined to the realms of the three body problem and celestial mechanics, and whose study of Poincaré's techniques resulted in further new discoveries in other related fields, although their work will not be discussed here. With regard to the theory of ordinary differential equations, Ivar Bendixson [1901] successfully extended some of Poincaré's ideas concerning the behaviour of solution curves near singularities, and included amongst his results is the theorem, now called the Poincaré-Bendixson theorem, which provides a positive criterion for the existence of a periodic solution in a dynamical system with one degree of freedom.

Another topic in which notable progress was recorded was Poincaré's theory of invariant integrals, as for example in the work of Koenigs [1895] who made a connection between it and Lie's theory of contact transformations<sup>3</sup>. While at the beginning of the 1920s Elie Cartan [1922] undertook an analysis in which he looked particularly at the relationship between invariant integrals and his own idea of integral forms. In studying the application of the theory relative to the integration of differential equations, Cartan also established the link with Lie's theory of transformation groups.

The selection of topics discussed in this chapter is not intended to be a comprehensive account of mathematics connected with Poincaré's work on the three body problem, neither is their treatment intended to be an in-depth mathematical analysis. Rather the objective is to put Poincaré's work into context with some contemporary and later research by both indicating how his work fitted in with corresponding mathematical ideas and giving an insight into the breadth of his influence with regard to the three body problem.

## 8.2 Stability

### 8.2.1 Introduction

As has been described in 3.3.2, Poincaré first discussed the stability of solutions to differential equations in [1885]. As he himself had explained, part of his original motivation for developing the qualitative theory had been his desire to tackle the problem of the stability of the solar system, and so stability per se was a natural topic for him to pursue. Furthermore, while he had been discussing stability in [1885], he had also been undertaking an investigation into stability of another sort: that of the different forms of rotating masses of fluid. This not only resulted in an important paper [1885b<sup>4</sup>] but it also provided the first connection with his work and the work of Liapunov. Only the year before Liapunov had completed his master's dissertation on the subject of the stability of figures of equilibrium, and, as Gray [1992] has pointed out, there were strong similarities between his methods and those

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<sup>3</sup> See Whittaker [1937, 275]. For a thorough systematic study of the early theory of invariant integrals, see De Donder [1901].

<sup>4</sup> For an appreciation of Poincaré [1885b] see Darwin [1900].

of Poincaré, and the publication of Poincaré's paper prompted Liapunov to initiate a correspondence between them<sup>5</sup>.

However, in the early 1890s when they each put forward their ideas about the stability of a given state of motion, there were marked differences between the two accounts. Although Liapunov freely acknowledged the influence of Poincaré's [1885] ideas, he used a different definition of stability to that adopted by Poincaré, and while Liapunov developed a precise theory which was quite general in its application, Poincaré's treatment was altogether less rigorous.

Liapunov's memoir, which had originally been prepared as his doctoral thesis, was completed in 1890 but since the process of publication had taken two years, it appeared after Poincaré's account in [P2] and in [MN I]. It was, therefore, entirely independent of Poincaré's work although, as Liapunov explained in the preface, the delay had allowed him the chance to add notes to the text indicating the analogies with [P2]; [MN I] appeared too late for a similar exercise.

### 8.2.2 Liapunov

Liapunov defined the solution of a system of differential equations as stable if other solutions which start at a given time sufficiently close to the given solution remain arbitrarily close to it at all later times. More formally, he stated that a solution  $\Phi(t, \alpha)$  is stable if given  $\varepsilon > 0$ , there exists a positive number  $N$  (not necessarily an integer) which depends on  $\varepsilon$  such that  $|\Phi(t, \alpha + \delta) - \Phi(t, \alpha)| < \varepsilon$  for all  $t$  provided  $|\delta| < N$ . This was in contrast to the rather freer definition employed by Poincaré (which he had originally used in [1885] and in [P2] had ascribed to Poisson) in which he regarded the motion of a point as stable if it returned infinitely often to positions arbitrarily close to its initial position.

The motivation for Liapunov's research was the desire to ascertain the domain of validity of a certain method of solving differential equations. In this method, in order to obtain a solution, the equations were reduced to linear approximations through the retention of only the first order terms in the dependent variables<sup>6</sup>. This was a considerable simplification of the original equations, and especially useful in the case of equations with constant coefficients, but, as Liapunov realised, there was

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<sup>5</sup> See the Liapunov-Poincaré correspondence published by V. I. Smirnov and A. P. Youshkevitch in *Cahiers* 8 (1987), 1-18.

<sup>6</sup> Liapunov quotes Thomas and Tait; Routh; and Zukovsky as being the main proponents of this method [1907, 204].



no *a priori* reason for such a linearisation procedure to be valid. He therefore set out not only to define the cases where the linear approximations could legitimately be used to replace the original equations, but also to derive procedures which would show when it was invalid. What resulted was a complete stability theory armed with a battery of techniques designed to deal with a variety of situations.

To make the problem more manageable, Liapunov limited himself to examining only those equations where the coefficients were either constant or periodic, the first case being regarded in essence as a particular case of the second. As he observed, this was not a severe restriction since, from the point of view of practical applications, many important examples consist of equations of these two types.

In his determination of stability of a system Liapunov derived two different methods. The first was applicable when it could be presupposed that an explicit solution to the equations of perturbed motion, generally in the form of infinite series, was known. While the second (known as Liapunov's *direct* method) could be used when there was no explicit knowledge of the solutions, the method being essentially based on energy considerations due to Lagrange and made rigorous by Dirichlet. Broadly speaking, the method exploits the intuitive idea that an equilibrium state of a physical system is stable if nearby the energy is always decreasing. The stability of the system can then be determined by means of the properties of a certain scalar function positive definite in the domain of the state of equilibrium<sup>7</sup>.

In the case where the coefficients of the equations were constant, Liapunov studied the equations of perturbed motion to discover that the stability of the solution was determined by the roots of a certain eigenvalue equation, these roots being equivalent to what Poincaré was later to call characteristic exponents. Liapunov found that if all the roots had negative real parts, stability was guaranteed, and, providing the initial perturbations were small enough, the perturbed solution asymptotically approached the original solution, and the linear approximation could be freely used. If only some of the roots had negative real parts, he found that the system did have a certain conditional stability which could be defined, while if the roots had positive real parts then the system was unstable and the approximation invalid.

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<sup>7</sup> For a clear and concise account of Liapunov's direct method with applications see La Salle and Lefschetz [1961].



Looking at the question from the other way round, his analysis showed that in most cases where the equations had constant coefficients, studying the linear approximation was sufficient to resolve the stability question, the only exception being when some of the roots, without having any positive real parts, had zero real parts. Although these cases were very difficult to analyse, Liapunov recognised, as Poincaré had done, that they were of particular interest, especially if the system of equations was canonical. For in this latter case, the roots are equal in magnitude but opposite in sign, and so absolute stability is only possible if all the roots have zero real parts.

Since the difficulties in the analysis were determined by the number and type of root generated by the equations, Liapunov made a detailed examination of the two simplest cases: the case in which one root was zero, and the case in which two roots were purely imaginary, in each case the remainder of the roots having negative real parts. In both these cases he was able to define the conditions for stability. In the first he found that it essentially depended on the form of a particular series obtained from the equations of the perturbed motion, while in the second he found it was the existence of a periodic solution to the original unperturbed equations which provided the key. Furthermore, as he discovered [1907,392], showing how this latter condition worked in practice provided a direct overlap with a result of Poincaré's from [P2]: for different reasons and using different methods they had each proved the existence of a periodic solution to a system of non-linear equations.

In the case of equations with periodic coefficients, Liapunov showed that the stability depended on the roots  $\rho$  of another eigenvalue equation which were related to the roots  $\lambda$  of the previous case by

$$\lambda = \frac{1}{w} \log \rho,$$

where  $w$  is the period of the coefficients. In this second case, the stability is determined by the modulus of the roots. If all the roots have modulus less than one then there is stability, and, as in the first case, if the initial perturbations are sufficiently small, the perturbed motion will asymptotically approach the original motion. Similarly, roots with a modulus of greater than one imply instability, and roots with modulus equal to one are the ones which require a more detailed analysis.

Thus there were certainly many similarities between the sort of results obtained by Liapunov and those obtained by Poincaré. However, the variance in their

definitions of stability meant that the scope of their analysis was substantially different. Liapunov's theory, while extremely rigorous and detailed, was limited in its range by his definition. The disadvantage with Liapunov's theory is that his definition is too demanding. For if a solution is Liapunov stable not only can the perturbed motion not stray far from the unperturbed motion, but also each point in the trajectory of the perturbed motion has to be close to its contemporaneous point in the unperturbed motion. In practical terms there are very few dynamical systems which completely satisfy Liapunov's criteria, and as a result the application of his theory is essentially confined to local analysis.

On the other hand, Poincaré's stability theory, being based on a less restrictive definition, could be applied to problems of a far more complex nature than those which could be considered by Liapunov. The point of departure for Poincaré was his theory of invariant integrals which, in conjunction with his definition, meant that he could attack general questions about the stability of dynamical systems, deriving results such as his recurrence theorem which allowed him an insight into the behaviour of the solutions of the restricted three body problem. His theory therefore led to knowledge about the global behaviour of systems, knowledge which would have been impossible to obtain within the constraints of Liapunov's theory, although this was to some extent counter-balanced by the accompanying imprecision in his local analysis. Moreover, Poincaré's ideas about stability provided George Birkhoff with the foundation for his theory of recurrent motion which is discussed in 9.3.1.

The initial inaccessibility of Liapunov's work meant that it was Poincaré's ideas which met with the first response<sup>8</sup>. However, with the publication of the French translation of Liapunov's memoir, Liapunov's stability theory became more widely known and the potential of his work began to be recognised. Liapunov's theory, as well as being capable of greater generalisation and having a definition which was intuitively more natural than that of Poincaré, provided a precise and conventional framework within which to work. Today the theory is generally regarded as one of the fundamental achievements within the qualitative theory of differential equations. A substantial literature has grown up around Liapunov's work<sup>9</sup>,

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<sup>8</sup> See Gray [1992, 520].

<sup>9</sup> For an extensive bibliography on the qualitative theory of differential equations and Liapunov stability in particular see Cesari [1959].

particularly recently in the area of control theory, and in 1992 the centenary of the memoir's original publication was commemorated by the appearance of an English translation<sup>10</sup>.

### 8.2.3 Levi-Civita

An alternative approach to stability theory was put forward by Levi-Civita in a series of abstracts in the *Comptes Rendus* [1900, 1900a, 1900b]. These were brought together in a long paper [1901] in which he placed the new ideas of both Poincaré and Liapunov into the structure of the classical analytical mechanics of Lagrange. As Dell'Aglio and Israel [1989] have eloquently argued, Levi-Civita's work on the qualitative theory of differential equations and related issues of stability provides a convincing example of Thomas Kuhn's "essential tension" between tradition and innovation.

Levi-Civita's interest in classical mechanics was combined with a deep geometrical insight which meant that the qualitative theory of differential equations formed a natural subject for his research. However this alignment of mechanics and geometry led him to a definition of stability which, although very similar to that of Liapunov, differed in one critical respect. While Liapunov's definition only took account of future stability, Levi-Civita's definition incorporated both past and future stability, reflecting the principle of reversibility in physical processes. Another important aspect of Levi-Civita's work is that, in contradistinction to that of Liapunov, it allows for the treatment of the case when the first order approximation is insufficient. Levi-Civita gave his definition in the following form.

He considered a system of differential equations with periodic coefficients

$$\frac{dx_i}{dt} = X_i(x_1, \dots, x_n, t) \quad (i = 1, \dots, n) \quad (8.2.i)$$

and said that the periodic solution  $x_i = 0$  was stable if and only if for any small neighbourhood  $E$  of the origin, there exists a second neighbourhood  $H$  such that if the initial position of the moving point is taken in  $H$ , the point remains in  $E$  for all positive and negative values of  $t$ . Although Levi-Civita had opened [1901] by acknowledging Liapunov's definition, when he gave his own definition he made no reference to Liapunov, and later in his classic work on rational mechanics he

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<sup>10</sup> See "Liapunov Centenary Issue", *International Journal of Control*, 55 (March 1992), 531-773.



referred to his definition as being “absolute stability in the sense of Dirichlet”, giving the explanation that it had been derived from a configuration space interpretation of the classic definition of a stable equilibrium<sup>11</sup>.

To deal with the question of the stability of the periodic solutions of equations (8.2.i) Levi-Civita employed a geometric model which reduced the question to an analysis of certain point transformations associated with the solutions. He applied the theory to the restricted three body problem and derived the interesting result that when a periodic solution in the restricted three body problem is such that the mean motions of the planetoid and the other two bodies are commensurable, then the motion is unstable and there will be solutions approaching and receding from the given periodic solution.

As Dell’Aglia and Israel have lucidly described<sup>12</sup>, there is clear parallel between Levi-Civita’s geometrical model and Poincaré’s method of transverse sections, and from this point of view, Levi-Civita’s work can be seen naturally as coming between that of Poincaré and Birkhoff

With regard to Poincaré and stability, although Levi-Civita did not use the concept of Poisson stability, he did explicitly state his agreement with Poincaré’s conclusion that instability is the rule and stability the exception<sup>13</sup>. Although Poincaré’s conclusion specifically related to differential equations of first order and first degree, in Levi-Civita’s work it was shown to be true in a more general sense.

## 8.3 Singularities and regularisation

### 8.3.1 Introduction

There were essentially two problems which arose in connection with the singularities of the differential equations of the  $n$  body problem. In the first place there was the question of the determination of the type of singularities which could arise and the corresponding investigation of their properties. In this respect, there was not only the subject of the singularities caused by collisions which, although

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<sup>11</sup> (With U. Amaldi) *Lezioni di meccanica razionale* 2 (1926/27), 464. See Dell’Aglia and Israel [1989, 297].

<sup>12</sup> Dell’Aglia and Israel [1989, 301-304].

<sup>13</sup> Levi-Civita [1901, 222]. See 3.2.2.



acknowledged still required a detailed study since it had largely been ignored as being of little practical consequence, but also the question of the existence of other types of singularities. Once the nature of the singularities was established the second problem arose: the task of trying to eliminate them, the so-called regularisation of the equations.

The three body problem eventually succumbed to resolution on both these issues. The collisions were analysed and a complete knowledge of the type of singularities was obtained. However, resolving these questions with regard to the general  $n$  body problem has proved much more elusive, and it is only very recently that significant progress has been made.

### 8.3.2 The three body problem

In the autumn of 1895 Oscar II once more showed his enthusiasm for mathematics by sponsoring a series of mathematical lectures held at the University of Stockholm. The King had originally intended for the lectures to be an extension to his competition, and so he had hoped to entice Poincaré to Stockholm with an invitation to lecture on recent progress in analysis. When Poincaré was unable to accept the offer, the position was filled (on the recommendation of Mittag-Leffler) by another French mathematician, Paul Painlevé<sup>14</sup>. The lectures, inaugurated by the presence of Oscar himself, were a great success and subsequently gained additional renown through their (beautifully lithographed) publication<sup>15</sup>.

Painlevé, following the brief to lecture on analysis, took as his principal subject the theory of transcendental functions defined by differential equations. In the final part of the lectures he considered the application of his earlier theory to the three and  $n$  body problems, and investigated the singularities in the differential equations. It was obvious from the equations that a collision point was a singular point, but what was not so clear was whether any other type of behaviour would also lead to a singular point.

In the three body case Painlevé supplied the answer: the only singularities are collisions. More precisely, he stated that starting from an initial time  $t$  and given initial conditions, singularities can only occur when at least one of the three mutual

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<sup>14</sup> *Œuvres de Painlevé I*, 199. See Cahiers 10, 1989, 194.

<sup>15</sup> Painlevé [1897].

distances tends to zero as  $t$  converges to a finite time  $t_1$ . In other words, either the motion is regular as  $t$  increases indefinitely or there is a collision.

What was especially important about the theorem was that it allowed Painlevé to conclude that the equations of motion of the three body problem were integrable using convergent power series (fundamentally equivalent to Taylor series), but only providing the initial conditions were such as to exclude the possibility of a two or three body collision within a finite time.

Thus it was clear to Painlevé that a mathematical solution to the three body problem could be found if it was possible to define precisely the initial conditions which corresponded to a collision. In [1896, 1897] he conjectured that these initial conditions should satisfy two distinct analytic relations (which would reduce to one in the case of planar motion). Then, having made a generalisation of Bruns' theorem on the existence of algebraic integrals for the three body problem [1897a, 1898], he was able to prove that the relations had to be transcendental [1897b], but proceeded no further<sup>16</sup>.

As far as singularities of the  $n$  body problem were concerned, Painlevé made little headway. He did manage to find a sufficient condition for a singularity to be a collision, but he was still left with the unresolved question of whether *pseudocollisions* (the name he gave to singularities which are not due to collisions) could exist for  $n \geq 4$ .

In 1903 Levi-Civita published the first of several papers on regularisation in the three body problem. It was a topic which maintained his interest for over twenty years and which began with two notes in the *Comptes Rendus* [1903, 1903a], the results in which were united in an important paper on the singular trajectories and collisions in the restricted three body problem [1903b]. In this paper he characterised the singular trajectories in the restricted problem, finding the analytic relation predicted by Painlevé.

Levi-Civita formulated the problem by considering the bodies as material points,  $S$  and  $J$  with masses  $1 - \mu$  and  $\mu$ , and planetoid  $P$  with negligible mass, and putting the equations of motion into canonical form

$$\frac{dr}{dt} = \frac{\partial \mathcal{F}}{\partial R}, \quad \frac{d\theta}{dt} = \frac{\partial \mathcal{F}}{\partial \Theta}, \quad \frac{dR}{dt} = -\frac{\partial \mathcal{F}}{\partial r}, \quad \frac{d\Theta}{dt} = -\frac{\partial \mathcal{F}}{\partial \theta} \quad (8.3.i)$$

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<sup>16</sup> See 7.3.1.

with the Hamiltonian

$$F = \frac{1}{2} \left[ R^2 + r^2 \left( \frac{\Theta}{r^2} - 1 \right)^2 \right] - U + \mu r \cos \theta - \frac{1}{2} r^2$$

where

$$U = \frac{1-\mu}{r} + \frac{\mu}{\Delta}, \quad r = SP, \quad \Delta = JP, \quad \theta = \angle JSP.$$

The conjugate variables  $R, \Theta$  are, respectively, the derivative of the radius vector  $r$ , and twice the (absolute) areal velocity.

As Levi-Civita knew from Painlevé's theorem, every singularity of the motion occurs only when, as  $t$  approaches a finite value  $t_1$ , the limit  $(t \rightarrow t_1) r = 0$  or the limit  $(t \rightarrow t_1) \Delta = 0$ , that is the differential equations have singularities at  $S$  and  $J$  respectively. Since  $S$  and  $J$  enjoy a symmetric role in the problem, it is sufficient to investigate the behaviour of the system about only one of these points, say  $S$ , and characterise the singular trajectories  $\Sigma$  along which a collision between  $S$  and  $P$  can occur.

The motion is regular before  $t = t_1$  and so  $r \neq 0$  for  $t < t_1$ . Therefore there exist values of  $t$  arbitrarily close to  $t_1$  for which  $dr/dt = R$  is not identically zero. Using the Jacobian integral and eliminating  $dt$ ,  $R$  can be defined as a function of  $r, \theta$  and  $\Theta$ , and the system of equations (8.3.i) takes the reduced form

$$\frac{d\theta}{dr} = -\frac{\partial R}{\partial \Theta}, \quad \frac{d\Theta}{dr} = -\frac{\partial R}{\partial \theta} \quad (8.3.ii)$$

Since along the trajectories  $\Sigma$  there are values of  $r$  arbitrarily close to 0 for which  $\theta$  and  $\Theta$  are analytic functions of  $r$  (since  $dr/dt$  is not identically zero), these trajectories can be separated from the other solutions to equations (8.3.ii).

Putting

$$\rho = |\sqrt{r}|, \quad \theta' = \frac{\Theta}{\rho^2} - 1, \quad H = -\rho R.$$

where  $\theta'$  is the relative angular velocity  $d\theta/dt$ , Levi-Civita arrived at the system

$$\frac{d\theta}{d\rho} = -2\rho^2 \frac{\theta'}{H}, \quad \rho \frac{d\theta'}{d\rho} = -4(\theta' + 1) - 2\mu\rho \frac{W}{H} \quad (8.3.iii)$$

where  $W = \sin \theta \left( 1 - \frac{1}{\Delta^3} \right)$ .

He then proved that the singular trajectories along which  $S$  and  $P$  collide within a finite time correspond to the single infinity of solutions of (8.3.iii) which are

analytic for  $\rho = 0$  and to those alone. If  $\theta_0$  is the value of  $\theta(0)$  and  $\theta'(0) = -1$ , then these solutions are of the form

$$\theta = \theta_0 + \rho\alpha(\rho, \theta_0) \quad \theta' + 1 = \rho\beta(\rho, \theta_0) \quad (8.3.iv)$$

$\alpha$  and  $\beta$  being power series in  $\rho$ .

Hence if a collision takes place, the motion must be along one of these trajectories: that is it is necessary and sufficient that at each instant  $\rho$ ,  $\theta$  and  $\theta'$  satisfy the equation derived from (8.3.iv) by eliminating  $\theta_0$ . Since the first expression in (8.3.iii) shows that  $\theta_0$  is an analytic function of  $\rho$  and  $\theta$ , the second expression can be written

$$\theta' + 1 = \rho f(\rho, \theta) \quad (8.3.v)$$

where  $f$  is an analytic function of  $\rho$  in the domain of  $\rho = 0$ , for all real  $\theta$ , and which, as Levi-Civita showed, is a periodic function of  $\theta$  and can be theoretically determined. He then proved that not only is the relation (8.3.v) algebraic in the velocities, periodic and single-valued but also, as predicted by Painlevé, it is unique. In [1915] he extended this result to the problem of three bodies in a plane.

Encouraged by Mittag-Leffler and Phragmén, Levi-Civita reworked [1903b] in [1906], and he now removed the singularities using the transformation defined by

$$x + iy = (\xi + i\eta)^2 \quad p - iq = \frac{\varpi - i\chi}{2(\xi + i\eta)'}$$

which had the advantage of being a simpler transformation than the one he had used in [1903b] as well as being canonical. To regularise the system at the point S, he simply used the auxiliary variable  $\tau$  defined by

$$d\tau = \frac{dt}{\rho^2} \quad (\rho^2 = \xi^2 + \eta^2).$$

In addition to rationalising his result from [1903b], Levi-Civita was also concerned about its theoretical nature. When the bodies are treated as material points regularisation only requires the absence of a collision. But from a practical point of view, if the bodies concerned are real celestial bodies, then for the motion to remain regular, it is necessary to know not only that there will not be a collision but also that the distances  $r$  and  $\Delta$  will not go below a certain given limit  $\varepsilon$ . Thus Levi-Civita wanted to establish the initial conditions which would ensure that these mutual distances remained greater than  $\varepsilon$ .



By regularising the equations he had obtained an analytic representation of all possible arcs  $A$  of a trajectory inside a sufficiently small neighbourhood  $D$  around  $S$ . Since every arc not passing through  $S$  remains a finite distance away from  $S$ , the minimum distance  $\delta$  from  $S$  to an arc  $A$  can be expressed as a function (single-valued inside  $D$ ) of the initial conditions. Thus either  $\delta = 0$  (impact) or  $\delta > 0$ , and if  $\delta > \varepsilon$ , then, as Levi-Civita required, physical sense can be given to the mathematical result. However, he realised that was possible for a trajectory to penetrate  $D$  infinitely often, leaving along one arc  $A$  and re-entering along another arc  $A^*$ , each arc giving rise to a new value of  $\delta$  (with the possibility of having  $\delta = 0$  as a minimum). The problem was then to find out exactly what the lower limit of  $\delta$  would be. Unfortunately, Levi-Civita was unable to obtain any information about this lower limit, which meant that he could not make long term predictions about the value of  $\delta$ . He could only conclude that if in the region  $D$  the distance  $\delta$  is greater than  $\varepsilon$ , then the motion is regular in the neighbourhood of  $S$ , but if the trajectory leaves  $D$  and later re-enters again, then it is impossible to forecast its behaviour.

Of special interest in [1906b] is a result which Levi-Civita derived at the end of the paper concerning a solution to the differential equations. When considering the arcs of the trajectories inside the region  $D$ , he found a new single-valued solution to the differential equations different to the one given by the Jacobian integral. This was an unexpected result since it appeared to be in contradiction to Poincaré's theorem on the non-existence of any new solutions.

However, as Levi-Civita himself explained, there was no contradiction because the domains of validity of the two results were quite different. Poincaré's theorem established the non-existence of integrals single-valued with respect to the Keplerian variables which implied they were single-valued in the neighbourhood of all trajectories which have the same osculating ellipse; whereas Levi-Civita's result implied the existence of single-valued integrals either for only a part of the trajectory, or in the neighbourhood of trajectories which are not entirely elliptic.

While Levi-Civita was concerned with defining the conditions for a collision in the restricted three body problem, Bisconcini, a young lecturer at the University of Rome, was working on the same problem but in the general three body case. His results [1906] were published just prior to Levi-Civita [1906b] in the same volume of *Acta*, although they had been completed some two years earlier.

Starting with a system of three bodies  $P_0, P_1, P_2$ , with  $\rho_1 = P_0P_1$ ,  $\rho_2 = P_0P_2$ , Bisconcini considered the relative motion of  $P_1$  and  $P_2$  with respect to  $P_0$  and derived the equations of motion in Hamiltonian form. He then concentrated on the case where in the limit as  $(t \rightarrow t_1)$   $\rho_1 = 0$  and  $\rho_2 \neq 0$ , i.e. the case of a collision between  $P_0$  and  $P_1$ . Making the appropriate change of variable he arrived at a system of equations, which he called (S), analogous to the equations of motion (8.3.iii) derived by Levi-Civita in [1903b].

In order to proceed further, he found that he had to make the additional independent assumption that in the neighbourhood of  $P_0$  the angular velocity of  $\rho_1$  in the motion relative to  $P_0$  must be finite. Although he was unable to prove that this was necessarily the case, he had two good reasons for believing it to be true. In the first place, the assumption was known to be true in the restricted problem, and, in the second, as  $\rho_1$  gets progressively smaller the influence of the point  $P_2$  on the relative motion of  $P_0$  and  $P_1$  tends towards zero, at which point  $P_0$  and  $P_1$  essentially move as a two body system, in which case the angular velocity of  $P_1$  tends towards a finite limit.

Having made this additional assumption he was able to show that it was possible to put the singular trajectories of the system along which the points  $P_0$  and  $P_1$  collide, in a one-to-one correspondence with the solutions of the equations (S) which are analytic in the neighbourhood of the collision. Finally, he deduced two distinct analytic relations between the initial conditions which when satisfied proved that the motion was taking place along a singular trajectory, thereby indicating the existence of a collision in finite time.

Bisconcini's result was an important contribution, but it did not provide an altogether satisfactory solution to the problem. In the first place, his solution involved a complicated infinite series (in powers of the distance  $\rho_1$ ) which was not easy to use. But rather more problematic was the fact that the series was not directly applicable except when the interval of time between the initial instant and the collision was sufficiently short, and he gave no condition for this latter criteria. There was, therefore, still a need both to simplify the solution and to increase the range of its application. Moreover, neither Levi-Civita nor Bisconcini had addressed the question of the conditions for a triple collision.

A complete solution of the three body problem was finally achieved by Karl Sundman, an astronomer at the Helsinki Observatory. Sundman originally

published the essential features of his work in *Acta Societatis Scientiarum Fennicae* [1907, 1909] and then later, in response to an invitation from Mittag-Leffler, brought them together in a single memoir published in *Acta* [1912]<sup>17</sup>. Not only was Sundman's result quite remarkable but the methods he used were surprisingly simple. Essentially they depended on the application of Picard's extension to Cauchy's well known theorem on the existence of solutions to differential equations. The memoir also included a more direct proof of Painlevé's result, as well as a proof of the validity of Bisconcini's postulate concerning the angular velocity of the radius vector in the case of a binary collision.

One of the best known of Sundman's results involves the case of a triple collision. He proved that such an event could occur only if all the constants of angular momentum (areal velocity constants) were simultaneously zero, in confirmation of Weierstrass' conjecture made some twenty years earlier<sup>18</sup>. This then led him to the result that if these constants are not all simultaneously zero, and the initial conditions are known, there is a positive limit below which the two greatest of the mutual distances between the bodies cannot go. He further established that if the three bodies collide at the same point in space they move in the same plane which passes through their common centre of gravity, and as they approach collision they asymptotically approach the equilateral triangle or collinear configuration.

In the case of a binary collision, he showed that the singularity of the differential equations is not essential and so can be removed by a suitable change of variables. Considering the case where the differential equations ceased to be regular for  $t = t_1$ , he introduced a new independent variable  $u$  defined by

$$dt = r du \qquad (t = t_0 \text{ for } u = 0)$$

from which

$$u = \int_{t_0}^t \frac{dt}{r}$$

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<sup>17</sup> Wintner [1947, 428] observed that Sundman [1909] which included the theory of triple collisions did not warrant a review in *Fortschritte* or reproduction in Sundman [1912].

<sup>18</sup> Mittag-Leffler [1912, 58]. See Chapter 6.



where  $t_0$  is a real constant which is chosen in an appropriate way each time the variable  $u$  is employed. Thus the system is regular for  $u = 0$ , and  $u$  is known as the regularising variable.

Having introduced  $u$  into the equations Sundman established that the coordinates of the bodies could then be expanded in powers of  $(t - t_1)^{1/3}$ , and his insight was to realise that an analytic continuation of this expansion could be used to define a continuation of the motion of the bodies after collision. The coordinates then satisfy the differential equations for  $t > t_1$  with the same values of the energy constant and the areal velocity constant.

Sundman's description of the motion after a binary collision, showed that the orbits of the colliding bodies have a cusp point at the point of collision whereas the orbit of the third body is continuous in the neighbourhood of the collision. Furthermore, his analysis also showed that the motion can be continued after each new collision providing not all three bodies collide. In other words, the successive times of binary collision,  $t_1, t_2, t_3, \dots, t_k, \dots$  cannot have a limit point, so if the sequence of collisions is infinite, then  $t^* = \lim t_k = +\infty$ . Thus the motion can be continued indefinitely for values of  $t$  as great as desired.

However, there was a limitation to Sundman's regularisation transformation,  $dt = rdu$ , since it was dependent both on the constant  $t_0$  and on whichever of the mutual distances was tending towards zero. To overcome this restriction Sundman introduced another variable  $\omega$  defined by

$$dt = \Gamma d\omega,$$

where

$$\Gamma = (1 - e^{-r_0/l})(1 - e^{-r_1/l})(1 - e^{-r_2/l})$$

so that  $\omega = 0$  when  $t = 0$ , and where the two greater of the mutual distances  $r_0, r_1, r_2$  is greater than  $l$ .

$\Gamma$  has a given value for each real value of the time satisfying  $0 \leq \Gamma \leq 1$ , and consequently the variables  $\omega$  and  $t$  increase and decrease together. Thus there exists a continuous one-to-one correspondence between the real values of  $t$  and the real values of  $\omega$ , so that when  $t$  varies from  $-\infty$  to  $+\infty$ ,  $\omega$  varies likewise.

Given a real and finite value of  $\omega$ , say  $\omega^*$ , the coordinates of the three bodies, their mutual distances and the time can be expanded as power series in  $(\omega - \omega^*)$ , where



the radius of convergence of these expansions is always greater than a positive limit independent of the value of  $\omega^*$ , i.e.

$$|\omega - \omega^*| \leq \Omega.$$

The coordinates of the three bodies, their mutual distances and the time are thus analytic functions of  $\omega$  in a band of breadth  $2\Omega$  contained between two lines parallel with the real axis and symmetric with respect to this axis.

Finally by introducing a new variable  $\tau$  defined by

$$\tau = \frac{e^{\pi\omega/2\Omega} - 1}{e^{\pi\omega/2\Omega} + 1},$$

and using the transformation

$$\omega = \frac{2\Omega}{\pi} \log \frac{1 + \tau}{1 - \tau},$$

analogous to the transformation used by Poincaré [1882a, 1886], the band in the  $\omega$  plane can be transformed into a circle of unit radius in the plane of the new variable  $\tau$ . The coordinates of the three bodies and the time are now analytic functions of  $\tau$  everywhere within the unit circle in the  $\tau$  plane and can be expanded in convergent series in  $\tau$  for all real values of the time. Furthermore, the same values of  $l$  and  $\Omega$  hold for a group of motions corresponding to different initial conditions, and the different terms of the expansions can be calculated by successive differentiation with respect to  $\tau$  as soon as the values of  $l$  and  $\Omega$  are determined.

Sundman summarised his achievement in the final theorem of [1912]:

*"In the three body problem, if the constants of angular momentum are not all zero and the initial coordinates and velocities of the bodies are given for a finite time, then two constants  $l$  and  $\Omega$  can be found such that by introducing the variable  $\tau$  instead of the variable  $t$ , the coordinates of the three bodies, their mutual distances and the time can be expanded in entire power series in  $\tau$ , which converge for  $|\tau| < 1$  and represent the motion for all time, whatever collisions occur between the bodies, provided that the motion is continued analytically as described above." [1912, 178].*

Sundman had thus provided a function theoretical proof to the problem which had engaged the minds of many great mathematicians and astronomers since the publication of Newton's *Principia*, a period of well over two hundred years. He had theoretically solved the three body problem. It was a remarkable achievement and

all the more so considering the simplicity of his solution: throughout his analysis depended only on classical results in the theory of differential equations. It is worth recalling that it was only some twenty years earlier that Poincaré had stated that he believed the complete resolution of the problem would require the use of new transcendental functions [P2, 6]. Furthermore, Tisserand, in the final volume of his *Mécanique Céleste* published in 1896, had said:

*"The rigorous solution of the three body problem is no further advanced today than during the time of Lagrange, and one could say that it is manifestly impossible."*<sup>19</sup>.

Although the significance of Sundman's achievement was certainly recognised by his contemporaries - Mittag-Leffler's encouragement had resulted in the rewriting and publication of his results in *Acta*; in 1913 the French Academy awarded him the *prix Pontécoulant* and doubled the value of the prize; and both Picard [1913] and Marcolongo [1914] wrote enthusiastic reviews - interest in his work was not consistently maintained. In the decade after the publication of the *Acta* paper minor corrections, simplifications and extensions to his results appeared, the most notable of which was a simplification due to Levi-Civita [1918] which provided a canonical regularisation of the three body problem in the neighbourhood of a binary collision<sup>20</sup>. But from then on and for the next thirty years, Sundman's work seem to have been almost forgotten. Why did such an important and long-awaited result almost fade into obscurity?

Firstly there was the practical limitations of his results. The rate of convergence of the series which he had derived was perceived to be extremely slow and so for practical purposes the classical divergent series were thought to be more useful<sup>21</sup>. For example, while George Birkhoff enthusiastically embraced Sundman's theoretical achievement:

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<sup>19</sup> Tisserand [1896, 463].

<sup>20</sup> See also Hadamard [1915] and Birkhoff [1922]. An analysis of the relationship between the work of Sundman and that of Levi-Civita is given in the article by L. Dell'Aglio and G. Israel "La regolarizzazione delle equazioni del problema dei tre corpi: Levi-Civita e Sundman, due diverse direzioni di ricerca" to appear in *Physis*.

<sup>21</sup> For an explanation of the slow rate of convergence see Saari [1990]. It is of interest to note a challenge to this traditional view put forward by Cesco [1961].

*"It is not too much to say that the recent work of Sundman is one of the most remarkable contributions to the problem of three bodies which has ever been made."*<sup>22</sup>.

his verdict on its application was in quite another vein:

*"Unfortunately these series are valueless either as a means of obtaining numerical information or as a basis for numerical computation, and thus are not of particular importance."*<sup>23</sup>.

Secondly, the results Sundman obtained furnished no qualitative information about the nature of the motion. He had provided a mathematical solution but not one which revealed general information about the form of the trajectories.

Forty years after the appearance of Sundman's *Acta* paper interest began to be revived in his work. Jean Chazy [1952] published an appreciation of Sundman's result in which he looked both at the contents of the memoir as well as its influence. Although not ignorant of the limitations imposed by the generality of Sundman's result, Chazy's account is a glowing testimonial to its effect on the direction of subsequent research into the three body problem:

*"Already this solution (Sundman's) has led to researches into collisions and close approaches between the three bodies, and prompted the study of infinite branches of the trajectories of the three bodies, and the study of motion as time goes towards infinity. Already the determination of singular trajectories has led to substantial results in the representation and the distribution of trajectories of the three body problem - the consideration of which is as necessary as the study of singular points in the study of an analytic function. Without having resolved in one go all the qualitative questions posed by the three body problem, Sundman's solution has given rise to essential progress in the resolution of these questions. And plenty of questions remain open following Sundman's work - just as in the work of Poincaré."*<sup>24</sup>.

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<sup>22</sup> Birkhoff [1927, 260].

<sup>23</sup> Birkhoff [1920, 53].

<sup>24</sup> Chazy [1952, 190].

Chazy himself had researched extensively into aspects of both the three and  $n$  body problems with notable success and so was well qualified to judge Sundman's achievement. In particular he had investigated the long term behaviour of the solutions of the three body problem making extensive use of both Sundman's regularising variable and Poincaré's theory of invariant integrals. By studying the 12-dimensional phase space defined by the positions and velocities of two of the bodies relative to the third, he provided a classification of the final motions of the problem [1922]. Apart from the bounded and oscillatory motions, he found three different types in which all three mutual distances became infinite: hyperbolic, parabolic, hyperbolic-parabolic; and two different types in which two of the mutual distances became infinite: hyperbolic-elliptic, parabolic-elliptic. In each case the different types were distinguished by the nature of the final velocities of all three bodies<sup>25</sup>.

Chazy's account of Sundman's work was followed in 1955 by a modernised version of Sundman's theorems in Carl Siegel's acclaimed text on celestial mechanics [1971]. Siegel was in no doubt as to the importance of Sundman's results, placing them as one of the most significant developments in the transformation theory of differential equations after the work of Poincaré.

### 8.3.3 The $n$ body problem

The first person to make an impression on the closing question in Painlevé's Stockholm lectures on whether non-collision singularities exist in the  $n$  body problem for  $n \geq 4$  was Hugo von Zeipel [1908]. Although von Zeipel ultimately devoted himself to the more practical aspects of astronomy, he had begun his academic career by studying periodic orbits for his doctoral thesis. His interest in singularities almost certainly stems from his stay in Paris (1904-1906) where he studied under both Poincaré and Painlevé<sup>26</sup>. As noted in 6.7, von Zeipel was the author of an extensive article on Poincaré's celestial mechanics which appeared in 1921 in the edition of *Acta* devoted to Poincaré.

Von Zeipel's theorem on singularities in the  $n$  body problem (as given by McGehee [1986]) states:

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<sup>25</sup> Further details of Chazy's research can be found in Arnold [1985, 67].

<sup>26</sup> See McGehee [1986].



*"If some of the particles do not tend to finite limiting positions as  $t$  approaches  $t_1$ , then one has necessarily*

$$\lim (t \rightarrow t_1) R = \infty,$$

*where  $R$  is the maximum of the mutual distances."*

In other words, a noncollision singularity can occur only if the system of particles becomes unbounded in finite time. At first sight, it would seem that such a singularity is an impossibility, since a particle escaping to infinity in finite time would have to acquire an infinite amount of kinetic energy. However, as Xia [1992] points out, since the potential energy of the system is not bounded from below, there is no reason why the kinetic energy should be bounded from above. Unfortunately, von Ziepel's work appears to have faded into obscurity, for twelve years after his theorem was published, Chazy [1920] published exactly the same theorem but with no reference to the earlier version, indicating that by that date von Ziepel's work had become forgotten<sup>27</sup>.

However, despite von Zeipel's result and the interest in it rekindled by Chazy, the proof of whether there exist noncollision singularities turned out to be particularly elusive, and it is only recently that definite results have been achieved. Mather and McGehee [1975] made a significant contribution by constructing a solution to the collinear four body problem in which the particles escape to infinity in finite time, although this was still not a complete resolution of the problem since their solution contained infinitely many elastic collisions prior to the appearance of the noncollision singularity.

The question was finally resolved in the affirmative by Zhihong Xia [1992] who, using "symbolic dynamics"<sup>28</sup> proved the existence of a noncollision singularity for a system of five particles moving in three dimensional space. Xia's example involves two binary pairs, the particles in the same pair having the same mass, and a particle oscillating between them. The single particle oscillates along a fixed axis and each binary pair orbits in a different plane at right angles to the fixed axis, each pair rotating in opposite directions. With this symmetric configuration Xia showed that it is possible to set the initial conditions so that the energy gain of the

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<sup>27</sup> McGehee's [1986] paper traces the history of Von Zeipel's result and gives a modern version of von Zeipel's proof.

<sup>28</sup> For further observations about "symbolic dynamics" see the discussion of Morse's work given in the Epilogue.

single particle and the corresponding energy loss in the binary pairs is such that all five particles tend to infinity in finite time, and furthermore, that the example can be modified for  $n > 5$ .

After Xia's result was announced, and using a different approach, Gerver [1991] proved the existence of a noncollision singularity in a planar  $3n$  body problem with  $n$  very large. The question for  $n = 4$  still remains open<sup>29</sup>.

## 8.4 Numerical investigations into periodic solutions

### 8.4.1 Darwin

George Darwin, the second son of Charles Darwin, was elected a fellow of Trinity College, Cambridge, in 1868, and from 1883 held the Plumian Chair of Astronomy<sup>30</sup>. Darwin was very much a traditional applied mathematician whose interest in a problem was stimulated by putting a mathematical hypothesis to the test by way of numerical calculations. Unlike some of his contemporaries, such as Adams and Hill, he was essentially a practical mathematician who, rather than calculating to an exceptional degree of accuracy, took the pragmatic approach assessing the situation and calculating accordingly.

Darwin was a great admirer of Poincaré's work, and their shared interest in the work of Hill and periodic orbits was not the first time that their work had overlapped<sup>31</sup>. Some ten years earlier they had both been involved in investigating the figures of equilibrium of a rotating liquid and comparing their work on this topic provides a good illustration of the complementary nature of their approaches to a problem. Although each investigation had resulted in the evolution of a "pear-shaped" figure, Poincaré's analysis had involved a process of evolution forwards, while that of Darwin consisted of working backwards through time<sup>32</sup>. Darwin's study of periodic orbits, although owing much to Hill, clearly shows the

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<sup>29</sup> A discussion of the research into the existence of noncollision singularities of the  $n$  body problem is given in Diacu [1993].

<sup>30</sup> For a biography of Darwin see Sir Francis Darwin [1916], and for an assessment of Darwin's scientific work see Brown [1916a].

<sup>31</sup> See Darwin [1900].

<sup>32</sup> Poincaré [1885b], Darwin [1887].

continuation of his interest in the work of Poincaré, his orbits giving clear and tangible illustrations of many of the features previously identified by Poincaré.

Despite the interest in periodic orbits which had been generated by Poincaré's memoir and the subsequent appearance of the first volume of the *Méthodes Nouvelles*, Darwin's [1897] paper contained the first systematic search for such orbits. The paper, which had taken him three years to complete, contained the numerical calculation of periodic solutions of the restricted three body problem, together with a discussion of their stability. It provided not only extensive details of the numerical results but also a full description of the mathematical methods used to obtain them.

He derived the equations of motion for the problem using a formulation in which  $S$ , the larger of the two primaries, was placed at the origin of a coordinate system which was rotating concurrently with the second primary  $J$ , with the planetoid  $P$  moving in the plane of  $J$ 's orbit.

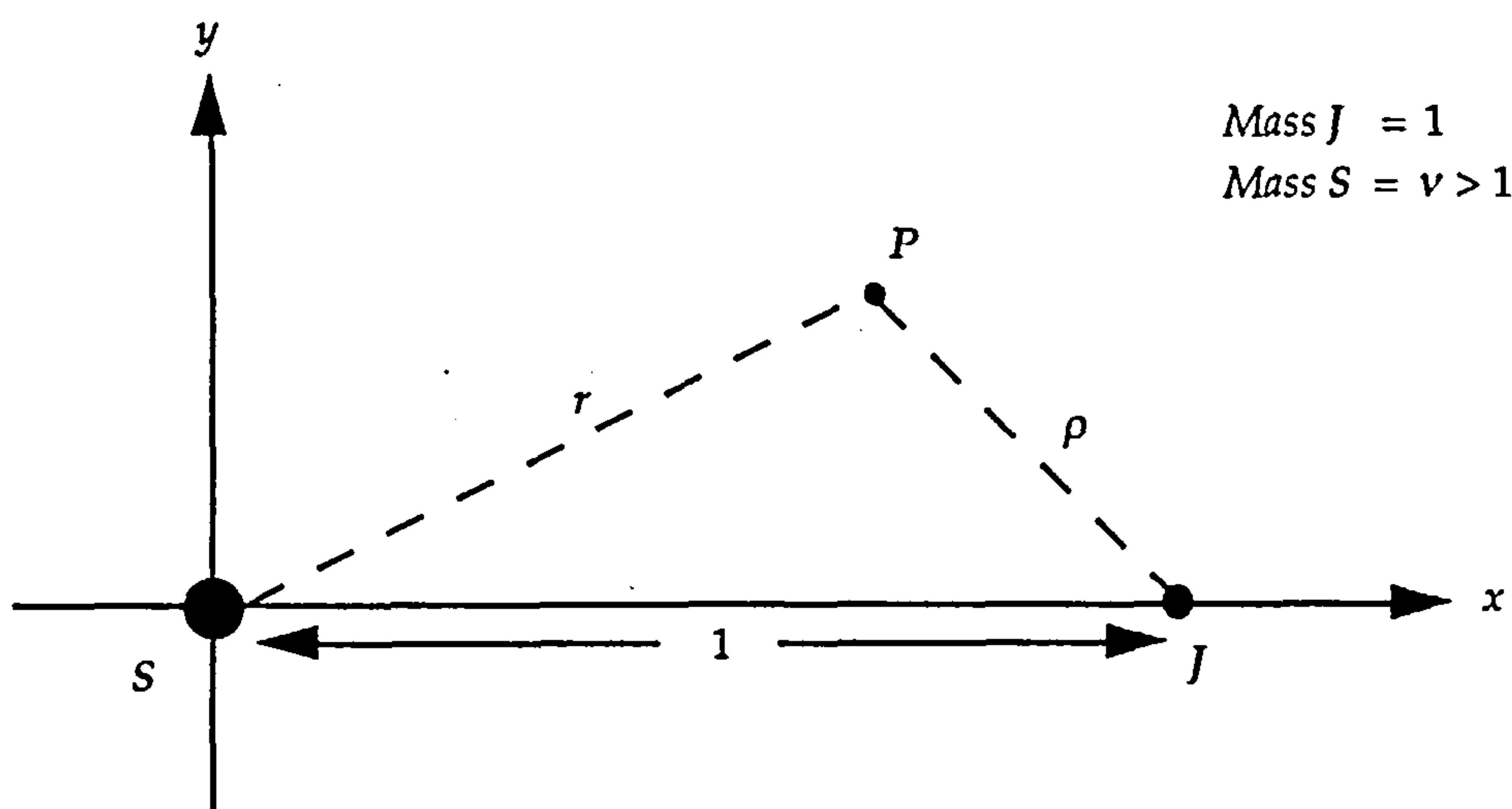


FIG. 8.4.i. Darwin's formulation of the restricted three body problem in rotating coordinates

Solving the equations he obtained the Jacobian integral

$$V^2 = 2\Omega - C, \quad \Omega = \nu \left( r^2 + \frac{2}{r} \right) + \left( \rho^2 + \frac{2}{\rho} \right)$$

where  $V$  is the angular velocity of the planetoid,  $\Omega$  is the overall potential of the system inclusive of its rotation, and  $C$  is the Jacobian constant.

Darwin then took up Hill's idea of partitioning space according to the value of the Jacobian constant  $C$ . Since for real motion  $V^2 > 0$ , which implies that  $2\Omega > C$ , and consequently the family of curves  $2\Omega = C$  (Hill's curves of zero velocity) define the regions of space in which the motion of the planetoid is in some way confined. The curves themselves are the locus of points for which the three bodies move for an instant as parts of single rigid body.

Darwin considered  $\rho$  (the distance between the planetoid and the body  $J$ ) as fixed and looked for solutions for  $r$  (the distance between the planetoid and the body  $S$ ). This led to cubic equations in  $r$  which could then be solved to get values of  $r$  and  $\rho$  to satisfy  $2\Omega = C$ . He then looked at what happened for different values of  $C$ . He found that as he changed the value of  $C$ , the curve went through four critical stages ( $\alpha$ ), ( $\beta$ ), ( $\gamma$ ) and ( $\delta$ ), each of which marked a transition of the shape of the curve, the points in ( $\alpha$ ), ( $\beta$ ) and ( $\gamma$ ) being situated on the line  $SJ$ , while the points in ( $\delta$ ) were symmetrically placed either side of  $SJ$  (see FIG. 8.4.ii). More specifically, at

- ( $\alpha$ ) the internal ovals coalesce to a figure-of-eight, and  $r = 1 - \rho$ ;
- ( $\beta$ ) the hourglass shape coalesces with the external oval, and  $r = 1 + \rho$ ;
- ( $\gamma$ ) the horse-shoe breaks at the toe, and  $\rho = r + 1$ ;
- ( $\delta$ )  $C$  is a minimum, and  $r = 1$ ,  $\rho = 1$ , and  $C = 3v + 3$ .

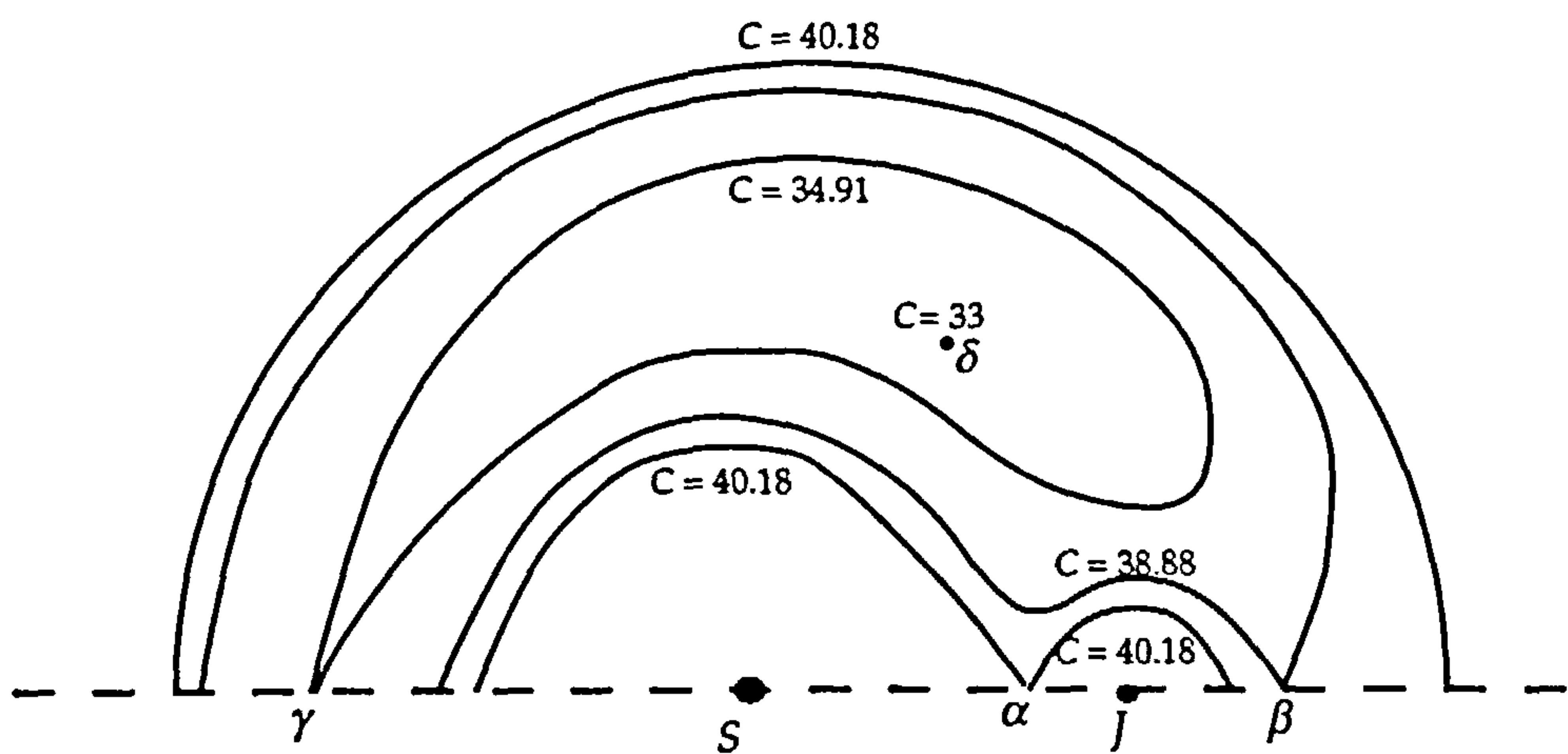


FIG. 8.4.ii. Curves of zero velocity,  $v = 10$ .



In the first three cases Darwin found that the motion at the points was dynamically unstable, while in the last case, which is in fact Lagrange's equilateral solution, he believed the motion to be always stable. (Later the astronomer S. S. Hough [1901] showed that Darwin had made a error with regard to this last conclusion and proved that the ( $\delta$ ) points were only stable for  $v > 24.9599$ , otherwise they were unstable).

Using the particular value  $v = 10$  (which is equivalent to a value for the mass parameter  $\mu$  of  $1/11$ ), Darwin made a classification of the possible periodic orbits depending on the value of  $C$ . To give some examples, he found that if  $C$  is greater than 40.1821 then the planetoid could either move as a superior planet around both  $S$  and  $J$ , or it could move as an inferior planet around  $S$ , or it could move as a satellite around  $J$ ; whereas if  $C$  is less than 40.182 and greater than 38.8760 then the planetoid could move in the three ways as above but in addition it could also move in an orbit which incorporated both of the two latter characteristics; or if  $C$  is less than 33, there is no region which is forbidden to the planetoid. All these and the other different possibilities can be deduced quite easily from FIG. 8.4.ii.

Darwin included a detailed exposition of his methods of integration as well as a discussion on the question of stability of the orbits. In the latter he found that his conclusions were in agreement with those of Poincaré concerning the disappearance in pairs of periodic solutions.

As far as actually calculating the orbits was concerned, due to the difficulties involved in discovering periodic orbits making more than one revolution around either of the primaries (or any other point in space) Darwin confined his attention to looking for what he called "simple" periodic orbits. These were periodic orbits which were re-entrant after a single circuit, although they could (and did) include loops. He conjectured that the periodic orbits were the critical cases which separated the orbits into different classes<sup>33</sup>. Thus to find these orbits it was necessary to trace an orbit through its transformation from one class to another.

Due to the potential extent of the field of investigations, Darwin limited himself further by ignoring both the superior planets and retrograde orbits, although later

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<sup>33</sup> As Szebehely [1967, 434] has pointed out, Darwin's conjecture if true provides a good motivation for studying periodic orbits, but unfortunately its imprecise formulation makes it impossible to establish its validity.

he did make start on investigating these two cases [1909]. In addition, he only considered a range of values for the Jacobian constant between 38 and 40.5.

Darwin's classification of periodic orbits included one family of planets orbiting  $S$ , two families of oscillating satellites each oscillating around a Lagrangian point on the line  $SJ$ , and three families of satellites orbiting  $J$ . He was however struck by the fact that one of these latter families appeared to exhibit a strange characteristic. As the value of the Jacobian constant decreased, the orbit seemed to develop from a simple closed oval into the form of a figure-of-eight in which one loop went round  $J$  and the other went round a Lagrangian point on the line  $SJ$ . Moreover, accompanying this change of form was a change from stability to instability, a discontinuity which Darwin was unable to explain. Clearly there was some aspect of the behaviour which Darwin's analysis had failed to capture. As mentioned in 7.2.4, this seemingly anomalous result attracted the attention of Poincaré who provided the explanation [MN III, 352]. There was in fact no anomaly, Darwin had simply been mistaken in classifying the two forms of orbit together when in reality they each belonged to independent families. However, although Poincaré had proved the existence of two different families, his account did not explain the disappearance of the stable orbits and the appearance of the unstable ones. These details were filled in by Hough [1901], who recognised that one of the sources of error was Darwin's failure to take into account that the retrograde orbits were the analytical continuation of the direct orbits. In addition, Hough also indicated the existence of another family of figure-of-eight orbits which Darwin himself had not detected.

In the meantime, Darwin had independently discovered his mistake and in [1909] added computational confirmation to Hough's theoretical results<sup>34</sup>. Also included in this second paper are further investigations into the periodic orbits of superior planets, retrograde orbits and orbits of ejection, the latter being those which provide the transitional form between direct and retrograde orbits.

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<sup>34</sup> Darwin's appreciation of Hough's contribution is marked by his inclusion of Hough's [1901] paper in his own collected works, sandwiched between [1897] and [1909].

### 8.4.3 Moulton and Strömgren

Taking their lead from Darwin, two important centres of numerical activity grew up which were involved in the study of periodic orbits and which made significant contributions to the quantitative analysis of the three body problem.

The first of these was in the United States where, between 1900 and 1917, under the leadership of F. R. Moulton, a research group prospered engaged in both analytical and numerical explorations of periodic orbits. Their work was published in a series of papers which were assembled in a substantial volume in 1920<sup>35</sup>. As far as the restricted three body problem was concerned, their numerical work centered largely on locating periodic orbits for the case where the two primaries have equal masses, that is when the mass parameter  $\mu = 0.5$ , which is important in stellar dynamics.

The second, and ultimately the more prolific of the two research groups, was based at the Copenhagen Observatory. Here from 1913 to 1939, Elis Strömgren and his colleagues calculated a comprehensive classification of the periodic orbits for the restricted three body problem, again most of them for the case when the masses of the primaries are equal. Indeed so extensive was their work in this particular case that it has now become known as the Copenhagen problem. Of special note is that the calculations of Strömgren and his fellow astronomers provided the foundations for the celebrated work of Maurice Hénon [1965] who studied the stability of periodic orbits by considering their intersections with Poincaré's transverse sections.

A clear and concise account of the work of the schools of both Moulton and Strömgren, together with comprehensive references, is given in Szebehely [1967].

Finally, it is interesting to note a point made by Szebehely concerning the value of the mass parameter  $\mu$ . By making a comparison between certain results of Darwin and Strömgren which shows, contrary to what might have been expected, that the value of  $\mu$  is not necessarily representative of the magnitude of perturbations, he provides an attractive example of the value of this kind of quantitative analysis<sup>36</sup>.

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<sup>35</sup> Moulton et al [1920].

<sup>36</sup> Szebehely [1967, 491].

## 9. Hadamard and Birkhoff

### 9.1 Introduction

In 1896 the prestigious *Prix Bordin de l'Académie des Sciences* was won by Jacques Hadamard, then a professor at the University of Bordeaux. The set topic had been to improve the theory of geodesics, the interest in the topic deriving from the use of geodesics on surfaces to represent the trajectories of motion in dynamical systems. Hadamard's response resulted in two papers [1897] and [1898]. The first, which contained most of the material he had submitted for the prize, was primarily a study of geodesics on surfaces of positive curvature, while the second, which was published after he had moved to the Sorbonne, expanded on ideas proposed in the prize paper and dealt with geodesics on surfaces of negative curvature. Both these papers are characterised by a qualitative analysis inherited from Poincaré. In the first Hadamard appealed to results from classical differential geometry, while in the second, in which Poincaré's influence is strikingly evident, Hadamard's discussion is dominated by topological considerations. Moreover, it was through working on these areas of mathematics directly derived from Poincaré that Hadamard was led to one of his most important and profound ideas: that of the "well-posed problem".

Hadamard's use of Poincaré's qualitative approach to the theory of differential equations in his Bordin paper provides a powerful illustration of the strength of Poincaré's new methods. However, although Hadamard continued to promote



Poincaré's ideas in the genre, he himself did no further active work on the topic<sup>1</sup>. This was not the case with George Birkhoff. Poincaré's *Méthodes Nouvelles* provided Birkhoff with inspiration which resulted in an abundance of remarkable research throughout his career. As Oscar Veblen remarked:

*"Birkhoff took up the leadership in this field (dynamics) at the point where Poincaré laid it down."*<sup>2</sup>.

Birkhoff's generalisations and extensions of Poincaré's ideas incorporated a vigorous use of topology and, as with Poincaré, the periodic motions play a central role in his theory. His ideas are presented with an admirable clarity of exposition and it was as a result of his efforts that the qualitative theory of dynamical systems emerged as a fully-fledged subject independent of its roots in the discipline of celestial mechanics. The second part of this chapter contains a discussion of three of Birkhoff's early papers on dynamics: [1912], [1915] and [1917].

In the spirit of the previous chapter, the following account is intended only to give an overall view of the different ways that Hadamard and Birkhoff took up and developed some of Poincaré's ideas, thereby providing the foundations for modern dynamical systems theory. As stated at the beginning of the thesis, consideration has been given predominantly to work which was produced prior to 1920.

## 9.2 Hadamard and geodesics

Hadamard's submission for the Bordin prize was the first major paper in which he tackled a subject other than analysis. He had been attracted to the qualitative theory of differential equations through studying Poincaré, and so was well-placed to take advantage of the opportunity presented by the Bordin competition to investigate a topic with importance for the qualitative understanding of dynamical problems. In particular he was drawn by Poincaré's idea of the centrality of the periodic motions, and in the context of the theory of geodesics he made an appealing

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<sup>1</sup> In 1920 and 1925, while at the Rice Institute in Texas, Hadamard gave a series of lectures on Poincaré's work, each of which included a discussion of Poincaré's qualitative theory of differential equations. See Hadamard [1922], [1933].

For Hadamard's views on Poincaré's mathematical œuvre, see Hadamard [1912], [1913], [1921].

<sup>2</sup> Veblen [1946, 282].

analogy in which he described the closed geodesics as fulfilling the role of a coordinate system to which all other geodesics are then related [1898, 775].

### 9.2.1 Geodesics on surfaces of positive curvature

Underlying Hadamard's first paper on geodesics on surfaces was the concept of partitioning the surface using properties of the force function of the dynamical system in order to categorise the behaviour of the trajectories of the system.

To illustrate the basic idea Hadamard considered the problem of the motion of a mass particle on a smooth surface of revolution with cylindrical polar coordinates  $(r, \theta, z)$  under the action of a force function  $U$  independent of  $\theta$ . In general the trajectory of the particle will remain between two parallels of the surface which, through a suitable choice of the constants of integration, can be chosen so that the smaller of the two values of  $r$  corresponds to the larger of the two values of  $U$  and vice versa, and so in the case of a geodesic the two parallels will have the same value of  $r$ . In this way a region of the surface is defined in which  $r$  and  $U$  vary inversely. Thus if the particle is moving on the surface of a sphere then it generally passes infinitely often into the lower hemisphere.

More specifically Hadamard considered the motion of a particle moving on a smooth surface under the influence of a single-valued potential function  $V$  of the coordinates of the surface. He then made a partition of the surface based on the distribution of the successive maxima and minima of the function  $V$ .

He began with the system of differential equations

$$\frac{dx_1}{X_1} = \dots = \frac{dx_n}{X_n} = dt \quad X_i = X_i(x_1, \dots, x_n)$$

in which  $X_i$  are analytic functions of the  $x_1, \dots, x_n$ , which are regarded as the coordinates of a point  $M$  in an  $n$  dimensional space  $E_n$ . Thus as  $M$  describes a trajectory,  $V$  will in general have an infinite number of successive maxima and minima. If

$$X(f) = X_1 \frac{\partial f}{\partial x_1} + \dots + X_n \frac{\partial f}{\partial x_n}$$

then these maxima and minima are described by

$$X(V) = 0 \quad (9.1.i)$$

which represents a manifold of  $n - 1$  dimensions. If  $V$  is a maximum, then  $X[X(V)] \leq 0$ , and if  $V$  is a minimum then  $X[X(V)] \geq 0$ , the former inequality defining that part of the surface (9.1.i) where the trajectory passes from the region  $X(V) > 0$

to the region  $X(V) < 0$ , and the latter inequality defining the converse. The boundary of these two parts of the surface is then composed of the points where the trajectory is tangent to the surface (9.1.i). Thus, excluding certain exceptional trajectories, each trajectory crosses the surface (9.1.i) infinitely often and passes successively into each of the regions determined by the two inequalities. The exceptions occur when the variation in  $V$  is always in the same direction. For example, if  $V$  is always increasing, then either it becomes infinite or it tends to a limit. Hadamard excluded the first possibility by assuming that  $V$  remained finite in the domain in which the  $x$  remain finite, but took account of the second with the lemma (now sometimes known as the derivatives theorem<sup>3</sup>):

*Lemma:* If, when  $t$  increases indefinitely, the function  $V(t)$  tends towards a limit and the first  $n + 1$  derivatives exist and are finite, then the first  $n$  derivatives tend to 0.

Thus if the function  $V$  and its partial derivatives up to 3rd order, and the functions  $X_i$  and their partial derivatives up to 2nd order, are finite as  $M$  moves along a trajectory, then either the trajectory crosses the regions of the surface (9.1.i) defined by the inequalities infinitely often or it is asymptotic to the boundary of these two regions.

In the particular case of a particle confined to move on a two-dimensional smooth surface, and where the function  $V$  has an infinite number of maxima and minima, Hadamard showed that the surface itself could be divided into two regions. The first, which he called the *attractive* region, contains all points of the trajectory where  $V$  has a minimum, i.e. it contains an infinite number of distinct parts of the trajectory each of which is of finite length. The second, which he called the *repellent* region, contains all points of the trajectory where  $V$  has a maximum and is a region in which the particle cannot indefinitely remain. The particle passes infinitely often through each of the regions and, consequently, crosses the boundary between the regions infinitely often. If the surface is regular at every point and  $V$  is a regular function of the coordinates of the surface, then in the exceptional cases where, after some given moment, the variation in  $V$  remains in the same direction, i.e. where there are only a finite number of maxima and minima, the theory shows

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<sup>3</sup> This lemma was also proved independently by both Kneser and Littlewood. See Cartwright [1965, 744].



that either the trajectory remains in the attractive region for an arbitrarily long period or it asymptotically approaches either a point of unstable equilibrium or a closed trajectory.

If the particle describes a geodesic, then the geodesic passes through each of the two regions infinitely often or it is asymptotic to a closed geodesic which represents the boundary between the two regions. Thus each geodesic which passes through each of the regions infinitely often cuts the closed geodesic infinitely often. Moreover, when the curvature of the surface is everywhere positive Hadamard was led to the stronger result that every closed geodesic is cut infinitely often by every other geodesic. In particular, a surface of positive curvature cannot have two closed geodesics which do not intersect. A result which is clearly demonstrated on the surface of a sphere where every great circle is cut by every other great circle.

A second important result which was contained both in the Bordin paper and in [1897] was Hadamard's proof of the converse of Dirichlet's theorem on the stability of equilibrium: that a position of equilibrium is unstable if the kinetic energy is not a maximum. In 1895 Kneser had proved the result for the particular case in which the kinetic energy is a minimum, and at the time of the competition a general proof was still believed to be unavailable. However, as Hadamard acknowledged in [1897], he had in fact been preceded in the general result by Liapunov [1907]<sup>4</sup>.

In the final part of [1897] Hadamard considered the question of the *domain* of a trajectory or a geodesic, and here he made explicit use of several of Poincaré's ideas. To explain what he meant by the domain he used the simple example of a geodesic on a surface of revolution. If the geodesic is not closed but oscillates in a strip between two parallels then following the geodesic in a given direction, the strip is gradually filled out by the geodesic and hence the strip is the domain of the geodesic.

Hadamard's ideas about the domain stemmed from a direct analogy with the sets introduced by Poincaré in [1885, 142] and also involved the use of Poincaré's theory of invariant integrals. To define the domain of a given trajectory Hadamard referred to the simplest case of a dynamical system with two degrees of freedom in which a

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<sup>4</sup> At the time of the competition, Liapunov's paper had only been available in Russian and was unknown to Hadamard, and it was only in the following year when an extract of Liapunov's results was published in the *Journal de Mathématiques* that Hadamard became aware of Liapunov's work. See 8.1.



state of the system is represented by the coordinates of a point in a four-dimensional space  $E_4$ . He considered the part of a surface  $\Sigma$  in a three dimensional space  $E_3$  which is crossed infinitely often by the trajectory. If the trajectory is not closed then the points of intersection will be distinct. This set of points will admit at least one limit point and, for increasing values of the time  $t$ , the trajectory will pass infinitely often through the neighbourhood of this point. By considering all possible surfaces  $\Sigma$ , this gives rise to a closed set of limit points which is then defined to be the domain of the trajectory in the space  $E_3$ . Moreover, if the trajectory returns infinitely often to the neighbourhood of an arbitrary one of the limit points then the set is not only closed but it is also perfect. Hadamard used Poincaré's recurrence theorem to show that the trajectories for which this is not the case can be considered exceptional.

Although Hadamard did not obtain many complete results in [1897], he was successful in establishing a new kind of framework from which a cogent theory could be developed. To echo Poincaré's sentiments expressed in his report on the competition<sup>5</sup>, the importance in Hadamard's paper lay in the abundance of new ideas it contained and the potential for future research which it provided, a potential which Hadamard himself developed in [1898]. Hadamard had not only boldly followed Poincaré in adopting a strictly qualitative approach to the problem but he had also demonstrated the power of Poincaré's ideas by showing how several of them could be applied to a particular class of problems.

### 9.2.2 Geodesics on surfaces of negative curvature

In terms of actual results Hadamard was much more successful in his second paper [1898] for it contained a full classification of the different types of geodesics that could exist upon surfaces with everywhere negative curvature. As Poincaré [1905a, 39] observed, Hadamard in responding to the Bordin committee's observations concerning future research on geodesics, provided a complete solution to the problem of geodesics on surfaces of negative curvature. Again Hadamard's approach was qualitative but this time he focused on the topology of the surface, in particular the order of connectivity, and used this to categorise the geodesics.

Hadamard opened the discussion by making the hypothesis that the surface under consideration consisted of  $n$  infinite independent *sheets*, each of which is limited by

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<sup>5</sup> *Comptes Rendus* 123 (December 1896), 1109-1111.

a curve  $C$  and is generated by the motion of the curve as it extends to infinity. Each infinite sheet can then be regarded topologically as bounded by the curve  $C$  in its initial position and by the same curve as it extends to infinity, and therefore topologically equivalent to a circular annulus. In other words it is a doubly-connected surface.

The best known examples of such surfaces are the hyperbolic paraboloid and the hyperboloid of one sheet, but, as Hadamard pointed out, surfaces of negative curvature can be formed having any number of infinite sheets. There are, as he observed, surfaces represented by equations of the type

$$z = k \log \frac{\delta_1, \dots, \delta_m}{\delta'_1, \dots, \delta'_n}$$

where  $k$  is a constant, and  $\delta_1, \dots, \delta_m; \delta'_1, \dots, \delta'_n$  are the distances projected on the  $x, y$  plane of the point  $(x, y)$  to the fixed points  $P_1, \dots, P_m; P'_1, \dots, P'_n$  on the plane. This surface has  $m + n + 1$  infinite sheets, with  $m$  directions on the side of  $z > 0$ ,  $n$  directions on the side of  $z < 0$  and one direction in the horizontal sense.

As an example, Hadamard included a diagram of the surface

$$z = k \log \frac{\delta}{\delta'}$$

which corresponds to  $m = n = 1$  and which has the general form of FIG. 9.2.i [1898, 741].

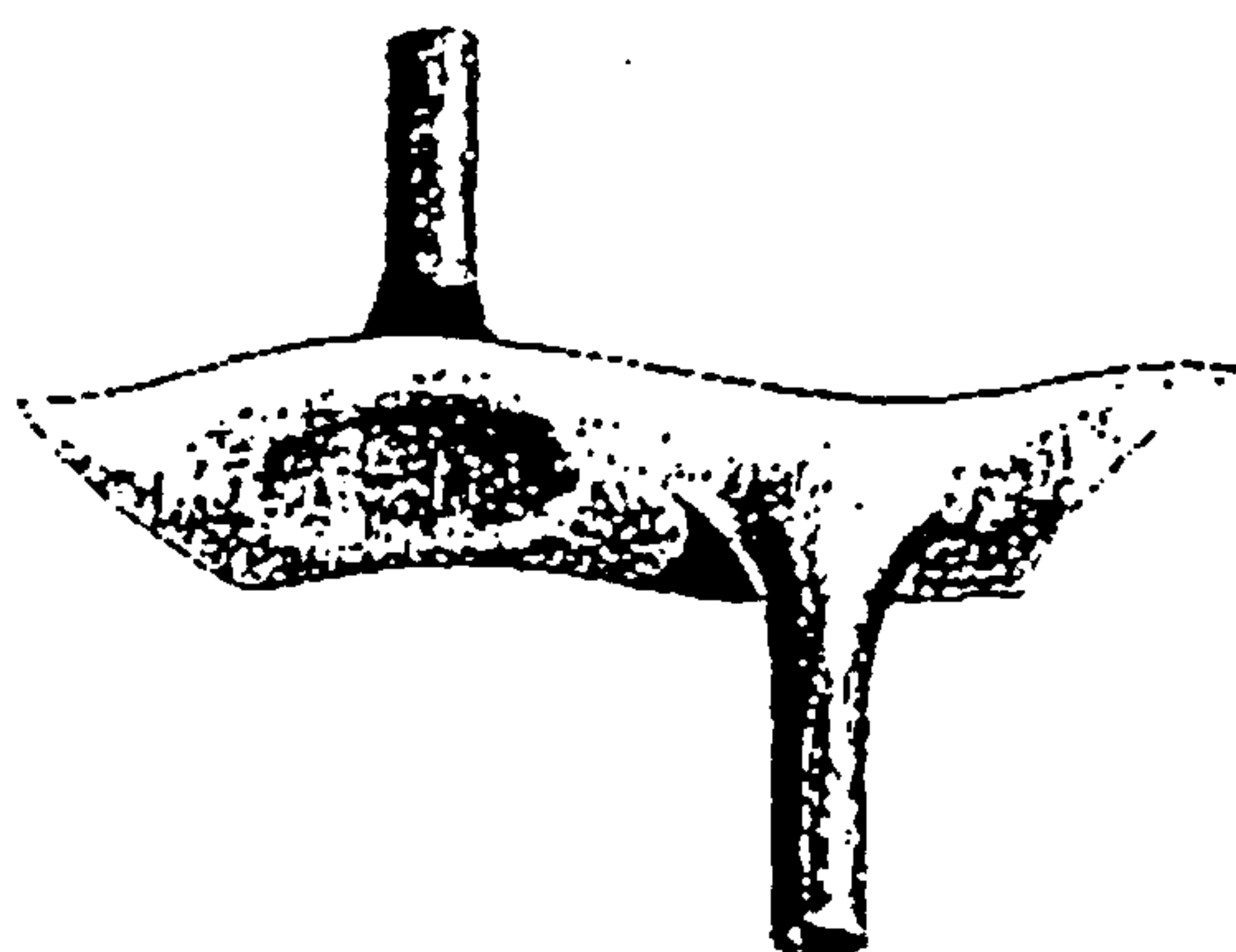


FIG. 9.2.i.

Furthermore, Hadamard observed that surfaces of negative curvature can also have an arbitrary number of holes and he considered the example of two hyperboloids

$U = 0, V = 0$ , which cut each other in a hyperbola. The part of surface  $UV = \varepsilon$ , where  $\varepsilon > 0$ , in the region  $U > 0, V > 0$ , has negative curvature and it has the general form of FIG. 9.2.ii, which is a surface with two infinite sheets and one hole [1898, 744]. It is easy to see that it is possible to construct a surface with an arbitrary number of holes and two infinite sheets simply by combining an arbitrary number of similar hyperboloids.



FIG. 9.2.ii.

Hadamard investigated the topology of these surfaces by considering the type of curves which could be drawn upon them. He said that two closed curves belonged to the same *species* if they were reducible one to the other by a continuous deformation on the surface, and distinguished between the different species by using two sorts of *elementary* curves: *simple* curves which were equivalent with respect to different edges of the surface, and curves which corresponded in pairs to different handles. Since any curve can be reduced to a sequence of elementary curves in a given direction and order, Hadamard had the powerful idea of representing the curves symbolically by using a sequence of symbols, each symbol in the sequence representing an elementary curve.

If  $a$  and  $b$  are any two points of the surface then Hadamard said that two paths that go from  $a$  to  $b$  belong to the same *type* if it is possible to pass from one to the other by a continuous deformation in which the points  $a$  and  $b$  remain fixed. By this definition, two paths  $ab, ac$ , which start from a point  $a$  and finish on a curve  $L$ , are also of the same type if it is possible to pass from one to the other by a continuous

deformation in which the point  $a$  remains fixed while the other extremity describes the curve  $L$ . If the curve  $L$  is closed, then there are an infinite number of ways of going from the point  $b$  to another point  $c$  of this curve without leaving it, and if the curve can be reduced to a point, then all the arcs are equivalent. In particular, on a doubly-connected surface all the paths from a given point to a given closed curve are reducible one to the other.

In accordance with Poincaré's dictum on the importance of periodic solutions<sup>6</sup>, Hadamard began his investigation into the different types of geodesics by considering the closed geodesics. He started by showing that corresponding to each type of line joining two distinct points, there is one and only one arc of a geodesic belonging to each type, a result which, as he observed, is equivalent to the theorem that on a surface of negative curvature two infinitely close geodesics cannot intersect more than once. By proving the impossibility of drawing, either between two points or from a point to a geodesic, two geodesics reducible one to the other, he showed that corresponding to each type of closed curve there is one and only one closed geodesic. Furthermore, he showed that on a surface of negative curvature there are no reducible closed geodesics; that if the surface is doubly-connected then it only has one closed geodesic; and if the connectivity is greater than two then the closed geodesics form a denumerable infinity.

Hadamard next considered the distance between two geodesics. It is straightforward to see that there are only three possibilities: either the two geodesics intersect, or the distance between them has a minimum absolute value, or the geodesics approach each other asymptotically. The existence of the first two possibilities is without question but what about the third? Clearly it only makes sense to think of geodesics asymptotically approaching a closed geodesic but do such asymptotic geodesics exist? To establish that they do, Hadamard appealed to non-Euclidean geometry.

Let  $a'$  be a point of the surface,  $A$  a geodesic joining this point to a point  $a$  of a geodesic  $L$ . Consider  $m$  which stretches indefinitely along  $L$  in a given direction, leaving from the point  $a$  and tracing the geodesic  $a'm$  of the type of  $A$  (see FIG. 9.2.iii). The angle  $ma'a$  is constantly increasing but it remains less than a given limit, namely the exterior angle  $a$  of the triangle  $maa'$ . Thus  $a'm$  tends towards a

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<sup>6</sup> Poincaré [MN I, 82]. See 7.2.2.



limit  $L'$  and the geodesic  $L'$  is asymptotic to  $L$ . It then follows that corresponding to each type of line joining a point to a geodesic there are two asymptotes of that particular type. These asymptotes can then be considered as geodesics which join the given point to the points at infinity on the given geodesic.

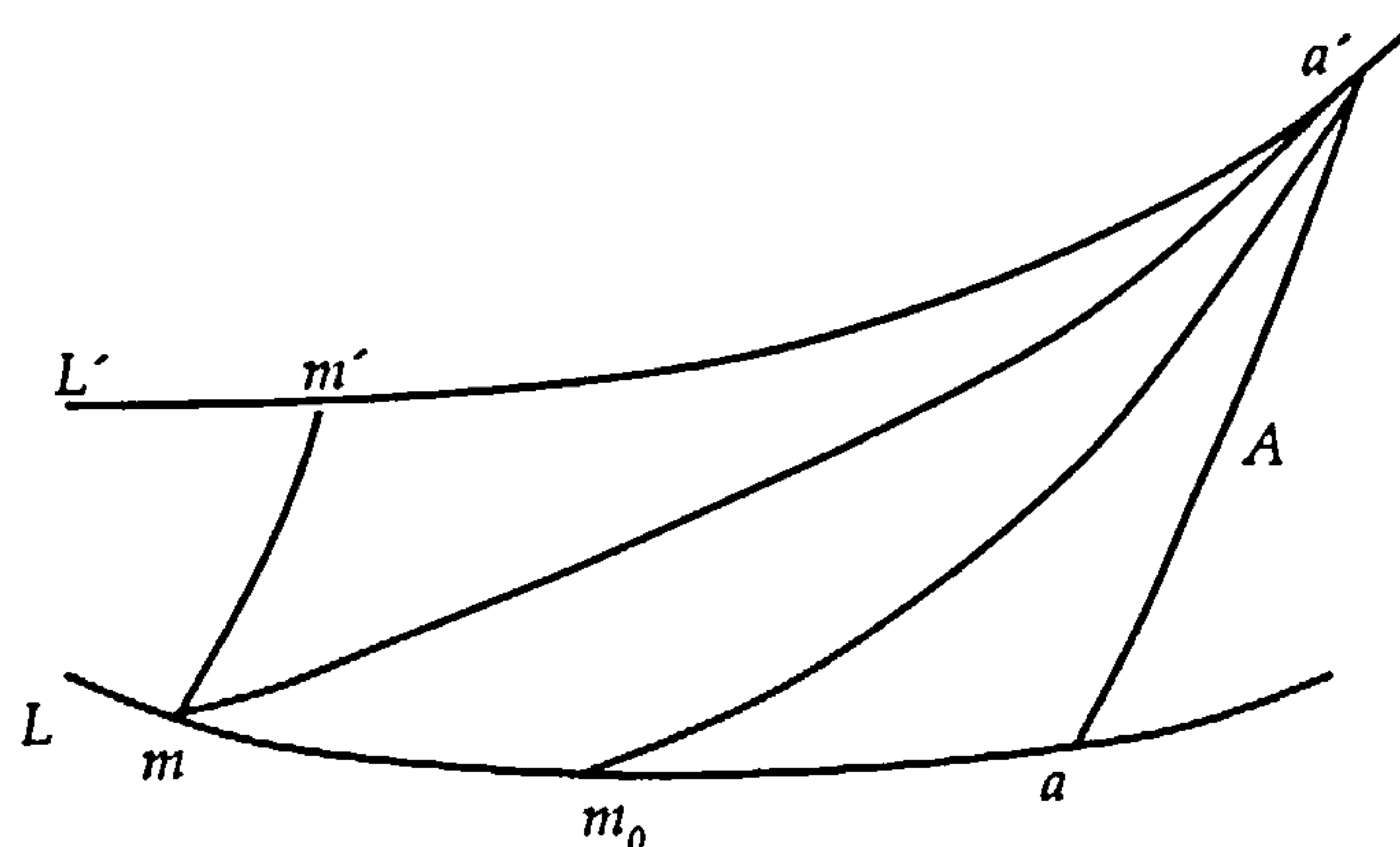


FIG. 9.2.iii.

With regard to his classification of geodesics, since the existence of asymptotic geodesics depends on the existence of closed geodesics, Hadamard put both these types of geodesic in the same category.

Hadamard next focused his attention on funnel-shaped infinite sheets  $\Pi_i$ . Corresponding to each infinite sheet  $\Pi_i$  of this sort is a simple curve and hence a closed geodesic  $\gamma_i$  which can be regarded as the initial position of the curve  $C_i$  which bounds the infinite sheet. Furthermore, the theory shows that on  $\Pi_i$  there is only one type of line going from an arbitrary point  $m$  to the bounded curve  $\gamma_i$ , hence the geodesic distance  $u$  of the point  $m$  to this curve is a completely defined single-valued function of the position of the point. Since this distance cannot have a maximum on a geodesic and must increase constantly and indefinitely if it increases at all, a geodesic which enters the sheet  $\Pi_i$  cannot leave it again: it is forced to extend to infinity along the sheet, and, moreover, it must do so regularly (i.e. without returning to a finite distance). It is clear that if one geodesic extends to infinity, then so does every geodesic infinitely close to it. These infinitely extending geodesics made up Hadamard's second category of geodesics.

If all  $n$  infinite sheets are funnel-shaped, then the corresponding set of  $n$  closed geodesics  $\gamma_i$  divide the surface into  $n$  infinite sheets and a bounded part  $S'$  which is the *finite part* of the surface. If a geodesic does not extend to infinity regularly on a

defined sheet, then it is always confined in the finite part of the surface. However, this is only true if the infinite sheets are funnel-shaped, since if the sheet is not funnel-shaped then there are no closed geodesics corresponding to the simple curves and hence any closed geodesics must be considered as being rejected to infinity.

With the above results Hadamard was able to draw some conclusions about the relationship between the geodesics and the order of connectivity of the surface considered.

If the surface is simply connected then there are no closed geodesics, and the distance from a point  $M$  to an arbitrary fixed point  $O$  (a distance which is a completely defined function of the position of  $M$ ) increases indefinitely as  $M$  describes a geodesic. Consequently, every geodesic goes to infinity, and the distribution is entirely analogous to that of the lines of a non-Euclidean plane. If the surface is doubly-connected then there is only one elementary curve, the simple curve corresponding to either one of the infinite sheets. The corresponding closed geodesic then divides the surface into two infinite sheets and there is no finite part of the surface. Hence every geodesic extends to infinity, except for the closed geodesic and its asymptotes and of these latter there are two through each point of the surface. However, if the order of connectivity is higher than 2 then not only are the closed curves infinite in number but also the simple curves relative to each infinite sheet are distinct.

Finally, Hadamard found a third category of geodesics quite different to any of those which he had previously described. These geodesics were bounded but they were neither closed nor asymptotes to closed geodesics. They appeared to approach a closed geodesic asymptotically but then move away before approaching another closed geodesic and so on. Furthermore he found that corresponding to each of these geodesics was an infinite sequence of closed geodesics which got progressively closer together and at the same time progressively increased in length. As he observed, including a direct quote from Poincaré:

*"... the geodesic in question possesses the property indicated by Poincaré [MN I, 82], namely that the equations of the problem admit 'a periodic solution (whose period may be very long), such that the difference between the two solutions is as small as desired for any given length of time'."*<sup>7</sup>.

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<sup>7</sup> Hadamard [1898, 768-769]

In other words Hadamard's discovery of this third type of geodesic was strong evidence to support Poincaré's conjecture that the periodic solutions are in fact dense.

Hadamard's proof of the existence of this third and final category of geodesics was especially notable in that it involved an early and novel use of Cantor's theory of transfinite sets<sup>8</sup>. Given a point  $O$  and the set  $E$  of tangents to the bounded geodesics passing through  $O$ , Hadamard found that the tangents to the geodesics he had already found, namely the closed geodesics and their asymptotes, were insufficient in number to form the totality of the set  $E$  and hence another category of geodesics must exist.

Furthermore, he found that in the neighbourhood of a tangent belonging to  $E$  there exists a geodesic which extends to infinity along an arbitrarily chosen sheet and in the neighbourhood of the same tangent there also exist geodesics belonging to third category. Hence the set  $E$  is perfect but nowhere dense. This remarkable result brought out an important distinction between the unbounded and bounded geodesics. For he had earlier shown that every unbounded geodesic is surrounded by a continuum of unbounded geodesics, but now he had shown that in the case of bounded geodesics an infinitesimal change in the initial direction of a geodesic is sufficient to make an absolutely arbitrary variation in the final behaviour of the curve. In other words, the boundedness property is not preserved by such a change.

This phenomena of sensitivity to initial conditions led Hadamard to propose the idea of the "well posed problem". Since in reality it is never possible to measure the initial data completely, he reasoned that it did not make sense to ascribe physical validity to a solution unless the solution had continuity with respect to the initial data. In a discussion of [1898] written only three years later he concluded:

*"Above all it must be acknowledged that the behaviour of these trajectories [geodesics] may depend on arithmetical discontinuous properties of the constants of integration. Secondly, as a result the important problems of celestial mechanics, such as the stability of the solar system, may belong to the category of ill-posed problems. If we substitute the search for the stability of the solar system with the analogous question related to geodesics of surfaces with negative curvature, we establish that each stable*

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<sup>8</sup> In his discussion of the formation of the set  $E$  Hadamard not only made reference to the work of Cantor and the similar sets encountered by Poincaré, but he also acknowledged an earlier contribution by Bendixson although without providing a reference [1898, 771].

*trajectory can be transformed, by an infinitely small variation in the initial conditions, into a completely unstable trajectory extending to infinity, or, more generally, into a trajectory of any of the types given in the general discussion: for example, into a trajectory asymptotic to a closed geodesic. But, in astronomical problems the initial conditions are only known physically, that is to say with an error which can only be reduced by improving the means of observation but which cannot be eliminated. However small it is, this error might cause a total and absolute perturbation in the result."*<sup>9</sup>.

### 9.3 Birkhoff and dynamical systems

Birkhoff's deep study of Poincaré's work on dynamics is evident from his first publication devoted to theoretical dynamics [1912] in which he introduced his idea of "recurrent motion" as a natural extension of periodic motion. This was followed by his resolution of Poincaré's last geometric theorem [1913] (described in 7.4.2), and two prize-winning papers in 1915 and 1917, the first on the restricted three body problem and the second on dynamical systems with two degrees of freedom. Many of the essential ideas from these early papers are collected together in his acclaimed book *Dynamical Systems* [1927], which was based upon his Colloquium Lectures delivered before the American Mathematical Society in 1920, and which was greatly influenced by Poincaré's work on celestial mechanics.

#### 9.3.1 Recurrent motion

Birkhoff's first paper on dynamics [1912] marks the beginning of a new phase in dynamical theory. Not only did Birkhoff explicitly consider a general dynamical system as opposed to addressing a particular dynamical problem, but instead of thinking simply in terms of a particular type of motion, he thought in terms of "sets" of motions which resulted in his novel concept of "minimal" or "recurrent" sets of motions.

Birkhoff considered the general class of dynamical systems defined by the differential equations

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<sup>9</sup> Hadamard [1901, 14].



$$\frac{dx_i}{X_i} = dt \quad (i = 1, \dots, n),$$

where the  $X_i$  are  $n$  real analytic functions and a state of motion can be represented by a point  $P$  in a closed  $n$ -dimensional manifold. A motion can then be represented by a trajectory in the manifold and its domain is its closed set of limit points. Birkhoff called the limit points of a motion as  $t$  becomes negatively or positively infinite  $\alpha$ - and  $\omega$ -limit points respectively. He defined a stable motion as one which never gets arbitrarily close to a singularity of the manifold.

If  $M'$  is a closed set of limit motions (i.e. trajectories composed of limit points) of a motion  $M$  and  $M'$  contains no proper subset then Birkhoff called the members of the set  $M'$  recurrent motions and the set itself he called a minimal set. More specifically he proved that a motion is recurrent if and only if for every  $\varepsilon > 0$ , there exists an interval of time  $T$  so large that the arc of the trajectory corresponding to the motion during the interval has points within a distance  $\varepsilon$  of every point of the trajectory. In other words, a motion is recurrent if during a sufficiently long time interval  $T$ , it comes arbitrarily close to all its states of motion, and thus a recurrent motion is stable in the sense given above.

A direct connection can be made between Birkhoff's concept of recurrent motion and Poincaré's ideas about stability. Since by definition every point on the trajectory of a recurrent motion is a limit point, hence the motion must approach every point on the trajectory infinitely often and arbitrarily closely. In other words, every recurrent motion must be Poisson stable. Clearly the simplest type of recurrent motions are the stationary and periodic motions, since in each case  $M'$  coincides with  $M$  and contains a single motion, whereas in every other case  $M'$  contains an infinite number of motions.

With regard to the general problem of determining all possible motions in a dynamical system, Birkhoff highlighted the value of the idea of recurrent motion with two particular results. On the one hand he proved that the set of limit motions of any motion contains at least one recurrent motion, and on the other he showed that any point  $P$  either generates a recurrent motion or it generates a motion which approaches with uniform frequency arbitrarily closely a set of recurrent motions. Thus the concept of recurrent motion can be used to derive definite results about the motion in an arbitrary dynamical system and one of the significant features of the theory is that it is valid for systems with any degree of freedom, in contrast to

Poincaré's theory of periodic motion which is only known to be valid for systems with two degrees of freedom.

Birkhoff also made a connection between recurrent motion and Hadamard's classification of geodesics on surfaces of negative curvature. It will be recalled that Hadamard's final category of geodesics contained those bounded geodesics which asymptotically approach a closed geodesic and then move away before returning to approach another closed geodesic. Since the motion corresponding to a geodesic of this type is necessarily stable, there must exist at least one geodesic corresponding to a recurrent motion. This cannot be either an asymptotic geodesic or an unbounded geodesic and hence either every geodesic in the third category approaches infinitely often and arbitrarily closely one particular closed geodesic, or there exists a recurrent motion given by a geodesic from Hadamard's final category.

In the case where the differential equations ceased to be analytic Birkhoff proved that recurrent motions do exist but they are neither periodic nor do they occur in the immediate neighbourhood of any periodic motion, and hence he called them *discontinuous* recurrent motions. The question of whether discontinuous recurrent motions exist in the analytic case was resolved by his student Marston Morse<sup>10</sup>.

### 9.3.2 The restricted three body problem

As discussed in 7.4.2, Birkhoff's first paper which concerned the restricted three body problem was his acclaimed proof of Poincaré's last theorem [1913] which appeared shortly after his paper on recurrent motion. With regard to his treatment of the problem in its generality, he published three principal papers [1915], [1935] and [1936], of which only the first will be discussed here. This paper, for which he won the Quirini Stampalia prize of the Royal Venice Institute of Science, provided the first major qualitative attack on the problem since Poincaré. But, unlike Poincaré, Birkhoff made little concession to analysis, and his investigation was founded almost entirely on topological ideas

Birkhoff formulated the problem in the standard way using a rotating coordinate system  $(x, y)$  with the two primaries  $S$  and  $J$  located on the  $x$  axis with the origin at their centre of gravity (see FIG. 2.2.ii)<sup>11</sup>, and, as customary, he reduced the system

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<sup>10</sup> Morse [1921a]. See the Epilogue.

<sup>11</sup> As Szebehely has observed [1967, 37], the formulation of the restricted three body problem seems peculiarly prone to error. In Birkhoff's formulation his description of the problem, his diagram, and one of his equations all contradict.

from fourth to third order by only considering the totality of motions for which the Jacobian constant  $C$  has a given value.

He first established a transformation of the variables which not only allowed him to derive Levi-Civita's [1906] equations and hence remove one of the singularities, but it also enabled him to derive a new form of the equations in which the singularities at both  $S$  and  $J$  are simultaneously removed and in which the equations are regular providing the planetoid is not rejected to infinity. From this new form of the equations he created a geometric representation in which the manifolds of motion are represented by the stream-lines of a three-dimensional flow and are without singularity unless  $C$  takes one of five exceptional values. Provided these five values are excluded the totality of the states of motion can then be represented by the stream lines of a flow occupying a non-singular manifold in a four-dimensional space. Since the five singular values mark the positions where two of the manifolds are about to join or separate, they act to distinguish between six different manifolds according to the value of  $C$ . By considering the representation from a topological point of view, Birkhoff demonstrated that each of these six manifolds required a different model, thereby giving an effective illustration of the problem's dependence on the value of  $C$ .

Birkhoff restricted most of his research to the case in which the planetoid is confined to move inside an oval about one of the primaries. This case, which is the simplest of the six, occurs when  $C$  is sufficiently large and positive. The relative simplicity of the case is associated with the fact that providing  $C$  is sufficiently large (or  $\mu$  is sufficiently small) the restricted problem closely resembles a two body problem. In this case Birkhoff's topological model shows that the states of motion are in one-to-one continuous correspondence with the points of a sphere, providing the diametrically opposite points are taken as identical.

If the motion is unperturbed the planetoid moves in a rotating ellipse with semi-major axis  $a$ , where the minimum value  $a_1$  of  $a$  corresponds to a retrograde circular orbit, and the maximum value  $a_2$  corresponds to a direct circular orbit. To consider the structure of the manifold of motion Birkhoff chose the variables to be the semi-major axis  $a$ , the longitude  $\theta$  of the line of apsides (the line joining the perihelion and the aphelion) of the ellipse with respect to the rotating  $x$  axis, and the mean anomaly  $\varphi$  of the planetoid taken in the same direction as the motion. The variable  $\theta$  then determines the instantaneous position of the ellipse while the variable  $\varphi$



determines the position of the planetoid on the ellipse. Thus, providing the circular orbits are excluded (since in this case  $\theta$  and  $\varphi$  are undetermined) the totality of states of motion can be represented in a one-to-one continuous way upon the interior of the hollow cylinder  $a_1 < a < a_2$ ,  $0 \leq \varphi \leq 2\pi$ , and the trajectories are spirals on the cylinders  $a = \text{constant}$ . The direct circular orbits are represented by the outer cylindrical surface  $\theta + \varphi = \text{constant}$  and the retrograde circular orbits are represented by the inner cylindrical surface  $\theta - \varphi = \text{constant}$ .

However, as Poincaré had shown [MN III, 372-381] the representation of the problem as a three-dimensional flow can be reduced to a representation which depends on the transformation of a two-dimensional ring into itself. For if  $K$  is any point on the ring  $\varphi = 0$  in a given cylinder and  $L$  is the point at which the trajectory through  $K$  first meets the ring  $\varphi = 2\pi$ , then corresponding points on the two rings will represent the same state of motion of the planetoid. In this way the two ends of the hollow cylinder are identified to form a torus with an internal hole of radius  $a_1$ . Thus the transformation  $T$  which takes  $K$  into  $L$  defines a transformation of the ring  $\varphi = 0$  into itself. Moreover,  $T$  preserves certain essential properties of the trajectories. For example, if the trajectory is periodic then a certain number of applications of  $T$  will take the point  $K$  into itself.

Birkhoff therefore constructed in the  $x, y$  plane a series of concentric retrograde (direct) circles of radius less than  $a_1$  ( $a_2$ ) about the primary. These can then be regarded as generating a ring of two leaves joined at the origin in the  $x, y$  plane. For any point  $K$  of a circle of the ring, there exists a positively tangent orbit which will again become positively tangent at a point  $L$  for the first time. This establishes a one-to-one continuous transformation of the ring into itself taking any point  $K$  into its corresponding point  $L$ , leaving radial distances unchanged and regresses each point by an amount dependent on the ellipse of motion of the planetoid. He then proved that for sufficiently small values of  $\mu$  and any value of  $C > \sqrt[3]{32}$ , it is possible to analytically continue the direct and retrograde periodic orbits and hence generalise the ring construction for the perturbed motion. In this case the transformation is a precise generalisation of the transformation for  $\mu = 0$ . It is one-to-one, continuous along the boundaries and varies continuously with  $\mu$  from the transformation for  $\mu = 0$ . He further proved that a necessary and sufficient condition for a ring bounded by a retrograde periodic orbit and a direct periodic orbit to exist which is cut by all the stream lines infinitely often is that every orbit in a sufficiently large time interval makes an arbitrary number of positive circuits of the retrograde orbit.



Birkhoff also showed that the transformation  $T$  possesses two important properties each of which can be used to form the basis for further results. Firstly,  $T$  is an area-preserving transformation. Birkhoff used this property to show that for a given value of  $C$  and a given point in the plane there are an infinite number of streamlines of the flow which pass through the point at a later time. Secondly,  $T$  is a product of two involutory transformations and from this property Birkhoff proved the existence of an infinite number of symmetric periodic orbits and also deduced results concerning their characteristic properties and distribution.

One other problem which Birkhoff addressed in [1915] concerned the restriction on the value of  $\mu$  which was inherent in his method for determining both the retrograde and direct periodic orbits. In the case of retrograde orbits he found that he could remove the restriction by returning to the differential equations of the problem and employing a transformation similar to the one used by Levi-Civita [1906]. In this way he was able to establish that for an arbitrary value of  $\mu$  there exists at least one retrograde periodic orbit symmetric with respect to the  $x$  axis which makes a single orbit around the primary, providing the value of  $C$  is such that the planetoid is confined to move within a closed oval of zero velocity about the larger of the two primaries. The case of direct periodic orbits was rather more difficult and required a different approach. In this case Birkhoff sought a new transformation whose construction was based on the existence of a retrograde orbit already known to exist rather than, as above, a transformation whose construction was based on the existence of both retrograde and direct orbits. The transformation he used was one in which the variables  $(a, \theta, \varphi)$  are replaced by

$$a^* = \frac{1}{a} - \frac{1}{a_2}, \quad \theta^* = \theta, \quad \varphi^* = \varphi - \theta.$$

and Poincaré's ring transformation is replaced by the transformation  $T^*$  of a disc into itself whose only boundary corresponds to the retrograde periodic orbit. A correspondence is then set up between the retrograde orbit for  $\mu = 0$  and the retrograde periodic orbit for  $\mu \neq 0$ . By Brouwer's fixed point theorem, the transformation  $T^*$  necessarily possesses an invariant point, and this point corresponds to a direct periodic orbit making a single revolution about the primary. Thus providing the conditions for the existence of  $T$  are fulfilled, there exists a transformation  $T^*$  and at least one direct periodic orbit.

Twenty years elapsed before Birkhoff's next publication on the problem. In the interim he had researched prodigiously into general dynamical systems, the

crowning result of which was another prize memoir *Nouvelles recherches sur les systèmes dynamiques* [1935a]. In his two later papers on the restricted problem, [1935] and [1936], of which the second was a continuation of the first, Birkhoff combined his ideas from [1915] together with some of the general results from [1935a], notably his development of Poincaré's idea of a transverse section. In [1935] he focused on the analytic properties of the transverse section and the transformation  $T$  used in [1915], while in [1936] he used qualitative methods to explore the results from [1935] in order to obtain further information about the different types of motion and the relationships which exist between them.

Birkhoff also contributed to another paper which concerned the restricted three body problem. In 1922 the National Research Council Committee on Celestial Mechanics, of which Birkhoff was a member, drew up a report on the state of celestial mechanics [1922]. Amongst other topics the report contained a short discussion on the restricted three body problem and there seems little doubt that Birkhoff himself was the author of this part of the report. It set out the particular difficulties associated with the problem and contained an admirably concise and accessible explanation of Poincaré's method of reducing the problem to the transformation of a transverse section.

### 9.3.3 Dynamical systems with two degrees of freedom

One of Birkhoff's most important papers on dynamics and a paper which was clearly inspired by Poincaré was his famous paper on dynamical systems with two degrees of freedom [1917] which won the Bôcher Prize of the American Mathematical Society in 1923. According to Morse, Birkhoff declared in about 1925 that he thought [1917] as good a piece of research as he would be likely to do<sup>12</sup>.

As previously mentioned, the interest in dynamical systems of this type stems from the fact that they represent the simplest type of non-integrable dynamical problems. Thus, as exemplified in the work of Poincaré and Hadamard, they form the natural starting point for qualitative explorations into questions of dynamics. Furthermore, as Birkhoff showed in [1917], they have the advantage that it is always permissible to consider the motion as the orbit of a particle constrained to move on a smooth surface.

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<sup>12</sup> Morse [1946, 380].

Birkhoff began with the equations of motion in standard Lagrangian form

$$\frac{d}{dt}\left(\frac{\partial L}{\partial \dot{x}} - \frac{\partial L}{\partial x}\right) = 0, \quad \frac{d}{dt}\left(\frac{\partial L}{\partial \dot{y}}\right) - \frac{\partial L}{\partial y} = 0,$$

where the function  $L$ , which is quadratic in the velocities, involves six arbitrary functions of  $x$  and  $y$ . By making an appropriate transformation of variables he reduced the equations to a normal form which only involved two arbitrary functions of  $x$  and  $y$ .

In the *reversible* case, i.e. when the linear terms in the velocities are lacking in  $L$  so that the equations remain unchanged when  $t$  is replaced by  $-t$ , this transformation was well known. In this case the equations of motion can be interpreted as those of a particle constrained to move on a smooth surface and the orbits of the particle interpreted as geodesics on the surface. But in the *irreversible* case, for example in restricted three body problem, Birkhoff's transformation was new and he gave a dynamical interpretation in which the motions can be regarded as the orbits of a particle constrained to move on a smooth surface which rotates about a fixed axis with uniform angular velocity and carries with it a conservative force field.

The central part of the paper concerned various methods by which the existence of periodic orbits could be established. In the first instance Birkhoff considered the method which he called the *minimum method*. Briefly, given a certain type of Lagrangian dynamical system and any closed curve  $l$  not deformable to a point on the surface then for a given value of the energy constant there exists a periodic orbit of the same type as  $l$  for which a certain integral is a minimum. In the reversible cases where the surface is closed and of positive genus then this integral corresponds to the arc length on the surface and the periodic orbit corresponds to a closed geodesic. In other cases, Birkhoff showed that the knowledge of boundaries of a particular type was required before the method could be used. In the irreversible case the situation is different because the integrand of the integral is no longer of one sign. As Birkhoff observed, the method only yields the completely unstable periodic orbits and hence has a limited application.

Secondly, Birkhoff developed his new *minimax* method<sup>13</sup>. This method which is applicable only to the reversible case establishes the existence of a large and

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<sup>13</sup> As noted by Birkhoff [1927, 139] and Veblen [1946, 283], Birkhoff's minimax method provided a starting point for Morse's work on calculus of variations in the large which introduces topological considerations into analysis.



completely different class of periodic orbits. As Birkhoff described in [1927, 133] this method can be most easily understood in an informal way by considering a torus in three-dimensional space. Clearly the minimum method will yield a closed geodesic having the type of a closed curve not deformable to a point on the surface of the torus. If now a closed curve of the same type is moved away from the minimising geodesic while at the same time one of the angular variables defining the torus is increased by  $2n\pi$  then the length of the closed curve will increase during this motion and the curve will have to reach a least upper bound in length in order for the motion to be possible. When the curve reaches this least upper bound length it will be taut and this is the position of the curve which corresponds to a closed geodesic of minimax type.

Thirdly, Birkhoff considered Poincaré's method of analytic continuation which is applicable to both reversible and irreversible periodic orbits. One of the problems with the method was that it was only valid for a small variation in the value of the parameter. The restriction was due to the possibility that the period of the orbit under consideration might become infinite. To increase the interval of the variation it is therefore necessary to show that this possibility cannot arise and this is precisely what Birkhoff did for a wide range of periodic orbits.

Finally, it was in this paper that Birkhoff first began to generalise Poincaré's idea of a transverse section and formally develop the theory attached to it. Poincaré had used the idea specifically to reduce the restricted three body problem to the transformation of a ring to itself but if the method was to have a general validity it was important to establish under what circumstances transverse sections exist. Birkhoff was able to show that not only do they exist in a wide variety of cases but also they can be of varying genus and have different numbers of boundaries.

With regard to the transformation of a transverse section, Birkhoff emphasised the fact that it possesses an invariant area integral. This is important because it means that the transformation only involves one arbitrary function of two variables as opposed to the normal form of the differential equations which involves two arbitrary functions. In other words, reducing a dynamical problem to a transformation of a transverse section into itself is both a qualitative and an analytic reduction.

By considering the invariant points of these transformations Birkhoff derived two important results about the periodic orbits. In the first place he found that the



difference between the number of unstable and stable periodic orbits is a constant, the constant depending only on the nature of the transformation and the genus and number of boundaries of the original surface. The second result involved a modification to Poincaré's last theorem. As has been described, the theorem involves the use of a ring-shaped transverse section to prove the existence of an infinite number of periodic orbits. Birkhoff now proved not only that the theorem can be used to establish the same conclusion for a general transverse section of genus  $p = 0$ , but also that a modified version of the theorem can be used to establish the same conclusion for a transverse section of genus  $p > 0$ .

#### 9.3.4 Later papers

Birkhoff's work on dynamical systems continued throughout his life and mention has already been made of his later papers which relate to Poincaré's last theorem and to his research on the restricted three body problem. The following is a very brief synopsis of some of his other work which is included to give an indication of the extent and direction of his later research.

In 1920 Birkhoff published a major paper on *Surface transformations and their dynamical applications* [1920] which was essentially an extensive elaboration and extension of some of the ideas he had broached at the end of [1917]. He began with a classification of different types of invariant points and then considered the behaviour of points in the neighbourhood of invariant points under the one-to-one, direct, analytic transformation of an analytic surface into itself. He also considered the problem of determining the behaviour of other different classes of points. The dynamical applications, which involved questions of integrability, stability and the classification of different types of motion, were only made briefly at the end of the paper.

In [1927a] Birkhoff considered the question of stability and the role of the Hamiltonian form of the equations. He argued that essentially the *only* significance of the Hamiltonian equations is that they possess the property of complete formal stability, i.e. any set of  $n$  equations possessing complete formal stability at a point of equilibrium can be given Hamiltonian form by an appropriate change of variables.

In [1927b] Birkhoff considered the distribution of periodic motions in dynamical systems with two degrees of freedom. As an example of how Poincaré's theorem and his own generalisation [1925] could be applied he discussed the motion of a billiard

ball on a convex table. He showed that if such a dynamical system admits a stable periodic motion then it admits an infinite number of other stable periodic motions within its immediate vicinity and the totality of these stable periodic motions form a dense set. Although he did not resolve Poincaré's question of whether the periodic motions are densely distributed throughout the possible motions, his example did show that this cannot be true unconditionally.

Finally, it is appropriate to mention Birkhoff's famous "ergodic theorem" [1931a], since its origins relate back to Poincaré's recurrence theorem<sup>14</sup>. The proof of the theorem is one of the major mathematical achievements of the period and in Birkhoff's œuvre ranks alongside his proof of Poincaré's last theorem.

The ergodic theorem states that for any dynamical system given by differential equations which possesses a certain  $n$ -dimensional invariant integral, there is a definite time probability  $p$  that any moving point, except those of measure zero, will be in an assigned region. Birkhoff's proof combined Poincaré's topological approach with the use of Lebesgue measure theory, and a clear explanation of the theorem and the nature of its applications is given in Birkhoff [1942].

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<sup>14</sup> The term "ergodic" originated with Boltzmann who used it to describe mechanical systems which had the property that each particular motion when continued indefinitely passed through every configuration and state of motion of the system which was compatible with the value of the total energy.

## 10. Epilogue

In discussing the impact made by Poincaré's memoir during the three decades following its publication a two-sided picture has emerged. On the one hand Poincaré's memoir stimulated interest in different aspects of the three body problem as exemplified most notably in the work of Painlevé, Levi-Civita, Sundman and Darwin, but, on the other, Poincaré's innovative topological approach to dynamics, although lauded by his contemporaries, found little expression in their work. Hadamard, despite the success of his Bordin paper did not use it as a focus for later research, and although Birkhoff made remarkable progress with his development of Poincaré's ideas, his investigations did not begin until 20 years after the publication of Poincaré's memoir. Furthermore, certain issues raised by Poincaré's work, such as the convergence of the series used in celestial mechanics and the strange characteristics exhibited by his doubly asymptotic solutions, were not the subject of any extensive research during the period under review. Much more recently each of these topics has resurfaced in the foundations of important new branches of mathematics, the former in KAM theory and the latter in chaos theory. This prompts questions about why these issues were not raised earlier and the general lack of contemporary response to Poincaré's dynamical ideas. In conclusion I want to consider these questions and in doing so look forward to some aspects of the next generation of research.

It is conspicuous that during the early years of the 20th century no serious attempt was made to understand further the behaviour of Poincaré's doubly asymptotic solutions - the complexity of which Poincaré so forcefully described in the final volume of the *Méthodes Nouvelles* published in 1899. Certainly this lack of research can to a great extent be explained by the inability to engage in a quantitative analysis due to inadequate computing techniques. The advent of the modern digital computer has meant that such an analysis is now possible with the result that during the last twenty years there has been an explosion of research into nonlinear systems. One of the results of this has been the unfolding of modern chaos theory, the roots of which go back to Poincaré's theory of doubly asymptotic solutions<sup>1</sup>. In addition to the problems of numerical computation, there was the further difficulty caused by the fact that the seemingly random behaviour exhibited by Poincaré's doubly asymptotic solutions did not fit in with the widely accepted Laplacian model of a clockwork universe. As observed earlier, this may at least partially explain why Poincaré himself originally missed the chaotic behaviour in his original memoir: he was simply expecting the solutions to evolve in a well-ordered way.

On a more general level, Poincaré's qualitative approach involved such a completely new way of looking at dynamical problems and such considerable conceptual difficulties that a period of assimilation was inevitable. One of the most striking examples of Poincaré's new ideas is his method of using a transverse section in order to reduce the dimension of a dynamical problem and more easily to investigate the behaviour of trajectories. Although the idea was acknowledged by Levi-Civita [1901], it was only through the work of Birkhoff that the potency of the method began to be realised<sup>2</sup>. Poincaré's creation and use of the method is a testimony to his remarkable talent for the visualisation of the long term evolution of a dynamical system.

With regard to the work of Birkhoff, although interest in his research was assured through his proof of Poincaré's last theorem, the actual response to it was slow. Although this again can be partly attributed to the novelty of his ideas, there were

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<sup>1</sup> See Ekeland [1988] and Stewart [1989] for informative popular accounts of Poincaré's work in relation to chaos theory.

<sup>2</sup> As for example in the work of Thomas Cherry who in the 1920s did extensive research into the general solutions of equations of dynamical systems. Of note is Cherry's paper on the periodic solutions of Hamiltonian systems [1928] in which he drew analogies with Poincaré's results.



also several indirect factors which inhibited the response. On the one hand there was the effect of the Great War which inevitably took its toll on mathematical activity, and on the other, by 1927, the time of the publication of Birkhoff's seminal book, dynamics had to compete in the mathematical arena not only with Einstein's general theory of relativity but also with the new ideas of quantum theory. Concomitant with these theories came a wealth of new mathematics which served to focus interest away from the traditional problems of celestial mechanics and the related dynamics<sup>3</sup>.

Nevertheless, one of Birkhoff's students, Marston Morse, was deeply influenced by his teacher's interest in Poincaré's topological approach to dynamics. Morse, who was born in 1892, the year of the publication of the first volume of Poincaré's *Méthodes Nouvelles*, became one of the foremost mathematicians of his generation and produced outstanding work in topology and the calculus of variations.

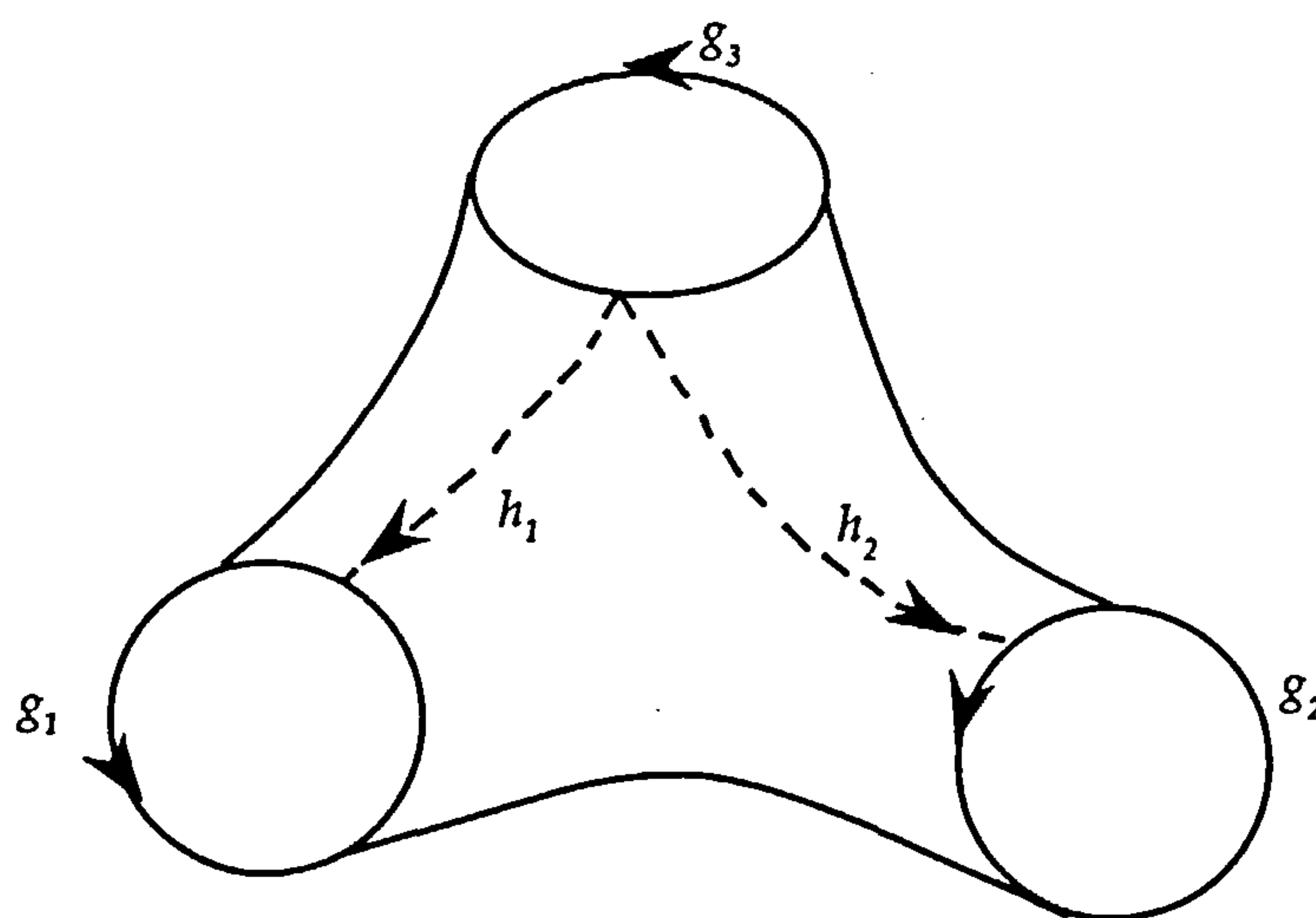


FIG. 10.i.

Morse's early papers on geodesics [1921, 1921a], which resulted from his thesis of 1917, drew on results both from Hadamard [1898] and Birkhoff [1912]. In these papers Morse discussed the behaviour of geodesics on surfaces of negative curvature embedded in three-dimensional space, the surfaces being of the type shown in

---

<sup>3</sup> That is not of course to say that dynamics and these topics were thought to be mutually exclusive. For example, Einstein showed his familiarity with Poincaré's ideas in [1917] where he discussed the quantisation conditions for nonseparable but integrable Hamiltonian systems considering the nature of motion of classical systems with more than one degree of freedom.

FIG. 10.i<sup>4</sup>. These surfaces have boundaries which have closed geodesics  $g$  and they are assumed to have genus at least equal to two.

Morse, using Hadamard's existence theorems and the fact that if the surface is cut along the geodesic segments  $h$  it becomes simply connected, created a symbolic representation of the geodesics lying entirely on the surface. He assigned to each geodesic a sequence of symbols where each symbol represented a geodesic segment and the whole sequence represented an unending ordered set of geodesic segments which he termed a *normal set*. Using this representation he obtained results about sets of geodesics and their limit geodesics. Notably he proved the existence of a certain class of geodesics which constituted a set of discontinuous recurrent motions and so resolved Birkhoff's question posed in [1912]. These particular geodesics are now known as *Morse trajectories*.

Morse's symbolic representation for the geodesic flow followed naturally from Hadamard's representation of curves on a surface given in [1898]. Morse took up the idea in more detail at the end of the 1930s when, with Hedlund, he presented a formalised account of "symbolic dynamics" [1938]. Their work provided the foundations for a powerful new method of dynamical investigation through which dynamical questions could be given an algebraic formulation quite distinct from the classical theories of differential equations.

Finally, turning to the questions concerning the convergence of Lindstedt's series. As described in 7.2.3, Poincaré's research had indicated, contrary to what Weierstrass had hoped, that Lindstedt's series were, apart from some exceptional cases, divergent. However, Poincaré had made it clear that he had not given a rigorous proof for the cases when the frequencies can be fixed in advance. With the work of Kolmogorov, Arnol'd and Moser it is now known that in these latter cases the majority of the formal series solutions are in fact convergent, and hence Weierstrass' intuition was after all correct. Their results form the basis for what is now known as Kolmogorov-Arnol'd-Moser (KAM) theory which provides methods for integrating perturbed Hamiltonian systems which are valid for infinite periods of time.

The basis of KAM theory can be understood by considering the autonomous Hamiltonian system with  $n$  degrees of freedom

---

<sup>4</sup> Bott [1980, 915].

$$\frac{dq}{dt} = \frac{\partial H}{\partial p}, \quad \frac{dp}{dt} = -\frac{\partial H}{\partial q}, \quad (p = p_1, \dots, p_n; q = q_1, \dots, q_n)$$

with an analytic Hamiltonian

$$H(p, q) = H_0(p) + \mu H_1(p, q) + \dots$$

where  $H$  is periodic in  $q$  of period  $2\pi$ , and  $\mu$  is a small parameter.

When the motion is unperturbed and  $\mu = 0$

$$\frac{dp}{dt} = 0, \quad \frac{dq}{dt} = \frac{\partial H_0}{\partial p} = \omega(p), \quad (\omega = \omega_1, \dots, \omega_n)$$

the system is integrable and the phase space is foliated by invariant tori  $p = \text{constant}$ . If the frequency ratios  $\omega_i$  are incommensurable then the motion is termed quasi-periodic with  $n$  frequencies  $\omega_1, \dots, \omega_n$ , and each trajectory  $p(t), q(t)$  is everywhere dense in the torus. The variables  $p, q$  are known as action-angle variables, and the unperturbed system is said to be nondegenerate if the motion is not described by a smaller number of frequencies than the number of degrees of freedom. In other words the system is nondegenerate providing the Hessian determinant  $\left(\frac{\partial^2 H_0}{\partial p^2}\right)$  does not vanish identically.

If now the system is slightly perturbed what happens to the invariant tori? The answer is given in the famous theorem of Kolmogorov [1954]. Clearly, the conditions under which the invariant tori are preserved are precisely the conditions under which solutions to the Hamiltonian equations exist (for fixed frequencies independent of the perturbation parameter) and the formal series expansions are convergent.

Kolmogorov's theorem states that if the unperturbed motion is nondegenerate then, under sufficiently small analytic perturbations of the Hamiltonian, the majority of invariant tori are not destroyed but only shifted slightly in the phase space, and the corresponding motion remains quasi-periodic. Moreover, these invariant tori form a closed nowhere dense set of positive measure whose complement has a measure which is small with respect to  $\mu$ .

More specifically, the tori which persist under small perturbations are those whose frequencies  $\omega_j$  are not only incommensurable but also satisfy the inequalities

$$\left| \sum_{j=1}^n k_j \omega_j \right| \geq K |k|^{-\nu}, \quad (\nu = n + 1) \quad (10.i)$$

for all integers  $k_j$  with  $|k| = \sum |k_j| \geq 1$ , and a suitable  $K(\omega) > 0$ . These relations give a condition under which the small divisors are bounded below in absolute value, and it is known by the theory of Diophantine approximation that for the majority of frequencies these relations are satisfied. These relations are naturally preserved under the perturbations since Kolmogorov's theorem asserts that the frequencies are independent of the perturbation parameter.

It is necessary to have relations of the type (10.i) which exclude commensurable frequencies since in any neighbourhood of an invariant torus of the unperturbed system there exists an invariant torus with frequencies  $\omega_j$  which are commensurable, and, in general, under a small perturbation such an invariant torus will collapse. These resonant tori consist of periodic solutions and, as Poincaré had shown when  $n = 2$ , only a finite number of periodic solutions persist for small values of  $\mu$ .

Furthermore, when  $n = 2$ , the two-dimensional invariant tori divide the three-dimensional energy level  $H = \text{constant}$  (FIG 10.ii) and a trajectory originating in the region between two invariant tori remains confined there. The measure of the gap between two invariant tori limits the magnitude of the oscillations of the corresponding action variables and hence these variables remain close to their initial values. When  $n > 2$ , the  $n$ -dimensional tori do not divide the  $(2n - 1)$ -dimensional energy level manifold  $H = \text{constant}$ .

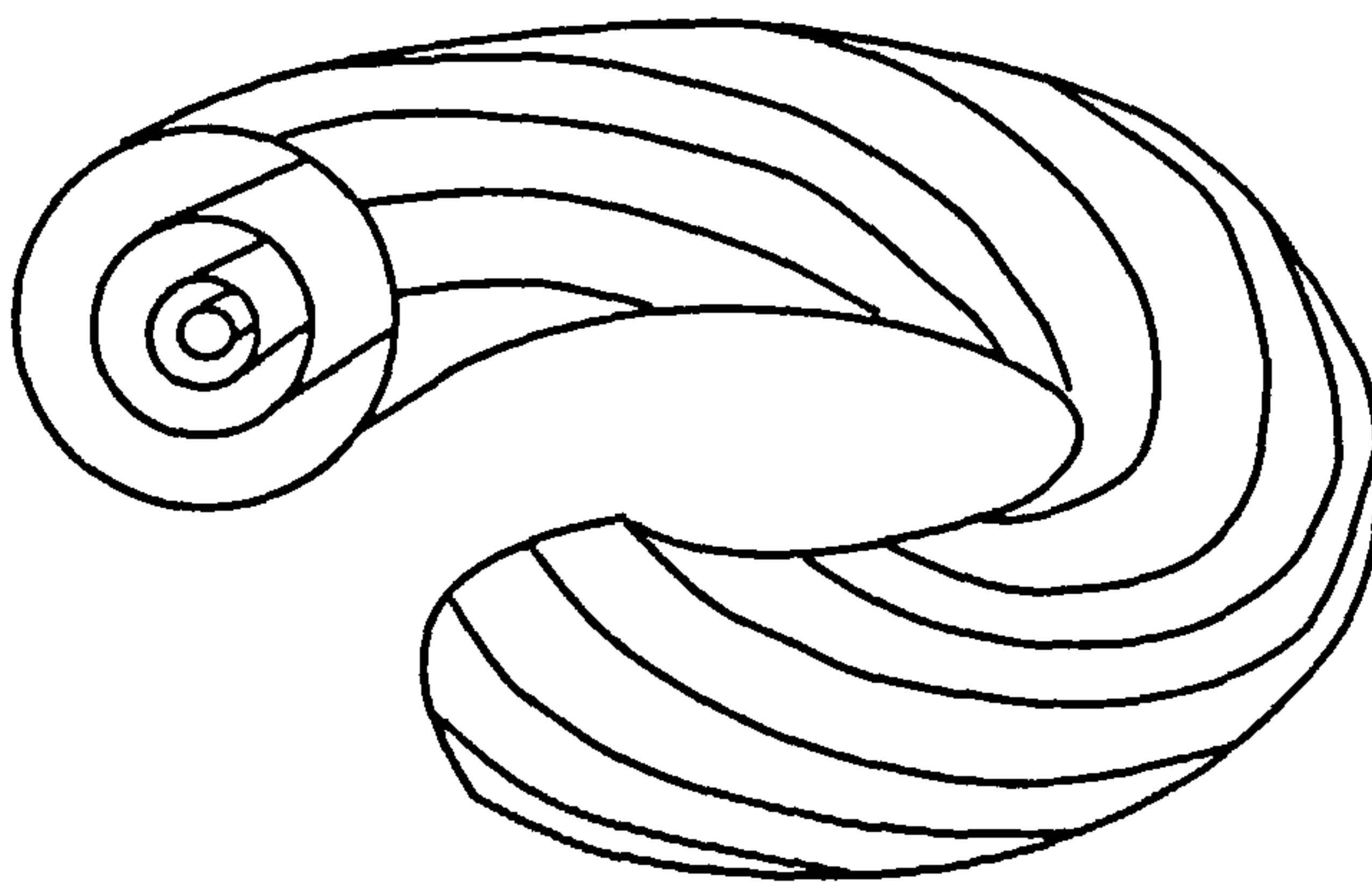


FIG. 10.ii.

The proof of Kolmogorov's theorem, which was proposed by Kolmogorov [1954] and made rigorous by his student Arnol'd [1963], was based on Lindstedt's method of the



construction of a succession of coordinate changes which progressively annihilate certain terms in the Hamiltonian in increasingly higher order of the parameter. Due to the presence of the small divisors, the convergence of the series satisfying the equations depends on the rate of contraction of the numerators. Kolmogorov's suggested form of proof used Newton's method of approximation which introduces quadratic convergence, i.e. the error  $\varepsilon_n$  of the  $n$ th approximation is of the order  $\varepsilon_{n-1}^2$  for  $n = 1, 2 \dots$ , and  $\varepsilon < 1$ . An important improvement to the theorem was made by Moser [1962] who showed that the requirement of the analyticity of the Hamiltonian can be abandoned and replaced by the condition that several hundred derivatives exist. In the two degree of freedom case he proved that it is sufficient that 333 derivatives exist!

Thus with the development of KAM theory Weierstrass' question was finally answered in the affirmative. The proof of Kolmogorov's theorem conclusively establishes the existence of convergent series solutions for the  $n$  body problem, and, moreover, these solutions are not exceptional in measure-theoretic terms.

Arnol'd's description of Kolmogorov's theorem in his introduction to the proof encapsulates the spirit of Kolmogorov's achievement:

*"A simple and novel idea, the combination of very classical and essentially modern methods, the solution of a 200 year old problem, a clear geometrical picture and great breadth of outlook ... ." [1963, 9].*

It is an appropriate tribute to an idea which incorporates much of the rich legacy inherited from Poincaré's great work on the three body problem.

## Appendix 1. A letter from Gösta Mittag-Leffler to Sonya Kovalevskaya

Mittag-Leffler Institute

Helsingfors, 7.6.1884

...

I agree with Weierstrass, if none of the answers on the set question are worthy of the prize, then the medal must be awarded to the mathematician who within recent years has made the best discoveries in higher analysis. But I cannot agree with the view that it will not further the progress of science to propose specific prize questions, in particular if they are stated reasonably. What about the significance for the development of the theory of linear differential equations resulting from the last prize question from the French Academy. This question provided the starting point for Poincaré's work. Furthermore, there does not exist a prize exclusively intended for pure analysis, hardly a prize exclusively intended for pure mathematics, apart from Steiner's prize in Berlin. The shortcoming of the Steiner prize is that it is awarded too often, every year. But we should not award our prize more frequently than every fourth year. Malmsten and the King want the prize jury to be appointed by the King and to consist of

1. The main editor of Acta Mathematica
2. A German or Austrian mathematician - = Weierstrass
3. A French or Belgian mathematician - = Hermite
4. An English or American mathematician - = Cayley? or Sylvester
5. A Russian or Italian mathematician - = the first time Brioschi or Tschebychef, the second time Mrs Kovalevskaya.

After each prize giving two of the prize judges should leave the jury and new ones should be appointed by King Oscar as long as he is alive - he must be able to appoint (substitutes) for both the leaving members. After King Oscar's death, the three remaining must appoint two new members but always in such a way as to fit the categories mentioned above. Imagine if one had to award a prize to the best mathematical work which has appeared during the last four years. Then the

national differences would certainly show up and the different views on what constitutes the essential substance of mathematics would be clearly expressed. Cayley and Brioschi might want to make the award to God knows which master of calculating and Tschebychef might opt for God knows what odd ideas. It is quite a different matter when one has to judge answers to a specific question. In this case one is forced to stick to much more objective criteria.

And finally one more reason. King Oscar is convinced that we should only announce specific prize questions and I doubt that it will be possible to change his mind unless I propose that the prize should be used to honour Swedish or Norwegian papers - like the anatomic prize you mentioned which is probably restricted to Russian works - but I do not want this at all. The competition would not get the international scientific reputation which I had imagined. I think that honouring only the works published in Acta would be more in the interests of the journal than in the interests of science. And this I do not want either. The interests of science must come first. If later I can do something for Acta at the same time then that is another matter and I will do it with all my heart.

...

Appendix 2.      Announcement of the Oscar Competition  
*Nature*, 30.7.1885

THE HIGHER MATHEMATICS

Prof. G. Mittag-Leffler, principal editor of the *Acta Mathematica*, forwards us the following communication, which will shortly appear in that journal:-

His Majesty Oscar II, wishing to give a fresh proof of his interest in the advancement of mathematical science, an interest already manifested by his graciously encouraging the publication of the journal *Acta Mathematica*, which is placed under his august protection, has resolved to award a prize, on January 21, 1889, the sixtieth anniversary of his birthday, to an important discovery in the field of higher mathematical analysis. This prize will consist of a gold medal of the eighteenth size bearing his Majesty's image and having a value of a thousand francs, together with a sum of two thousand five hundred crowns (1 crown = about 1 franc 4- centimes).

His Majesty has been pleased to entrust the task of carrying out his intentions to a commission of three members, Mr. Carl Weierstrass in Berlin, Mr. Charles Hermite in Paris, and the chief editor of this journal, Mr. Gösta Mittag-Leffler in Stockholm. The commissioners having presented a report on their work to his Majesty, he has graciously signified his approval of the following final propositions of theirs.

Having taken into consideration the questions which from different points of view equally engage the attention of analysts, and the solution of which would be of



the greatest interest for the progress of science, the commission respectfully proposes to his Majesty to award the prize to the best memoir on one of the following subjects:-

(1) A system being given of a number whatever of particles attracting one another mutually according to Newton's law, it is proposed, on the assumption that there never takes place an impact of two particles to expand the coordinates of each particle in a series proceeding according to some known functions of time and converging uniformly for any space of time.

It seems that this problem, the solution of which will considerably enlarge our knowledge with regard to the system of the universe, might be solved by means of the analytical resources at our present disposition; this may at least be fairly supposed, because shortly before his death Lejeune-Dirichlet communicated to a friend of his, a mathematician, that he had discovered a method of integrating the differential equations of mechanics, and that he had succeeded, by applying this method, to demonstrate the stability of our planetary system in an absolutely strict manner. Unfortunately we know nothing about this method except that the starting point for its discovery seems to have been the theory of infinitely small oscillations<sup>1</sup>. It may, however, be supposed almost with certainty that this method was not based on long and complicated calculations but on the development of a simple fundamental idea, which one may reasonably hope to find again by means of earnest and persevering study.

However, in case no one should succeed in solving the proposed problem within the period of the competition, the prize might be awarded to a work in which some other problem of mechanics is treated in the indicated manner and completely solved.

(2) Mr. Fuchs has demonstrated in several of his memoirs<sup>2</sup> that there exist uniform functions of two variables which, by their mode of generation, are connected

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<sup>1</sup> See p.35 of the Panegyric on Lejeune-Dirichlet by Kummer, "Abhandlungen der K. Akademie der Wissenschaften zu Berlin," 1860.

<sup>2</sup> These memoirs are to be found in (1) "Nachrichten von der K. Gesellschaft der Wissenschaften zu Göttingen," February, 1880, p.170; (2) Borchardt's "Journal," Bd.89, p.251 (a translation of this memoir is to be found in the "Bulletin" of Mr. Darboux, 2me série, t.iv); (3) "Nachrichten von der K. Gesellschaft der Wissenschaften zu Göttingen," June, 1880, p.445 (translated into French in the "bulletin" of Mr. Darboux, 2me série, t.iv); (4) Borchardt's "journal," Bd.90, p.71 (also in the "Bulletin of Mr. Darboux, 2me série, t.iv); (5) "Abhandlungen der K. Gesellschaft der

with the ultra-elliptical functions, but are more general than these, and which would probably acquire great importance for analysis, if their theory were further developed.

It is proposed to obtain in an explicit form those functions whose existence has been proved by Mr. Fuchs, in a sufficiently general case, so as to allow of an insight into and study of their most essential properties.

(3) A study of the functions defined by a sufficiently general differential equation of the first order, the first member of which is a rational integral function with respect to the variable, the function, and its first differential coefficient.

Mr. Briot and Mr. Bouquet have opened the way for such a study by their memoir on this subject (*Journal de l'École polytechnique*, cahier 36, pp.133-198). But mathematicians acquainted with the results attained by these authors know also that their work has not by any means exhausted the difficult and important subject which they have first treated. It seems probable that, if fresh inquiries were to be undertaken in the same direction, they might lead to theorems of high interest for analysis.

(4) It is well known how much light has been thrown on the general theory of algebraic equations by the study of the special functions to which the division of the circle into equal parts and the division of the argument of the elliptic functions by a whole number lead up. That remarkable transcendant which is obtained by expressing the module of an elliptic function by the quotient of the periods leads likewise to the modular equations, that have been the origin of entirely new notions and highly important results, as the solution of equations in the fifth degree. But this transcendant is but the first term, a particular case and that the simplest one of an infinite series of new functions introduced into science by Mr. Poincaré under the name of "*fonctions fuchsiennes*," and *successfully* applied by him to the integration of lineary differential equations of any order. These functions, which accordingly have a *rôle* of manifest importance in analysis, have not as yet been

---

Wissenschaften zu Göttingen," 1881 ("Bulletin" of Mr. Darboux, t.v); (6) "Sitzungsberichte der K. Akademie der Wissenschaften zu Berlin" 1883, i, p.507; (7) The memoir of Mr. Fuchs published in Borchardt's "Journal," Bd.76, p.177, has also some bearings on the memoirs quoted.

considered from an algebraical point of view as the transcendant of the theory of elliptic functions of which they are the generalisation.

It is proposed to fill up this gap and to arrive at new equations analogous to the modular equations by studying, though it were only in a particular case, the formation and properties of the algebraic relations that connect two "fonctions fuchsiennes" when they have a group in common.

In case none of the memoirs tendered for competition on any of the subjects proposed above should be deemed worthy of the prize, this may be adjudged to a memoir sent in for competition that contains a complete solution of an important question of the theory of functions other than those proposed by the Commission.

The memoirs offered for competition should be furnished with an epigraph and, besides, with the author's name and place of residence in a sealed cover, and directed to the chief editor of the *Acta Mathematica* before June 1, 1888.

The memoir to which his Majesty shall be pleased to award the prize as well as that or those memoirs which may be considered by the Commission worthy of an honorary mention, will be inserted in the *Acta Mathematica*, nor can any of them be previously published.

The memoirs may be written in any language that the author chooses, but as the members of the Commission belong to three different nations the author ought to subjoin a French translation to his original memoir, in case it is not written in French. If such a translation is not subjoined the author must allow the Commission to have one made for their own use.

THE EDITORS OF ACTA MATHEMATICA

### Appendix 3.        Entries received in the Oscar Competition

Acta 11, 401-402

The titles of the entries received for the competition, listed in the order in which they were received.

1.    *Mémoire sur l'équation trinôme de degré impair  $x^m \pm x = r$ .*
2.    *Nuova Teoria des Massimi e Minimi degli Integrali definiti.*
3.    *Allgemeine Entwicklung der Functionen (Développement général des fonctions).*
4.    *Les Fonctions Pseudo- et Hyper-Bernoulliennes et leurs premières applications. - Contribution élémentaire à l'intégration des équations différentielles.*
5.    *Über die Bewegungen in einem System von Massepunkten mit Kräften der Form  $-\frac{1}{r^2}$ .*
6.    *Intégration des équations simultanées aux dérivées partielles du premier ordre d'un nombre quelconque de fonctions de plusieurs variables indépendantes.*
7.    *Über die Integration der Differentialgleichungen, welche die Bewegungen eines Systems von Punkten bestimmen (Sur l'intégration des équations différentielles qui déterminent les mouvements d'un système de points matériels).*
8.    *Sur les intégrales de fonctions à multiplicateurs et leur application au développement des fonctions abéliennes en séries trigonométriques.*
9.    *Sur le Problème des trois Corps et les Équations de la Dynamique.*
10.   *Sur le Problème des trois Corps.*
11.   *Über die Bewegung der Himmelskörper im widerstehenden Mittel.*
12.   *Recherches sur la formule sommatoire d'Euler.*



## Appendix 4. Report of the Prize Commission

Poincaré *Œuvres* XI, 286-289

French translation sent to Poincaré after the announcement of the competition result.

### Traduction

Procès-verbal dressé par devant S. M. le Roi au palais de Stockholm, le 20 Janvier 1889, en présence de S. Exc. M. le Comte Ehrensvärd, Ministre des Affaires Etrangères, M. G. Wennerberg, Ministre des Cultes et de l'Instruction Publique, M. R. O. Schjött, Ministre Norvegien et de M. G. Mittag-Leffler, professeur a l'université de Stockholm.

§1. La commission, nommée par S. M. le Roi, en date du 25 Novembre 1884, pour examiner des mémoires, ayant concouru pour le prix en mathématiques offert par Sa Majesté, et composé de M. Carl Weierstrass, professeur à l'université de Berlin, M. Charles Hermite, professeur à la Sorbonne à Paris, et M. Gösta Mittag-Leffler, professeur à l'université de Stockholm, ayant terminé ses travaux, le rapport de la commission fut soumis au Roi.

Il ressort de ce rapport que la commission a été de l'opinion unanime, que le mémoire qui est intitulé *Sur le problème des trois corps et les équations de la dynamique* avec la devise "Nunquam præscriptos transibunt sidera fines", est l'œuvre profonde et originale d'un génie mathématique dont la place est marqué parmi les grands géomètres du siècle. Les plus importantes et les plus difficiles questions, comme la stabilité du système du monde, l'expression analytique des coordonnées des planètes par des series de sinus et de cosinus des multiples du temps, puis l'étude on ne peut plus remarquable, des mouvements asymptotiques, la découverte de formes de mouvement où les distances des corps restant comprises entre des limites fixes, on ne peut cependant exprimer leurs coordonneés par des séries trigonométriques, d'autres sujets encore que nous n'indiquons point, sont traités par des méthodes qui ouvrent, il n'est que juste de le dire, une époque nouvelle dans la mécanique céleste. Les notions analytiques inconnues de Lagrange et de Laplace, qui n'ont été acquises que de notre temps, ont un rôle essentiel dans ces questions si difficiles où le talent de l'auteur se montre dans tout son éclat. Une fois de plus se trouve ainsis confirmé cette observation que les plus grands progrès en astronomie, en physique et les découvertes

qui étendent le domaine des mathématiques abstraites, se produisent simultanément, comme si elles étaient appelées à se seconder en concourant à un même but, et que la commission de même a été unanime dans l'opinion, que l'auteur du mémoire qui porte pour titre *Sur les intégrales des fonctions à multiplicateurs et leur application au développement des fonctions abéliennes en séries trigonometriques*, et a pour devise

“Nous devons l'unique science  
Que l'homme puisse conquérir  
Aux chercheurs dont la patience  
En a laissé les fruits mûrir.”<sup>1</sup>

a montré un talent mathématique de premier ordre, et que son mémoire est extrêmement digne de l'attention des géomètres.

§2. S. M. le Roi daigna décerner le prix offert par Sa Majesté et composé d'une médaille en or évaluée à environs 1,000 francs ainsi que la somme 2,500 couronnes à l'auteur de mémoire muni de l'épigraphe “Nunquam præscriptos transibunt sidera fines” et un exemplaire de la médaille à l'effigie de Sa Majesté et portant l'inscription “in sui memoriam” à l'auteur de mémoire portant l'épigraphe:

“Nous devons l'unique science ...”

§3. S. M. le Roi ayant en suite ouvert les bulletins accompagnant les dit mémoires, il a été constaté que le bulletin à l'épigraphe: “Nunquam præscriptos transibunt sidera fines” portait le nom M. H. Poincaré, Paris”, et celui à l'épigraphe:

“Nous devons l'unique science ...”

le nom de “Paul Appell, Paris”.

Ainsi passé: Au Château de Stockholm le 20 Janvier 1889.

Oscar

Alb. Ehrensvärd

G. Wennerberg

P. O. Schjött

G. Mittag-Leffler

Otto Printzsköld

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<sup>1</sup> Sully-Prudhomme, *Le Bonheur*.

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## *Abbreviations used in the text*

*Acta* Acta Mathematica

BAAS British Association for the Advancement of Science

*Cahiers* Cahiers de Séminaire d'Histoire des Mathématiques

*Comptes Rendus* Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences

M-L I Mittag-Leffler Institute

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