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Whittaker's analytical dynamics: a biography

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Abstract Originally published in 1904, Whittaker's *A Treatise on the Analytical Dynamics of Particles and Rigid Bodies* soon became a classic of the subject and has remained in print for most of these 108 years. In this paper, we follow the book as it develops from a report that Whittaker wrote for the British Society for the Advancement of Science to its influence on Dirac's version of quantum mechanics in the 1920s and beyond.

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1 Introduction

Mathematics as a whole changed quite dramatically during the twentieth century, with the appearance of new areas, like topology, and the complete refashioning of already established ones, like algebra. Another area that underwent considerable changes in this period was classical mechanics. Once a flourishing subject, where a remarkable cross-breeding of mathematics and physics took place, classical mechanics was considered by many to have reached a dead end by the first decades of the twentieth century, except for eventual applications to other fields. By the 1950s, some physicists considered classical mechanics as useful only as "the springboard for the various branches of modern physics" and because it afforded "the student an opportunity to master many of the techniques necessary for quantum mechanics" (Goldstein 1950, p. ix).

At about the time that physicists were expressing such thoughts, the tide was already turning in favour of classical mechanics. This was spearheaded by the needs of the age of space exploration and by new advances on the theoretical side. Chief among these were Kolmogorov's results concerning Poincaré's "Problème Général de la Dynamique" (Poincaré 1899, vol. 1, chapter 1, §13, p. 32). Presented at the 1954 *International Congress of Mathematicians*, these results would give rise to what is now called KAM theory; see Arnold (1989, Appendix 8, p. 405) or Arnold et al. (1997, Chapter 5, §3, p. 182). This new phase in the history of classical mechanics was characterized by a growing use of geometrical methods, especially those of symplectic geometry. Although the word symplectic was only coined by H. Weyl in 1939 Weyl (1939), the subject has its roots in the work of Lagrange and Poisson Marle (2009), with important contributions by G. Darboux and S. Lie. The present approach, in terms of differential forms, goes back to Lie's theory of contact transformations (Berührungstransformation) Geiges (2001); Hawkins (1991). Given the cradle in which it was born, it is not surprising that symplectic geometry offers the perfect language in which to express classical mechanics.

So dramatic have been the changes that mechanics has undergone in the twentieth century that the style and even the contents of most books on dynamics written before the 1930s look hopelessly dated to present-day readers. But there are exceptions.

A remarkable one is Whittaker's *Analytical Dynamics* or, to give it its full title *A treatise on the analytical dynamics of particles and rigid bodies, with an introduction to the problem of three bodies*. Published originally in 1904, the book went through four editions and remains in print to this day. Moreover, it is not being used merely as a historical document. Of the 114 papers and books listed in MathSciNet between 2000 and 2012 that cite *Analytical Dynamics* as one of their references, only three are of a historical nature. Even a cursory look at the table of contents of *Analytical Dynamics* suggests why it is still useful: although couched in an older language, one finds there most of the topics dealt with in modern tracts, including several that we commonly associate with the symplectic approach that became prevalent since the 1960s.

The main question we try to answer in this paper is what caused this book's remarkable longevity. We begin with some biographical information on Whittaker's life, with emphasis on those aspects of it that seem most relevant to the history of the book. This is followed by a sketch of the chronological history of the book itself, its editions and translations. Next, we discuss the book's content, with emphasis on the chapters that deal with Hamiltonian dynamics. There are two reasons for this choice. The first is that they present, in incipient form, the symplectic approach that we now use; the second is that it provided the concepts from which Dirac developed his version of quantum mechanics in 1925. Any book that has seen so many editions and that has been in print for so long has also been often reviewed. In §4, we analyze some of these reviews and the way they present *Analytical Dynamics* in the light of the changes that mechanics underwent between 1904 and the 1990s. In the final section, we discuss the book's style and its impact, and attempt to answer the question posed above.

2 The author and his book

We begin with a short biographical note with the aim of calling attention to those aspects of Whittaker's life that help to explain both the choice of contents and the style of his *Analytical Dynamics*.

2.1 The author

Edmund Taylor Whittaker was born in Southport on the 24 of October 1873. Because of his frail health as a child, he was taught by his mother until he was eleven years old, when he was sent to study at Manchester Grammar School. As Temple writes in (1956, p. 299):

> [Whittaker] was on the classical side, which meant that three-fifths of his time was devoted to Latin and Greek: in the lower forms, where the study was purely linguistic, he did well: but his lack of interest in poetry and drama caused a falling-off when he was promoted to the upper school, and he was glad to escape by electing to specialize in mathematics. Only after he had left school did he

> discover the field of Latin and Greek learning that really appealed to him—ancient and mediaeval theology, philosophy and science.

He kept this interest in classical learning throughout his life, and it must have been of great help in his many scholarly pursuits. The most clear expression of Whittaker the scholar is probably his book *A history of the theories of aether and electricity* Whittaker (1910) first published in 1910. However, his scholarship is also apparent in *Analytical Dynamics* where, as we shall see, he displays an impressive knowledge of classical works by Newton, Euler, Lagrange, Laplace and many others.

Whittaker entered Trinity College, Cambridge, as a scholar in 1892, where he was a pupil of G. H. Darwin and A. R. Forsyth. For the first two years, he was coached by R. R. Webb, whom he left because he disliked the way he crammed his students with tripos problems. As an undergraduate, he was more interested in applied than in pure mathematics, and in 1894, he was the winner of the Sheepshanks exhibition in astronomy. He was bracketed second wrangler (with J. H. Grace) in the tripos of 1895 with T. J. I'A. Bromwich as senior wrangler. In 1896, he was elected a Fellow of Trinity, and a year later, he was first Smith prizeman on a topic of pure mathematics. His students at Cambridge included G. H. Hardy, J. H. Jeans, A. S. Eddington, G. I. Taylor, J. E. Littlewood and G. N. Watson, who would collaborate with him in the second (1915) edition of *Modern Analysis* Watson and Whittaker (1963). The first edition, published in 1902, had Whittaker as its sole author.

In 1898, the Council of the British Association for the Advancement of Science requested that Whittaker write a "report on the planetary theory". His *Report on the progress of the solution of the problem of three bodies* was published in the proceedings of the Association's 69th meeting Whittaker (1900) which took place in Dover in 1899. This report, which we will analyze in Sect. 2.2, formed the basis of the second part (Chapters XIII to XVI) of *Analytical Dynamics*, whose first edition appeared in 1904.

Whittaker remained in Cambridge until 1906, when he was appointed Royal Astronomer for Ireland and Andrews Professor of Astronomy at the University of Dublin, the same posts held by W. R. Hamilton between 1827 and his death in 1865. Among his duties were those of giving twelve lectures "open to the public" at every session. This "public" consisted basically of students from Trinity College, University College and the Royal College of Science of Ireland. *A history of the theories of aether and electricity* was partly based on some of these lectures. According to McCrea (1957, p. 237):

> For what Whittaker regarded as the comparative leisure of his conditions of life together with the very fine library facilities of Dublin, particularly of Trinity College, permitted him to do the enormous amount of reading required to produce the book.

In 1912, Whittaker left Dublin to take up the post of Professor of Mathematics at the University of Edinburgh, where he succeeded George Chrystal. He stayed in Edinburgh for the rest of his life and died there in 1956.

2.2 The report

As mentioned above, the second half of *Analytical Dynamics* derives from a report that Whittaker wrote for the British Association. In 1898, "the Council of the British Association resolved that 'Mr. E. T. Whittaker be requested to draw up a report on the planetary theory'" (McCrea 1957, p. 236). Whittaker's paper, delivered the next year, was called *Report on the Progress of the Solution of the Problem of Three Bodies*. In the very first paragraph, he explains that the "above title has been adopted in place of that originally chosen, as indicating more definitely the aim of the Report". And he goes on to explain that:

> The fundamental problem of dynamical astronomy is that of determining the motion in space of any number of particles which attract each other according to the Newtonian law. [...] The theory has hitherto been developed chiefly with the object of determining the motion of the moon and the planets. While, however, the lunar and planetary theories are, both of them, attempts to solve the problem of three bodies, yet the results of the two theories are quite different in form; this is owing to the fact that the assumptions on which the approximations are based are not the same in the two cases.

He then explains that in the case of two planets "the mass of one body preponderates, and the other bodies circle round it," while in the lunar theory, "it is assumed that two of the bodies circle around each other, while circling together round a preponderating third body". This gives rise to two different solutions in terms of infinite series.

The report covers "the last thirty years, 1868–1898", a period that "opens with the time when the last volume of Delaunay's 'Lunar theory' was newly published" and closes with the publication of "the last volume of Poincaré's 'New Methods of Celestial Mechanics" (p. 122). The second volume of Delaunay's *La théorie du mouvement de la lune* was published in 1867 and the third volume of Poincaré's *Les Méthodes Nouvelles de la Mécanique Céleste* appeared in 1899, the same year the Report was published. As Whittaker explains at the beginning of the Report:

> The work will be distributed under the following seven headings:–
> §I.– The differential equations of the problem.
> §II.– Certain particular solutions of simple character.
> §III.– Memoirs of 1868–1889 on general and particular solutions of the differential equations, and their expressions by means of infinite series (excluding Gyldén's theory).
> §IV.– Memoirs of 1868–1889 on the absence of terms of certain classes from the infinite series which represent the solution.
> §V.– Gyldén's theory of absolute orbits.
> §VI.– Progress in 1890–98 of the theories of §§III. and IV.
> §VII.– The impossibility of certain kinds of integrals.

Since these headings give a fair description of the contents of each section, we restrict our comments to those topics that made their way into the second part of *Analytical Dynamics*. As was to be expected, Whittaker begins §I explaining that

> The motion of three mutually attracting bodies is determined by nine differential equations, each of the second order, or, as it is generally expressed, by a system of the eighteenth order.

He then shows that it is possible to reduce the problem to a system of the sixth order using a number of elementary first integrals. This is followed by a precise description of "several problems of a more special character", such as the *problem of three bodies in a plane* and the *restricted problem of three bodies*, after which he describes various ways in which the equations for these problems can be formulated. The very short §II deals with generalizations of Lagrange's work on bodies that move on a line or that occupy the vertices of an equilateral triangle (Lagrange Points). The next section is devoted to the "derivation, nature, and properties of the infinite series by means of which the problem [of three bodies] can be solved" and includes a sketch of G. W. Hill's work. Hill's paper of 1877 Hill (1886) is described by Whittaker as marking "the beginning of a new era in Dynamical Astronomy" (p. 130). The section also includes a discussion of the *problem of small divisors* (p. 134). Section IV reports on miscellaneous results related to the solutions mentioned in its title, while section V discusses the method introduced by H. Gyldén (1841–1896) to calculate the motions of planets. Whittaker begins section VI stating that "a new impetus was given to Dynamical Astronomy in 1890 by the publication of a memoir of Poincaré" (p. 144). Not surprisingly, this section, which discusses the results obtained by Poincaré (1890) and further polished in Poincaré (1899), is the longest of the whole *Report*. Among the topics mentioned by Whittaker are the definition of integral invariant, the existence of periodic and asymptotic solutions and the question of the stability of the solar system. However, as Barrow-Green points out (Barrow-Green 1997, p. 147), he does not mention some of the ideas that we now regard as most characteristic of Poincaré's work, such as the Poincaré map, and he says very little about Poincaré's *doubly asymptotic solutions*, the ones that display the behaviour that has become known as *chaotic*. The final section (VII) is less than three pages long and is mostly concerned with Bruns's theorem on the algebraic integrals of the three body problem, which we discuss in more detail in Sect. 3.3.

2.3 The book

The publication history of the book, whose full title is *A treatise on the analytical dynamics of particles and rigid bodies; with an introduction to the problem of three bodies*, is as follows:

First edition: Cambridge University Press, 1904, xiii + 414.
Later editions: second edition, 1917; third edition, 1927; fourth edition, 1937, all of them by Cambridge University Press.
First American printing: Dover Publications, 1944.
Reprint of the fourth edition: with a foreword by Sir William McCrea, Cambridge University Press, 1988.
German edition: Analytische Dynamik der Punkte und starren Körper. Mit e. Einf. i.d. Dreikörperproblem u. mit zahlr. Übungsaufg, Grundlehren der mathematischen

Wissenschaften in Einzeldarstellungen, based on the second edition, Springer, Berlin, 1924.
Russian edition: Analiticheskaya dinamika, [B] Izhevsk: Nauchno-Izdatel'skij Tsentr "Regulyarnaya i Khaoticheskaya Dinamika" (1999).

The first edition consisted of sixteen chapters, subdivided into 188 consecutively numbered sections. Although Whittaker added many new sections to the second and third editions, he never altered the chapter structure. Even the chapter titles were kept, with two exceptions. Chapter IX, which was originally called *The principles of Hamilton and Gauss*, became *The principles of least action and least curvature* in the second edition, and Chapter XVI had its original title *Integration by trigonometric series* shortened to *Integration by series* in the third edition. Both changes were kept in later editions.

The book can be naturally divided into two parts. Part I, which consists of the first twelve chapters, is an exposition of the basic principles of dynamics. The second part (Chapters XIII to XVI) draws heavily on the report Whittaker had written for the British Association. While the first part hardly changed from the second edition on, the second part kept growing until the third edition. The fourth and final edition differs from the third only in the correction of some errors and the addition of a few references.

3 Contents

A list of the chapters that make up the book, with their corresponding titles, is given in Table 1. Throughout this section, all page numbers refer to the first edition of *Analytical Dynamics* unless stated otherwise.

3.1 The principles of dynamics

The purpose of part I is to give a state-of-the-art introduction to the principles of dynamics as they stood in the first years of the twentieth century. As we will see, this was considered as one of the book's strong points by the reviewers of the first edition.

The first chapter deals basically with the mathematical formalism required for describing rigid motions, namely rotations (§§1 to 12) and translations. The latter are introduced in terms of vectors (§13). More precisely, a *vector quantity* is defined to be "any one of the equal and parallel lines of space which have a given length" and which can be added together. Whittaker then proceeds to explain that not only displacements but also velocities and accelerations are vector quantities (§14). Since Whittaker defines vectors as what we would call equivalent classes of directed line segments under parallelism, his vectors do not all have the same origin, as vectors are now meant to have. This causes a problem when it comes to explaining the vectorial character of angular velocity for, as he says in §15, "an angular velocity about one line is not equivalent to an angular velocity of the same magnitude about a parallel line". He concludes that angular velocity "must therefore be regarded as a vector which is *localised* along a definite line". It should be noted that although he defines vectors,

Table 1 The chapter heads of *Analytical Dynamics*

Chapter	Title
I	Kinematical preliminaries
II	The equations of motion
III	Principles available for the integration
IV	The soluble problems of particle dynamics
V	The dynamical specification of bodies
VI	The soluble problems of rigid dynamics
VII	Theory of vibrations
VIII	Nonholonomic systems, dissipative systems
IX	The principles of Hamilton and Gauss (1st edition)
	The principles of least action and least curvature (2nd edition)
X	Hamiltonian systems and their integral invariants
XI	The transformation theory of dynamics
XII	Properties of the integrals of dynamical systems
XIII	The reduction of the problem of three bodies
XIV	The theorems of Bruns and Poincaré
XV	The general theory of orbits
XVI	Integration by trigonometric series
	Integration by series (3rd edition)

Whittaker never uses the concept systematically, in the way that has become standard since the 1930s, so that his equations are always expressed in terms of coordinates. We will discuss Whittaker's use of vectors in *Analytical Dynamics* in Sect. 5.1.

With Chapter II, it becomes clear that this is by no means an elementary introduction to mechanics. Although the chapter begins with a rather leisurely discussion of the meaning of such concepts as rest, motion, frame of reference (§19), mass (§20) and force (§21), it soon picks up momentum so that by §26 we are introduced to the equations of motion in Lagrangian form. Other concepts discussed in this chapter include work (§22), rigid body (§23), holonomic and non-holonomic systems (§25), kinetic energy (§26), conservative forces (§27) and impulsive motions (§35).

There is very little in Chapter II about the integration of the equations of motion. The closest we get to it is Whittaker's explanation in §32 that, in the absence of singularities, power series provide solutions that "will give any information which may be required about the initial character of the motion". However, the topic is exhaustively studied in Chapter III, where after stating (§37) that a problem is *soluble by quadrature* when it "can be completely solved in terms of the known elementary functions or the indefinite integrals of such functions", he explains (p. 53) that

> The problems of dynamics are not in general soluble by quadratures; and in those cases in which a solution by quadratures can be effected, there must always be some special reason for it,—in fact, the kinetic potential of the problem must have some special character. The object of the present chapter is to discuss those peculiarities of the kinetic potential which are most frequently found in

problems soluble by quadratures, and which in fact are the ultimate explanation of the solubility.

Topics discussed in this chapter include systems with ignorable coordinates (§§38–40), conservation of energy and its use in reducing the number of degrees of freedom of a system (§§41–42) and the method of separation of variables (§43). It is only in Chapter IV that Whittaker gives examples of equations for concrete dynamical systems, and then he proceeds to integrate them using the methods of Chapter III—among these, the pendulum (§44), a particle free to move in a smooth tube whose motion is constrained in some way (§45), central forces (§§46–50 and §53) and motion on a surface (§§54–55). Not surprisingly, this is the first chapter where he uses elliptic functions (in §§48 and 54). Note that up to this point only systems of point masses have been considered.

Chapter V prepares the ground for the study of the dynamics of rigid bodies by introducing "a number of the constants which can be assigned to a rigid body, and which depend on its constitution" (§57), among them the moment of inertia (§§57–61) and the angular momentum (§62). The eleven articles of Chapter VI are concerned with the solution of problems of the dynamics of rigid bodies. These range from exercises like describing the "motion of a rod on which an insect is crawling" (§65) to the motion of a top (§§71–74). As usual, Whittaker is not shy at introducing special functions whenever they are required. Thus, Weierstrass's $\wp$-function, Rieman's ζ-function and the Jacobi elliptic functions sn and cn are all used to solve various problems concerned with the "motion of a body about a fixed point under no forces" (§69). However, some functions were too exotic even for Whittaker. In his study of Kovalevskaya's top (he calls her Mme S. von Kowalevski) in §74, he derives the equations but stops short of solving them with the help of hyperelliptic functions.

Chapter VII is concerned with another standard theme of classical dynamics, the theory of vibrations. Whittaker presents the approach that has become standard; see, for example Arnold (1989, Ch. 5) and Landau and Lifschitz (1976, Ch. V). He begins explaining that only small oscillations will be considered, which are those whose "divergence from the equilibrium-configuration will never become very marked" (§76). This hypothesis implies that both the kinetic and the potential energies of the system are quadratic forms. Assuming also that no coordinates have been ignored, Whittaker shows that these two quadratic forms can be simultaneously diagonalized using orthogonal transformations. The examples given in §85 include "vibrations about steady circular motion of a particle moving under gravity on a surface of revolution whose axis is vertical" as well as a top on a perfectly rough plane and a sleeping top. As we will see in Sect. 4.3, Whittaker's use of the expression "perfectly rough plane" caused an interesting exchange of letters in the pages of *Nature* a year after the book was published.

While the systems considered in previous chapters have been holonomic and conservative, those discussed in Chapter VIII are either non-holonomic or dissipative. Holonomic systems had already been defined in §25, where it is said that they are "characterised by the fact that the number of degrees of freedom is equal to the number of independent coordinates required to specify the configuration of the system". In §§88–90, the machinery developed in the previous chapters is used to study a number of non-holonomic systems. A system described in §90 illustrates very nicely G. H. Bryan's comment in his review of the first edition (Bryan 1905, p. 603) that some of the examples in *Analytical Dynamics* contain "the substance of minor papers that have been published abroad":

> A heavy homogeneous hemisphere is resting in equilibrium on a perfectly rough plane with its spherical surface downwards. A second heavy homogeneous hemisphere is resting in the same way on the perfectly rough plane face of the first, the point of contact being in the centre of the face. The equilibrium being slightly disturbed, it is required to find the vibration of the system.

In a footnote (p. 218), this problem is attributed to "Madame Kerkhoven-Wythoff". This is Geertruida Kerkhoven (neé Wythoff), a Dutch mathematician who published six papers between 1893 and 1902, several of which on analytical mechanics; see Creese and Creese (2004, p. 111). The study of dissipative systems begins in §91 with the discussion of frictional forces and includes articles on resisting forces that depend on the velocity (§§92 and 93), vibrations of dissipative systems (§94) and impact (§§95 to 97).

Having applied the techniques introduced in the first three chapters to the analysis of a large number of systems, Whittaker now proceeds to develop more advanced topics beginning, in Chapter IX, with action principles. This chapter opens with the very short §98, whose aim is to convey a single message: it is useful to consider n-coordinates as defining a point in n-space, because that (p. 241) "makes it natural to use geometric terms such as 'intersection', 'adjacency', etc when speaking of the relations of different states or types of motion of the system". Although a present-day reader will automatically interpret the many n-tuples of the form $(q_1, \ldots, q_n)$ that have appeared in previous articles as points or vectors in an n-dimensional space, Whittaker waits until he gets to the more advanced chapters to make this fact explicit. Up to this point such coordinates were considered as degrees of freedom, which is essentially the rôle they play in Lagrange's *Mécanique Analytique* (Lagrange 1889, vol. 2, Seconde Partie, Section Septième, p. 2); see also Kline (1990, p. 589).

The next two articles introduce the principles of least action, for conservative holonomic systems that are now usually named after Hamilton (§99) and Maupertius (§101). In the book, the appellation *principle of least action* is reserved for the second of these principles; the other one is simply called *Hamilton's principle*. However, unlike present-day accounts, such as those in Arnold (1989, §13A, p. 59) or Landau and Lifschitz (1976, §2, p. 2), these principles are not taken as starting points, from which the whole of dynamics can be derived. Hamilton's Principle, for instance, is shown to follow from Lagrange's equation of motion, which in §26 Whittaker had derived directly from Newton's Second Law. More surprisingly still, Whittaker never mentions the calculus of variations in this chapter; indeed, in the first edition the expression occurs only in §110 (see below). The reason is probably that Whittaker's approach is essentially geometrical. This becomes clear in §103 where, after extending the two principles of least action to non-conservative systems (§101) and non-holonomic systems (§102), he attempts to answer the question: "are the stationary integrals [that appear in these principles] actual minima?". His discussion is based, down to the very notation he uses, on the paper of Culverwell (1892), a fellow of Trinity College Dublin. Given that the avowed aim of Curvewell's paper is the "discrimination of maxima and minima values in the calculus of variation", one is somewhat surprised to read in his very first paragraph that although this subject "has possessed considerable interest for mathematicians", it is "as yet, at all events, of little if any practical utility". After explaining that

> Except for the simplest case [...] the investigation supplied have been so difficult, and have required such an amount of analytical skill, as to deter any but the most competent mathematicians from considering cases of any degree of complexity,

he points out that:

> The simplicity of the Jacobian result seemed to indicate that there must be some simple way of obtaining it, and, guided by the desire to ascertain the degree of continuity required in the variation, the simple, almost geometrical, method explained in the following pages presented itself to me.

The "Jacobian result" is the criterion described by Jacobi (1837), see also Jacobi (1838) and Todhunter (1962, pp. 243–253), to determine whether a given curve is a maximum of a given integral. For a system with two degrees of freedom, Culverwell's version of Jacobi's result can be described as follows. Let

$$U = \int_{x'}^{x''} f(x, y, \dot{y})\mathrm{d}x$$

be "the integral which is to be made a maximum". Culverwell denotes the variation δy by $\alpha\phi$, where α is a "small constant, the order of magnitude of which determines, generally speaking, the order of magnitude of δy, since the function ϕ is supposed to be finite, at least never infinite". Next he defines the coefficients U_0, U_1, U_2, U_{00}, U_{01}, U_{11} using the Taylor expansion

$$\begin{aligned} f(x, y+\alpha\phi, \dot{y}+\alpha\dot{\phi}) = {} & f(x, y, \dot{y}) + \alpha(U_0\phi + U_1\dot{\phi}) \\ & + \frac{1}{2}\alpha^2(U_{00}\phi^2 + U_{01}\phi\dot{\phi} + U_{11}\dot{\phi}^2) \\ & + \text{terms involving}\, \alpha^3 \text{ and higher powers} \end{aligned} \tag{3.1}$$

At the end of §1 of Part I of his paper, Culverwell defines *conjugate points*:

> When it is possible to draw between two points P and Q two *consecutive* stationary curves having at P and Q the same values for y and its [...] first fluxion, the two points may be called *conjugate*.

The key result is his Proposition V (Culverwell 1892, p. 247):

> When the limits are fixed, and when $U_{11}dx$ is negative throughout the integration, the stationary value of U is a maximum, provided that x'' is not beyond the point "conjugate" to x'.

In § 103 of *Analytical Dynamics*, Whittaker essentially repeats Curvewell's proof (with maximum replaced by minimum) of this result in the case that

$$U = \int (a_{11}\dot{q_1}^2 + 2a_{12}\dot{q_1}\dot{q_2} + a_{22}\dot{q_2}^2)dt,$$

where q_1, q_2 are the coordinates of the mechanical system. Taking into account that the quadratic form in the integrand defines the kinetic energy of the system and, as such, must be positive definite, Whittaker concludes that U_{11} is positive, so that "for finite ranges the [Maupertius] Action is a minimum, provided the final point is not beyond the kinetic focus of the initial point", where kinetic focus is used here as a synonym for conjugate point. A modernized version of the Culverwell-Whittaker approach can be found in Gray and Taylor (2007), where the "simple illustrative example" that appears at the end of § 103 of *Analytical Dynamics* is quoted in full.

The three final articles of Chapter IX present the principle of least constraint of Gauss and that of least curvature of Hertz. In § 107 Gauss's Principle is shown to be (p. 253) "the basis of a form in which Appell has proposed to write the general equations of dynamics".

3.2 Hamiltonian systems and contact transformations

We now turn to Whittaker's presentation of Hamiltonian dynamics, which extends from Chapters X to XII and includes *the transformation-theory of dynamics* (Chapter XI), which, as we shall see in Sect. 5.2, greatly influenced Dirac's version of quantum mechanics. The exposition begins in § 109 where Whittaker derives Hamilton's equations

$$\frac{dq_r}{dt} = \frac{\partial H}{\partial p_r}, \quad \frac{dp_r}{dt} = -\frac{\partial H}{\partial q_r}, \quad (r = 1, 2, \ldots, n) \tag{3.2}$$

from the Lagrangian formalism by equating coefficients of two differential 1-forms, in much the same way as it is still done in modern textbooks such as Arnold (1989, Chapter 3, section 15, p. 65) and Landau and Lifschitz (1976, Chapter 7, §40, p. 131). To justify the importance of these equations Whittaker proves in § 110 a theorem of Jacobi according to which "all the differential equations which arise from problems in the Calculus of Variations, with one independent variable, can be expressed in Hamiltonian form". Most of the remaining articles of Chapter X are concerned, directly or indirectly, with integral invariants. Although these invariants are introduced with a view to their application to Hamiltonian systems, Whittaker begins his exposition with two articles in which he considers a general system of differential equations of the first order and the first degree which he regards as "defining the motion of a point whose coordinates are $(x_1, \ldots, x_n)$ in space of n dimensions". He continues (p. 262):

> If now we consider a group of such points, which occupy a p-dimensional region ζ_0 at the beginning of the motion, they will at any subsequent time t occupy another p-dimensional region ζ. A p-tuple integral taken over ζ is called an

integral-invariant, if it has the same value at all times t; the number p is called the order of the integral-invariant.

This is illustrated by the best known example of such an invariant, the volume of an incompressible fluid. An article (§112) on variational equations is followed (§113) by a discussion of the relation between integral invariants of order one (those defined by the integration of 1-forms) and the existence of a first integral for the given system of differential equations. In §114, Whittaker considers

> Integrals which have the invariantive property only when the domain over which the integration is taken is a *closed* manifold (using the language of n-dimensional geometry); these are called *relative* integral-invariants.

He then shows how Stokes's theorem can be used to construct an (absolute) integral invariant from a relative one. The application of integral invariants to Hamiltonian systems begins in §115, where Whittaker shows that "the quantity $\int \sum_{r=1}^{n} p_r dq_r$ is a relative integral-invariant of any Hamiltonian system of differential equations". Today, this invariant is named after Poincaré, who introduced it and the notion of integral invariant, see Barrow-Green (1997, p. 40), Poincaré (1899, vol. 3, Chapter XXII) and Arnold (1989, Chapter 9, section 44, p. 238). Having shown that every Hamiltonian system has a Poincaré invariant, Whittaker considers, in the next two articles, ways by which a system with an integral invariant can be reduced to the Hamiltonian form. Indeed, the main result of §116 is that (p. 269):

> If a system of equations
>
> $$\frac{dq_r}{dt} = Q_r, \quad \frac{dp_r}{dt} = P_r \quad (r = 1, 2, \ldots, n)$$

possesses the relative invariant

$$\int (p_1\delta q_1 + p_2\delta q_2 + \cdots + p_n\delta q_n),$$

then the equations have the Hamiltonian form

$$\frac{dq_r}{dt} = \frac{\partial H}{\partial p_r}, \quad \frac{dp_r}{dt} = -\frac{\partial H}{\partial q_r} \quad (r = 1, 2, \ldots, n);$$

which is the converse of the result considered in the previous article. The final articles of this chapter are concerned with Jacobi's method of the last multiplier; for a modern treatment see Goriely (2001, §2.11, p. 74). The method is described in great generality in §§119 and §120 and applied to Hamiltonian systems in §121. In §122 Whittaker shows that the last multiplier method can be used to construct "integral-invariants whose order is equal to the order of the system"; that is integral invariants obtained by integrating a differential n-form when the system of equations has dimension n. In the final two articles of this chapter, last multipliers are applied to the reduction of second order equations to Lagrangian form.

That brings us to Chapter XI, arguably the most celebrated of the whole book. In his obituary of Whittaker, published in 1957, McCrea writes, concerning the subject of this chapter, that it "seems to have been largely [Whittaker's] own formulation" (McCrea 1957, p. 248). I see no reason to believe this statement. A careful look at the sources listed in the fourth edition of *Analytical Dynamics* shows that Whittaker merely organized material that he found in the works he cites, mostly papers by Poisson, Jacobi and Lie. For example, the presentation of contact transformations in terms of differential forms comes from Lie (1872) and is very similar to those found in expositions of the same material by contemporary mathematicians like Poincaré (1910, p. 3) and Liebmann (1914). Indeed, no similar claim is made in the Foreword McCrea wrote when the book was reprinted in 1999 Whittaker (1999). Chapter XI is also the first one to have changed substantially between the first and the second editions. We begin with an exposition of the contents of §125 in the first edition, which includes a formal definition of contact transformation.

Following Lie's approach (Hawkins 1991, p. 419), Whittaker defines contact transformations in terms of differential forms:

> Let $(q_1, \ldots, q_n, p_1, \ldots, p_n)$ be a set of $2n$ variables, and let $(Q_1, \ldots, Q_n, P_1, \ldots, P_n)$ be $2n$ other variables which are defined in terms of them by $2n$ equations. If the equations connecting the two sets of variables are such that the differential form
>
> $$P_1 dQ_1 + P_2 dQ_2 + \cdots + P_n dQ_n - p_1 dq_1 - p_2 dq_2 - \cdots - p_n dq_n$$
>
> is, when expressed in terms of $(q_1, \ldots, q_n, p_1, \ldots, p_n)$ and their differentials, the perfect differential of a function of $(q_1, \ldots, q_n, p_1, \ldots, p_n)$, then the change from the set of variables $(q_1, \ldots, q_n, p_1, \ldots, p_n)$ to the other set $(Q_1, \ldots, Q_n, P_1, \ldots, P_n)$ is called a *contact-transformation.*

A couple of paragraphs later, he rephrases this definition in a less verbose way as follows:

> A contact-transformation leaves the differential form $\sum p_r dq_r$ invariant, to the modulus of an exact differential.

This surprisingly modern-sounding statement was omitted in later editions. Next, Whittaker points out that:

> The result of performing two contact-transformations in succession is to obtain a change of variables which is itself a contact-transformation; this is generally expressed by the statement that *contact-transformations have the group-property.*

From our point of view, this definition leaves much to be desired, for no mention is made of inverses and, furthermore, the examples show that these transformations do not always have the same domain, so composing them may not even be possible. By the time *Analytical Dynamics* was written, the modern concept of group had already been defined, for instance, in Jordan's *Traité des substituitions* (1870, p. 22, §27) and in Burnside's *Groups of finite order* (1897, §12, Chapter II, p. 11). However, both these books dealt with finite groups while, as we have seen, Whittaker wrote from the point of view of Lie's theory of continuous groups of transformations. Now, as pointed out by Hawkins (1991), a group for Lie consisted of a set of invertible transformations of an "m-dimensional manifold" whose elements were taken to be m-tuples". Moreover, Hawkins (1991, p. 191):

> It was tacitly assumed that any group **G** possesses the defining property of a permutation group: if T_1 and T_2 belong to **G**, then so does the composite transformation $T_1 \circ T_2$. [Lie and Klein] took it for granted that, as in the case of permutation groups, closure under composition implies closure under inversion. Thus, if **G** had "the group property" of closure under composition, it was assumed that T^{-1} is in **G** whenever T is.

Article 125 is the first one to have changed substantially between the first and the second editions of *Analytical Dynamics*. As Whittaker explains in the preface to the second edition, "the new explanation of the transformation-theory of Dynamics in §125 sprang from a desire to do justice to the earliest great work of Hamilton's genius", by which he means Hamilton's *Theory of Systems of Rays* Hamilton (1828) and its three supplements Hamilton (1830, 1831, 1837). Whittaker begins the new §125 pointing out that Hamilton's work is based on the "connexion between dynamics and optics" which arises by comparing Fermat's Principle of least time to the Principle of Least Action, "a connexion which is perhaps less obvious in our day [1917] than in his, when the corpuscular theory of light was still widely held", (Whittaker 1999, p. 288). This "connexion" allows one to conclude that "*the trajectories of the particles in the dynamical problem are the same as the paths of the rays in the optical problem*" (Whittaker 1999, p. 289) (italicized in the original). Moreover, Whittaker (1999, p. 289)

> The statement in itself is true whatever hypothesis regarding light be adopted: therefore it supplies a means of connecting dynamics with the *undulatory* hypothesis. This idea is the starting-point of Hamilton's theory.

Now, the undulatory theory allows one to discuss the propagation of light in terms of Huygens method of wave-fronts (Whittaker 1999, p. 289):

> Consider a wave-front, or locus of disturbance in an optical medium, as it exists at a definite instant t, having the form of a surface σ. Each element of this wave-front may be regarded as the source of a secondary wave, propagated outwards from it; so that at a subsequent instant t', the disturbance originating in any point (x, y, z) of the original wave-front will extend over a surface. To obtain the equation of this surface, we observe that the time taken by light to travel through the medium from an arbitrary point (x, y, z) to another arbitrary point (x', y', z') depends only on the six quantities (x, y, z, x', y', z') : let it be denoted by $V(x, y, z, x', y', z')$. This function $V(x, y, z, x', y', z')$ was called by Hamilton the *characteristic function* for the medium in question.

Therefore, at time t', the equation of the wave caused by the disturbance that occurred in (x, y, z) at time t has the form

$$V(x, y, z, x', y', z') = t - t'. \tag{3.3}$$

However, by Huygen's Principle (Whittaker 1999, p. 289),

> The wave-front that represents the whole disturbance at the instant t' is the envelope of the secondary waves which arise from the various elements of the original wave-front.

Let Σ be the wave front at time t' and denote by (l, m, n) the unit vector normal (or "direction-cosines of the normal" in Whittaker's terminology) to σ at (x, y, z) and by (l', m', n') the unit vector normal to Σ at (x', y', z'). Assuming that the medium is isotropic, we have that the ray is normal to the wave front. Since "Σ is the envelope of the surfaces V corresponding to points of σ", it follows that there exist constants κ and λ such that

$$\left(\frac{\partial V}{\partial x}, \frac{\partial V}{\partial y}, \frac{\partial V}{\partial z}\right) = \kappa(l, m, n) \text{ and } \left(\frac{\partial V}{\partial x'}, \frac{\partial V}{\partial y'}, \frac{\partial V}{\partial z'}\right) = \lambda(l', m', n'). \tag{3.4}$$

Eliminating κ and λ from these equalities we find that

$$\frac{1}{l}\frac{\partial V}{\partial x} = \frac{1}{m}\frac{\partial V}{\partial y} = \frac{1}{n}\frac{\partial V}{\partial z} \text{ and } \frac{1}{l'}\frac{\partial V}{\partial x'} = \frac{1}{m'}\frac{\partial V}{\partial y'} = \frac{1}{n'}\frac{\partial V}{\partial z'}$$

which together with

$$l^2 + m^2 + n^2 = l'^2 + m'^2 + n'^2 = 1,$$

give rise to a system of six equations from which we can determine the primed variables in terms of the unprimed ones. Whittaker (1999, p. 290) observes that by these equations

> The behaviour of rays of light in the medium is completely specified in terms of the single function $V(x, y, z, x', y', z')$. It will be observed that they are not differential equations, but that they give directly, in the integrated form, the changes in any system of rays after a finite interval of propagation through the medium.

Then comes the punch line:

> From the point of view of Pure Mathematics, we regard the change from the set of variables (x, y, z, l, m, n) to the set of variables (x', y', z', l', m', n'), or (to express it geometrically) from the surfaces σ, to the surfaces Σ, as a *transformation*. The function V is thus to be regarded as determining a transformation of space which changes any surface σ into a new surface Σ. It is evident that if two surfaces σ and σ' touch at a point, the corresponding transformed surfaces Σ and Σ' also touch at the corresponding point: on this account the transformation has been called by S. Lie a *contact-transformation*. Thus *any function* $V(x, y, z, x', y', z')$ *defines a contact-transformation, which transforms*

any wave-front σ into the wave-front Σ which is derived from σ by propagation through the medium in the interval of time $t' - t$.

But it follows directly from (3.4) that the total differential of V satisfies the equation

$$dV = \kappa(ldx + mdy + ndz) + \lambda(l'dx' + m'dy' + n'dz'). \tag{3.5}$$

Considering the time it would take to move an infinitesimal distance along the radial from the given points, Whittaker concludes that if the units are chosen such that the speed of light in the vacuum (or "free aether" in his terminology) is one, then $\kappa = -\mu$ and $\lambda = \mu'$, where μ and μ' represent the refractive indices of the optical medium at (x, y, z) and (x', y', z'), respectively. Therefore, writing

$$\mu l = \xi,\ \mu m = \eta,\ \mu n = \zeta,\ \mu' l' = \xi',\ \mu' m' = \eta',\ \mu' n' = \zeta',$$

the total differential of V takes the form

$$dV = (\xi' dx' + \eta' dy' + \zeta' dz') - (\xi dx + \eta dy + \zeta dz). \tag{3.6}$$

Hamilton called the vectors (ξ, η, ζ) and (ξ', η', ζ') the *components of the normal slowness of propagation* of the wave at (x, y, z) and (x', y', z'), respectively; see Arnold (1989, p. 251) for more details. This illustrates how the definition of contact transformations used by Whittaker can be accounted for in the context of Hamilton's work on optics, for (3.6) can be interpreted as saying that the differential $\xi dx + \eta dy + \zeta dz$ is invariant, modulo the exact form dV, under the transformation (defined by V) that takes $(x, y, z, \xi, \eta, \zeta)$ to $(x', y', z', \xi', \eta', \zeta')$.

Assuming next that the interval $t' - t = \Delta t$ is very small, Whittaker deduces from (3.6) that there exists a function H such that

$$dH = \frac{dx}{dt}d\xi + \frac{dy}{dt}d\eta + \frac{dz}{dt}d\zeta - \frac{d\xi}{dt}dx - \frac{d\eta}{dt}dy - \frac{d\zeta}{dt}dz. \tag{3.7}$$

The usual Hamiltonian system can be derived by equating the coefficients of $dx, dy, dz, d\xi, d\eta, d\zeta$ on both sides of the equation. Whittaker ends the "new §125" stating that (Whittaker 1999, p. 292) (italicized in the original):

> *Our investigation shews that [the Hamiltonian system derived from* (3.7)*] may be regarded as representing an infinitesimal contact-transformation, that is to say, the motion of a wave-front from one position to a position indefinitely near it.*

As we shall see, he discusses this remark in more detail in §134 which, unlike §§125 and 126, remained unchanged between the first and second editions.

With §125 taken over by Whittaker's effort to commemorate the work of his illustrious predecessor as Royal Astronomer, the formal introduction of contact transformations in the second edition was moved to §126. In the first edition, this article contains only a short discussion of ways under which a contact transformation can be defined when it happens to be (p. 283) "possible to eliminate $(P_1, P_2, \ldots, P_n, p_1, \ldots, p_n)$ completely, so as to obtain one or more relations between the variables

$$(Q_1, Q_2, \ldots, Q_n, q_1, \ldots, q_n)\text{''}.$$

The remaining articles of Chapter XI were not changed in any essential way between the first and later editions. Articles 127 and 128 are concerned with the *bilinear covariant* of the form $\alpha = \sum p_r dq_r$. In modern parlance this is the 2-form $d\alpha$. As usual, Whittaker begins (§127) by defining the bilinear covariant of a general 1-form

$$X_1 dx_1 + X_2 dx_2 + \cdots + X_n dx_n,$$

where "$(X_1, X_2, \ldots, X_n)$ denote any functions of $(x_1, x_2, \ldots, x_n)$" which are implicitly assumed to be differentiable. He denotes the above expression by θ_d and writes

$$\theta_\delta = X_1 \delta x_1 + X_2 \delta x_2 + \cdots + X_n \delta x_n,$$

where "δ is the symbol of an independent set of increments". A formal computation "using the relations $\delta dx_r = d\delta x_r$, which exist since the variations d and δ are independent" shows that

$$\delta\theta_d - d\theta_\delta = \sum_{i=1}^{n} \sum_{j=1}^{n} a_{ij} dx_i \delta x_j \tag{3.8}$$

where "a_{ij} denotes the quantity $\partial X_i / \partial x_j - \partial X_j / \partial x_i$". He finishes §127 proving that this "quantity" is independent of the choice of coordinates, thus justifying the name *bilinear covariant* by which he calls it. In §128, Whittaker shows that since the "bilinear covariant of a differential form is not affected by the addition of an exact differential to the form", it follows that

> If the transformation from
>
> $$(q_1, \ldots, q_n, p_1, \ldots, p_n) \text{ to } (Q_1, \ldots, Q_n, P_1, \ldots, P_n)$$
>
> is a contact-transformation, the expression
>
> $$\sum_{r=1}^{n} (\delta p_r dq_r - \delta q_r dp_r)$$
>
> is invariant under the transformation.

The next three articles are concerned with the bracket expressions named after Lagrange and Poisson. Lagrange's brackets were originally introduced as part of his application of the *method of variations of coefficients* to the problem of the movement of bodies in the solar system, see Lagrange (1760/1761, p. 730). His approach takes as its starting point the fact that the description of the elliptic orbit of a planet around the sun requires six parameters, or constants of the movement, if the sun and planet system remains isolated. Taking into account the effect of the attraction of the other planets, "the planetary orbits must be considered as ellipses whose dimensions and positions in space change little by little"[1] (Poisson 1809, p. 267). In order to determine these changes, one must introduce into the differential equations terms that take into account the effect of the other planets. Poisson explains in (1809, p. 267) that

> In order to solve these equations, one considers as variables the arbitrary constants of the integrals relative to the elliptical movement. It is remarkable that this procedure, one of the most fertile of analysis, consists in taking as variable quantities that have been until then considered as constants.[2]

Lagrange's approach required him to write the derivatives of each of the six keplerian elements[3] of the planet's orbit in terms of the derivatives with respect to these selfsame elements of the potential that defines the perturbative force. The brackets that we now name after Lagrange were introduced to simplify these formulae. Assuming that $q_1, \ldots, q_n, p_1, \ldots, p_n$ are functions of the variables u and v, Whittaker defines the *Lagrange bracket* of u and v as

$$[u, v] = \sum_{r=1}^{n} \left(\frac{\partial q_r}{\partial u} \frac{\partial p_r}{\partial v} - \frac{\partial p_r}{\partial u} \frac{\partial q_r}{\partial v} \right).$$

In 16 October 1809, Poisson read at the Institute de France a memoir "relative to that same theory [Lagrange's 1808 work on the variation of parameters], that I regard from a whole different point of vue" (Poisson 1809, p. 268).[4] Comparing this memoir of Poisson with his own earlier work, Lagrange writes (1760/1761, p. 812):

> This memoir contains an expert analysis that is like the inverse of mine, and whose object is to avoid the eliminations that mine required.[5]

Indeed, Poisson writes the derivative of the potential with respect to a keplerian element in terms of the derivatives of those elements with respect to the time. Following Lagrange, Poisson introduces a bracket notation in order to simplify his formulae. Assuming that f and g are functions of $q_1, \ldots, q_n, p_1, \ldots, p_n$, Whittaker defines the *Poisson bracket* of f and g by

[1] les orbite planétaires doivent être regardées comme les ellipses dont le dimensions et les position dans l'espace, varient par dégres insensibles.

[2] l'on imagina, pour résoudre ces nouvelles équations, de considerer comme variables les constants arbitraires des intégrales relatives au mouvement elliptique. Il est remarquable que ce procédé, l'un des plus feconds de l'analyse, qui consiste à faire varier des quantités regradées d'abord comme constantes.

[3] Eccentricity, semimajor axis, inclination, longitude of the ascending node, argument of periapsis and mean anomaly at epoch.

[4] relatives à cette même théorie, que je reprends en entier sous un nouveau point de vue.

[5] Ce Mémoire contient une savant analyse qui est comme l'inverse de la mienne, et dont l'objet est d'eviter les éliminations que celle-ci exigeait.

$$(f, g) = \sum_{r=1}^{n} \left(\frac{\partial f}{\partial q_r} \frac{\partial g}{\partial p_r} - \frac{\partial g}{\partial p_r} \frac{\partial g}{\partial q_r} \right).$$

Whittaker's notation for these brackets is the same introduced by Poisson in his memoir of 1809; see Poisson (1809, p. 281 and 290). In *Analytical Dynamics* both brackets are used to characterize contact transformations. Let $(Q_1, \dots Q_n, P_1, \dots, P_n)$ be functions of $(q_1, \dots, q_n, p_1, \dots, p_n)$. Whittaker shows that "*the conditions which must be satisfied in order that the transformation from one set of variables to the other may be a contact-transformation may be written*" (p. 289, italicized in the original) in the form

$$\begin{array}{lcll} [P_i, P_j] = [Q_i, Q_j] = 0 & & (P_i, P_j) = (Q_i, Q_j) = 0 & \\ [Q_i, P_j] = 0 \text{ if } i \neq j & \text{or} & (Q_i, P_j) = 0 \text{ if } i \neq j & \text{where} \quad 1 \leq i, j \leq n, \\ [Q_i, P_i] = 1 & & (Q_i, P_i) = 1 & \end{array}$$

depending on which bracket one uses. Actually he derives the second of these sets of conditions from the first using that if "$(u_1, \dots, u_{2n})$ are $2n$ independent functions of $(q_1, \dots, q_n, p_1, \dots, p_n)$, so that conversely $(q_1, \dots, q_n, p_1, \dots, p_n)$ are functions of $(u_1, \dots, u_{2n})$" then

$$\sum_{t=1}^{2n} (u_t, u_r)[u_t, u_s] = 0 \text{ when } r \neq s \text{ and } \sum_{t=1}^{2n} (u_t, u_r)[u_t, u_r] = 1.$$

These relations make explicit what Lagrange meant when he wrote (see above) that Poisson's analysis was the inverse of his own. For a discussion of the rôle of the Poisson bracket in the work of Lie on contact transformations see Hawkins (1991).

Article 132 is devoted to the study of the special case of those contact transformations that leave the differential form $\sum p_r dq_r$ invariant, rather than "invariant to the modulus of an exact differential", a case first studied by Mathieu (1874, p. 273ff). Whittaker points out that these transformations form a subgroup of the whole group of contact transformations. However, as it might be expected, given his definition of group, he defines a subgroup as what we would call a subset closed under the composition of transformations.

Whittaker then turns to infinitesimal contact transformations (§133), defined as "those in which the new variables [...] differ from the original variables [...] by quantities which are infinitesimal". He concludes that (p. 292)

> The most general infinitesimal contact-transformation is defined by the equations
>
> $$Q_r = q_r + \frac{\partial K}{\partial p_r} \Delta t, \quad P_r = p_r - \frac{\partial K}{\partial p_r} \Delta t \quad (r = 1, 2, \dots, n),$$
>
> where K is an arbitrary function of $(q_1, q_2 \dots, q_n, p_1, \dots, p_n)$ and Δt is an arbitrary infinitesimal quantity independent of $(q_1, q_2, \dots, q_n, p_1, \dots, p_n)$.

He then points out that if f is a function of the ps and qs then its increment is equal to $(f, K)\Delta t$ when the arguments are subject to the transformation defined by the above equations. He calls the Poisson bracket (f, K) the *symbol* of the given infinitesimal transformation.

Whittaker begins the very short §134 explaining that, taking into account that the motion of a Hamiltonian system is governed by the equations (3.2),

> It follows that we can interpret [equations (3.2)] as implying that the transformation from the values of the variables at time t to their values at time $t + dt$ is an infinitesimal contact-transformation.

From which he concludes what has become the book's most often quoted statement (p. 293):

> The whole course of a dynamical system can thus be regarded as the gradual self-unfolding of a contact-transformation.

Indeed, "this result, taken together with the group-property of contact-transformations, is the foundation of the transformation theory of dynamics". The next major result is proved in article 136, where it is shown that "a contact-transformation of the variables $(q_1, q_2, \ldots, q_n, p_1, \ldots, p_n)$ of any dynamical system conserves the Hamiltonian form of the equations of the system". The same result is proved again in article 137 using what Whittaker calls *the first Pfaff's system of equations* corresponding to the 1-form

$$\alpha = X_1 dx_1 + X_2 dx_2 + \cdots + X_{2n+1} dx_{2n+1},$$

which is the system of equations

$$\sum_{i=1}^{2n+1} a_{ij} dx_i = 0 \text{ for } 1 \leq j \leq 2n+1$$

obtained by equating to zero the coefficients of $\delta x_1, \ldots, \delta x_{2n+1}$ in the bilinear covariant of α defined in (3.8). These equations are "from the mode of their formation [...] invariantively connected" with α. Applying this to the form

$$p_1 dq_1 + p_2 dq_2 + \cdots + p_n dq_n - H dt \tag{3.9}$$

he shows that (p. 297)

> The dynamical system whose Hamiltonian function is H is invariantively connected with the differential form [(3.9)] inasmuch as the equations of motion of the dynamical system, in terms of any variables $(x_1, x_2, \ldots, x_{2n}, \tau)$ whatever, are the first Pfaff's system of the differential form
>
> $$X_1 dx_1 + X_2 dx_2 + \cdots + X_n dx_n - T d\tau,$$
>
> which is derived from the form [(3.9)] by the transformation from the variables $(q_1, q_2, \ldots, q_n, p_1, \ldots, p_n, t)$ to the variables $(x_1, x_2, \ldots, x_{2n}, \tau)$.

This is used, in articles 138 and 139, to determine how the Hamiltonian of a given dynamical system changes under contact transformations and, in article 140, to show that:

> The integration-problem [of a system whose Hamiltonian is H] is thus reduced to the determination of a transformation for which the last term of the differential form [(3.9)] becomes a perfect integral.

Chapter XII collects a number of results on Hamiltonian systems, mostly related to their integrals. Whittaker begins (§142) by showing that if the Hamiltonian H of a system is independent of time, then it can be used to reduce by one the number of degrees of freedom of the system. He then (§143) deduces the Hamilton-Jacobi differential equation

$$\frac{\partial W}{\partial t}+H\left(q_1,q_2,\ldots,q_n,\frac{\partial W}{\partial q_1},\frac{\partial W}{\partial q_2},\ldots,\frac{\partial W}{\partial q_n},t\right)=0 \tag{3.10}$$

by assuming the existence of a contact transformation with $W = W(q_1,\ldots,q_n, Q_1,\ldots,Q_n,t)$ as generating function (a terminology not used in *Analytical Dynamics*) that transforms the given system into one whose Hamiltonian is constant. The next article is devoted to proving that "the integral of the kinetic potential" (the Hamilton action integral) is a solution of the Hamilton-Jacobi equation. He then turns to infinitesimal contact transformations and shows that (p. 308):

> Integrals of a dynamical system, and [infinitesimal] contact-transformations which change the system into itself, are substantially the same thing.

This result also brings back the Poisson bracket, for if ϕ is an integral of the system then the corresponding infinitesimal transformation has (ϕ, H) as its symbol. The rôle of the Poisson bracket is further developed in §145, where it is shown that "if ϕ and ψ are two integrals of [a] system, the Poisson-bracket is constant throughout the motion" (p. 309). This result, first observed by Poisson (1809, §7, p. 281), allows one to deduce new integrals from old ones whose Poisson brackets are not constant. Whittaker notes (§146) that the same holds for the Lagrange bracket, but that

> Lagrange's result, unlike Poisson's, does not enable us to find any new integrals; for we have to know all the integrals before we can form Lagrange's bracket-expressions.

Suppose now that $u_1,\ldots,u_r$ are functions of the ps and qs. If all the brackets (u_i,u_k) are zero, these functions are said to be *in involution.* Using infinitesimal transformations, Whittaker shows in §147 that (p. 311, italicized in the original)

> *If* $(u_1,u_2,\ldots,u_r)$ *are in involution, and the equations*
>
> $$v_1=0, v_2=0,\ldots,v_r=0$$
>
> *are consequences of the equations*
>
> $$u_1=0, u_2=0,\ldots,u_r=0,$$
>
> *then the functions* $(v_1,\ldots,v_r)$ *are in involution.*

This is used (§148) to prove a theorem of Liouville (1855) which we now state as: if a system of n degrees of freedom has n integrals in involution then it is integrable by quadratures; see Arnold (1989, Chapter 10, section 49, p. 271) and Lützen (1990, pp. 670–679). This is followed by a discussion of a theorem of Levi-Civita's which, as explained in the introduction of Ahmed and Ghori (1984), "provides a procedure for constructing particular solutions of a [...] dynamical system by means of time independent invariant relations in involution". The last three articles are devoted to the integration "of systems which possess integrals of certain special kinds"; namely, integrals that are linear in the momenta (§150) and integrals quadratic in the velocity (§§152 and 153).

As we have pointed out before, Chapters XI and XII were the first to which Whittaker made substantial changes between the first and second editions, the most obvious of which concerns the first article (§125) of Chapter XI. As we saw above, the new version of §125 had its origin in Whittaker's wish to do justice to Hamilton's work on dynamics. This may also be the reason why the *Hamilton–Jacobi equation* of the first edition became the *Hamilton equation* in all later ones, matched by a corresponding change in the title of §142 from *the Hamilton-Jacobi equation* to *Hamilton's partial differential equation*.

3.3 Celestial mechanics

Chapter XIII marks the beginning of what we called in Sect. 2.3 the second part of the book, which is devoted to the application of the principles explained in Part I to the problem of three bodies. In this chapter, Whittaker introduces the differential equations that describe the problem (§155) and shows that this system of the 18th order (made up of nine equations of the second order) admits a number of first integrals that allow one to reduce it to a system of the 6th order (§§157–159). In his words (p. 346):

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

In the next articles he discusses the *problem of three bodies in a plane* (§161) and the *restricted problem of three bodies* (§162).

Although the next chapter is 27 pages long, it contains only two articles, the first of which is subdivided into eleven parts, the second into nine. Article 164 contains the proof of Brun's theorem. It turns out that the ten integrals of the three body problem mentioned in the quotation above are all of them algebraic functions. In a paper published in 1887, H. Bruns (1848–1919) showed that "the classical integrals are the only independent algebraic integrals of the problem of three bodies" (p. 346). In his Report to the British Association, Whittaker had noted that "a defect in Bruns's proof [...] was pointed out and remedied by Poincaré in 1896" (Whittaker 1900, p. 159). Curiously, Whittaker's proof in *Analytical Dynamics* contains the very same mistake fixed by Poincaré and mentioned in the Report (Whittaker 1900, p. 159). A modern proof that addresses the many flaws found in Bruns's and all subsequent proofs, including Whittaker's, can be found in Julliard-Tosel (2000). The second article of Chapter XIV is concerned with (p. 368):

> Another theorem on the non-existence of a certain type of integrals in the problem of three bodies, which is in many respects analogous to that of Bruns, and was discovered in 1889 by Poincaré.

This is one of the results for which Poincaré won the King Oscar Prize in 1889 Poincaré (1890). It is also the content of the chapter called *Non-existence des intégrales uniformes*, of Poincaré's *Méthodes Nouvelles* (Poincaré 1899, Chapitre V, p. 233). See also (Barrow-Green, 2002, p. 71ff) and Verhulst (2012, p. 133ff).

Chapter XV is concerned with the description of the "motion of a particle which is free to move in a plane under the action of conservative forces". In an introductory article (§166) Whittaker explains that (p. 374)

> The principal results which have been obtained hitherto relate to periodic orbits (§§167–171), to the stability of a given orbit (specially of a periodic orbit) with respect to small displacements from it (§§172–176), and to the stability of a given group of orbits with respect to the time, i.e. the question as to how far the orbits preserve their general character after the lapse of a very great time (§§177–179).

The topics studied in this chapter include Lagrange's special solution of the three body problem when the "mutual distances of the bodies are invariable throughout the motion" (§170) and Poincaré's study of stability in terms of the system's characteristic exponents (§§175–176).

The last chapter opens with an article where Whittaker explains the difficulties one faces in finding a solution of a dynamical system in terms of convergent series. He mentions analytical continuation of solutions and a transformation, based on Poincaré's work on zetafuchsian functions Poincaré (1884), that maps a band of a given width on each side of the real axis into a circle of radius one. However, as he explains at the beginning of the next article (p. 397):

> The series discussed in the preceding article are all open to the objection that they give no evident indication of the nature of the motion of the system after the lapse of a great interval of time [...]. Under these circumstances we are led to investigate expansions of an altogether different type.

These are trigonometric series, and he motivates their introduction with examples of their use in the "problem of the simple pendulum" and in celestial mechanics. The remainder of the chapter is devoted to Whittaker's work Whittaker (1902) (p. 398) on

> A method which is applicable to all dynamical systems and leads to solutions in the form of trigonometric series: the method consists essentially, as will be seen, in the repeated application of contact-transformations, which ultimately reduce the problem to the equilibrium-problem.

This method applies to a system whose Hamiltonian H is independent of time. Solving the equations

$$\frac{\partial H}{\partial p_r} = \frac{\partial H}{\partial q_r} = 0 \quad (r = 1, 2, \ldots, n),$$

one obtains

> One or more sets of values $(a_1, a_2, \ldots, a_n, b_1, b_2, \ldots, b_n)$ for the variables $(q_1, q_2, \ldots, q_n, p_1, \ldots, p_n)$; and each of these sets of values correspond to a form of equilibrium or (if the above equations are those of a reduced system) steady motion of the system.

One then chooses one of these equilibrium points and translates the variables so that the chosen point is at the origin. Thus, for sufficiently small values of the variables, it is possible to write H as a power series whose homogeneous component of degree k we will denote by H_k. Moreover, $H_0 = H_1 = 0$ because the origin is an equilibrium point of the system. Assuming that H_2 is positive definite and that the eigenvalues of the corresponding bilinear form are imaginary and distinct, Whittaker shows that there exists a contact transformation that changes H_2 into

$$\frac{1}{2}\sum_{r=1}^{n}(p_r^2 + s_r^2 q_r^2).$$

Applying another (§184) contact transformation, the Hamiltonian can be written in the form

$$a_{0,0,\ldots,0} + \sum a_{n_1,n_2,\ldots,n_n} \cos(n_1 p_1 + n_2 p_2 + \cdots + n_n p_n),$$

where the coefficients a are functions of $q_1, \ldots, q_n$ only. Moreover, the periodic part of H is small when compared with the non-periodic part $a_{0,0,\ldots,0}$, so that (p. 403) "when the variables $q_1, q_2, \ldots, q_n$ are small they vary very slowly, while the variables $p_1, p_2, \ldots, p_n$ vary almost proportionally to the time". In §§186–187, he shows that by applying yet more contact transformations to the system one obtains a Hamiltonian H whose periodic part "will become so insignificant that it may be neglected" (p. 187). Therefore, we end up with a system whose Hamiltonian depends only on the variables $(q_1, \ldots, q_n)$. Hence,

$$\frac{dq_r}{dt} = 0 \text{ and } p_r = -\int \frac{\partial H}{\partial q_r} dt \quad (r = 1, \ldots, n);$$

which allows us to integrate the system, obtaining

$$p_r = \mu_r t + \epsilon_r, \text{ where } \mu_r = -\frac{\partial H}{\partial q_r} \quad (r = 1, \ldots, n).$$

As we have mentioned before, the chapters on celestial mechanics were those that changed the most between the first and the fourth editions. In both editions, Chapter XV begins in article 166. However, in the first edition, Chapters XV and XVI have, respectively, 14 and 9 articles, while in the fourth edition these numbers have increased to 22 and 18. Instead of discussing the various topics that constitute the subjects of the new articles, some of which are of a very technical nature, we will be satisfied with pointing out some of the more conspicuous changes. From now on, article numbers refer to the fourth edition of *Analytical Dynamics*. In Chapter XV, these include the definition of Poincaré's *doubly asymptotic orbits* (§169), a study of the orbits of the planets in the "general relativity-theory" (§170) and a whole article on *The theory of matrices* (§176). The new articles in Chapter XVI are concerned with Whittaker's theory of *adelphic integrals*, originally published in Whittaker (1917). In §200, Whittaker defines *adelphic transformations* as (Whittaker 1999, p. 442, italicized in the original)

> Those infinitesimal transformations which change each trajectory of the system into an adjacent trajectory, *in such a way that every ordinary periodic solution is changed into an adjacent periodic solution of the same family*, i.e. having the same period and the same constant of energy.

As he explains in a footnote (Whittaker 1999, p. 442), the name comes from the Greek for brotherly because the original orbit and the transformed one are closely related and also because "the integral corresponding to the transformation stands in a much closer relation to the integral of energy than do other integrals of the system". For the sake of simplicity, he studies in detail only systems with two degrees of freedom. There are three cases depending on the ratio s_1/s_2 of the coefficients of q_1 and q_2 in the linear component of the Hamiltonian. In article 196, he derives the terms of the adelphic integral up to order four in $\sqrt{q_1}$ and $\sqrt{q_2}$ when s_1/s_2 is irrational. The resulting formula covers pages 434 and 435 of the fourth edition. Whittaker admits that no proof of the convergence of these series "has yet been devised" (Whittaker 1999, p. 437), but expresses the hope that the analogy with similar series devised by Bruns, that have been proved to be convergent, "is favourable to the convergence" of his adelphic integrals (Whittaker 1999, p. 438). As we will see in Sect. 4.4, Whittaker's optimism concerning the convergence of these series was not shared by T. M. Cherry in his review of the third edition Cherry (1928). He thought instead that they would be "generally divergent and only exceptionally convergent".

4 Reception

In this section, we use various reviews of the four editions of *Analytical dynamics* to examine how the mathematical community reacted to Whittaker's book. The fact that the book remains in print helps us to trace the evolution of the attitudes of that community both to the subject and to the style of the book.

The first edition of *Analytical Dynamics* was reviewed very thoroughly in the *Bulletin of the American Mathematical Society* by E. B. Wilson and in *Nature* by G. H. Bryan. We discuss these long reviews in detail and then briefly mention a few more reviews up to the 1980s.

4.1 An American point of view: E. B. Wilson

Edwin Bidwell Wilson (1879–1964) graduated from Havard in 1899 and received a Ph.D. from Yale four years later. In (Hunsaker and MacLane, 1973, p. 287)

Wilson is described as "the last student of Gibbs", whose lecture notes he brought "to a full and polished written form" in the book Wilson (1901). Wilson spent the period 1902–1903 studying mathematics in Paris, mainly at the École Normale Supérieure. On his return to the United States, he became assistant professor at Yale, but moved to the Massachusetts Institute of Technology in 1907, becoming a full professor there in 1911. He had a wide range of interests that extended beyond mathematics and physics and included statistics and economics. The *Biographical Memoir* (Hunsaker and MacLane 1973, p. 291) mentions his "extraordinary activity in reviewing books": between 1911 and 1914, for example, he wrote thirty reviews for the *Bulletin of the American Mathematical Society*. Wilson's six-page-long review Wilson (1906) of *Analytical Dynamics* was published in the *Bulletin* in 1906. The review opens with a lament for what Wilson sees as the imminent encroachment by pure mathematics of territory that traditionally belonged to applied mathematics:

> At Cambridge, England, mathematics means for the most part mechanics, mathematical physics, or even physics sometimes not so very mathematical. The famous tripos—the mathematical tripos, of course, which goes back at any rate to 1747—seems, at least to an outsider, to lay its main stress on the theoretical applications of mathematics rather than on pure mathematics. Very likely this is a tradition that has come down from the time of Newton, and it is certainly maintained by the eminent physicists such as Stokes, Kelvin, Maxwell, Rayleigh, J. J. Thomson, who have been high wranglers. This tripos with its great prestige gives an attractive and distinctive touch to the university and although the ever-increasing pressure of pure mathematics, with its possibilities for various kinds of unessential and mediocre work infinitely wider than those to be found in theoretical applied mathematics, will probably tell on the training at Cambridge sooner or later, may we not look forward to that date with some regrets on the general uniformizing that is taking place and lament the fact that other realms than physics are possessed of an entropy? (Wilson 1906, p. 451)

But he immediately changes tone: "At present, however, there seems no immediate danger" and the reason for that is Wilson (1906, p. 452)

> The publication by Cambridge University Press of three large and highly important volumes by as many recent graduates of Cambridge typically cantabrigian in that they exhibit great mathematical power and attainements directed firmly and unerringly along the direction of physical research.

The books are James Walker's *Analytical Theory of Light*, James Jeans's *Dynamical Theory of Gases* and Whittaker's *Analytical Dynamics*. According to Wilson:

> Mechanics is more largely a part of pure mathematics than the other branches of physics because the data are fewer and simpler; the transition to the systems of differential equations less recondite (Wilson 1906, p. 452).

He justifies his opinion by pointing out that most of the recent progresses in this area have been attained by mathematicians. However,

> For the mathematician [...] the way has unfortunately been barricaded by the kind of treatise on mechanics which has been available to the intermediate student.

Among these, he mentions the "famous works of Routh", by which he means a series of treatises on various aspects of mechanics that Routh published in the second half of the nineteenth century; see Routh (1860, 1882, 1884, 1891, 1898). His opinion of Routh's books, whose subject he considers "not so much mathematical as distinctly mechanical", is not very high: "the reader learns how to do difficult and often artificial problems rather than how problems are done". Although he admits that the kind of training provided by Routh's books is "highly valuable and should precede other work", he considers that Whittaker's book "breaks the barricade and opens the way to fruitful advance". However, he also makes clear that this book "starts at the beginning of the subject—but it is not for the beginner". Indeed, "the book is mathematical in nature, written with a precision and developed with a logic sure to appeal to mathematicians". Moreover, the "diversity of method taken with the compact style makes the book hard reading for any but the somewhat advanced student". Since "to solve the exercises requires on the part of the reader a similar versatility", he concludes that

> One may see why and how it is that the Cambridge student well versed in mechanics is also in reality thoroughly equipped in pure mathematics in so far as present applications are concerned.

After these initial considerations, Wilson discusses the contents of the book. His description of Chapters I to IX extends to four pages, halfway through which he points out that from Chapter VII on

> The amount of mathematics which the reader must know, or assimilate from the meager indications in the text, is by no means inconsiderable—calculus of variations, elementary divisors, integral invariants, and contact transformations may be mentioned as examples. It is, then, just at this point that modern mechanics begins.

In comparison, the last eight chapters are dealt with in just one paragraph, of which we quote the beginning and the end:

> The remaining one hundred pages of the book become successively more and more full of the most recent researches. [...] How much the student of mechanics proper may be interested in this matter [the subjects of the last four chapters] is a question. The advisability of including such a treatment here cannot, however, be well open to doubt. Mathematical astronomy as one conclusion of mechanics is quite in the spirit of the present time.

Having said that, he concludes by suggesting two topics that he "should have been happy to see introduced". The first is "the study of the application of Lagrange's equations and the kinetic potential to problems of physics and chemistry" and the second is "statistical mechanics". These choices are not at all surprising, given that Wilson was a student of Gibbs and that he prepared Gibbs's lecture notes for publication.

4.2 A British point of view: G. H. Bryan

G. H. Bryan (1864–1928) was brought up by his mother and his grandparents after the death of his father, a Cambridge don, when he was still very young. Mrs Bryan lived in the continent for long periods and, as a consequence, Bryan acquired a good knowledge of several languages. He went up to Cambridge in 1883, graduated as fifth Wrangler in 1886 and stayed there, as a fellow of Peterhouse, until 1895, the year he was appointed Professor of Pure and Applied Mathematics in the University College of North Wales, in Bangor. He stayed in Bangor until his retirement in 1926. Bryan's research was mostly concerned with problems in hydrodynamics and thermodynamics, but he also contributed important results to the bourgeoning science of aeronautics. His book *Stability in aviation*, published in 1911, is described in Hunsaker (1916, p. 278) as "the first rational theory of the dynamical stability of aeroplanes". According to Bairstow (1933, p. 139), Bryan was a "friendly, kindly, very eccentric individual with a keenness for 'unsolved problems' in dynamics and hydrodynamics". His eccentricity was "attributed largely to the circumstances of his early upbringing". His strengths and weaknesses were summed up by Love (1929, p. 240) as follows:

> To understand Bryan, his achievement and his limitations, one must think of him as a man with a passion for accuracy, a remarkably clear head, and a severely practical outlook on life. His writings reflect his mind—always lucid and to the point, never vague or mysterious, generally seeking for something of practical value. He took little interest in laboratory work in physics, and perhaps less in speculations as to the constitution of the universe or the nature of time and space. He should be remembered as a man with a very uncommon gift for mathematics, who was actuated by an intense desire to utilize it for the purpose of promoting material progress.

As we will see this goes a long way towards explaining his criticisms of *Analytical Dynamics*.

Whittaker's book was the last one of three "Cambridge Mathematical Works" that Bryan reviewed for the issue of *Nature* published on April 27, 1905, see Bryan (1905), the other two works being Grace and Young's *The algebra of invariants* and James Jeans's *The dynamical theory of gases*, which is also mentioned in Wilson's review. The very first paragraph sets the tone for the whole review:

> Whatever opinions may be felt as to the desirability of University Presses competing with private firms in swelling the already too large flood of school geometries or issuing cram books for compulsory Greek examinations, there can only be one opinion as to the series of standard treatises on higher mathematics emanating at the present time from Cambridge. In a country which, in its lack of national interest in higher scientific research, particularly mathematical research, stands far behind most other important civilised countries, it necessarily devolves on a University Press to publish advanced mathematical works. We may take it as certain that the present volumes will be keenly read in Germany and America, and will be taken as proofs that England contains good mathematicians, though

> Englishmen as a nation may be unaware of their existence, with the exception of the senior wrangler of one year, who is forgotten the next.

Bryan's first mention of Whittaker's book is in a paragraph which one can safely call polemical:

> It is remarkable that physicists strain at gnats when put down to study kinetic theory or thermodynamics, and yet they swallow camels with complacency when they read the subject of Mr. Whittaker's book, "Analytical Dynamics." Some writers even go so far as to introduce pages and pages of the most unreal dynamical problems into what they call treatises on physics.

The next paragraph elaborates this further

> "The soluble problems of particle dynamics" [which is the title of Chapter IV of *Analytical Dynamics*] mostly represent things which have no existence. It is impossible for a particle to move on a smooth curve or surface because, in the first place, there is no such thing as a particle, and in the second place there is no such thing as a smooth curve or surface. What constitutes the chief interest of "Analytical Dynamics" is the possibility of forming clear mental pictures of its results by *imagining* bodies capable of performing the motions discussed.

He summarizes the contents of the book in two paragraphs. The first begins with "Mr. Whittaker's treatment is essentially mathematical and advanced" and the second is followed by: "The book is thus written mainly for the advanced mathematician". The "large number of examples both in the text and at the end of the chapters" include "a good many" that contain "the substance of minor papers" (cf. Sect. 3.1) while others are followed by the reference *Coll. Exam.*:

> Some of the questions bearing these references may give foreign mathematicians a little insight into the unpalatable nuts which Cambridge students are expected to waste time in trying to crack for examination purposes. The antics of insects crawling on epicycloids, or the vagaries of particles moving along the intersections of ellipsoids with hyperboloids of one sheet, are of no scientific interest, and the time spent in "getting out" problems of this character might better be employed in learning something useful. Moreover, Cambridge college examiners have a habit of endowing bodies with the most inconsistent properties in the matter of perfect roughness and perfect smoothness. A perfectly rough body placed on a perfectly smooth surface forms as interesting a subject for speculation as the well-known irresistible body meeting the impenetrable obstacle. What the average college don forgets is that roughness or smoothness are matters which concern two surfaces, not one body.

He proposes to replace such "fictitious problems" by "the more frequent introduction of simple problems in resisted motion". Moreover,

> Those who have the ability to do more difficult work should pass on to the advanced parts of a book like Mr. Whittaker's and learn what foreign mathematicians have been doing; this is much more useful.

The high regard for what continental mathematicians had been doing and the insistence on problems of interest in applications immediately remind one of A. E. H. Love's opinion of Bryan, quoted above. It is also interesting to compare Bryan's comments with those of Wilson. As it is so often the case, the outsider Wilson is touched by the mystique of the mathematical tripos, whose "great prestige gives an attractive and distinctive touch to the university". Bryan, the fifth wrangler, sees much to criticize in a system under which a student must spent a lot of his time solving problems about "ellipsoids rolling on perfectly smooth surfaces formed by the revolution of cissoids or witches about their axes".

4.3 The "Fictitious Problem" polemic

The Cambridge establishment must have been piqued by Bryan's comments, for a letter was published (1905, p. 56) in *Nature* on May 18, 1905 under the title *Fictitious Problems in Mathematics*. Taking his cue from the comment by Bryan repeated above, the writer signs himself simply "An Old Average College Don". After briefly summarizing Bryan's criticisms, the anonymous writer adds

> Will your reviewer give a reference to some page of Whittaker's book (that under review), or to some page of any other text-book used in the last half-century at Cambridge, in support of his charge against Cambridge examiners? Fifty years ago, William Hopkins was still directing the mathematical teaching of Cambridge, and enforcing the conservation of energy where friction is taken into consideration. A perfectly rough sphere moving on a rough surface is intended to mean that, during the motion considered, the sphere rolls without any slip. "A perfectly rough sphere moving on a smooth surface" would no doubt be equivalent to "A sphere moving on a smooth surface": but where does the phrase occur?

This is followed by a response from Bryan who claims that

> The alleged inaccuracies of language in stating the assumed conditions of smoothness or roughness prevailing between two bodies in contact are unfortunately so common that it is the exception rather than the rule to find any problem in which these conditions are correctly worded.

Bryan claims to have come across two examples of such imprecisions in a chapter of Besant's *A Treatise on Dynamics* but he does not give any exact reference. A search through a digitized version of Besant's book Besant (1893) comes up with several instances of *perfectly rough*, but *perfectly smooth* appears only twice and none of them supports Bryan's claim. However, the example he quotes from Routh's *Elementary rigid Dynamics* (Routh 1882, p. 69, example 4), where a "person is placed at one end of a perfectly rough board which rests on a smooth table", seems a perfect fit. Routh had been Bryan's Tripos coach (Warwick 2003, p. 517) and perhaps to soften the blow, Bryan adds:

> At the time of writing the review I was quite unaware that such an example had found its way into a text-book written by so careful a teacher of applied

> mathematics as Dr. Routh, and it says much for the prevalence at Cambridge of these erroneous forms of statement that this wording failed to attract the writers attention.

He finishes by saying that a similar misuse of the terms perfect elastic and inelastic has been brought to his attention.

Besant and Routh had both retired in the 1880s (Warwick 2003, p. 280), but while "Besant was too much engrossed by his proper work to add much to mathematical literature" (GBM 1917, p. 310), Routh had actually made important contributions to dynamics. Perhaps that goes some way towards explaining why Routh was the only one to feel the need to reply to Bryan. In a note published on the May 25th, 1905 issue of *Nature* (1905, p. 78), Routh explains that

> When bodies are said to be perfectly rough, it is usually meant that they are so rough that the amount of friction necessary to prevent sliding in the given circumstances can certainly be called into play. [...] The board in question has therefore no special peculiarity. All that is stated is that the coefficient of friction between the man and the board exceeds a certain finite quantity.

He points out that such expressions are only abbreviations used to "make the question concise" to allow the students attention to be "specially directed to the dynamical principle involved in the solution". He concludes by claiming that the problem mentioned by Bryan has been "understood by so many students in the sense above described" without any objection been raised that he thinks its "meaning must be perfectly clear", so much so that he "cannot imagine what other meaning it could have".

Routh's letter on the 25th May 1905 issue of *Nature* is actually preceded by one of A. B. Basset on the same subject. Basset notes that the statements about which Bryan complains designate "an ideal state of matter" assumed to exist to simplify the problem and make it "sufficiently easy for the beginner". He then explains that similar "fictitious hypothesis", like that of a *frictionless liquid* are required in hydrodynamics, because

> Viscosity leads to such formidable difficulties, that nobody has yet succeeded in solving such a simple problem as the motion due to a doublet situated at the centre of a sphere; and the solution, if it could be obtained, would throw much light on the mode of attacking more difficult problems

Hydrodynamics was Bryan's main field of research.

The June 1st, 1905 issue of *Nature* (1905, p. 102) brought two more contributions to the debate. The first, by C. B. Clarke, begins by saying that Routh set the example to "test (inter alia) whether the pupil knew that, for any friction to arise, both the surfaces must be rough". He then repeats the quote from Bryan's review reproduced in the anonymous letter that started the exchange and adds that "this is the statement which I called (and still call) in question", thus implicitly identifying himself as the "average college don". Like Basset, Clarke explains that such simplifying hypotheses are not restricted to exercises in textbooks:

> In considering the pendulum, I probably begin by assuming no friction on the axis of suspension, and, if I try afterwards to apply a correction for this friction,

> I probably make an assumption which is inaccurate. Friction = pressure × a constant is inaccurate, statically and dynamically.

Clarke's letter is followed by another one from Bryan where he tries to make clear what it is that he is objecting to. His argument is essentially that if a "perfect rough body" is one that never slips when brought in contact with another body and a "perfect smooth body" is one that never offers "tangential resistance" then it does not make sense to ask what happens when a "perfect rough" body slides against a "perfect smooth" body. The problem, he says, is that terms such as "perfect roughness" and "perfect smoothness" are not "attributes of a single body" because friction depends on the properties of the two bodies that are being brought into contact. At the end of his letter, he proposes ways by which this kind of "misleading language" can be avoided. He suggests, for instance, that the example from Routh's book could be rephrased as a "man walks without slipping along a plank which can slip without friction on a horizontal table".

The issue of *Nature* published a week later (June 8, 1905) (1905, p. 127) brings another letter of Routh on the same subject. He argues that Bryan's definition of a "perfectly rough body" in (1905, p. 102) as one "which never by any chance slips on any other body it is placed in contact" is not that in common use. Thus, the "proper inference" is not that "'the average college' don forgets an elementary law of friction" but that Bryan's definition is not "that in common use". Indeed, he says, the "various letters sent to NATURE sufficiently show what meaning is usually attached to the words".

The last letter *Nature* published on the polemic (on June 22, 1905) came from Bryan. Turning to Routh's definition of "perfectly rough," he points out that it refers to *bodies* being perfectly rough, while his "attack" is directed against saying that *a body* is "perfectly rough". In his view

> The definition given in the book in which the problem occurs is inapplicable to the problem as at present worded. Otherwise we appear to be dealing with a plank such that in the given circumstances, one [side] of which is resting on a smooth table, the amount of friction necessary to prevent sliding can certainly be called into play, and this is apparently inconsistent with Dr. Routh's interpretation.

He ends by challenging "An Average College Don" to "point to any text-book containing an explicit definition of a perfectly rough body (not bodies)" and adds:

> If he succeeds, I anticipate no difficulty in furnishing him with examples of questions which are either inconsistent with his definition, are ambiguously worded, or are open to some equally serious objection.

Reading these letters 100 years after they were written, it is difficult not to view the whole polemic as prompted by a bout of hair-splitting on the part of Bryan. His initial claim is undoubtedly correct: friction is a property that has to do with how bodies behave when they come into contact. Thus, *a body* on its own cannot be "perfectly rough": that expression can only be applied when two bodies come into contact with each other. As it often happens in such polemics, the boundary between the two factions tends to get blurred when one looks at concrete examples. Although it is true that Routh refers to a body and Whittaker to a plane (Whittaker 1904, p. 33) as "perfectly rough", they do so only as a kind of shorthand, because both of them correctly define only what it means for *bodies* (Routh 1882, p. 280) or surfaces (Whittaker 1904, p. 31) to be "perfectly smooth". So, from Bryan's point of view, they give correct definitions but later use them improperly. Routh, Basset and Clarke, on the other hand, claim that it is tacit knowledge (at least in Cambridge) that one writes "a perfectly rough body" to stand for the formal definitions correctly given by both Routh and Whittaker. It is hard not to ask what it was *really* all about. Comparing Routh's letter of the 8th of June with Bryan's final letter, one might even get the impression that those who opposed Bryan simply misunderstood what he was complaining about. Although the whole polemic derived from a comment of Bryan's in his review of *Analytical Dynamics*, the author of that book never got involved in the polemic.

Bryan also wrote a review of the second edition (1917) of *Analytical Dynamics* Bryan (1918). He begins by observing that at the time the first edition was published aeroplanes "only existed in people's imagination and in reports of successes by the Wright brothers", so that no one expected to find applications to "aerial navigation" in a treatise on analytical dynamics. However, although since then "there has been plenty of time for pure and applied mathematicians to provide material" to allow the aeroplane to be "a predominating feature" of a book like Whittaker's, he finds in that book no mention of the aeroplane or of any matter that he judges to be "suggestive, even vaguely, of the existence of aerial navigation". He goes on to explain that he thinks mathematicians have done very little on the subject and that most of the relevant contributions have come from physicists and engineers. The way in which Bryan's own interest and enthusiasm for aviation seems to fill his whole field of vision is rather amusing. He refers to the problem of "stability of steady motion" as "out-and-out the most important development of theoretical dynamics". And he thinks it "rather a pity" that Whittaker did not treat of the subject in the second edition of *Analytical Dynamics*. He concludes that:

> The present work will be found of much use by such students of a future generation as are able to find time to extend their study of particle and rigid dynamics outside the requirements of aerial navigation, and it will also afford a valuable source of information for those who are in search of new material of a theoretical character which they can take over and apply to any particular class of investigation.

4.4 Other reviews

Besides the two detailed reviews discussed above, the first edition of *Analytical Dynamics* was also reviewed in the *Jahrbuch über die Fortschritte der Mathematik* (JFM 35.0682.01). The review was written by Emil Lampe (1840–1918), a professor at the *Technischen Hochschule Charlottenburg*, who was also the Jahrbuch's editor. In his review of this "excellent work" (vortrefflichen Werks) Lampe says that

> The careful treatment aimed at the training of students—that for half a century has characterized the science of analytical mechanics in England, and which

> produced such works as Walton's collection of examples and the textbooks of Routh, Thomson and Tait, Lamb, Love, Basset and many others—has had, as a consequence, that the English student is directed with great energy towards the study of mechanics in which he displays excellent performance, as can be gauged from the many, and not at all easy, problems appended at the end of each chapter of this book.[6]

The second edition was reviewed by E. P. Jourdain (1879–1919) in both *The Mathematical Gazette* Jourdain (1917) and *Science in Progress* (Jourdain 1917). The reviews are very similar, but the *Gazette* one is longer and more detailed. After listing the subjects of the various chapters, Jourdain makes a number of very specific criticisms. The first concerns a statement on the aether and the principle of relativity, which he considers "of no value when put in such a magisterial form". In the second, he complains that the "introduction (p. 29) of the equations of motion suffers from a defect". The only "equations of motion" in p. 29 of the second edition are those that define Newton's first law for cartesian coordinates in 3-space and the "defect" seems to be that Whittaker did not give sufficient motivation so that, as Jourdain puts it, the student is "told by implication that these equations [...] are identities". The third criticism concerns a point of the history of mathematics, a subject that interested Jourdain so much that he was preparing a book on it at the time of his death (1921). According to a footnote on p. 34 of the second edition of *Analytical Dynamics*, Lagrange first formulated his equations of motion in Lagrange (1760/1761), a paper published 28 years before his *Mécanique Analytique*. Jourdain disagrees with Whittaker's claim and suggests that "Prof. Whittaker should have given a rather more detailed justification for such a revolutionary statement". For a recent discussion of the relation between these two works of Lagrange see (Pulte 2005, 212ff). The last point concerns, as he puts it at the end of the *Science in Progress* review,

> A rather noticeable [...] neglect of work published from 1904 to 1908 on the questions that arise in formulating Hamilton's principle and the principle of least action for non-holonomic conditions and generalised co-ordinates.

This work was done by Jourdain himself, as we learn from the more detailed *Gazette* review, which ends thus:

> However, all these criticisms do not touch the very great value of the book which has been and will be the chief path by which students in English speaking countries have been and will be introduced to modern work on the general and special problems of dynamics.

G. D. Birkhoff wrote a very short review Birkhoff (1920) of the second edition of *Analytical Dynamics* for the *Bulletin of the American Mathematical Society* in which he praises the book for being "invaluable as a condensed and suggestive presentation of the formal side of analytical dynamics" but suggests that there is "serious need

[6] Es ist kein Lehrbuch für den Anfänger, sondern vielmehr ein reichhaltiges Kompendium, in welchem der Verf. mit grosser Sorgfalt und vielem Geschick alles verarbeitet hat, was die Forschung der besten Gelehrten bis in die jüngste Vergangenheit über den Gegenstand zutage gefördert hat.

for a complementary type of treatise in which the main emphasis is laid upon the deeper qualitative side which has played an increasingly larger part since the work of Poincaré". As a "single instance of the incompleteness of Whittaker's treatment in this respect" he cites the discussion of trigonometric series in Chapter XVI which is done "in the spirit of Delaunay and does not even mention that these series are generally divergent nor refer to their asymptotic properties".

Not surprisingly, the first book in English to complement *Analytical Dynamics* in the way proposed by Birkhoff was his own *Dynamical systems* Birkhoff (1927), whose first edition appeared in 1927, the same year that the third edition of *Analytical Dynamics* was published. The *Gazette* featured a double review of these books by T. M. Cherry (1898–1966) in 1928, see Cherry (1928). Cherry begins by explaining that "General Dynamics as it has been developed during the past century is simply the theory of differential equations of Lagrangian and Hamiltonian form". Of the books he is reviewing, Whittaker's deals "largely" and Birkhoff's "almost entirely" with "this 'higher' aspect of Dynamics". A brief discussion of Poincaré's work on the stability of the solar system and the divergence of the trigonometric series used to solve the three body problem sets the scene for the two reviews. Cherry's comments are concerned mostly with the last two chapters of *Analytical Dynamics* which, he explains, were "completely re-written in order to take account of recent developments in this field". However, in his view, although Chapter XV (on the theory of orbits) is a "great improvement on preceding editions", on the whole it "hardly lives up to its title". Moreover, the discussion of Whittaker's *adelphic integrals* in Chapter XVI "must be regarded as suggestive rather than conclusive" because "a process is given for constructing the "adelphic integral" series [...] without the necessary proof that it will be successful". As mentioned in Sect. 3.3, he also criticizes what he considers as Whittaker's "optimistic view" concerning the convergence of these series. Unlike Whittaker, he thinks that they will be "generally divergent and only exceptionally convergent".

The most recent review of *Analytical Dynamics* appeared in Zentralblatt (Horneffer 1988) when the fourth edition was reprinted in the *Cambridge Mathematical Library*. The following quote is from its first paragraph:

> First published in 1904 [the book] reflects the style of late 19th century and the state of the mathematical apparatus of those days. The excessive use of elliptic functions in the elaboration of explicit formulas, for example, is a fact maybe surprising to the modern reader. The bulk of calculations contrasting with the lack of figures (not so bad as with Lagrange 200 years ago: there none, here three!), the newcomer in the discipline will be bewildered, but soon will appreciate the clearness of physical thought and the development of formulas seldom found in other texts. Nonetheless, as a first reading one would better give him another text.

Analytical Dynamics has also been mentioned in the reviews of other books that treat roughly the same material. For example, the same issue of the *Bulletin of the American Mathematical Society* (volume 2, issue 2, 1980) brought two different reviews of books on mechanics, both of which mention Whittaker's book. Reviewing both Arnold's *Mathematical methods of classical mechanics* Arnold (1989) and volume one of Walter

Thirring's *A course in mathematical physics* Thirring (1978), Ian Sneddon claims in Sneddon (1980) that

> The theoretical work of the century and more after the death of Lagrange was crystallized by E. T. Whittaker in a treatise Whittaker (1904) which has not been superseded as the definitive account of classical mechanics.

However,

> The time was now ripe for a presentation of the theory of classical mechanics in terms of the theory of differentiable manifolds which had been developed since the appearance of the fourth edition of Whittaker's treatise.

The other one is a review, written by S. Sternberg, of the second edition of *Foundations of mechanics* by Abraham and Marsden. Sternberg begins the review noting that

> This excellent book is one of several superb books on mechanics which have appeared in the past decade, such as those of Souriau [Souriau (1970)], Siegel-Moser [Siegel and Moser (1971)], Arnold [Arnold (1989)] and Thirring [Thirring (1978)], indicating a revitalized interest in the venerable subject of classical mechanics.

At the end of the review, he points out that:

> All the above books should be on the shelf of every serious student of mechanics. One would like to be able to report that such a collection would be complete. Unfortunately, this is not so. There exist topics in the classical repertoire, such as Kowalewskaya's top which are not covered by any of these books. So hold on to your copy of Whittaker (1904).

Analytical Dynamics also had its share of adverse criticism, the most notorious being probably the comments in Goldstein's *Classical Mechanics* (Goldstein 1950, p. 27), where it is called

> A well-known treatise which presents an exhaustive treatment of analytical mechanics from the older viewpoints. The development is marked, regrettably, by an apparent dislike of diagrams (of which there are only four in the entire book) and of vector notation, and by a fondness for the type of pedantic mechanics problems made famous by the Cambridge Tripos examinations. It remains, however, a practically unique source for the discussion of many specialised topics.

We will examine Goldstein's criticism in more detail in the next section.

5 Discussion

In the last two sections we described the contents of *Analytical Dynamics* and examined the way its various editions were received by the community. We will finish by examining the book's style and discussing its impact on the development of mechanics in the twentieth century.

5.1 Style

One characteristic that pleased the reviewers of the first edition of *Analytical Dynamics* was Whittaker's ability in presenting the very latest results in dynamics. Wilson (1906, p. 452) puts it very nicely, when he says that *Analytical Dynamics* "is modern, thoroughly modern with its bibliographical references running quite up into the year 1904". Even G. H. Bryan, whose review is the most critical of those discussed in Sect. 4, writes that (Bryan 1905, p. 603):

> It will thus be seen that Mr. Whittaker's treatise collects into book form the outlines of a long series of researches for which hitherto it has been necessary to consult English, French, German, and Italian transactions.

Both reviewers also note that, although the book does not assume any previous knowledge of dynamics on the part of the reader, it is by no means elementary. G. H. Bryan points out that the "book is thus written mainly for the advanced mathematician" (Bryan 1905, p. 603) and E. B. Wilson observes that it "starts at the beginning of the subject—but it is not for the beginner"; (Wilson 1906, p. 452) A similar opinion is expressed by Lampe (1918),

> This is no textbook for the beginner, but rather a rich compendium, in which the author has handled with great care and much skill all the research that the best scholars have brought to light on the subject until very recently.[7]

Indeed, the treatment is thoroughly analytical and presupposes a good knowledge of special functions, the calculus of variations, and the theory of ordinary differential equations. As Wilson (1906, p. 452) says in his review, the "book is mathematical in nature, written with a precision and developed with a logic sure to appeal to mathematicians".

Although this is not attested in any of the reviews that I have found, Whittaker's strict mathematical approach seems to have led to the book being criticized for being too abstract. In his obituary of Whittaker McCrea (1957, p. 248) says that at the time the first edition was published Whittaker "was criticized for wasting space on the transformation theory of dynamics". This is echoed by C. Truesdell in the exordium [reprinted in Truesdell (1984, p. 30)] of *The classical field theories*, a book he co-authored with R. Toupin. Writing about their book, he says they expect that some people "will reproach us with too much abstract and useless formalism"; and adds as an afterthought "not forgetting that such deprecation was directed towards Whittaker's *Analytical Dynamics*"Truesdell (1984). Which also indirectly testifies to the status that Whittaker's book had attained by the 1960s, when Truesdell's book with Toupin was published. Whittaker himself writes in Whittaker (1940, p. 156) that

> When my book Analytical Dynamics was published in 1904, I was criticised severely for devoting a large part of it to such topics as the duality between

[7] Es ist kein Lehrbuch für den Anfänger, sondern vielmehr ein reichhaltiges Kompendium, in welchem der Verf. mit großer Sorgfalt und vielem Geschick alles verarbeitet hat, was die Forschung der besten Gelehrten bis in die jüngste Vergangenheit über den Gegenstand zutage gefördert hat.

> coordinates and momenta, contact-transformations, Poisson-brackets, integral-invariants, action, and so forth—mere mathematical playthings, the critics called them.

That *Analytical Dynamics* is abstract cannot be denied. Not only is it not an introduction for beginners, as the reviewers rightly pointed out, it is a presentation of mechanics as a mathematical theory. Even a cursory look at the book shows that the discussion of the physical principles is kept to a minimum. Moreover the presentation is unapologetically analytic, in Lagrange's sense of the word, with very little use of geometric language. As the latter's *Mécanique Analytique*, the first edition of *Analytical Dynamics* did not contain any pictures, although by the fourth edition Whittaker had added four, all of them in Chapter XV (pages 392, 394 and 411 of Whittaker (1999)).

Despite the fact that this is a book for mathematicians, it is not rigorous in the sense we now use the word. One obvious flaw is the constant use of old style infinitesimal arguments; a more subtle one is that often there is no clear separation between results that hold only locally, and those that hold globally. A typical example occurs in §141 (p. 302), where Whittaker wants to solve an equation of the form $H + h = 0$, for some Hamiltonian function H of $q_1, \ldots, q_n, p_1, \ldots p_n$ and constant h, with respect to the variable p_1. Faced with a similar equation, we would now apply the Implicit Function Theorem, which would allow us to conclude that the equation can be solved, as desired, in the neighbourhood of any point where the required Jacobian is nonzero. Not so Whittaker; he simply says "let this equation be solved for the variable p_1". Although this is by no means an isolated example, Whittaker is obviously aware that a series often converges only in a small neighbourhood of a point. This is particularly evident in the chapters concerned with celestial mechanics, where convergence of the series that describe the orbits was one of the outstanding problems of the day. Thus, as we saw in Sect. 3.3, he begins Chapter XVI explaining that (Whittaker 1904, p. 396)

> We have already observed (§32) that the differential equations of motion of a dynamical system can be solved in terms of series of ascending powers of the time measured from some fixed epoch; these series converge in general for values of t within some definite circle of convergence in t-plane, and consequently will not furnish the values of the coordinates except for a limited interval of time. By means of the process of continuation it would be possible to derive from these series successive sets of other power series, which would converge for values of the time outside this interval.

A different attitude to a similar problem occurs in Whittaker's discussion of stationary integrals (§103). As we explained in Sect. 3.1, Whittaker first shows that "for small ranges the Action is a minimum for the actual trajectory" and then determines that the action is in fact a minimum "provided the final point is not beyond the kinetic focus of the initial point" (p. 248). Therefore, although this result holds only locally, it is possible to characterize the bounds within which it works; cf. (Chorlay 2011, §3.2). All of this is fairly typical of the way mathematics was practiced in the early twentieth century; a time when, as Chorlay shows in (2011), mathematicians were just beginning to become aware of the local-global dichotomy.

Although, to my knowledge, the book has not been criticized for its lack of rigour with regard to the domains of convergence of the various series that it contains, the same cannot be said of some characteristics that it also shares with contemporary works. Predictably, such criticisms have been advanced in a book and in a review published after 1950. As we saw in Sect. 4.4, Goldstein (1950, p. 27) speaks of Whittaker's "apparent dislike of diagrams (of which there are only four in the entire book) and of vector notation", while Horneffer (1988) talks of the "excessive use of elliptic functions in the elaboration of explicit formulas" and also of the "bulk of calculations contrasting with the lack of figures (not so bad as with Lagrange 200 years ago: there none, here three!)"; actually four, as Goldstein says. Since this kind of criticism often arises from a work being taken out of the context in which it was written, which should include both its intended public and the historic period, we will first consider the rôle of vectors and of diagrams in mechanics at the beginning of the twentieth century.

To begin with, one should remember that although Newton introduced several quantities of a vectorial nature in *Principia* (Crowe 1967, p. 128), the concept of vector and many of the vector operations we use today originated in W. R. Hamilton's works on quaternions (1843) and in H. Grassman's *Ausdehnungslehre* (1844). Despite being critical of quaternions, J. C. Maxwell realized that vectors helped to "fix the mind at once on a point of space instead of its three coordinates, and on the magnitude and direction of a force instead of its three components" as he put it in his *Treatise on electricity and magnetism* (1873) (Maxwell 1954, p. 9). Given the influence of Maxwell's book, it is not surprising that J. W. Gibbs and O. Heaviside, the creators of vector analysis, became interested in vectors after reading it (Crowe 1967, p. 152 and 162). Gibbs first lectured on his vector system in 1879, but his *Elements of Vector Analysis* was only privately published (in two volumes that appeared 1881 and 1884). Heaviside developed his ideas, independently of Gibbs, in papers published between 1882 and 1883 and got acquainted with Gibbs's *Elements* only in 1888. The first volume of his *Electromagnetic Theory*, which "contained the first extensive published treatment of modern vector analysis" (Crowe 1967, p. 169), appeared in 1893. Crowe (1967, p. 225) points out that "the most influential force in producing acceptance [of vector analysis] stemmed from the association of vectorial analysis with electrical theory, an association to be credited to Maxwell and Heaviside". The first book solely devoted to vector analysis to be commercially published was E. B. Wilson's *Vector Analysis* Wilson (1901), (Crowe 1967, p. 228); see also Sect. 4.1 above.

Thus, at the time (1896–1904) Whittaker was giving the lectures that would later become *Analytical Dynamics*, the use of vectors was mostly restricted to applications in electromagnetism. Moreover, Maxwell's comment in the *Treatise* that "the methods of Des Cartes are still the most familiar to students of science" (Maxwell 1954, p. 9) continued to hold in 1904, for the first book completely devoted to vector analysis had been published only three years before. A search through Poincaré's *Leçons de mécanique céleste* Poincaré (1910), whose first volume was published a year after *Analytical Dynamics*, shows the same sparing use of vectors and an almost complete absence of vector equations. In fact, most uses of the word "vecteur" in volume I of Poincaré (1910) occur in the expressions "rayons vecteurs" and "vecteur des aires", although Poincaré also mentions "la somme géométrique des deux vecteurs" (p. 32) and "les vecteurs qui représentent en grandeur et direction les quantités de mouvement" (p. 251).

Passing now to the question of diagrams one immediately sees that it cannot be settled by any simple appeal to the historical context. Even a cursory look at Routh's books Routh (1860, 1882, 1884, 1891, 1898) or Besant (1893) show that their texts are peppered with diagrams to help the student along. However, these are rather elementary texts, aimed at students, which is not the case of *Analytical Dynamics*, as both Wilson, Bryan and Horneffer make clear in their reviews. Whittaker himself says in the Preface to the first edition that the "connected account" of recent advances that he presents in his book will "prove a stimulus to further research". McCrea observes in (1957, p. 248) that this was Whittaker's "modest way of saying that this was the first book on what was then and still remains [in 1957], modern fundamental dynamical theory". In the same preface, Whittaker says that:

> It would ill become of me to omit mention of the debt which I, in common with all English students of Dynamics, owe to the influence which Dr Routh has so long exercised on the study of the science.

About this, McCrea comments in McCrea (1957) that "although Routh's books are useful even today [1957], Whittaker's book disclosed immeasurably wider horizons". These remarks show that *Analytical Dynamics* should be considered as a research monograph, and that it is with similar contemporary works that it should be compared. The title of Whittaker's book and Horneffer's parenthetical remark that it is "not so bad as" Lagrange's *Mécanique Analytique* suggest that it should be compared with other works in dynamics written in the tradition of Lagrange's great treatise. In this respect, three books immediately come to mind: Jacobi's *Vorlesungen über analytische Mechanik* Jacobi (2009) and Poincaré's *Methodes nouvelles de la mécanique celeste*, both of which are cited in *Analytical Dynamics*, and the latter's *Leçons de mécanique celeste*. None of these books have anywhere near as many pictures as Routh's books or most modern (post-1960) books on dynamics. For example, the first volume of Poincaré (1910), published in 1905, a year after the first edition of *Analytical Dynamics*, contains only three pictures (pp. 37, 69 and 363) in a total of 365 pages. One concludes that although Whittaker's book may be a rather extreme case (no figures in 409 pages of the first edition), it is by no means atypical. Even Poincaré who, by all accounts, was of a more geometric bent than Whittaker, included very few pictures in his books devoted to mechanics.

As far as elliptic functions are concerned, a comparison of the works mentioned in the previous paragraph with *Analytical Dynamics* reveals that the latter abounds in them, while they do not appear at all in the first volume of *Leçons de Mécanique Céleste*. As might be expected we fare better when we turn to Jacobi's Lectures on Dynamics Jacobi (2009), where elliptic and abelian integrals appear in lectures 25, 27 and 30 (Das Abelsche Theorem). However, Jacobi's work was, by then, almost 50 years old and Poincaré's geometrical approach would soon move dynamics away from exact solutions. So it seems that Whittaker's abundant use of elliptic functions is both an echo of a bygone era and a reflection of his interest in what are now called the special functions of analysis.

Another characteristic of the book often mentioned by its reviewers is the *examples* or exercises as we would now say. Some come with solutions and appear interspersed throughout the actual text, though printed in smaller type. But there are also many that come at the end of each chapter under *Miscellaneous examples* and are posed as problems to be solved by the reader. As we have seen in Sect. 4.2, G. H. Bryan points out that (Bryan 1905, p. 603)

> An interesting feature [of Analytical Dynamics] is the large number of examples both in the text and at the end of the chapters. Of these a good many really contain the substance of minor papers that have been published abroad.

These examples are followed by the name of the author of the paper they come from. However, most of the examples are followed by "Coll. Exam." or "Camb. Math. Tripos" showing that they were originally posed as questions in examinations. Bryan considered many of these questions artificial and said so very forcibly in his review, see Sect. 4.3. The artificial nature of many of these problems was also noted by Goldstein (1950, p. 27). It is difficult to argue against the view that, in choosing many problems that even to his contemporaries seemed contrived, Whittaker was being not only too much of a Cambridge man, but a somewhat old fashioned one at that. In this respect, one should remember that those who opposed Bryan in the *Fictitious Problems* polemic were men very much older than Whittaker: in 1904 Basset was 50 years old, Clarke was 72 and Routh 73.

Finally, when reading *Analytical dynamics* one cannot help noticing the large number of references to works that have become classical. In the first edition these are mostly works by contemporary authors. Whittaker himself mentions "Lie, Rayleigh, Klein, Hertz, Lorentz, Poincaré, Siacci, Bruns, Boltzmann, Larmor, Greenhill, Appell, Painlevé, Stäckel and Levi-Civita" in the preface of the first edition, adding that a "large proportion of the subject matter has hitherto been accessible only in scattered memoirs" by these mathematicians. In later editions, however, he goes further back in time, systematically quoting, with very specific references, from works by Newton, Euler, the Bernoulli brothers, Lagrange and Laplace. This was, perhaps, also a fruit of those hours he spent reading at the library during his Dublin years, see Sect. 2.1 above.

5.2 Impact

Analytical Dynamics has been very successful by most of the criteria used to judge a book: it went through four editions, it was translated into both German and Russian and the reviews published at the time of the various editions are full of praise for the book. The main reason for this has to do with the fact, pointed out in all the reviews of the first edition, that it presented the very latest results in dynamics. Moreover, Whittaker's choice of topics, specially in the more advanced parts of the first half of the book, proved to be along two of the main lines that dynamics would take in the twentieth century. As McCrea puts it in McCrea (1957),

> With an inspired appreciation of what is in the best sense useful in mathematics, [Whittaker] has included in his books much that was found to be needed in

> the development of quantum mechanics and wave-mechanics more than twenty years afterwards. The part that British workers in particular were thus enabled to contribute to this development owes a debt to Whittaker which seems scarcely to have been sufficiently acknowledged.

Of course by "books" he means *Modern Analysis* and *Analytical Dynamics*.

The debt mentioned by McCrea was explicitly acknowledged by Dirac in 1977. The events to be related occurred in 1925, while Dirac was a PhD student working under Ralph Fowler. In August of that year, Fowler received from W. Heisenberg the proof-sheets of Heisenberg's latest paper (van der Waerden 1967, pp. 261–276), and passed them on to Dirac, who read them but at first found them not very interesting. It was only a week later, when Dirac read the paper for the second time, that he realized how revolutionary Heisenberg's ideas really were. However, he felt that Heisenberg's formulation was not very satisfactory and he began to try to rework it "in a Hamiltonian scheme that would conform with the theory of relativity" (Kragh 1990, p. 16). However, this required that Dirac find an expression in classical mechanics that would correspond to the commutators introduced by Heisenberg's theory. In the course of one of the long walks he usually took on Sundays, Dirac began to think about Poisson brackets. As Dirac reminisced in 1977 [quoted in Kragh (1990, p. 17)]:

> I remembered something which I had read up previously in advanced books on dynamics about these strange quantities, Poisson brackets, and from what I could remember, there seemed to be a close similarity between a Poisson bracket of two quantities, u and v, and the commutator $uv - vu$. The idea first came in a flash, I suppose, and provided of course some excitement, and then of course came the reaction "No, this is probably wrong." I did not remember very well the precise formula for a Poisson bracket, and only had some vague recollections. But there were exciting possibilities there, and I thought that I might be getting to some big new idea. It was really a very disturbing situation, and it became imperative for me to brush up my knowledge of Poisson brackets and in particular to find out just what is the definition of a Poisson bracket. Of course I could not do that when I was right out in the country. I just had to hurry home and see what I could then find about Poisson brackets. I looked through my notes, the notes that I had taken at various lectures, and there was no reference there anywhere to Poisson brackets. The textbooks which I had at home were all too elementary to mention them. There was just nothing I could do, because it was a Sunday evening then and the libraries were all closed. I just had to wait impatiently through that night without knowing whether this idea was really any good or not, but still I think that my confidence gradually grew during the course of the night. The next morning I hurried along to one of the libraries as soon as it was open, and then I looked up Poisson brackets in Whittaker's *Analytical Dynamics*, and I found that they were just what I needed.

However, this is not the only version of these events left by Dirac. van der Waerden reproduces in (van der Waerden 1967, p. 41) what Dirac purportedly told him in an interview on June 26, 1961:

At first I could not make much of [Heisenberg's paper], but after about two weeks I saw that it provided the key to the problem of quantum mechanics. I proceeded to work it out by myself. I had previously learned the Transformation Theory of Hamiltonian Mechanics from lectures by R. H. Fowler and from Sommerfeld's book *Atombau und Spektrallinien*.

In the second (1921) edition of Sommerfeld's book Hamiltonian mechanics is discussed in the appendix, which includes a section on contact transformations (Sommerfeld 1921, §6, pp. 468–472). However, Poisson brackets are not mentioned anywhere in the book, not even in this section. Since it is clear that, right from the beginning, the brackets played a key rôle in Dirac's version of quantum mechanics (Dirac 1926), it seems safe to trust Dirac's later recollection of events, rather than his claims in the interview with van der Waerden.

The exposition of Hamiltonian dynamics and contact transformations in *Analytical Dynamics* also contains, in incipient form, many of the results that would play an important rôle in the geometrization of dynamics that took place in the second half of the twentieth century. However, although the key players in this process were certainly aware of the book's existence, it does not seem to have directly influenced them. Asked whether *Analytical Dynamics* had any influence on his work, R. Abraham, whose book on mechanics has become a classic presentation of the symplectic approach, said (Abraham, Private communication):

I can only say that yes, I read Whittaker as a student and made much use of his books during my work on mechanics ... but no, they did not influence directly my work in translating mechanics into modern symplectic geometry. In my book of 1967, e.g., I did not include his portrait in the historical gallery at the beginning of the book. Indirectly, perhaps ... My own history of the symplectic approach is given on page 131 (notes for Chapter 3) of the first edition [of Abraham (1967)].

In these notes the history of geometrical mechanics is summarized in one paragraph as follows:

This version of mechanics has been slowly evolving since Cartan [Cartan (1922)]. The first modern exposition of Hamiltonian systems on symplectic manifolds seems to be due to Reeb [Reeb (1952)] in 1952. An early version of Lagrangian systems in this context appears in Mackey [Mackey (1963)]. This formulation of mechanics was widely known in mathematical circles by 1962, and is explained in a letter by Richard Palais that circulated privately at about that time. This and other material from §14 and §15 [of Abraham (1967)] can also be found in Sternberg [Sternberg (1964)], Jost [Jost (1964)], and Hermann [Hermann (1962)].

When asked the same question, S. Sternberg, whom Abraham lists as one of his direct influences, wrote (Sternberg, private communication):

I believe that Wintner, in his book *The Analytic Foundations of Celestial Mechanics* was the first to tie in (linear) "canonical relations" with the symplectic group as discussed in the indigestible book *The Classical Groups* by Hermann Weyl.

Indeed, the paper van Kampen and Wintner (1936), which Aurel Wintner wrote with E. R. van Kampen, is an extension to general canonical transformations of the results in Wintner (1934), whose subject is what is now called linear symplectic geometry. In van Kampen and Wintner (1936, §4, p. 855), a canonical transformation is defined as follows:

> Suppose that the $2n$ functions (of $2n + 1$ variables [$X = (x_1, \ldots, x_{2n})$ and t]) which occur in [the] transformation formulae [$Y = Y(X; t)$] have continuous partial derivatives of the second order and that the transformation determines for every fixed t a locally one-to-one correspondence between the phase-spaces, the $2n$-rowed Jacobian being everywhere distinct from zero. A transformation [$Y = Y(X; t)$] which satisfies these requirements is said to be a canonical transformation if it transforms every canonical system [(3.2)] into a system of differential equations which is again canonical.

As the authors note, although the transformed system is required to be canonical, its Hamiltonian function need not coincide with that of the target phase-space; when it does, they call the transformation *completely canonical.* In van Kampen and Wintner (1936, §7, p. 857) characterize canonical transformations are characterized in terms of the Jacobian with respect to $X = (x_1, \ldots, x_{2n})$ of the transformation $Y = Y(x; t)$. Let C be the Jacobian and let

$$G = \begin{pmatrix} \omega & -\epsilon \\ \epsilon & \omega \end{pmatrix}$$

where ϵ is the identity matrix and ω the zero matrix, both of size $n \times n$. Assuming that the Hamiltonian h is time independent, their characterization amounts to the statement that

> $Y = Y(x)$ is a canonical transformation if and only if there exists a constant s such that $C'GC = sG$, where the prime is taken to mean transposition.

In this case, the Hamiltonian of the transformed system is sh. Thus, a transformation will be completely canonical when $s = 1$. Wintner and van Kampen warn their readers that the traditional approach to canonical transformation, as presented, for instance, in Prange's article Prange (1935) in the *Encyklopädie der Mathematischen Wissenschaften* starts with the tacit assumption that $s = 1$. As Arnold points out (Arnold 1989, p. 239), in the volume *Mechanics* of their famous *Course of Theoretical Physics* Landau and Lifschitz (1976, §145, p. 143) mistakenly "prove" that all canonical transformations are completely canonical. No similar mistake will be found in *Analytical Dynamics*, where all contact transformations, as defined in §125 of the first edition, are automatically completely canonical, a fact proved by van Kampen and Wintner in §12 of their paper (van Kampen and Wintner 1936, p. 861).

Incidentally, Whittaker's book is not listed as a reference in van Kampen and Wintner (1936) or Wintner (1934); however, both papers cite Birkhoff's *Dynamical Systems* Birkhoff (1927) and *Lezioni di meccanica razionale* by T. Levi-Civita and U. Amaldi Levi-Civita and Amaldi (1927). More surprisingly, *Analytical Dynamics* is also not a reference in Wintner's *The Analytical Foundations of Celestial Mechanics* Wintner

(1941), where an exposition of his joint work with van Kampen on canonical transformations can be found in Chapter 1. It should also be noted that in both van Kampen and Wintner (1936) and Wintner (1941) Wintner understands by *phase space* a $2n$-dimensional euclidean space. Although manifolds are absent from Wintner's work, he has full command, unlike Whittaker, of the difference between local and global properties. Thus, in the Preface of Wintner (1941), he says that:

> In chapter I, emphasis is laid on a careful distinction between formal questions, which are always local in nature, and questions in the large, which are the actual problems of mathematical dynamics.

For a discussion of the use of the expression "in the large" in the early twentieth century see Chorlay (2011).

Having called attention to Wintner's priority in relating symplectic maps to canonical transformations, Sternberg (Private communication) adds:

> Although Prof. Wintner was my teacher and PhD advisor, I had not read his *Analytic Foundations* as a student. My introduction to celestial mechanics came from Carl Ludwig Siegel. Prof. Siegel gave a course in 1953 on celestial mechanics at Johns Hopkins where I was a student. It was of course, amazing, dealing mainly with his mastery of the problem of small divisors, but also with Bruns theorem etc. I remember this course vividly, and had some correspondence with him. He wrote the book *Vorlesungen über Himmelsmechanik* [Siegel (1956)], based on this course, and this later morphed into Moser-Siegel [Siegel and Moser (1971)].

This brings us to another main contributor to the development of the symplectic approach, C. L. Siegel. His paper *Symplectic Geometry* Siegel (1943), begins with an introduction to linear symplectic geometry that covers essentially the same ground as Wintner had done in Wintner (1941). However, although Siegel had published on celestial mechanics, his aim in Siegel (1943) is to contribute to the then incipient theory of several complex variables. More precisely, he decomposes an $n \times n$ symmetric matrix with complex coefficients into real and imaginary parts and considers the domain H formed by those symmetric matrices whose imaginary part is positive definite. This domain, he claims, "is a generalization of the upper half-plane". The symplectic group first appears in Theorem 1: "Every analytic mapping of H onto itself is symplectic", (Siegel 1943, p. 3). Given Siegel's motivation, it is not surprising that although he devotes Part II of his paper (Siegel 1943, pp. 8–16) to the symplectic group, he never refers to its being connected with the canonical transformations of classical mechanics. It is only in his book Siegel (1956), mentioned by Sternberg in the comment quoted above, that Siegel makes clear the connection between the symplectic group and canonical transformations. Siegel's presentation prompted S. P. Diliberto to note in his review Diliberto (1958) of Siegel (1956) that:

> The first sections of Part I have a short but sound introduction to Lagrangian derivatives, canonical transformations, and Hamilton- Jacobi theory, all treated in a style to which they are not accustomed.

Diliberto's review is full of superlatives, right from the first paragraph, where he claims that the appearance of Siegel's book is "certainly one of the great mathematical events of the century".

Despite of his enthusiasm for Siegel's course, Sternberg (Private communication) says:

> What really inspired me, however, was a visiting lecture by Prof. S.S. Chern. I was so taken by his version of mathematics that I managed to spend several summers at the University of Chicago where I attended his lectures, and was later fortunate to have been appointed an instructor there 1957–1959. Prof. Chern (who had been a student of Elie Cartan) introduced me to the works of Cartan which, in one form or another, have been a major portion of my preoccupations. My introduction to what developed into the symplectic approach to mechanics came from Cartan's great book, "Leçons sur les Invariants Intégraux" [Cartan (1922)].

Cartan's book is also mentioned by Sternberg in the introduction of Sternberg (1964, p. ix), where he says:

> This book, like all Cartan's work, is rich in geometrical ideas. It is also quite readable—once the reader gets accustomed to the language of infinitesimals.

Indeed, although the language of Cartan's *Leçons* may be somewhat antiquated, it contains an exposition of the theory of differential forms and its application to problems of mechanics that has a far more modern feel than that of *Analytical Dynamics*.

The reminiscences of J. Marsden in Marsden (1993) nicely complement these comments of Abraham and Sternberg. According to Marsden,

> In the period 1960-1965 geometric mechanics was "in the air". Some key papers were available, such as Arnold's work on KAM theory and a little had made it into textbooks, such as Mackey's book on quantum mechanics and Sternberg's book on differential geometry. In this period Steve [Smale] was working on his dynamical systems program. His survey article Smale (1967) contained important remarks on how geometric mechanics (specifically Hamiltonian systems on symplectic manifolds) fits into the larger dynamical system framework. In 1966 at Princeton, Abraham ran a seminar using a preprint of the survey article and it was through this paper that I first encountered Smale's work.

As it might be expected, the published version of Smale's survey contains the modern definition of symplectic manifold (Smale 1967, section III.1, p. 805). Smale gives two references where the interested reader will find "a complete discussion of this material", one of them is Sternberg's book Sternberg (1964), the other one is Abraham's Abraham (1967).

The conclusion seems to be that the rôle that *Analytical Dynamics* played at the time geometrical mechanics was being developed was that of a useful and self-contained source for many classical results. Indeed, this is a rôle it continues to play, as a search in MathSciNet will reveal. Between 2000 and 2012, Whittaker's book was cited in 111 papers (excluding those works of a purely historical nature). Moreover, these citations are not limited to works on mechanics and astronomy, but also includes papers on linear algebra, ordinary and partial differential equations, dynamical systems, differential geometry, global analysis, control systems and quantum mechanics.

Finally, it should be noted that in his short summary of the history of geometric mechanics, quoted above, Abraham mentions Jost's paper Jost (1964), which begins:

> It was P. A. M. Dirac who emphasised most strongly the significance of the *Poisson brackets* in classical analytical dynamics [see, e.g., P. A. M. Dirac, *Quantum Mechanics* [sic] (Claredon Press, Oxford, 1947), 3rd. ed., Sec. 21, p. 84 and earlier editions]. He not only did this but also gave what turns out to be a complete axiomatic characterization of the Poisson brackets [see Dirac, Eqs. (2)–(6); see also our Sec. 4 (a–f)].

Of course, the book mentioned by Jost is Dirac's *The Principles of Quantum Mechanics* Dirac (1947). Further on, Jost adds that

> [...] Diracs [sic] axioms lead to a phase space with coordinates and canonically conjugate momenta [...] Once this is established, the "correct procedure" consists of course in a complete elimination of any kind of coordinates. The principle [sic] results of general analytical dynamics have to be derived by the exclusive use of Poisson brackets.

Given, as we have seen, that Dirac's approach to quantum mechanics was greatly influenced by Whittaker's exposition of Poisson brackets, *Analytical Dynamics* has arguably exerted an indirect influence on the development of geometrical mechanics through its rôle in the development of Dirac's version of quantum mechanics. More details on the history of symplectic geometry and the development of geometric mechanics can be found in Kosmann-Schwarzbach (2013).

5.3 Conclusion

As the evidence gathered in this paper shows, *Analytical Dynamics* had a substantial rôle to play in the development of mechanics in the twentieth century, although the character of this rôle kept changing with the passing of time. At the time its first edition was published, *Analytical Dynamics* was praised for expounding the state of the art in the most advanced parts of mechanics, especially in Hamiltonian dynamics and celestial mechanics; see Sect. 4 above. The early reviewers also noted that the book presented the fundamental theory of dynamics in great generality, instead of developing tools to deal with specific systems. This led to the book being criticized for what was considered its excessive abstraction, with some suggesting that the inclusion of a chapter on the transformation theory of dynamics (Chapter XI) was wasted space; see Sect. 5.1 above. Whittaker's approach would be vindicated in the 1920s when this very chapter turned out to contain the tools that allowed Dirac to develop his version of quantum mechanics. This was possible because Whittaker turned to continental authors like Jacobi, Lie and Poincaré, who influenced both the subject matter and the style of the book. The great degree of generality of Whittaker's approach has also been one of the keys to the remarkable longevity of his book. Indeed, Routh's treatises, even the more advanced ones, look very dated when compared with most books on mechanics written since 1950. This applies both to Routh's choice of topics and to his presentation. The same cannot be said about *Analytical Dynamics*. Except for the absence of any tools of global analysis (manifolds, tangent and cotangent bundle), the choice of topics is very close to that of a modern text like Arnold (1989).

Of course, like all human artifacts, Whittaker's book is limited, in many ways, by the practices of the time and place at which it was written; in this case, early twentieth century Cambridge. For example, convergence questions are usually handled in a very cavalier fashion, and no clear distinction is made between results that hold globally and those that hold only locally. Actually, some limitations were clear even to the reviewers of the first edition, as is the case with G. H. Bryan's criticism of the artificial nature of many of the exercises proposed by Whittaker. Chosen from college examinations and the mathematical tripos, they were a remnant of nineteenth century Cambridge and would soon be relegated to the past. Paradoxically, some of the characteristics that make the book seem dated, like the frequent use of elliptic functions, may be one of the reasons why it has remained useful to this day. As one reviewer Sneddon (1980) put it, *Analytical Dynamics* "crystallized" the "theoretical work of the century and more after the death of Lagrange" and remains a rich repository of results that otherwise would have been scattered in hard to find historical periodicals published in several different languages.

One final question remains: what were the qualities that allowed Whittaker to write a book—actually two, for *Modern Analysis* has proved to be as longevous as *Analytical Dynamics*—that remains useful to mathematicians working in several different areas, more than one hundred years after it was written? The analysis in the previous sections suggests that this was in good measure due to Whittaker's great knowledge of the literature and to his ability to organize this knowledge in a systematic way. Moreover, his reading was not limited to contemporaneous works, it also encompassed the classics of the 18th and 19th centuries. The most notorious result of Whittaker's familiarity with the classical mathematical and physical literature was his *A history of the theories of aether and electricity* Whittaker (1910), which he revised in 1951 Whittaker (1951) and to which he added a new volume Whittaker (1953) two years later, to cover the period from 1900 to 1926. However, Whittaker's scholarship is present in all of his books, markedly so in *Analytical Dynamics*, where it is most evident in his choice of topics and in the huge number of very precise citations of books and papers dating back to the eighteenth century that appear in the footnotes of most pages of all editions after the second. It seems to me that the success of Whittaker's books owes much to the fact that he was one of that rare breed, a scientist who is also a scholar, of which D'Arcy Thompson is probably the best known representative. People whose research may not have been exceptional, but whose great knowledge of the literature, including historical works, allowed them to "crystallize" in their books a vision of a whole subject that would greatly influence later generations.

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