The dramatic episode of Sundman

Journal Article

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Abstract
In 1912 the Finnish mathematical astronomer Karl Sundman published a remarkable solution to the three-body problem, of a type which mathematicians such as Poincaré had believed impossible to achieve. Although lauded at the time, the result dimmed from view as the twentieth century progressed and its significance was often overlooked. This article traces Sundman’s career and the path to his achievement, bringing to light the involvement of Ernst Lindelöf and Gösta Mittag-Leffler in Sundman’s research and professional development, and including an examination of the reception over time of Sundman’s result. A broader perspective on Sundman’s research is provided by short discussions of two of Sundman’s later papers: his contribution to Klein’s *Encyklopädie* and his design for a calculating machine for astronomy.

Zusammenfassung
Im Jahre 1912 veröffentlichte der finnische mathematische Astronom, Karl Sundman eine bemerkenswerte Lösung des Dreikörperproblems, von einer Art, wie sie Mathematiker wie Poincaré für unmöglich gehalten hatten. Obwohl Sundmans Ergebnis in seiner Zeit gefeiert wurde, verschwand es im Laufe des Jahrhunderts allmählich aus dem wissenschaftlichen Bewusstsein, und seine Bedeutung wurde meistens unterschätzt. Der Artikel spürt der Karriere Sundmans nach bis hin zu seiner großen Leistung. Er zeigt den Einfluss von Ernst Lindelöf and Gösta Mittag-Leffler auf Sundmans Forschung und berufliche Entwicklung. Er beschreibt auch die wechselnde Rezeption des Ergebnisses. Zwei spätere Publikationen Sundmans werden in einem Anhang beschrieben.

1. Introduction

In 1914 Sir George Greenhill,[1] in his Presidential address to the Mathematical Association of Great Britain, used ‘the dramatic episode of Sundman’ as a rallying call to young mathematicians to brave the frontiers of their subject [Greenhill, 1914, 259].[2] Greenhill was just one among a number of mathematicians who, in the second decade of the twentieth century, drew attention to Karl Sundman’s analytical solution to the three-body problem [Sundman, 1912]. Some, such as Greenhill, simply referred to the main result, while others, such as Jacques Hadamard [1915] and Hugo von Zeipel [1917], explored the mathematics itself.

Although Greenhill was no stranger to hyperbole, in this instance his words were not misplaced. Sundman’s success did indeed have a dramatic quality to it. The three-body problem was one of the most famous mathematical problems of the day, and one that had

[1] George Greenhill was Professor of Mathematics at the Royal Military Academy Woolwich from 1876 to 1908, and was best known for his work on elliptic functions, ballistics, and aeronautics.
[2] Greenhill’s comments will be discussed further in Section 7.

exercised many of the greatest mathematical minds for over two hundred years. That a solution should have been provided by a little-known Finnish mathematical astronomer working in comparative isolation in Helsinki (Helsingfors) was not in the script. Furthermore, Sundman's solution was in a form which mathematicians of the calibre of Poincaré had believed impossible to achieve. No wonder then that the mathematical world was excited and responded. The drama also had a prologue, but one that had passed virtually unnoticed until after the main act had played out. The paper which drew Greenhill's remarks appeared in *Acta Mathematica* in 1912, but it was not Sundman's first publication of his solution. That epithet goes to two papers Sundman published in *Acta Societatis Scientiarum Fennicae* in 1907 and 1909, but which elicited little response. It was not until the solution appeared in its final form in *Acta Mathematica* that the plaudits began.

Although Sundman's achievement was lauded by his contemporaries, as the century progressed, the responses became increasingly mixed and references to it began to dry up. Furthermore, it is evident that contemporary commentators on his work had little knowledge of Sundman or of the circumstances in which he produced and published his results.

However, in the last decade or so, renewed interest in Poincaré's work on the three-body problem (due to its connection with the development of chaos theory) has meant that the significance of Sundman's solution has once again been recognised [Barrow-Green, 1997, 187–192; Lehti, 2001b; Henkel, 2001; Saari, 2005, Chap. 4]. But these studies concentrate on Sundman's mathematics and they leave a number of questions concerning the context of his achievements unresolved. Namely, why and how did Sundman, a virtually unknown mathematical astronomer (as opposed to mathematician), working outside the mathematical mainstream, manage to solve the three-body problem in a way that was thought to be impossible? Why, given the celebrity status of the problem, did Sundman first publish his solution in a journal with low mathematical visibility? What did Sundman do for the rest of his career, and why was he largely forgotten during the course of most of the 20th century?

In this paper I trace Sundman's career and the path to the publication of his paper in *Acta Mathematica*, beginning with a discussion of the Finnish mathematical environment within which Sundman worked, since his location, both scientifically and physically, was a significant factor with respect in the events in his professional life. An outline of his career is followed by a detailed consideration of the period he spent abroad just prior to his first publication on the three-body problem. Having explained the significance and difficulty of the three-body problem, I then discuss the development of Sundman's interest in it and briefly describe his solution, before examining in detail the publication of his papers and bringing to light the key roles played by Ernst Lindelöf and Gösta Mittag-Leffler in his work. This is followed by an examination of the reactions to Sundman's solution, both on publication and subsequently, which shows how attitudes towards Sundman and his work, which varied over time, reflect the differing concerns of mathematicians and astronomers. A broader perspective on Sundman's research is provided by short discussions of two of Sundman's later papers—his contribution to

Klein's *Encyklopädie der Mathematischen Wissenschaften* and his design for a calculating machine for astronomy—both of which were published in 1915 and each of which presents a complement to his work on the three-body problem.

2. Mathematics in Finland in the nineteenth and early twentieth centuries

In 1828 Finland's first University, the Imperial Alexander University in Finland, was opened as the continuation of the Academy of Turku (Åbo) which had been destroyed by the great fire of Turku the previous year.[3] The effect of the fire was so devastating—three-quarters of the city was destroyed—that it provided the ideal excuse to implement a plan already under discussion, namely to move the University some 160 kilometres to the country's new capital Helsinki.[4] With Finland's independence from Russia which occurred at the end of 1917—Finland had been an autonomous Grand Duchy of Russia since 1809—the 'Imperial' was dropped from the University's name and then, in 1919, the University was officially renamed the University of Helsinki. The Imperial Alexander University was Finland's only university until 1908, when the Helsinki Technology Institute, founded in 1849, received university status and was renamed the University of Technology.

During the 19th century and the first decades of the 20th, the mathematics department at the University of Helsinki consisted of one professor and one or two docents, although at times the professor taught alone.[5] From 1857 to 1874 the chair was occupied by Lorenz Lindelöf (1827–1908), a graduate of the University. Lindelöf's best known mathematical work was in the calculus of variations, but he also published substantially on astronomy and was twice acting Professor of Astronomy. An influential figure—a former rector of the University and head of the Societas Scientiarum Fennica (Finnish Society of Sciences)—Lindelöf left the University to take up an important position as head of the new School Administration.[6]

In 1877 the Swedish mathematician Gösta Mittag-Leffler (1846–1927) was appointed as Lindelöf's successor. The appointment process had taken almost 18 months to complete. Although in terms of mathematical ability, Mittag-Leffler had been ranked by the Faculty as the best candidate for the position—the ranking being based on a report by Lindelöf in which Lindelöf stressed that it was scientific competence that mattered the most—the fact that Mittag-Leffler was a foreigner[7] and that he had had to obtain special exemption from

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[3] I am grateful to Olli Lehto for providing me with many of the historical details in this section.

[4] This plan was in line with the Russian policy of weakening the ties of Finland to Sweden, one aspect of which was to move the important institutions closer to St Petersburg and away from Sweden.

[5] For a detailed study of the history of mathematics in Finland in the period 1828–1928, see [Elfving, 1981].

[6] For further information on Lorenz Lindelöf see Olli Lehto's recently published double biography (in Finnish) of Lorenz and Ernst Lindelöf [Lehto, 2008].

[7] Rather ironically, in 1875 Mittag-Leffler had turned down the offer of 'an extraordinary chair' in Berlin, arranged through Weierstrass's intervention, because, as he said, 'the atmosphere was unbearable for a foreigner. It was shortly after Germany's victorious war with France, and German arrogance had reached

Finnish language requirements, had counted against him.[8] Several members of the University, and students too, felt strongly that the position should, if possible, be awarded to a Finnish national, and since the second best candidate, Ernst Bonsdorff (1842–1936), was an able Finnish mathematician, they felt that the chair should go to him. While Mittag-Leffler came with testimonials from Hermite, Weierstrass, Kronecker and Schering, those in favour of Bonsdorff, who from the outset had to assume a defensive position, drew attention to the fact that Bonsdorff 'occupied by a demanding office and living far from scientific exchange and libraries had been able to devote only a short time to studies abroad' in contrast to the advantages enjoyed by Mittag-Leffler [Elfving, 1981, 75]. Feelings ran high and when the final vote was cast in the Senate to confirm the ranking of the candidates, Mittag-Leffler retained his place at the top of the rankings but only by the narrowest of margins.

The provincial attitude demonstrated by those who opposed Mittag-Leffler's appointment on national (rather than academic) grounds is not surprising. Helsinki was a small university located far away from the mathematical heartlands of France and Germany.[9] Foreign travel and international communication were difficult, and the political situation was bound to intensify feelings of nationalism. Those members of Faculty, who, like Bonsdorff, had had little chance to travel abroad were not only poorly placed to judge the value of bringing in talent from outside but also naturally more inclined to be protective of talent that was home-grown.

While in Helsinki, Mittag-Leffler published vigorously both inside and outside Finland—he generally wrote in Swedish or French—and nurtured a number of promising young doctoral students, including Hjalmar Mellin, whom he encouraged to study in Berlin with Weierstrass, thereby helping to raise the visibility of Finnish mathematics within Scandinavia and beyond. Although Mittag-Leffler left Helsinki after 4½ years, having been tempted back to Sweden by the chair of mathematics at the newly founded Stockholm Högskola, he developed an attachment to Finland and to Finnish mathematicians, in particular the Lindelöf family, which he maintained for the rest of his career.[10]

In 1882, the year after his return to Sweden, Mittag-Leffler embarked on a project which ultimately would turn out to be his most important contribution to international mathematical life, and one which would also have a significant effect on Scandinavian

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its peak. Foreigners were treated with supercilious condescension, and one heard nothing but Der grosse Kaiser, Bismarck, and Moltke.' The quotation is given in [Elfving, 1981, 74] but without citation.

[8] On the question of language, a major reason for the rise of the Finnish language (over Swedish) was the order of Alexander II to establish secondary schools. It was not in the political interest of Russia that the Finnish elite were Swedish-speaking. From the middle of the 19th century until 1937, the language issue divided the Finnish people, and gradually the University became the main arena for the fight.

[9] The question of science in the European periphery is cogently discussed in [Gavroglu et al, 2008]. For discussions of early 20th-century Scandinavian science, see [Siegmund-Schultze and Sørensen, 2006].

[10] There was also a personal side to Mittag-Leffler's attachment to Finland. It was while he was working in Helsinki that he met and became engaged to his future wife, Signe Lindefors, a Finnish woman who later inherited a substantial fortune. See [Stubhaug, 2007] for further details about Mittag-Leffler's domestic life.

mathematics: the founding of the journal *Acta Mathematica*.[11] From the outset Mittag-Leffler intended the journal to be international—the languages of publication were stated to be French and German, or in exceptional cases English or Latin (in fact no papers were ever published in Latin)—and to provide a showcase for Scandinavian mathematics. And he succeeded immediately in both respects. Early issues of the journal contained articles by Poincaré and Cantor alongside ones by the Finnish mathematical astronomer Hugo Gyldén (who had been appointed as head of the Stockholm Observatory in 1871) and the young Mellin, as well as Mittag-Leffler himself.[12] Each of the Scandinavian countries was represented on the editorial board, with Lorenz Lindelöf the first to take on the mantle for Finland.

Until the appearance of *Acta Mathematica*, Finnish mathematicians rarely published outside Finland unless they had had the opportunity to travel and to establish international contacts. For those mathematicians who published in Finland, the journal of choice was usually the *Acta Societatis Scientarum Fennicae* (Proceedings of the Finnish Society of Sciences). Although this journal circulated beyond Finland, it appears not to have been particularly well known to mathematicians outside Scandinavia, probably due to the fact that it contained articles from a mixture of disciplines and was not a mathematics journal per se.

A further way in which Mittag-Leffler helped to raise awareness of Scandinavian mathematics was through the Scandinavian (later Nordic) Congresses of Mathematicians, the first of which was held at his instigation and under his guiding hand in Stockholm in 1909. Again politics comes into the picture, the backdrop being the dissolution in 1905 of the Union of Sweden and Norway, after years of increasing dissatisfaction, particularly from the Norwegian side. The Congresses both provided an opportunity for scientific cooperation between the four Scandinavian nations and help to heal differences between them [Sørensen, 2006]. By publishing the proceedings in French, the organizers ensured that the content was accessible to an international audience.

When the competition opened for the successor to Mittag-Leffler's chair, the leading contenders were Edvard Neovius (1851–1917),[13] a geometry docent at the University and nephew by marriage of Lorenz Lindelöf, and, Bonsdorff. Although, as in the previous contest, Bonsdorff was the somewhat weaker candidate, there was once again a real possibility that nationalism might prevail over mathematical ability: Bonsdorff was a member of the Fennoman party, the most visible element of the Finnish national movement, whereas Neovius was not [Elfving, 1981, 112]. In the event, Bonsdorff withdrew his application before the final disputations took place, and Neovius took up the position in 1883.

[11] For a detailed discussion of Mittag-Leffler's role in founding *Acta Mathematica*, see [Barrow-Green, 2002].

[12] By 1918, 21 papers by Finnish mathematicians had been published in *Acta Mathematica* [Elfving, 1981, 79].

[13] In 1906 Edvard Neovius' two brothers, Lars and Otto, as a mark of their support for Finnish independence, changed their surname from Neovius to the Finnish Nevanlinna. Otto was the father of Rolf Nevanlinna, the famous founder of value distribution theory. A family tree of the Neovius-Nevanlinna family is given in [Elfving, 1981, 106].

Neovius had studied at the ETH (Eidgenössische Technische Hochschule) in Zurich where he had become friends with his teacher Hermann Amandus Schwarz. Following Schwarz, Neovius began work on minimal surfaces and it was the topic that absorbed him throughout his mathematical career. But it was a career cut short by political activity. At the end of the 1890s Finland's autonomy was coming under threat, and in 1900 Neovius, still not a Fennoman but a sympathiser to the cause and increasingly involved in political activities, left the chair to join the Finnish Cabinet as head of the Finance Department, where he remained until 1905. With the chair having already been reassigned, he moved to Denmark, the home of his wife's family.

In the same year that Neovius was awarded the chair of mathematics, the chair of astronomy and directorship of the University Observatory was awarded to Anders Donner. Donner, a mathematics graduate of the University, had undertaken postgraduate study in astronomy, traditionally a strong field in Nordic countries, and mathematics. The latter he had done with Weierstrass in Berlin, writing his doctoral thesis on elliptic function theory, which he had defended in Helsinki in 1879. Before gaining the professorship in 1883, he had assisted Gyldén at the Stockholm Observatory working on perturbation theory as well as helping with observations [Elfving, 1981, 88–89]. That Donner should have had such a strong mathematical training was no accident. Research at the University Observatory was largely theoretical, as was typically the case for small observatories with limited resources for observations, and strong links existed between astronomy and mathematics at the University, as exemplified by Lorenz Lindelöf who had held both chairs and under whom Donner had studied as an undergraduate. After his return to Helsinki, Donner devoted himself almost entirely to practical astronomy, retaining the chair until 1915.

Meanwhile in the mathematics department, Ernst Lindelöf (1870–1946), son of Lorenz Lindelöf, had been appointed in succession to Neovius.[14] Lindelöf had gained his doctorate, for a thesis on Lie's theory of transformation groups, at the University in 1893 and in the ensuing decade he spent several periods studying abroad, first in Stockholm, then in Paris, and later in Göttingen, which enabled him to develop many important contacts. These contacts, among the most significant of whom were Mittag-Leffler, Paul Painlevé and Emil Borel, would be important to Lindelöf, both from the perspective of his own research on the theory of functions and for the help it would enable him to give to students and colleagues with whom he was working. In 1895 he had become a docent in the department and was promoted to associate professor in 1902, before being awarded the full professorship in 1903. In 1907 he became a member of the editorial board of *Acta Mathematica*.

Lindelöf's research, almost all of which was done by 1915, was mainly concerned with analytic function theory, although at the beginning of his career he did important work on existence proofs for differential equations.[15] With regard to analytic functions, his principal interest lay in the theory of entire functions, where he notably gave a complete

[14] For further information on Lindelöf see [Elfving, 1981, 133–152; Myrberg, 1947; Oettel, 1973].
[15] For a bibliography of Lindelöf's work, see [Myrberg, 1947, II–IV].

treatment to the question of interdependency between the growth of the function and the coefficients of the Taylor expansion. He also worked on the behaviour of analytic functions in the neighbourhood of a singular point, writing an important paper together with the Swedish mathematician Edvard Phragmén in which they developed what is now known as the Phragmén-Lindelöf Principle [Phragmén and Lindelöf, 1908]. He is, however, best known for his work on analytic continuation, in which, by appealing to summation formulas, he achieved important results and which are published in his monograph *Le calcul des résidus* [1905]. His later papers deal with conformal mapping. A dedicated and thorough teacher, Lindelöf spent most of the second half of his career teaching and developing his lecture courses into a series of text books that formed the basis of mathematical literature in the Finnish language [Myrberg, 1947, II].

Although a patriot, Lindelöf tended not to get involved in political matters. But he lived through a precarious period of his nation's history and, as Elfving [1981, 135–136] has described, not all effects of increasing Russification passed him by unheeded. But whatever trouble had gone on in Finland before, it was as little when compared to the civil war which took place in the first half of 1918 in the aftermath of the declaration of independence from Russia. Lindelöf's letters to Mittag-Leffler describe the worsening situation:

> *In February 1918 (when Lindelöf stayed as a guest of Mittag-Leffler in Djursholm while the latter was himself in Tällberg): "Times are getting more and more awful in Finland,. There is a great danger that Helsingfors will be altogether destroyed by the Russians …" In June 1918: "The older people are sound and safe, but of my younger pupils, at least four have been killed in battle or murdered … Food may have been scarce in Sweden, but here there is starvation for everyone, and famine for the poorest ones."* [Elfving, 1981, 134].

Like many of his class, Lindelöf was drawn to the conservative German-supported "White" side of the conflict.[16] Later he would be vocal in his opposition to the exclusion of German mathematicians from the International Congress of Mathematicians held in Toronto in 1924, and successfully spearheaded a campaign for Finnish mathematicians to boycott the Congress in protest [Lehto, 1998, 37].

[16] There were fervent conflicts and the terror was on both sides, with the losing socialist Russian-supported "Red" side taking the brunt of the casualties. After the victory of the Whites, Finland came under the influence of Germany for a short while, until the latter's defeat in the First World War, after which it emerged as an independent democratic republic.

3. Overview of Sundman's career

Figure 1. Karl Sundman. Reproduced by permission of the University of Helsinki Observatory

Karl Sundman (see Fig.1), the son of a customs officer, was born in 1873 in the small coastal town of Kaskö [Järnefelt, 1953, 3].[17] Self-taught, he passed his baccalauréat in 1893 and entered the University of Helsinki. In 1897 he moved to the Pulkovo Observatory of the Russian Academy of Sciences near St Petersburg,[18] to work as an assistant to Oskar Backlund, the Director of the Observatory, and in particular to help Backlund with the completion of Hugo Gyldén's monumental research on the motion of the large planets.[19] Sundman returned to Helsinki in 1899, and two years later his dissertation on perturbations of small planets [Sundman, 1901] was published.[20] In 1902 he was put in charge of the astronomy course at the university, and the following year he was awarded a Rosenberg grant[21] to travel to France and Germany. (Sundman's travels are discussed in the following section). He returned to Helsinki in 1906, and in 1907 was promoted to associate professor of astronomy, the promotion indicating that he had been singled out as a potential successor to Anders Donner, the incumbent professor and Director of the Observatory. 1907 was also the year the first of his papers on the three-body problem was published.

In 1911 Donner became Rector of the University and Sundman took over the chair of astronomy in his absence. When Donner became acting Chancellor in 1915, the chair and directorship of the Observatory became vacant and Sundman was elected to both positions. But the election was not without controversy. The recommendations from the foreign referees[22] were at variance and when Sundman was chosen, the other two candidates, Ilmari Bonsdorff (son of Ernst) and Ragnar Furuhjelm, both observational astronomers, complained about the decision [Linnaluoto, Markkanen, and Poutanen, 1984]. The point at issue was whether the main criteria for election should be ability in theoretical astronomy (in which Sundman was judged to have a clear edge on his rivals) or leadership in practical astronomy. Diplomatically the election board found a way to satisfy the complainants without retracting their decision. A personal chair was created for Furuhjelm and Bonsdorff was appointed to head the newly established Geodetic Institute. Sundman took up his positions in 1918 and held them until his retirement in 1941.

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[17] According to [Järnefelt, 1953, 3] Sundman's parents thought their son should be trained as a fisherman and were concerned for his future when he proved unsuited for such an occupation.

[18] Recall that at that time Finland was still a grand duchy under Russian sovereignty, thus Sundman's move to Pulkovo was quite a natural one.

[19] Gyldén had died in 1896 leaving the second volume of his *Traité analytique des orbites absolues des huit planètes principales* unfinished. For further information on Sundman's work as an astronomer see [Lehti, 2001a].

[20] The Swedish astronomer Hugo von Zeipel, in his article on perturbation theory for Klein's *Encyklopädie*, described Sundman's method for calculating the coefficients of certain types of perturbation function [Sundman, 1901, 24-36] as "outstanding" (vorzüglich) [Von Zeipel, 1912, 584].

[21] The Rosenberg travel grant fund was set up by H.F. Antell (1847-1893), the illegitimate son of Herman Rosenberg, governor of Vaasa County, Finland, in memory of his father. I am grateful to Tapio Markkanen of the University of Helsinki for supplying me with this information.

[22] The foreign referees were the Dutch astronomers J.C. Kapteyn and H.G. van de Sande Bakhuyzen, and the Danish astronomer Elis Strömberg.

Sundman was a frequent speaker at the Finnish Mathematical Society where in 1906 and 1909 he reported on his work on the three-body problem.[23] Apart from topics connected with mathematical astronomy, he spoke also on the zeros of the ζ function and the work of Fermat [Elfving, 1981, 191]. He died in 1949.

4. Sundman's travels during 1903-1906

In his application for the Rosenberg grant Sundman stated that he wanted time to concentrate fully on his research in celestial mechanics without the distraction of other work-related obligations.[24] The title of his proposed research was "The investigation of the motion of small planets and their moons, especially Saturn's moons, in order to treat the case of near commensurability between the mean motions of the perturbing and perturbed bodies,"[25] a topic closely related to the subject of his thesis. As he described in the application, his interest in the topic had been stimulated by Gyldén's work on the stability of planetary systems. He explained,

> *Gyldén's work, more than that of anyone else, has shed light on this problem, and made it one of the central problems of celestial mechanics. This is because the correctness of astronomy's conclusions concerning the past and future of the solar system depend on it. In order to find a solution to the question of stability one has to have an expression [in power series] for the planets' coordinates which is valid for all time. …*
>
> *If the mean motions of the perturbing and the perturbed bodies are approximately commensurable the trajectory develops certain characteristic properties. The closer the mean motions are to being commensurable and the greater the mass of the perturbing body, the greater the perturbation and the more difficult it is to find a satisfactory solution.*
>
> *Gyldén believed that the whole matter was based on the wrong method of integration. He developed a new method of integration which he called "horistic" and through the application of which infinitely small divisors could be avoided. However, this method has been contested by Poincaré, arguably the leading mathematician of our time, who has also founded an astronomical school.*[26] *There is now a struggle for dominance in celestial mechanics between the approach of Gyldén and that of Poincaré.*

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[23] Abstracts books, Finnish Mathematical Society. The meetings of the Society in those days were combined colloquium/research seminars. The talks were given by the few faculty or the advanced students of the University of Helsinki (and, to a lesser extent, the Technical University). According to the abstracts books, Sundman gave nine talks during the period 1895-1916. I am grateful to Hans-Olav Tylli of the Mathematics Department of the University of Helsinki for supplying me with this information.
[24] Sundman's application for the Rosenberg grant, 4th December 1902, Central Archives of the University of Helsinki, Keskushallinnon Arkisto, Saapuneet Anomukset 1902, Signum EC 97.
[25] For any planet orbiting the Sun, its mean motion is taken to be its mean angular velocity of revolution. "[T]he case of near commensurability between mean motions" refers to the phenomena of "small divisors", a well-known problem in celestial mechanics, an example being the long-period inequality of Jupiter and Saturn, whose mean motions are approximately in the ratio 5:2. See [Barrow-Green, 1997, 17-18].
[26] The usual methods of solving problems in celestial mechanics result in solutions with certain terms which have coefficients with infinitely small divisors. Gyldén tried to show that if when using traditional methods a more exact calculation was made, then these terms will not arise and instead there will be "horistic" terms in which the coefficients have very small, but nevertheless non-vanishing, divisors. The

There is much uncertainty about the true nature of the motion of the small planets. In the case of very close commensurability between the mean motions of the perturbing and perturbed bodies, the formulae cease to be valid and one could easily conclude that the motion has to be unstable.[27]

He outlined some specific problems connected with the calculation of the mean motions of the small planets which, he believed, would provide results not "without significance for the stability problem and, in any case, would provide a good attack on the subject". Having observed that trigonometric functions, the ones traditionally used by mathematical astronomers, were still the best option when it came to representing planetary trajectories—he noted that Gyldén had tried unsuccessfully to use elliptic functions—, he suggested it might be profitable to "develop the given trigonometric forms for the coordinates." He continued,

With this planned investigation it is most important, from a mathematical point of view, to be as well equipped as possible. In particular, one must be in a position to follow the leads offered by function theory as well as those offered by the theory of differential equations.

The reference to function theory (and the implication that he was interested in it) would not have been lost on the University Committee evaluating the grant application, since it was a subject of particular interest to one of its members, Sundman's colleague Ernst Lindelöf.

Having spent most of his career in Helsinki, Sundman had had little opportunity for personal contact with mathematicians and astronomers outside Finland. By going to France and Germany—he proposed dividing his time equally between each—he would be able to see at first hand just how the "struggle for dominance" within celestial mechanics was playing out. In France, he wished to go only to Paris since all the people he hoped to meet—Poincaré, Paul Appell, Paul Painlevé, and the astronomer Octave Callandreau—were there. In Germany, he had in mind a more extensive tour, with most of his time being spent in Munich and Göttingen. In Munich the attraction was Hugo von Seeliger, the renowned head of the Munich Observatory, while in Göttingen it was the mathematicians, although he mentioned no names. His intended itinerary also included Berlin, in particular the Astronomical Calculation Institute, and, if time allowed, Leipzig and Kiel. In drawing up his plans, he would have had an ideal source of advice in Lindelöf who had spent time in both Paris and Göttingen.

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point was that if he was right, then these terms could be used to prove the convergence of the series. As Poincaré was to show, Gyldén's conclusion was false. Poincaré's analysis and criticism of Gyldén's horistic methods can be traced through a series of notes and papers which began with a note in the *Comptes Rendus* [1901] and culminated in a memoir in *Acta Mathematica* [1905]. See [Barrow-Green, 1997, 164-166].

[27] Sundman's original travel plan (in Swedish) for the Rosenberg stipend. Helsingin Yliopisto, Keskushallinnon Arkisto, Saapuneet Anomukset 1902, Signum EC 97. I am indebted to Reinhard Siegmund-Schultze for providing translations from Swedish for this document and the Lindelöf-Mittag-Leffler correspondence.

Although Sundman met many mathematicians and astronomers on his travels, the only names to appear in the travel report he submitted on his return to Finland are those of Poincaré and (the deceased) Gyldén, and then only in the context of their publications.[28] However, his correspondence with Donner, recently studied by Carl Källman, does provide concrete information about his meetings and also makes it possible to recreate his actual itinerary: Göttingen–Paris–Munich–Paris–Munich–Paris–Göttingen–Padua–Göttingen–Leipzig–Berlin.[29]

On Christmas Day 1903, Sundman wrote from Paris to tell Donner that he had met Callandreau who had introduced him to Poincaré and Emile Picard. He also mentioned that he had attended Poincaré's lectures on perturbation theory and had visited Poincaré to discuss the three-body problem, which for his part meant "simple cases with possible singular events", and had found that the two of them shared similar views.[30] However, within a few weeks his opinion had changed. Writing from Munich, he now told Donner that "For him [Poincaré] 'it was of less importance whether or not the formulae could be calculated within a finite (möjlig) time. For myself it has always been a necessary condition in astronomy to acknowledge only those formulas which can be calculated".[31]

On 13 April 1904, Sundman was elected as a member of the *Société Mathématique de France*, having been proposed for membership by Painlevé and Emile Borel;[32] the connection with Borel no doubt coming through Lindelöf since the two were close friends [Elfving, 1981, 137].

At the end of December 1904, Sundman reported to Donner that he would shortly be leaving Munich to go to Paris in order to discuss perturbation theory with Poincaré. And it is clear from a letter written by Poincaré to Mittag-Leffler that Poincaré and Sundman did meet in Paris at the end of February 1905:

> *How far on are you with the publication of my memoir on Gyldén? M. Sundman]n[of Helsingfors asked me about it yesterday because it will help him with his article* [on planetary theory] *for Klein's Encylopaedia.*[33]

In Germany, Sundman visited Göttingen, Munich, Leipzig and Berlin, although he mentioned only Göttingen in his travel report. In Göttingen he met Karl Schwarzschild

[28] I am grateful to Tapio Markkanen for providing me with this information. Sundman's travel report is in the Central Archives of the University of Helsinki.
[29] I am grateful to Carl Källman for generously sharing the contents of the Sundman-Donner correspondence with me. In all further references to the correspondence I benefited from his advice. The correspondence is lodged at the University of Helsinki Observatory Archives.
[30] Sundman to Donner, 25 December 1903, University of Helsinki Observatory Archives.
[31] Sundman to Donner, 2 February 1904, University of Helsinki Observatory Archives.
[32] Unusually Sundman was elected by a majority of members rather than unanimously, although this may have been because he was not well known. I am grateful to Hélène Gispert for providing me with this information.
[33] Original: "Où en est la publication de mon mémoire sur Gyldén? M. Sundman]n[de Helsingfors me le demandait hier parce que cela lui faciliterait la rédaction de son article pour l'Encylopédie de Klein." Poincaré to Mittag-Leffler, 1st March 1905. See [Nabonnand, 1999, 330-331]. Poincaré's memoir on Gyldén is [Poincaré, 1905]; Sundman's article on planetary theory is [Sundman, 1915a].

who invited him to contribute to the *Encyklopädie* volume on astronomy,[34] an invitation which eventually resulted in [Sundman 1915a] (discussed in Section 9),[35] and in Munich he had a very fruitful visit with von Seeliger.[36] On his visit to Leipzig he met with Heinrich Bruns, the professor of astronomy and Director of the Observatory, who had done important work on the three-body problem [Bruns, 1887], and whom he described as having "a clear view of the difficulties in celestial mechanics".[37] As far as his trip to Berlin was concerned, Sundman told Donner that he had spent time working on the three-body problem but did not mention any names.[38]

Sundman also made at least one visit not on his original plan. At the end of April 1905 he went to Padua to see Tullio Levi-Civita,[39] the two week stay presumably prompted by Levi-Civita's recently published paper on the three-body problem [Levi-Civita, 1903].

5. The significance and difficulty of the three-body problem

The three-body problem is seductively simple to state. Given three bodies interacting gravitationally, with known positions and velocities, determine their subsequent motion. Although the problem is necessarily an idealised one—the bodies are ideal Newtonian centres of force—it represents a good approximation to actual physical problems, the classical example being the motion of the Sun-Earth-Moon system. The problem has long captivated mathematicians, and not least because of its connection with the question of the stability of the solar system. The connection arises because if the solar system is considered to be made up of the Sun and the eight planets, then it can be considered as a nine-body system.[40] A solution to the three-body problem would thus provide an important step towards a solution to the stability problem. In 1904 the British mathematician E.T. Whittaker described the problem as the "most celebrated of all dynamical problems" and noted that more than 800 papers relating to the problem had been published since 1750 [Whittaker, 1904, 327].[41]

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[34] Schwarzschild was a co-editor of the astronomy volume together with Samuel Oppenheim.

[35] Sundman to Donner, 10th June 1906, University of Helsinki Observatory Archives. Report (in Swedish) of the Rosenberg Committee, A. Donner, E. Lindelöf, 10.April 1905. Helsingin Yliopisto, Matemaattis-Luonnontieteellisen, Osaston Arkisto. Pöytirjat 1905, Signum Ca 11.

[36] See [Carpelan and Tudeer, 1925, 942], a biographical entry on Sundman written for the University of Helsinki while Sundman was in the University's employment; and 'Report of the Rosenberg Committee' A. Donner, E. Lindelöf, 10.April 1905. Helsingin Yliopisto, Matemaattis-Luonnontieteellisen, Osaston Arkisto. Pöytirjat 1905, Signum Ca 11.

[37] Sundman to Donner, 27th December, 1905, University of Helsinki Observatory Archives.

[38] Sundman to Donner, 10th June 1906, University of Helsinki Observatory Archives.

[39] Sundman to Donner, 26th May 1905, University of Helsinki Observatory Archives.

[40] The solar system is of course made up of the sun and all its attendant bodies—meteors, comets and satellites as well as the major and minor planets—but in the context of the stability problem it is the planets that dominate.

[41] For a detailed history of the three-body problem see, for example, [Gautier, 1817; Whittaker, 1899; Lovett, 1912; Marcolongo, 1919]. Whittaker's report describes the situation from 1868 to 1898, while that of Lovett is concerned with the following decade. Marcolongo's book is a full account and deserves to be better known.

Newton himself had tackled the problem but the difficulties he encountered, in marked contrast to his success with the two-body problem,[42] led him to declare that a solution "exceeds, if I am not mistaken, the force of any human mind" [Newton, 1684].[43] Similar sentiments were still being expressed some 200 years later. In 1890 Poincaré, in the introduction to his celebrated memoir on the problem, wrote that he thought the possibility of finding a complete solution looked very remote:

> *Many other circumstances lead us to believe that a complete solution, if we can ever discover it, will require completely different and infinitely more complicated analytical tools than those we already possess.* [Poincaré, 1890, 6] [44]

While six years later Felix Tisserand, Poincaré's predecessor as professor of celestial mechanics at the Sorbonne, went so far as to proclaim that:

> *The rigorous solution of the three-body problem is no further advanced today than during the time of Lagrange, and one could say that it is manifestly impossible.* [Tisserand, 1896, 463] [45]

David Hilbert, in the introduction to his famous speech at the International Congress of Mathematicians in Paris in 1900, mentioned the problem in the same breath as Fermat's Last Theorem:

> *I remind you of the three-body problem. The fruitful methods and the far-reaching principles which Poincaré has brought into celestial mechanics and which are today recognised and applied in practical astronomy are due to the fact that he undertook to treat anew that difficult problem and to come nearer to a solution.*
>
> *The last two mentioned problems—that of Fermat and the three-body problem—seem to us almost like opposite poles—the former a free invention of pure reason, belonging to the region of abstract number theory, the latter forced upon us by astronomy and necessary for an understanding of the simplest fundamental phenomena of nature.* [Hilbert, 1901, 61]

Unlike the two-body problem, which can be solved fairly simply, the three-body problem is a complicated non-linear problem. It contains 18 variables—each body has three position and three velocity components—and the equations of motion (derived using Newton's Laws) are a set of nine second order differential equations. By choosing the appropriate units, these equations can be rewritten in Hamiltonian form as a set of 18 first order differential equations. The difficulty is that while there are 18 variables, there are only ten independent algebraic integrals: the 6 integrals of the motion of the centre of

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[42] Newton found a geometric solution to the two-body problem [Newton, 1687, Book I, Section XI]. The first complete solution to the two-body problem was given in 1710 by Johann Bernoulli. See [Wintner, 1947, 422].

[43] The English translation is from [Wilson, 1989, 253].

[44] Original : "Bien d'autres circonstances nous font prévoir que la solution complète, si jamais on peut la découvrir, exigera des instruments analytiques absolument différents de ceux que nous possédons et infiniment plus compliqués." For a discussion of Poincaré's memoir see [Barrow-Green, 1997].

[45] Original : "La solution rigoureuse du problème des trois corps n'est pas plus avancée aujourd'hui qu'à l'époque de Lagrange, et l'on peur dire qu'elle est manifestement impossible."

mass, the 3 integrals of angular momentum and the integral of energy.[46] These 10 integrals were known to Euler and Lagrange in the middle of the 18th century, and in 1887 Heinrich Bruns proved that there are no others [Bruns, 1887], a result sharpened by Poincaré in [1890, 259-265].[47] By the use of these 10 integrals, together with the "elimination of the time" and the "elimination of the nodes", the original system of order 18 can be reduced to one of order 6 but it can be reduced no further.[48] This means that in general there is no closed form solution to the set of differential equations that describe the problem.

Once mathematicians had realised that it was impossible to find a closed form solution, they turned to looking for a solution in terms of infinite series. Finding series that would work sufficiently well for some limited time span presented little trouble; indeed mathematical astronomers had been finding and using such series for decades. The difficulty was to find series that would work for any initial configuration and for any time span, no matter how long.

Since a complete solution to the problem has to take account of all possible motions of the bodies, it must also take account of collisions between them.[49] And since collisions are described by singularities in the differential equations, a way has to be found to eliminate the singularities. But trying to resolve this problem only gave rise to another one. It was obvious from the equations that a collision gave rise to a singularity, but what was not known was whether there could be any other type of singular behaviour.[50] It turned out that if this issue could be resolved then it was a theoretical possibility that a complete (analytical) solution to the problem could be found.

In 1886 Poincaré had indicated that if it was known in advance that the distance between any two of the three bodies always remained above a certain given bound then it would be possible to prove that the coordinates of the three bodies could be expanded in convergent series in powers of

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

[46] A standard way of decreasing the complexity of a system of differential equations is to find an algebraic integral for it: that is a quantity which will remain constant for any given solution and that can be expressed as an integral that gives rise to an algebraic dependence between the variables. This allows us to reduce the number of variables by expressing some of them in terms of others.

[47] For a discussion of Poincaré's contribution, see [Barrow-Green, 1997, 127-129].

[48] In 1772 Lagrange had shown that the system could be reduced to a system of order 7 [Lagrange, 1772]; the reduction to sixth order was made by Jacobi in [1843].

[49] A discussion, in measure-theoretic terms, of the likelihood of collisions is given in [Saari, 2005, 207-221].

[50] As Diacu [1993, 8] has described, the motivation for considering non-collision singularities was the possible appearance of large oscillations; for example, one body might oscillate between the other two getting progressively closer to a collision with each oscillation, but never actually colliding.

for all real values of the time *t*, and a positive constant α [Poincaré, 1886, 172].[51] In other words if it was known in advance that no collisions were going to occur then the problem could be solved. Despite the fact that this appears a remarkable result, it is of little practical use since for any given initial conditions, it is not possible to know in advance whether or not a collision will occur. And Poincaré himself did not believe that it would be possible to make much use of the method within celestial mechanics.

In 1895 Painlevé, while lecturing in Stockholm on the invitation of King Oscar II of Sweden and Norway,[52] proved that all singularities are collisions and showed that the problem could be solved using convergent power series fundamentally equivalent to Taylor series but only providing the initial conditions precluded the possibility of any kind of collision between the bodies [Painlevé, 1897].[53] The hunt was then on to find the initial conditions which corresponded to a collision. Painlevé himself conjectured that these initial conditions should satisfy two distinct analytic relations but was unable to make further progress.

Another step was taken by Levi-Civita who found Painlevé's analytic relation for a particular case of the three-body problem known as the *restricted three-body problem* [Levi-Civita, 1903, 1906]. (This is the case in which two of three bodies have a mass much larger than the mass of the third such that the motion of the two larger bodies is not influenced by the motion of the third body, while the motion of the third body is influenced by the motion of the other two. The task is to ascertain the motion of the third body.) Further progress was made by another Italian mathematician, Giulio Bisconcini, who, in the case of the general three-body problem, deduced two distinct analytic relations between the initial conditions which, when satisfied, proved that the motion was taking place along a singular trajectory, thereby indicating the existence of a collision in finite time [Bisconcini, 1906]. Bisconcini's result was important, but it did not provide a satisfactory solution to the problem. In the first place, his solution involved a complicated power series that was not at all easy to use. But rather more problematic was the fact that the series was only applicable when the interval of time between the start of the motion and the collision was sufficiently short, and he gave no conditions for this latter criterion. So there was still a need both to simplify the solution and to increase the range of its application. Moreover, Bisconcini had considered only the problem of a binary collision and not that of a triple collision.

6. The development of Sundman's interest in the three-body problem

Sundman's interest in the question of the stability of the solar system, declared in his application for the Rosenberg grant, provides the link to his work on the three-body

[51] Poincaré introduced this transformation for the first time in [Poincaré, 1882], one of his earliest publications.

[52] Although the lectures were sponsored by the King, a strong supporter of mathematics, the decision to choose Painlevé as lecturer was made by Mittag-Leffler. See [Barrow-Green, 1997, 183].

[53] Painlevé proved this result for the three-body case but his efforts to extend it to the *n*-body problem for $n > 3$ were unsuccessful. This led Painlevé to conjecture, correctly as it turned out, that pseudo-collisions (Painlevé's name for noncollision singularities) do exist for $n \geq 4$. For a discussion of pseudo-collisions in the *n*-body problem, see [Diacu, 1993].

problem. Recall that he articulated both the need to "have an expression for the planets' coordinates which is valid for all time" and the importance of being able to "follow the leads offered by function theory". Nevertheless, Sundman's attack on the three-body problem took him into realms of mathematics hitherto unseen in his previous work. What prompted him to become interested in resolving such a purely mathematical (as opposed to astronomical) question, albeit one directly related to his earlier research, and indeed where and from whom did he learn the relevant mathematics?

It seems that from the start of his career, Sundman had a broad knowledge of and interest in mathematics. His first publication [Sundman, 1897] concerned continued fractions, and it is reported that he displayed considerable ability as a mathematician while he was in Pulkovo working as Backlund's assistant completing Gyldén's magnum opus [Linnaluoto, Markkanen, and Poutanen, 1984]. Gyldén's work was notoriously obscure [Poincaré, 1905, 271] and editing it certainly required considerable mathematical dexterity. In addition, the year Sundman moved to Pulkovo, 1897, was the year of publication of Painlevé's Stockholm lectures [Painlevé, 1897], the text through which Painlevé's results on the three-body problem became widely known. Since Backlund was Swedish by birth and early in his career had worked as Gyldén's assistant at the Stockholm Observatory,[54] it is likely that he knew about Painlevé's lectures at the time of their delivery in 1895, and he would certainly have been aware of their publication. Thus Backlund, who was considered to be "a man of the first rank in both astronomy and mathematics" with "wonderful geniality and sound common sense" [Turner, 1918, xxiii, xxiv], was the right sort of person to provide Sundman with a fitting environment in which to nurture mathematical aspirations.

It is evident that during his time in Helsinki, both as a student from 1893 to 1897 and after his return from Pulkovo in 1899, Sundman benefited greatly from the presence of Ernst Lindelöf, who was three years his senior and with whom he became good friends. Not only was Lindelöf an accomplished mathematician, but also he was a clear expositor, and, significantly from the point of view of Sundman's work, Lindelöf was the leading exponent in Finland on complex function theory and it was function theoretic methods which led Sundman to his main results. Furthermore, Lindelöf's highly praised text on applications of the calculus of residues to function theory [Lindelöf, 1905][55] was published not long before Sundman's first paper on the three-body problem, indicating that Lindelöf was working in the field at the time when Sundman would have been formulating his ideas. In addition, Lindelöf was known to be generous with comments and suggestions to colleagues and doctoral students, while being modest about his own contribution.[56] Indeed Sundman, at the beginning of his papers on the three-body problem [Sundman, 1907; 1909; 1912], gives Lindelöf substantial praise for his editorial help, making the particular point in [Sundman, 1909] that Lindelöf had helped him

[54] See [Baker, 1917, 310].
[55] In reviewing Lindelöf's book, G.H. Hardy wrote, "For M. Lindelöf's *Calcul des Residus* I have nothing but praise. The applications of Cauchy's 'calculus' to the theory of functions, and in particular to the summation of series and the theory of analytic continuation, are of the most far-reaching character, and, so far as I know, no one before M. Lindelöf has attempted to give a systematic account of them." [Hardy, 1905, 233].
[56] I am grateful to Olli Lehto for this information given in a private email.

simplify several proofs. Elfving remarked rather cryptically that Sundman "got much help from Lindelöf" in "his famous work on the three-body problem" but elaborated no further [1981, 191]. However, since it can be safely assumed that Elfving met both Sundman and Lindelöf,[57] Elfving's words, albeit not very enlightening, are not without weight.

But Lindelöf was not only a mathematical resource for Sundman. He also had a strong network of foreign contacts. Having studied in Stockholm, Paris, and Göttingen, he had established good relationships with several of Europe's leading and most influential mathematicians. Of these, one of the most important for Sundman was Mittag-Leffler whom he visited frequently in Stockholm and with whom he maintained an extensive correspondence until Mittag-Leffler's death in 1927. It is likely that the two first met in Helsinki in the late 1870s when Lindelöf was only a young boy and Mittag-Leffler was visiting his father.

Another of Sundman's colleagues with a solid mathematical background was his immediate superior, Anders Donner, the Professor of Astronomy. Although, by this date, Donner was immersed in practical astronomy, his previous position as Gyldén's assistant in Stockholm meant that he would have been familiar with Gyldén's methods and hence Sundman's earlier work. From Sundman's point of view, therefore, Donner was someone to whom he could turn for both mathematical and astronomical advice.

Although the statements made by Sundman in his Rosenberg grant proposal do not specifically mention the three-body problem, his plan of research, with its emphasis on mathematics and on questions of stability was certainly closely connected to it. It is hardly a coincidence that two of the people he most wanted to meet, and indeed did meet, while on his travels, Poincaré and Painlevé, had made important contributions to the problem. And his interest in the problem was clearly further stimulated by other mathematicians and astronomers he met while he was abroad, such as Levi-Civita and Bruns, who had also worked on the problem.

Since Sundman's scientific publications during the period 1904 to 1912 consisted solely of his four articles on the three-body problem and since his article for Klein's *Encyklopädie*, which was commissioned while he was abroad, was not close to completion until 1912,[58] it seems that from a mathematical point of view he worked almost exclusively on the three-body problem while he was away.[59] At all events, by the time of his return there is no doubt that his ideas were substantially developed and by

[57] I am grateful to Hans-Olav Tylli for this information. Dr Tylli also informed me that it is believed that Elfving borrowed some of Lindelöf's notebooks while he was writing his *History* but that the whereabouts of these notebooks is now unknown.
[58] Lindelöf to Mittag-Leffler, 30 January 1913. Letter No. 64, Institut Mittag-Leffler.
[59] While abroad Sundman also worked on theoretical aspects of joining overlapping photographic plates to minimise errors of position for the Carte du Ciel project. See the Sundman-Donner correspondence, 1903-1906, University of Helsinki Observatory Archives.

June 1906, as Lindelöf reported to Mittag-Leffler, the first of his papers was nearly finished.[60]

7. Sundman's Solution

Sundman's first paper on the three-body problem was published in *Acta Societatis Scientarum Fennicae* (henceforth *Acta SSF*) in 1907, and the second followed two years later [Sundman, 1907; 1909]. In the same year, 1909, he gave a talk on his results at the first Scandinavian Congress of Mathematics, Stockholm, which was published as [Sundman, 1910]. His fourth and final paper on the subject, which was a revised and combined version of the first two, was published in *Acta Mathematica* [Sundman, 1912]. Since Sundman's solution of the three-body problem has been nicely described in [Henkel, 2001] and in [Saari, 2005], and discussed with varying degrees of detail in a number of other works,[61] only a brief description will be given here. In contrast to his predecessors, Sundman attacked the problem by considering triple as well as binary collisions, and fundamental to his solution was the introduction of an auxiliary variable by which the coordinates and the time were generalised to complex values.[62]

In the first paper Sundman focused on the case of triple collision for, as he himself observed, it had not been the subject of any publication [Sundman, 1907, I]. Seeking the conditions for such a collision, he found what he described as a "remarkable" theorem, namely, that a triple collision can occur only if all three integrals of angular momentum are simultaneously zero [p.17]. Following on from this, it was straightforward to show that all three bodies remain constantly in the same plane, defined by their common centre of gravity, and, with a little more work, to show that as they approach collision, they asymptotically approach one of the central configurations (or so-called Lagrangian solutions), which is either an equilateral triangle or collinear configuration.[63] By considering the case in which at least one of the integrals remains non-zero and the initial conditions are known, he further showed that there is a positive limit below which the greatest of the three mutual distances between the bodies cannot go (in confirmation of conjectures made by Weierstrass in 1889).[64] Additionally, he proved that if only two of the bodies collide then the angular velocity of the motion of one of the bodies around the

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[60] Lindelöf to Mittag-Leffler, 12 June 1906. Letter No. 33, Institut Mittag-Leffler. The letter will be discussed in more detail in Section 7 below.

[61] For further discussion of the content of Sundman's papers, see [Birkhoff, 1927, Chapter 9; Chazy, 1952; Siegel and Moser, 1971, Chapter 1; Saari, 1990; Barrow-Green, 1997; Lehti, 2001b].

[62] In its use of complex variables, Sundman's solution brings to mind Hadamard's famous dictum "The shortest way between two truths of the real domain often passes through the complex one" [Hadamard, 1954, 123]. Although the dictum—which of course has a much broader meaning for function theory—was written long after the publication of Sundman's work, Hadamard was well acquainted with the latter, having been the first to make improvements to it [Hadamard, 1915].

[63] In the equilateral triangle case, the initial positions of the three bodies are at the vertices of an equilateral triangle, and the bodies continue to move as though attached to the triangle that rotates about the centre of mass. In the collinear case, the initial positions of the bodies are on a straight line and they continue to stay on that line while the line rotates in a plane about the centre of mass of the bodies.

[64] At this time Sundman was unaware of the conjectures which Weierstrass had made in a letter to Mittag-Leffler. The letter, which is dated 2 February 1889, is published in [Mittag-Leffler, 1912, 55-58].

other is finite, thereby filling in a gap in one of Bisconcini's proofs. However, in reaching these results, Sundman had made certain assumptions with regard to binary collisions—in particular, that it was possible to analytically define an extension of the motion after collision—but he announced that these assumptions would be justified in a later paper. He concluded by remarking that the methods he had used could, with minor modification, be extended to the *n*-body problem and that he would soon return to this issue.

In [1909] Sundman delivered on his promise to deal with binary collisions. As he announced on the opening page, the aim of the paper was to prove the following theorem:

> *If the constants of angular momentum in the motion of three bodies with respect to their common centre of gravity are not all zero, one can find a variable τ such that the coordinates of the bodies, their mutual distances and the time can be expanded in convergent series in powers of τ which represent the motion for all real values of the time, whatever collisions occur between the bodies.* [Sundman, 1909, 3] [65]

His insight was to realise that analytic continuation could be used to define a continuation of the motion of the bodies after collision, and by a change of variables he was able to demonstrate that the singularity at binary collision is of removable type. Taking the case where at least one of the integrals of angular momentum remains nonzero, he expressed the nine Cartesian coordinates together with the time *t* as holomorphic functions of a single variable τ, using a similar transformation to that proposed by Poincaré in 1886. He then proved that these 10 functions, starting with real initial values, are holomorphic within the unit circle $|\tau| = 1$ of the complex plane, and, since $\tau = \pm 1$ corresponds to $t \pm \infty$, the 10 corresponding functions can be represented by uniformly convergent series for all values of $|t| < \infty$. Although no physical meaning can be ascribed to the analytic continuation in the complex domain, it is, as Siegel [1941, 432] observed, "important for the mathematical investigation of the differential equations" involved in the problem.

By showing that there exists a convergent series expansion for the coordinates of the bodies valid for all time, Sundman had provided an analytic solution to the problem. Not only was his achievement quite remarkable, not least when considered in the context of the mathematicians before him who had been unable to solve the problem, but the ideas he used were surprisingly simple. Essentially they depended on Picard's extension to Cauchy's well-known theorem on the existence of solutions to differential equations; a topic, incidentally, on which Lindelöf had published in the 1890s.

Sundman's lecture in Stockholm in 1909 also focused on collisions between the bodies. Although the published version [Sundman, 1910] is brief and lacking detailed proofs, it is well organised, includes the main results from [Sundman, 1907; 1909], and gives due

[65] Original: "Si les constantes des aires dans le mouvement des trois corps par rapport à leur centre commun de gravité ne sont pas toutes nulles, on peut trouver une variable τ telle que les coordonnées des corps, leurs distances mutuelles et le temps soient développables en séries convergentes suivant les puissances de τ qui représentent le mouvement pour toutes les valeurs réelles du temps, et cela quels que soient les chocs qui se produisent entre les corps."

credit to his predecessors (Painlevé, Levi-Civita, Bisconcini). However, the lecture itself failed to impress Mittag-Leffler who noted in his diary that it was 'dåligt' (bad) and that it was difficult to get from it any idea what Sundman had actually done.[66] While there is no way of comparing the spoken and written versions of the lecture, it is possible that Mittag-Leffler was being a little harsh in his judgement since he also noted in his diary that he left the auditorium before Sundman had finished speaking because he was hosting a conference dinner at home that evening.

At all events, whatever Mittag-Leffler thought of Sundman's lecture, he was already familiar with its results and appreciated their significance. As will be described below, by the time of the lecture Mittag-Leffler had exchanged several letters with Sundman and later he oversaw the publication in *Acta Mathematica* of Sundman's final paper on the three-body problem [Sundman, 1912]. In this last paper Sundman combined the results from [Sundman, 1907; 1909] into a coherent whole. It was a substantial paper and, although containing no new results, it was slightly longer than the sum of the earlier two. The treatment was altogether more systematic—it dealt first with binary collisions—and it no longer included the claim about results for the *n*-body problem, a topic to which Sundman never returned.[67] In introducing the contents of the paper, Sundman reiterated the theorem with which he had opened his 1909 paper, but this time it was elevated to the "résultat principal" of his research [Sundman, 1912, 107].

8. The publication of Sundman's results

The lack of knowledge about Sundman and the circumstances in which he produced his publications is perhaps nowhere more evident than in Greenhill's remarks (referred to in the Introduction):

> *The dramatic episode of Sundmann* (sic) *of Helsingfors should encourage the young mathematician as exhibiting the inexhaustible nature of our subject.*
>
> *No sooner had Poincaré declared the Problem of Three Bodies insoluble*[68] *than Sundmann showed how the divergency of the series required to hold for infinite eternity of Time, past and future, could be cured by a simple change of the variable.*
>
> *But as Sundmann's memoir was published in the Finnish language of Helsingfors, the copy sent to Poincaré remained on his desk unread, and Sundmann's name is not mentioned in the Presidential Address at the Cambridge Congress of Mathematicians.*[69]

[66] Diary of G. Mittag-Leffler, Mittag-Leffler Collection, Royal Library, Stockholm; ref. L62: 41. I am grateful to Arild Stubhaug for providing me with the information from Mittag-Leffler's diary.

[67] In 1991 Qiu-Dong Wang, using a new transformation, elegantly generalized Sundman's theory to the cases $n > 3$, and $n = 3$ with zero angular momentum [Wang, 1991].

[68] Although there is an interval of some 16 years, Greenhill is presumably referring to [Poincaré, 1890, 6] quoted above.

[69] The President of the Fifth International Congress of Mathematicians, held in Cambridge in 1912, was Sir George Darwin. His address contained the following remarks on the three-body problem: "To the layman the problem of the three bodies seems so simple that he is surprised to learn that it cannot be solved completely, and yet we know what prodigies of mathematical skill have been bestowed upon it. My own

It was not till Mittag-Leffler published a French version in the Acta Mathematica that the full importance dawned on the world, and Sundmann's merit was recognised by a prize of the Paris Academy. [Greenhill, 1914, 259]

Since both of Sundman's memoirs published in *Acta SSF* were written in French, Greenhill had obviously seen neither of them, despite the fact that there were copies deposited in Cambridge and London which would have been available to him.[70] The error about the language of the memoirs invalidates the assertion about Poincaré, and there is no other evidence to suppose that Greenhill had any idea whether or not Poincaré had seen either of the original memoirs, although given Sundman's discussions with Poincaré it would seem unlikely that Poincaré had not seen them. Nevertheless, Greenhill was right in saying that it was not until Sundman's paper appeared in *Acta Mathematica* that Sundman's work became well known, since it was only the latter publication that provoked any significant reaction. And he was also correct in saying that it was as a result of the publication in *Acta Mathematica* that Sundman was awarded a prize, the Prix Pontecoulant, by the Académie des Sciences, the value of the prize being doubled because of the importance of the result [Picard, 1913a, 1212].

Given the result's importance, why then did Sundman initially choose to publish his work in *Acta SSF*, a Finnish journal which published articles (albeit usually in French or German) from a variety of disciplines, including literature and history as well as science and mathematics, rather than in a mainstream mathematics journal with an international reputation?

It turns out that *Acta SSF* was not in fact Sundman's first choice for the publication of his results. On 10 June 1906, Sundman reported to Donner than Lindelöf had spoken to Mittag-Leffler about his work and that Mittag-Leffler wanted to publish it.[71] In other words, his 1907 paper was originally destined for *Acta* Mathematica, as the following letter from Lindelöf to Mittag-Leffler, written on 12 June 1906, confirms:

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work on the subject cannot be said to involve any such skill at all, unless you indeed describe as skill the procedure of a housebreaker who blows in a safe door with dynamite instead of picking the lock. It is thus by brute force that this tantalizing problem has been compelled to give up some few of its secrets, and great as has been the labour involved I think it has been worth while. Perhaps this work too has done something to encourage others, such as Störmer, to similar tasks as in the computation of the orbits of electrons in the neighbourhood of the earth, thus affording an explanation of some of the phenomena of the aurora borealis. To put at their lowest the claims of this clumsy method, which almost excite the derision of the pure mathematician, it has served to throw light on the celebrated generalisations of Hill and Poincaré." [Darwin, 1913, 36].

[70] Sundman's papers in *Acta SSF* were consulted by F.P. White, a graduate student, in Cambridge on 19th February 1916, who also observed "[The papers] are in French after all, which disposes of Sir George Greenhill' statement in his presidential address that Poincaré could not read them as they were in Finnish." Diary of F.P. White, 1916. St. John's College Archives. *Acta SSF* is also held by the Royal Society (of which Greenhill was a Fellow) and the British Library, both of which would have been easily accessible to Greenhill, a London resident.

[71] Sundman to Donner, 10 June 1906, University of Helsinki Observatory Archives.

> *Following our last conversation I have written to Dr. Sundman and mentioned that you, dear* Farbror,[72] *have offered to publish his result on the three-body-problem in the next volume of* Acta. *He replied a few days ago that he gratefully accepts the offer, but at the same time expressed the hope that not too much time will pass before his paper is printed. This is important to him for special reasons, particularly for his promotion at the University. I would therefore like to ask* Farbror, *before I respond definitely to Sundman, if it would be possible to print his paper (which probably won't be long) in one of the first issues of volume 31.*[73] *All this is of course under the condition that his result is correct and worthwhile publishing, of which I for my part have no doubt, given Sundman's great talent. I will myself go through Sundman's paper, before I send it on, and I will do this as meticulously as possible.* [74]

Mittag-Leffler replied almost immediately confirming his acceptance of Sundman's paper, even though he had not seen it:

> *Thanks for your letter of June 12. Sundman's paper will appear in volume 31, if it is not too long. I will also see what I can do to provide him with offprints quickly.*[75]

Three weeks later Sundman wrote to Mittag-Leffler giving an outline of the paper and reiterating Lindelöf's request for rapid publication:

> *Professor E Lindelöf has informed me that you have been so kind as to accept a paper in Acta Mathematica on the results I have discovered concerning the motion in the three-body problem when a collision between the bodies takes place within a limited time. With reference to this, I have prepared a paper in French which will be completed in a couple of weeks.*
>
> *Allow me to send you my heartiest thanks for your offer of space in* Acta *for this paper.*
>
> [Outline of contents of paper]
>
> *I would be grateful if you could confirm when I might have my paper published. If I may make a request in the matter, it would be to have it published as soon as possible; the sooner the better.*[76]

But Sundman's optimism about the state of the paper was misplaced. Two months later he was writing to Mittag-Leffler to tell him that the paper was still not ready because, on Lindelöf's advice, he was making some revisions:

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[72] The Swedish word *farbror* means uncle (literally brother of the father), however in this context it is used to show respectful familiarity. It also indicates the age difference between the correspondents: Mittag-Leffler was some 25 years older than Lindelöf.
[73] At the date of Lindelöf's letter, Volume 30 of *Acta Mathematica* was midway through publication; the final article was printed in November 1906. The printing for Volume 31 began in March 1907 and was completed in March 1908.
[74] Lindelöf to Mittag-Leffler, 12 June 1906. Letter 33, Institut Mittag-Leffler. This and all subsequent letters between Lindelöf and Mittag-Leffler are in Swedish.
[75] Mittag-Leffler to Lindelöf, 18 June 1906. Letter 4086, Institut Mittag-Leffler.
[76] I am indebted to Christine Peel for providing translations from Swedish of the Sundman-Mittag-Leffler correspondence. Sundman to Mittag-Leffler, 9 July 1906. Letter 1, Institut Mittag-Leffler.

> *I refer to your kind communication telling me that you will publish my paper as soon as possible if the results contained in it prove to be correct. I waited for Professor E Lindelöf's return in order to hear whether after studying the results he considered them to be correct. He has declared himself convinced that they are but he has requested that I revise the paper. Unfortunately, as a result there will be a delay of another two weeks before I can send it to you. I hope that after a thorough study by you it can be included in Acta.*[77]

But Sundman's revisions raised further difficulties, at least from Lindelöf's perspective. It seems that the revisions not only had extended the paper, which inevitably compromised the date of its appearance in *Acta Mathematica*, but also cast doubt in Lindelöf's mind as to whether the paper was sufficiently complete for publication in *Acta Mathematica*. On 5 November, Lindelöf wrote to Mittag-Leffler suggesting that an alternative publishing route via *Acta SSF* might be appropriate:

> *I would like to have a copy of the first issue of Acta's volume XXX, where Bisconcini's paper appears. I have for some reason lost my copy which I badly need for Sundman's paper. The latter is by the way so long (probably more than 50 pages) that no publication* [two words illegible] *in the foreseeable future in Acta, which is why we have chosen to introduce it in the proceedings* ["*skrifter*"] *of our Academy of Sciences. Maybe later* Farbror *will find it appropriate to publish a more comprehensive and improved version of this and other papers of Sundman. As far as I can see he has made decisive progress in the three-body-problem and found a very deep result.*[78]

Mittag-Leffler immediately replied making it clear that he had no objection to Lindelöf's suggestion:

> *Thanks for your letter from November 5. It does not matter if Sundman's paper is 50 pages long if it marks a real progress. I have reserved space for it in Acta. If, however, he wants to publish it first in Finland and later an improved version in the Acta that is also o.k.*[79]

On 17 December 1906, Sundman submitted his paper to *Acta SSF*, and it was published the following year. Sundman's need for rapid publication, which was tied to his case for promotion, had made the choice of journal inevitable.

From the above it is evident that Mittag-Leffler was not pressing to keep the paper exclusively for *Acta Mathematica*. This may well have been due, at least in part, to the fact that all along Mittag-Leffler would have known that the chances of the paper appearing in *Acta Mathematica* in 1907 were slim. He had sufficient papers to fill the next volume of the journal—his authors included Poincaré and Lindelöf—and due to the way the issues were scheduled he would have known that the volume would not complete publication until well into 1908. Moreover, 1906 had been a particularly busy year for him. *Acta Mathematica* aside, he had been involved in law suits, waterfall projects, erecting monuments, finding a summer residence, rebuilding his villa in Djursholm, and

[77] Sundman to Mittag-Leffler, 14 September 1906. Letter 2, Institut Mittag-Leffler.
[78] Lindelöf to Mittag-Leffler, 5 November 1906. Letter 38, Institut Mittag-Leffler.
[79] Mittag-Leffler to Lindelöf, 8 November 1906. Letter 4141, Institut Mittag-Leffler.

advising the government on how to count votes.[80] It is likely therefore that he would have had less time to spend on the manuscript than he would have liked. Since the paper was on such a celebrated topic and by a relatively unknown author, he no doubt felt it better to err on the side of caution and, rather than publish it without giving it full attention, let it first be published elsewhere. Furthermore, at this stage he had seen only Sundman's summary of his results and although he did not explicitly express doubt about their validity,[81] the exchanges with Lindelöf could have given him additional cause to hold back on publication. In addition, Lindelöf knew that Mittag-Leffler's suggestion of republishing the article in *Acta Mathematica* would not be an empty promise. It was a tactic Mittag-Leffler had used successfully in the past, most notably with his republication (and translation from German into French) in the early 1880s of Georg Cantor's articles on set theory, and with the republication in 1886 of George William Hill's paper on the motion of the lunar perigee [Hill, 1886], originally privately published in the United States in 1877.[82]

Two years later, on 28 November 1908, Lindelöf, who by this time had been made an editor of *Acta Mathematica*, wrote again to Mittag-Leffler to inform him that Sundman had done further work on the problem:

> *Sundman has continued his investigations on the three-body-problem and obtains ... curious power series which (under the condition that the three area constants do not vanish at the same time) represent the motion for all time, in case two bodies collide* [illegible] *after collision.*[83]

Mittag-Leffler took the initiative and almost immediately wrote to Sundman inviting him to submit a single paper containing all his results on the problem:

> *I have heard from Lindelöf that you have produced further new important work on the three-body problem. I suggest that you edit it for Acta Mathematica and submit it together with your first paper published in the Society's Acta* [SSF]. *The latter should be re-edited so that both papers form a single whole which I could publish. I would then put it in the same volume as Weierstrass' letters to me and Frau Kovalevskaya about the three-body*

[80] The evidence for this comes from Arild Stubhaug, who is the author of a biography of Mittag-Leffler [Stubhaug, 2007], and to whom I am most grateful for this information. In July 1906 Mittag-Leffler was in St Moritz seeing business partners concerning waterfall projects in Sweden and Norway; in August he was involved in a trial in Falun in which he had been accused of having stolen the right to produce a steel pen from a court calligrapher, and in the same month he also went to Hjo to put up a monument for his grandfather. Meanwhile, at the same time, he and his wife were rebuilding the villa at Djursholm (now the Institut Mittag-Leffler), as well as travelling around Småland with his two secretaries trying to find a summer residence. He was also very involved with politics, foremost as an advisor to the government on how to count votes.

[81] Ten years later, however, Mittag-Leffler did ask Lindelöf some questions about Sundman's results on the three-body problem which reveals that by that time he had decided it was easier to question Lindelöf than to study Sundman's paper again for himself. Mittag-Leffler to Lindelöf, 7 October 1916. Letter 6048, Institut Mittag-Leffler.

[82] The translations of Cantor's papers were published in *Acta Mathematica* 2 (1883). For a discussion of Mittag-Leffler and the foundation of *Acta Mathematica*, see [Barrow-Green, 2002].

[83] Lindelöf to Mittag-Leffler, 28 November 1908. Letter 48, Institut Mittag-Leffler.

problem.[84] *I believe that you are interested in such a publication since it would be seen by more mathematicians than the Society's Acta* [SSF]. *This would especially be the case if Weierstrass's letters are published in the same volume.*

Would it not be possible for the Doctor [Sundman] *to visit us here at some point during the Christmas vacation which I intend to pass at home? For my own part, I would like to receive as soon as possible a short review of your latest work.*

And, to give Sundman encouragement, he concluded by praising him for the clarity of his earlier paper:

Your paper "Recherches sur le problème des trois corps" is very appealing to me. When one wishes to communicate with the mathematical public one should not use a lot of terminology from mechanics the meaning of which is often not fully clear to the reader. But one should, as you always do, say clearly what one means. One does not write only for those who are conversant with the question but one also wants to interest the wider mathematical public.[85]

With his reference to the "mathematical public," Mittag-Leffler is here referring to the readership of *Acta Mathematica*. Since *Acta Mathematica* was (and still is) a journal primarily devoted to mathematical analysis, Mittag-Leffler was doing what he could to ensure that Sundman would continue to write as a mathematician (i.e. using complex function theory) rather than straying into the language of a mathematical astronomer.

However, by the time Mittag-Leffler had put pen to paper, he was probably too late. For six weeks later, on 18 January 1909, Sundman submitted his second paper to the *Acta SSF*, where it was published as [Sundman, 1909] later that year. Meanwhile, on 17 January Lindelöf had written to Mittag-Leffler telling him about Sundman's new results. He was responding to earlier questions from Mittag-Leffler but belatedly because he "first wanted to go thoroughly into Sundman's work in order to be able to vouch for its correctness."[86]

The discussions between Lindelöf and Mittag-Leffler with respect to Sundman's work continued. Mittag-Leffler, who was in the process of editing Weierstrass's letters on the three-body problem, suggested the possibility that "most of Sundman's results" had already been found by Weierstrass. Presumably he wanted to know whether Sundman had in fact gone further than Weierstrass on the question of conditions for triple collision. Lindelöf, who had not seen Weierstrass's letters, (correctly) doubted Mittag-Leffler's

[84] Weierstrass's letters to Mittag-Leffler on the subject of the three-body problem were written while Weierstrass and Mittag-Leffler (together with Charles Hermite) were in the process of judging Poincaré's memoir on the topic submitted to King Oscar II's prize competition (see [Barrow-Green, 1997, Chapter 4]). They were written during 1888 and 1889 when Sonya Kovalevskaya, who was a close friend of Mittag-Leffler's and a member of the editorial board of *Acta Mathematica*, was living and teaching in Stockholm. In the event, the letters were published in [Mittag-Leffler, 1912] in volume 35 of *Acta Mathematica*, the volume immediately preceding the one in which Sundman's 1912 paper was published.

[85] Mittag-Leffler to Sundman, 7 December 1908. Letter 4479, Institut Mittag-Leffler.

[86] Lindelöf to Mittag-Leffler, 17 January 190[8] (certainly 1909). Letter 49, Institut Mittag-Leffler.

suggestion, adding that he was convinced that Weierstrass had not had the "astronomical instinct" which had led Sundman to his "very deep result."[87]

Not long after the appearance of [Sundman, 1909], Sundman began to prepare a synthesis of his two *Acta SSF* papers for publication in *Acta Mathematica*, during the course of which Mittag-Leffler sent him a copy of the relevant part of the Weierstrass correspondence [Sundman, 1912, 106]. In January 1910 Sundman handed over a final version to Lindelöf for language editing before sending it to Mittag-Leffler.[88] After some delay (due to external factors), Lindelöf sent the paper to Mittag-Leffler in June. As he wrote to Mittag-Leffler, he was now convinced about the paper's merits:

> *The treatise has become rather long and will probably comprise 100 pages. But it has all details of the perhaps most important problem of theoretical astronomy, performed with real mathematical rigor. ... I am therefore convinced that the paper will justify its place in Acta.*[89]

However, it was a further two years before the paper, which extended to 75 pages, appeared. It was eventually printed on 8 July 1912.

As well as charting the progress of Sundman's papers through the vagaries of the publication process, the Lindelöf–Mittag-Leffler correspondence also reveals that there was an added incentive for Mittag-Leffler to publish Sundman's paper in *Acta Mathematica*. In the spring of 1911 Lindelöf had applied for a subsidy for *Acta Mathematica* from the Finnish state budget, basing his application partly on the fact that an article by a Finn, i.e. Sundman, was already in print in the journal.[90] By October 1911, it had become imperative for the article to appear, as he wrote to Mittag-Leffler:

> *It would be in Sundman's and my interest if his paper is printed quickly ... It is necessary to have it finished in the spring in order to have further subsidies for Acta from the Finnish state budget. Last spring Mellin*[91] *and I applied for that money and used the argument that an extensive work of a Finn is in print in Acta, and next time we have to be sure that it is really being printed.*[92]

The following year Mittag-Leffler mentioned in passing to Lindelöf that he was considering inviting Sundman to be an editor of *Acta Mathematica*, which prompted Lindelöf to remark that this too 'would be very helpful for further subsidies'.[93] Mittag-Leffler duly obliged and in January 1914 Sundman wrote to Mittag-Leffler to say:

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[87] Lindelöf to Mittag-Leffler, 26 February 1909. Letter 50, Institut Mittag-Leffler.
[88] Lindelöf to Mittag-Leffler, 12 January [1910]. Letter 55, Institut Mittag-Leffler.
[89] Lindelöf to Mittag-Leffler, 15 June [1910]. Letter 61, Institut Mittag-Leffler.
[90] Lindelöf to Mittag-Leffler, 19 June 1911. Letter 62, Institut Mittag-Leffler.
[91] Robert Hjalmar Mellin, a former student of both Mittag-Leffler and Weierstrass, was professor of mathematics at the Technical University of Finland, Helsinki. See [Elfving, 1981, 98-104].
[92] Lindelöf to Mittag-Leffler, 1 October 1911. Letter 63, Institut Mittag-Leffler.
[93] Lindelöf to Mittag-Leffler, 8 November 1913. Letter 71, Institut Mittag-Leffler.

> *Naturally I have nothing against it if you wish to propose me as a member of the Finnish editors of Acta. I feel myself however completely unworthy of this honour and it is only with great diffidence that I dare accept it.*[94]

In Volume 41, published in 1916, Sundman's name appears, alongside those of Lindelöf and Mellin, as one of the three Finnish members of the editorial board.[95]

At the same time as inviting Sundman to join the board of *Acta Mathematica*, Mittag-Leffler also asked Sundman if he would write a commentary on Poincaré's prize-winning paper on the three-body problem [Poincaré, 1890]. Although he did not actually say so, presumably Mittag-Leffler wanted the commentary for the Poincaré memorial volume of *Acta Mathematica* he was compiling, and which eventually appeared in 1921 as Volume 38. However, Sundman declined the offer due to other commitments.[96] As well as working on [Sundman, 1915; 1915a] he was organising solar eclipse expeditions and had 50 astronomy students to teach. Mittag-Leffler tried hard to persuade him to change his mind but Sundman was resolute in his refusal, appreciating the difficulty and delicacy of the task:

> *I must first thank you very much for your kind letter including the analysis of Poincaré's work. For the last two days I have again looked through Poincaré's prize paper in order to see if I possibly could satisfy your desire concerning an essay about it. I find this matter very delicate. The thing requires a thorough investigation. I do not think that the prize paper corresponds to what Weierstrass wanted. It is probably for that reason he had such difficulty in accounting for his statements about it. That the prize paper is of high value is without question but when it is a matter of expressing oneself in connection with the question of the prize one has to be very careful not to make a rash judgement. Pressed as I am by other unavoidable work, it is impossible for me to find sufficient peace and quiet to write on such a demanding question.*[97]

Sundman was right to be cautious about writing such a commentary. Poincaré's paper [1890] is very long and full of innovatory ideas. Although writing a commentary on it would have provided Sundman with an opportunity for publicising his own work on the three-body problem, doing so would have been a challenging and time-consuming task. Weierstrass himself had originally promised Mittag-Leffler that he would write a commentary to accompany the publication of Poincaré's paper but, despite persistent cajoling from Mittag-Leffler, never did so.[98] It is not known whether Mittag-Leffler asked anyone other than Sundman to write a commentary but no such article was ever published.

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[94] Sundman to Mittag-Leffler, 4 January 1914. Letter 6, Institut Mittag-Leffler.

[95] Volume 38 of *Acta Mathematica* which was devoted to the work of Poincaré and due to be published in 1915 was delayed by the War and did not appear until 1921, and Volume 39 which was devoted to the work of Weierstrass did not appear until 1923. Sundman's name was supposed to appear in Volume 40, also published in 1916, but was inadvertently omitted. Mittag-Leffler to Lindelöf, 8 January, 1916. Letter 5780, Institut Mittag-Leffler.

[96] Sundman to Mittag-Leffler, 4 January 1914. Letter 6, Institut Mittag-Leffler.

[97] Sundman to Mittag-Leffler, 4 January 1914. Letter 7, Institut Mittag-Leffler.

[98] For a discussion about the publication of [Poincaré, 1890] and the lack of Weierstrass's commentary, see [Barrow-Green, 1997, Chapter 4].

9. The reception of Sundman's work

Sundman's first paper on the three-body problem [Sundman, 1907] was not listed in the *Jahrbuch über die Fortschritte der Mathematik* (*JFM*) and there is little evidence that it drew much attention. One person who was aware of it was Edgar Odell Lovett, the head of the department of astronomy at Princeton. In 1908 Lovett gave a lecture on progress in the *n*-body problem to the annual meeting of the American Association for the Advancement of Science, in which he listed Sundman's main results:

> *Sundman has found the condition for the simultaneous collision of all three bodies to consist in a vanishing of all three integrals of area in the motion of the bodies with respect to their common centre of gravity; if the constants of area are not all zero, Sundman has assigned a positive limit below which, of the three distances, the greatest always remains so. The same writer has announced the extension of his results to the n-body problem, including explicit expressions for the coordinates in the vicinity of the equilibrium.* [Lovett, 1909, 90]

Lovett also observed that Sundman's work had stimulated a renewed "theoretical interest in the Lagrangian solutions," which indicates that Sundman's paper had been noticed by others, but he gave no further details. That Lovett himself had heard about Sundman's work may well have been as a result of connections he had made when studying with Sophus Lie, whom he had followed from Leipzig (where Bruns was professor of astronomy) to Kristiania in the late 1890s [Stubhaug, 2002, 505]. But while Lovett spoke positively about Sundman's work, he does not appear to have appreciated its full significance.

In terms of public acclaim, Sundman's second paper [Sundman, 1909] fared little better than the first. It did at least get a mention in the *JFM* but there was no synopsis and no review.[99] It would seem therefore that either the editors of the *JFM* were unable to find a reviewer or they thought it not worth reviewing (possibly without having seen it). The *JFM* had previously noted [Sundman, 1897; 1897a] and provided a short synopsis of [Sundman, 1901], but these earlier papers would not have given the editors reason to think that [Sundman, 1909] contained anything of particular note. Sundman's 1910 paper from the Stockholm Congress did get a notice in the *JFM* but it was a short factual report which gave no hint of the significance of the results.[100]

The situation with respect to [Sundman, 1912] was entirely different. Unlike *Acta SSF*, *Acta Mathematica* was a leading mathematics journal and any paper published in it was assured an international audience. The paper was enthusiastically reviewed in the *JFM*, with the author of the review, the editor Emil Lampe, describing Sundman's solution as "a towering milestone in mathematical research in celestial mechanics."[101] And the paper received a glowing reception across Europe and in the United States. Emile Picard used his report for the committee of the Prix Pontecoulant as a basis for two other (near

[99] Rather curiously the on-line version of the *JFM* mistakenly states that [Sundman, 1909] is written in "Finnish" even though the mistake is not in the original printed version.

[100] As was customary with *JFM* the report appeared three years after the publication itself and hence after [Sundman, 1912] in any case.

[101] Original: " ist ein ragender Markstein in der mathematischen Forschung der Himmelsmechanik."

identical) articles, in all of which he described Sundman's memoir as "an epoch-making work for analysts and mathematical astronomers." [Picard, 1913a, 1211; 1913b, 320; 1913c, 725].[102] Penetrating to the heart of the result, he made reference to Sundman's "profound insights on what one can call the *analytic continuation* of the problem after [binary] collision."[103]

In Italy Robert Marcolongo in his survey article drew attention to the "extraordinary simplicity"[104] of Sundman's method [Marcolongo, 1914, 175], while in the United States Forest Ray Moulton called Sundman's work "remarkable" and "of the very highest order of excellence" [Moulton, 1914, 198].[105] In Britain Arthur Eddington also described the work as "remarkable":

> *The reference to KF Sundmann* (sic) […] *is a reminder that we have hitherto shirked the task of giving to our readers any account of his remarkable researches. The delay is not due to any lack of appreciation but the fact that the memoir is not to be read hastily.* [Eddington, 1915, 429]

Eddington was perhaps a little disingenuous in his explanation of the delay, since Sundman's *Acta Mathematica* article had been in circulation for over three years at the time of his remarks. Nevertheless, it was probably his article that stimulated his Cambridge colleague, Henry Frederick Baker, to give a special lecture at the University Observatory in February 1916 on Sundman's paper.[106] Meanwhile Hadamard in [1915] simplified Sundman's proof concerning the conditions for a triple collision, as well as correcting a minor error (which did not affect the final result). While von Zeipel made Sundman's results available to German readers in a rather long article in which he provided Sundman's proof pared of "everything insignificant" in order to give a clearer idea of Sundman's "train of thought" [Von Zeipel, 1917, 56].

However, while Sundman's mathematical dexterity was widely lauded, it had not gone unnoticed that his result gave no practical help to astronomers, whose agenda was rather different to that of the mathematicians.[107] Picard [1913a, 1211] pointed out that

[102] Original : "Nous pouvons dire alors que le Mémoire de M. Sundman]n[est un travail faisant époque pour les Analystes et les Astronomes mathématiciens."
[103] Original : "Vues profondes de M. Sundman]n[sue ce qu-on peut appeler le *prolongement analytique* du problème après le choc."
[104] Original : "Orbene, un astronomo de Helsingfors, Karl Sundman, [...] ha compinto quanto Poincaré stimava se non impossible, certamente difficilissimo, e inoltre coi soli istrumenti analitici che offre l'analisi moderna e con una semplicità di mezzi veramente straordinaria! "
[105] It seems that Moulton did not find [Sundman, 1912] immediately easy to follow. In January 1914 he wrote to G.D. Birkhoff to ask him to inform him if, when he had gone through the details, he found 'anything questionable about any of the conclusions'. Moulton did, however, presume that the memoir was 'absolutely correct' since it had been 'very carefully examined by competent people.' F.R. Moulton to G.D. Birkhoff, 19 January 1914. Papers of George David Birkhoff, Harvard Archives. HUG 4213.2, Box 7.
[106] The lecture was given on 17 February 1916 to the Observatory Club at the University Observatory. The lecture was attended by White, who mentions it in his diary for that day, describing the contents of Sundman's paper as "mostly pure mathematics." See: Diary of F.P.White, St. John's College Archives.
[107] The difference in requirements of mathematicians and astronomers was succinctly captured by Poincaré who, in his *Mécanique Céleste*, considered the example of the two mathematical series:

Sundman's series were unlikely to be more suitable for providing qualitative information about virtually periodic astronomical phenomena than trigonometric series, despite the divergence of the latter. Moulton [1914, 207] observed that Sundman's solution "gives no properties of the motion, no answer to the questions the astronomer raises, and there is no hope of its being practically applied." While the Scandinavian astronomers Carl Burrau and Elis Strömgren, writing in the *Astronomische Nachrichten* remarked:

> *Sundman has proved that a transformation can be found such that the coordinates (and the time) can be expanded in theoretically always convergent series. But he has not developed these series, let alone said anything about their practical convergence, and it is the practical convergence alone which is important for the application of the theory to concrete cases.* [Burrau and Strömgren, 1916, 185-186][108]

In 1917 Whittaker referred to the result in the new edition of his *Analytical Dynamics* but described it as "a considerable advance" [Whittaker, 1917, 424] rather than a solution, indicating a hesitance about classifying the result. Likewise Florian Cajori in his *History of Mathematics* described Sundman's work as "material advances in the problem of three bodies," again with no mention of solution [Cajori, 1919, 454]. (As a side remark, Whittaker and Cajori's ambivalence about the status of Sundman's result naturally raises the question of what counts as a "solution" in the context of an applied mathematics problem. Exactly this topic is explored explicitly in the context of Levi-Civita's and Sundman's work in [Dell'Aglio, 1993].)

Meanwhile G.D. Birkhoff, in a popular article in *Science* on recent advances in dynamics, qualified the value of the result:

> *Finally, there is Sundman's remarkable work on the unrestricted problem* […] *he has "solved" the problem of three bodies in the highly artificial sense proposed by Painlevé in 1897. Unfortunately these series are valueless either as a means of obtaining numerical information or as a basis for numerical computation, and thus are not of particular importance.* [Birkhoff, 1920, 53].

Birkhoff, no doubt mindful of his audience, clearly wanted to deter anyone from trying to calculate with Sundman's series. But this was not Birkhoff's last pronouncement on the subject. A few years later he devoted a large part of the final chapter of his seminal *Dynamical Systems* [Birkhoff, 1927, 260-292] to it, examining the mathematics in considerable detail and casting Sundman's contribution in a very positive light:

> *It is not too much to say that the recent work of Sundman is one of the most remarkable contributions to the three-body problem which has ever been made.* [Birkhoff, 1927, 260]

$\sum \frac{(1000)^n}{n!}$ and $\sum \frac{n!}{(1000)^n}$. If, Poincaré argued, you were a mathematician, then you would consider the first series convergent and the second divergent, but if you were an astronomer you would label them the other way around [Poincaré, 1893, 1-2]. See [Barrow-Green, 1997, 156].

[108] Original: "Sundman hat bewiesen, daß eine solche Transformation definiert werden kann, daß die Koordinaten (und die Zeit) in theoretisch immer konvergente Reihen entwickelt werden können; er hat aber diese Reihen nicht entwickelt, noch weniger etwas über die praktische Konvergenz dieser Reihen gesagt, und auf diese praktische Konvergenz allein kommt es bei der Anwendung der Theorie auf konkrete Fälle an."

Furthermore, it was presumably Birkhoff's analysis of Sundman's work on the three-body problem which led to Sundman's name appearing as the "leader in applied mathematics in Finland" in a list dated 26 January 1926, compiled by Birkhoff, Oswald Veblen and Solomon Lefschetz, of "Leaders in the field of mathematics listed under countries and institutions." Although Sundman had published very little since 1915, it seems that at that time Birkhoff was unaware of Sundman's reduction in productivity. However, in February 1926, Birkhoff left for a trip to Europe—his first—to visit mathematics departments,[109] and while away must have learnt of Sundman's true standing because later he crossed through Sundman's name on his copy of the list.[110]

Needless to say, it was not long before someone decided to find out just how "valueless," from the point of view of numerical computation, Sundman's series actually were. In 1930 David Beloriszky [1930] calculated that if Sundman's series were going to be used for astronomical observations then the computations would involve at least $10^{8,000,000}$ terms! It seems likely that Sundman himself knew about Beloriszky's calculation because the very same year, at a meeting of the *Astronomische Gesellschaft* in Budapest, he announced that he believed numerical integration to be the only tractable method for treating the problem [Strömgren, 1935, 127]. It is of course possible that this announcement was prompted by further work of his own on the problem, although there is no evidence to this effect.

But it was not only from a quantitative point of view that applied mathematicians and astronomers saw limitations in Sundman's result. In 1937 the American mathematician and astronomer H.E. Buchanan, having acknowledged the importance of the result for mathematicians, observed that its practical value was "not very great" since it gave "no properties of the motion, no shapes of the orbits, no proof of periodicity or of non-periodicity"; the problem "was not solved as thoroughly or as completely" as he wished [Buchanan, 1937, 80].

One of the most stalwart champions of Sundman's result was Carl Ludwig Siegel who, from 1941, campaigned to make the result better known, noting that at that time "[Sundman]'s important papers have been studied by only very few people" [Siegel, 1941, 433], and, in his acclaimed text on celestial mechanics, provided a modernised account of Sundman's theorems [Siegel, 1956, Chapter 1]. Siegel was in no doubt as to the importance of Sundman's work, considering it to be one of the most significant developments in the transformation theory of differential equations post-Poincaré. Strong support for the result also came from the mathematical astronomer Jean Chazy who in [Chazy, 1952] published an appreciation of [Sundman, 1912] in which he looked at the

[109] A copy of Birkhoff's report on his visit to Europe is in G.D. Birkhoff Papers, Harvard University Archives, HUG 4213.2 (Box 6). Birkhoff's report is reproduced in [Siegmund-Schultze, 2001, 265-271].
[110] G.D. Birkhoff Papers, Harvard University Archives, HUG 4213.2 (Box 6). Apart from Sundman, the only other name to appear under Finland is that of F.E.H. Nevanlinna who was designated as a leader in analysis and whose name was also crossed out in Birkhoff's copy. Note that this Nevanlinna was Frithiof, the slightly older brother of Rolf Nevanlinna (referred to in Footnote 9), the latter's crucial work of 1925 in which he laid the foundations of value distribution theory having apparently not yet come to Birkhoff's attention [Nevanlinna, 1925].

influence of the memoir as well as its contents. Although cognisant of the limitations imposed by the generality of Sundman's result, Chazy's account describes the positive effect of the result on the direction of research into the three-body problem:

> *The integration of the three-body problem by Sundman, beyond its intrinsic value, has had a moral effect. After so many vain efforts, after the enormous work of Poincaré, researchers turned away from the three-body problem: Sundman's integration has shown that in this problem one is able to 'make something'. Fundamental progress in the question has produced new aspects of the question, and inevitably had indirect and profound consequences. …*
>
> *But already this solution* [Sundman's] *has on the one hand led to research into collisions and close approaches between the three bodies, and on the other hand prompted the study of infinite branches of the trajectories of the three bodies, and the study of the motion as time tends to infinity. Likewise the determination of singular trajectories has led to substantial results in the representation and the distribution of trajectories of the three-body problem—the consideration of which is as necessary as the study of singular points in the study of an analytic function. Without having resolved in one go all the qualitative questions posed by the three-body problem, Sundman's integration has given rise to essential progress in the solution of these questions. And plenty of questions remain open after Sundman's work—just as after the work of Poincaré.* [Chazy, 1952, 189–190][111]

Chazy, who had researched extensively into the three-body problem, was well qualified to judge Sundman's achievement. In an acclaimed paper [Chazy, 1922], he had made extensive use of Sundman's regularising variable in order to investigate the long term behaviour of the solutions of the three-body problem and, by taking a more complete account of the quasi-periodic character of planetary motion, filled a gap in Sundman's solution.[112] Nevertheless, despite Chazy's positive spin on Sundman's contribution, it is hard to find evidence that Sundman was getting as much credit as Chazy seemed to indicate.

Certainly Siegel and Chazy were not entirely successful in converting to their way of thinking either those of a more practical persuasion, who continued to dismiss Sundman's

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[111] Original: "L'intégration du problème des trois corps par Sundman, en outré de sa valeur intrinsèque, a eu un effect moral. Après tant de vains efforts, après l'œuvre gigantesque de Poincaré, les chercheurs se détournaient du problème des trois corps : l'intégration de Sundman a montré que dans ce problème on pouvait 'faire quelque chose'. Un progrès fondamental dans une question fait apparaître de nouvelles faces de cette question, et nécessairement a des conséquences indirectes et profondes. … Mais dès maintenant cette solution a entraîné d'une part des recherches sur les chocs et les approches des trois corps, et d'autre part a provoqué l'étude des branches infinies des trajectoires des trois corps, et l'étude du mouvement quand le temps tend vers l'infini. Dès maintenant des résultats substantiels ont été acquis dans la représentation et la répartition de l'ensemble des trajectoires du problème des trois corps, par la détermination des trajectoires singulières—dont la considération est aussi nécessaire que, dans l'étude d'une fonction analytique, la considération des points singuliers. Sans avoir résolu d'un coup toutes les questions qualitatives que pose le problème des trois corps, l'intégration de Sundman a provoque dans la solution de ces questions des progrès essentiels. Et quantité de questions restent ouvertes à la suite de l'œuvre de Sundman—comme à la suite de l'œuvre de Poincaré."

[112] See [Chevenard, 1955, 158]. In 1922 Chazy won the Prix Benjamin Valz for his work in celestial mechanics and particularly for [Chazy, 1922]; see [Costabel, 1971, 220].

result ignoring its mathematical merits, for example [Roy, 1978, 120], or those, such as Abraham and Marsden [1978, 699], who mention it but only in a footnote; and in 1990 Donald Saari was still campaigning for proper recognition of the result [Saari, 1990, 114]. One theoretical astronomer who did see Sundman's result in a wider context was Victor Szebehely:

> *The credit for using regularizing variables is usually given to Sundman (1912), who introduced regularization to show the existence of solutions of the differential equations of motion. It is interesting to note that such pure mathematical exercises led to everyday practical techniques used today in our applied orbit mechanics. The combination of a mathematical existence proof and increased accuracy of numerical integration of the orbits of space probes represents an important message to promote the cooperation of engineers and mathematicians.* [Szebhely, 1997, 112].

However, by the date of Szebhely's remarks, Sundman's name had begun to reappear in the literature, the resurgence of interest in his work being linked, through Poincaré's research on the three-body problem, to developments in chaos theory.

The reception of Sundman's solution can thus be divided into four distinct phases. In the first, which encompasses the publication of [Sundman, 1907; 1909], the results were virtually ignored. The relative obscurity, at least from a mathematical and an international point of view, of both the author and the journal in which he published meant that these papers were largely invisible to the mathematical community. Sundman was a shy personality working outside—both academically and physically—the mainstream of mathematics, and *Acta SSF* was a journal without a special reputation for mathematics. Lindelöf and Mittag-Leffler, the two people who, through their knowledge of Sundman's work and their standing in the mathematical community, could have promoted Sundman's solution did not do so, instead focussing their efforts on helping Sundman to produce a polished version of it for *Acta Mathematica*.

In the second phase, the decade following the publication of the *Acta Mathematica* paper, the solution was lauded, finessed, and extended. Its practical limitations were noted but did nothing to lower the high regard in which it was held. The result was timely. Poincaré's magisterial work on celestial mechanics *Les Méthodes Nouvelles de la Mécanique Céleste*, which included his ground-breaking work on the three-body problem, had been completed in 1899 while the final volume of his teaching text *Leçons de Mécanique Céleste* had appeared in 1910. Painlevé and Levi-Civita had been publishing vigorously on the problem, and Hilbert's endorsement at the Paris ICM in 1900 served only to add to the problem's attraction. There was even the added poignancy of Poincaré's sudden death less than a fortnight after Sundman's paper was actually printed. Added to which, for a Finnish author the paper could not have appeared in a better journal. *Acta Mathematica* was not only one of the world's leading mathematical journals but had at its helm Mittag-Leffler, a zealous promoter of Scandinavian and Finnish mathematics.

But in the third phase, which began after the First World War and continued up until the early 1990s, the situation was different. Interest in celestial mechanics in general

diminished.[113] Poincaré's work had changed the nature of the subject, at least in the highly theoretical sense, and without fast computation to illuminate his ideas (which happened much later with the birth of the digital computer and the advent of computer graphics) it was not obvious in which direction the subject should go. Poincaré's mathematics was fiercely difficult and held little immediate promise for the practical mathematical astronomer. Sundman's solution was in a similar vein. While not of the same level of difficulty as the work of Poincaré, it nevertheless encompassed an analytical result: a result in pure mathematics and without practical application. For applied mathematicians the problem remained ostensibly unsolved and their appreciation of Sundman's achievement was correspondingly muted. Furthermore, Sundman himself published little after 1912, his only other significant papers (discussed in Section 9) appearing inauspiciously during 1915. As far as the pure mathematicians were concerned, Sundman had been unknown to them before the publication of his *Acta Mathematica* paper and he became lost to most of them again after it. As an unobtrusive professor of astronomy in a small country ravaged by civil war he was easy to forget. He had produced a startling result in pure mathematics but it involved nothing new in the way of mathematical techniques. And it was his only significant contribution to pure mathematics. Pure mathematicians, therefore, had no compelling reason to keep him in mind. Nevertheless, there were some, notably Birkhoff and Siegel, and later Chazy and Saari, who continued to appreciate what Sundman had achieved and through them knowledge of his work was kept alive.

In the fourth phase, which began in the early 1990s, the situation changed again. Progress in chaos theory stimulated a renewal of interest in Poincaré's work on the three-body problem. This, in turn, drew attention to Sundman's solution and since then it has been increasingly cited and discussed in a variety of publications, showing that it is now widely known and appreciated.[114] The last word here should go to Saari who in 2005 wrote:

> *While Sundman's series are of no practical use, almost a century later his contributions toward our understanding of collisions and the N-body problem remain valued tools in our analysis of celestial mechanics.* [Saari, 2005, 138].

9. Two later papers

Up until 1907, the year of his first publication on the three-body problem, Sundman's publication record had not been especially remarkable.[115] Apart from his doctoral thesis of 1901, he had had three other papers published: the one on continued fractions [Sundman,1897], another on a question connected with using a ring micrometer [Sundman, 1897a] and a third dealing with an aspect of Gyldén's work [Sundman, 1903]. However, these papers do not take account of the work he did at the Pulkovo Observatory

[113] This was attributable in part to competing new areas of activity, e.g. relativity theory and quantum theory, as well as the difficulty of engaging in quantitative analysis due to the inadequacy of computing techniques.
[114] *Mathematical Reviews* for 1990-2008 lists nearly 40 publications that include Sundman's name in the abstract.
[115] A full bibliography of Sundman's publications can be found in [Järnefelt, 1950; 1953].

helping Backlund finish Gyldén's research on planetary orbits. Completing the latter turned out to be a major undertaking and it continued to occupy Backlund for several years after Sundman had left the Observatory. It was eventually published in 1908 and although the publication appeared under Backlund's name, Backlund made it clear that much of the credit for the work should go to Sundman [Gyldén, 1908, Preface].

Nor did Sundman publish prolifically after 1912, being busy with teaching and practical work at the observatory, and most of his subsequent papers were directly connected with astronomy. Two publications, however, stand out and in different but connected ways can be considered as complementary to his publications on the three-body problem. The first is his article on the theory of the planets which he contributed to the astronomy volume of Klein's *Encyklopädie der Mathematischen Wissenschaften* [Sundman, 1915a], and the second is his plan for a machine to determine planetary perturbations [Sundman, 1915b].

As noted above, Sundman had been invited to contribute to the *Encyklopädie* while he was on his European tour during 1903-1906. Since at that time he had published little, and all his publications had appeared in Scandinavian journals, it seems likely that he was recommended to Schwarzschild by Lindelöf, on the basis of personal knowledge and the success of his thesis [Sundman, 1901]. Nevertheless, he was a relatively unknown figure and it was a prestigious project, so the fact that he was invited to contribute to it would seem to indicate that he had made a positive impression while abroad. At all events, Sundman's article [1915a], which explains the principles of planetary perturbation theory, fits closely with his stated research objectives for the Rosenberg grant, and so in terms of both his interests and his career, it would have been an invitation natural for him to accept. As the letter from Poincaré to Mittag-Leffler quoted in Section 5 makes clear, Sundman began working on the article at least as early as 1905. But it was a long time in the writing since he was still working on it in January 1913.[116]

The article, which is a wide-ranging, substantial piece of work and one of the longer ones in the astronomy volume of the *Encyklopädie*, provides an overview of the principles applied by Sundman in much of his own astronomical work and represents a return to his original interest in perturbation theory as laid out in [Sundman, 1901]. The motion of a planet encircled by satellites is divided into three components: the motion of the centre of gravity of the whole system, the motion of the planet around the centre of gravity of the system, and the motion of the planet around its own centre of gravity. Since the resulting differential equations cannot be accurately integrated, approximations which take into account the actual conditions prevailing in the planetary system have to be used. The lack of exact solutions then points towards the need for (faster) approximation methods.

Sundman cited the work of several Scandinavian astronomers, notably Gyldén, Hansen, and Backlund, and the benefit of his travels is evident in his use of the work of Poincaré (including Poincaré [1905]) and von Seeliger. Notably, he made no special reference to

[116] Lindelöf to Mittag-Leffler, 30 January 1913. Letter 64, Institut Mittag-Leffler. Sundman had the proofs by early January 1914, so the article must have been completed during 1913. Sundman to Mittag-Leffler, 4 January 1914. Letter 6, Institut Mittag-Leffler.

his own work. While the article is at a different point on the mathematical spectrum to his papers on the three-body problem—the techniques it contains can be used for practical calculations—it is concerned with the same meta-question: the motion of celestial bodies.

In [Sundman, 1915b], in a logical step on from the *Encyklopädie* article, at least from a practical point of view, Sundman considered a way of speeding up the perturbation calculations. But instead of devising new mathematical techniques he drew up plans for a machine, a "perturbographe," to mechanise the calculations. He wanted a machine that would calculate the perturbations of small planets both quickly and easily. Having started by looking for a mechanical method for obtaining the perturbing forces, he quickly saw the possibility of extending the idea to the direct determination of the perturbations themselves. He also realised that if several such machines could be used in parallel, then it should be possible to record simultaneously the perturbations of several perturbing planets, or to treat several planets exerting reciprocal perturbations on one another.

It seems that Sundman began working on the design of the perturbographe sometime around the end of 1911 or beginning of 1912. On 30 January 1913, Lindelöf reported to Mittag-Leffler that "for one year he [Sundman] has been occupied with the construction of an apparatus which records directly the perturbations of the small planets' orbits which are caused by Jupiter."[117] Indeed Sundman had given his first account of the machine at a meeting of the Helsinki Mathematical Society in October 1912 [Sundman, 1915b, 6]. He spoke about it in detail at the Scandinavian Congress of Mathematicians, Kristiania in September 1913, and by February 1914, Sundman having given three talks on it, Lindelöf wrote to Mittag-Leffler to say that the astronomers were waiting for it "with impatience."[118]

Sundman's goal was to construct a machine which would be capable of determining the perturbations of the small planets caused by Jupiter—a topic that connected closely to his thesis—and to this end "restricted" himself to designing a machine capable of providing solutions to second order differential equations. On the assumption that the machine could be constructed to the necessary degree of precision, he estimated that it should be able to obtain the perturbations to an accuracy of "less than one centimetre of their value" [Sundman, 1915b, 6]. He summarised the fundamental idea behind the machine in the following propositions:

1. *A mechanism imitates the motions of the perturbing planet and the perturbed planet in such a way that their relative positions in space are indicated by a scale given by the relative positions of certain components of the machine.*
2. *According to the relative positions of these components, other components must be mechanically constrained to indicate the positions relative to the size of the perturbing forces, and, according to the positions of those second components, a third series of components should indicate the derivatives of the perturbations.*

[117] Letter from Lindelöf to Mittag-Leffler, January 1913. No. 64. Institut Mittag-Leffler
[118] Letter from Lindelöf to Mittag-Leffler, 7 February [1914]. No. 73. Institut Mittag-Leffler.

3. *By combining these last components with planimetres, one obtains the perturbations, which one then records by a suitable device for the desired periods.*
4. *As the registered perturbations successively increase, the position of the perturbed planet must be automatically corrected in the machine for the values of these perturbations.* [Sundman, 1915b, 7] [119]

As well as describing the mathematical theory behind the machine—the mathematical operations being represented by different components of the machine—Sundman provided detailed drawings of how the machine could be constructed. He estimated that the speed of the machine could be set so that an orbit of Jupiter could be completed in about seven minutes—Jupiter's orbit in real time is approximately 12 years—and, taking everything else into consideration, the required perturbations could then be obtained within an hour. As Lindelöf remarked to Mittag-Leffler, the machine was designed to be capable of producing in seven minutes "values as exact as a human computer could find after several weeks of calculation."[120] Although the purpose of the machine was to calculate astronomical perturbations, its actual function would have been the integration of second order differential equations, and so it would have been adaptable to a wide range of problems.

To date no evidence has come to light to suggest that any attempt was ever made by Sundman or his contemporaries to construct the machine,[121] although Mittag-Leffler, ever the entrepreneur, was certainly interested in doing so, as he wrote to Lindelöf:

> *What you say about Sundman's plans for a machine to calculate the perturbations of planets interests me very much. Ask him to send me an estimate of the costs to construct such a machine, and I will see what can be done.*[122]

Whether Sundman ever did convey his estimate of the cost to Mittag-Leffler is not known, but nothing further came of Mittag-Leffler's interest. In any case, Sundman's timing could hardly have been worse. The paper was published in the middle of the First World War, when engineering minds in Europe were occupied with more pressing matters. Added to which, the paper was published in the *Festskrift* for Anders Donner which meant that in any case it would have had low visibility outside Finland. Furthermore, after the War, the situation in Finland itself was little better, the civil war of

[119] Original : "1. Un mécanisme imite les mouvements de la planète perturbatrice et de la planète perturbée de telle façon que leurs positions relatives dans l'espace sont indiquées à une échelle donnée par les positions relatives de certaines organes de la machine.
2. Selon les positions relatives de ces organes, d'autres organes doivent être contraints mécaniquement à indiquer par leurs positions relatives la grandeur des forces perturbatrices, et, selon la position de ces seconds organes, une troisième série d'organes doit indiquer les dérivées des perturbations.
3. En réunissant ces derniers organes à des planimètres, on obtient les perturbations, que l'on enregistre ensuite par un dispositif approprié pour des époques voulues.
4. A mesure que croissent ces perturbations enregistrées, la position de la planète perturbée doit être automatiquement corrigée dans la machine pour les valeurs des perturbations."

[120] Letter from Lindelöf to Mittag-Leffler, 2 December 1915. No. 82. Institut Mittag-Leffler.

[121] Carl Källman tells me that according to Sairo [1974] some parts of the machine have been constructed but neither Källman nor I have seen Sairo's book (which is in Finnish).

[122] Letter from Mittag-Leffler to Lindelöf, 8 January 1916. No. 5780. Institut Mittag-Leffler.

1918 having left the country in turmoil. Sundman himself appears not to have promoted the machine, making no further references to it in his work. As a result, the paper seems to have been completely overlooked for 35 years until the numerical analyst, E.J. Nyström, wrote a description of it for Sundman's obituary [Järnefelt, 1950, iv-v]. But by 1950 Sundman's idea had been superseded: Vannevar Bush's differential analyzer at M.I.T. had been built in 1931 and the world of mathematical machines had moved on. There was no need to resurrect Sundman's design.

10. Conclusion

Sundman's solution of the three-body problem is one of the significant mathematical achievements of the early 20th century. The fact that an almost unknown mathematical astronomer from Helsinki should be the author of such a purely mathematical result makes it all the more remarkable. However, Sundman's success cannot be seen in isolation from those around him. In particular, the central contribution of his colleague Ernst Lindelöf should not be underestimated. Lindelöf had precisely the right mathematical background to support Sundman in his endeavour, and, as Sundman gratefully acknowledged, he gave freely of his expertise. But Lindelöf did not only assist Sundman with mathematics, he also guided him through the publication process, being the essential go-between with Mittag-Leffler. It is even possible that by encouraging Sundman to apply for the Rosenberg grant, which he surely would have done, he was the initial catalyst for Sundman's research. Lindelöf had had the benefit of foreign travel and was well placed to provide Sundman with the appropriate introductions.

The mathematics in Sundman's first papers on the three-body problem [Sundman, 1907; 1909] is essentially little different from that in his final paper on the topic [Sundman, 1912] and yet the former passed by virtually unheeded while the latter was lauded and garnered a substantial prize. While it is certainly the case that *Acta SSF* was not a journal impatiently seized upon by mathematicians awaiting the very latest research in analysis, the fact that Sundman, despite his trip to France and Germany, was an unknown author was also a factor contributing to this initial lack of recognition. It is tempting to speculate how different this part of the story might have been had Poincaré rather than Sundman been the author of the original papers. The contrasting enthusiasm for Sundman's *Acta Mathematica* paper is not hard to understand. Sundman had "solved" a celebrated problem, one that had been attempted by many famous mathematicians, and his solution had been published in a distinguished international journal. Moreover, it was a journal that had already published one of the most celebrated articles on the three-body problem [Poincaré, 1890], and it was edited by one of mathematics' most active proponents. With Mittag-Leffler's support, Sundman's solution was assured exposure of the widest sort.

That the solution then sank largely from view was due both to Sundman's position and to his personality. He had produced a highly theoretical result but it was a result which had little by way of lasting appeal for the majority of mathematicians. Moreover, his work on the three-body problem took him out of his métier and the excursion was only a temporary one: he was a mathematical astronomer not a pure mathematician. External factors—the cataclysm of the First World War with its associated events (especially in

Finland) and the lessening of interest in celestial mechanics—too played their part, especially with respect to his paper on the perturbographe. It is little wonder that Sundman and his work slipped all but out of sight.

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