Oscar II's Prize Competition and the Error in Poincaré's Memoir on the Three Body Problem

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Introduction

In the autumn of 1890 HENRI POINCARÉ's memoir on the three body problem [1] was published in the journal *Acta Mathematica* as the winning entry in the international prize competition sponsored by OSCAR II, King of Sweden and Norway, to mark his 60^{th} birthday on January 21, 1889. Today POINCARÉ's published memoir is renowned both for providing the foundations for his celebrated three-volume *Méthodes Nouvelles de la Mécanique Céleste* [2] and for containing the first mathematical description of chaotic behavior in a dynamical system.

A combination of royal patronage and carefully planned public relations meant that the competition achieved the unusual distinction of gaining recognition that stretched well beyond the world of mathematics. However, despite appearances to the contrary, correspondence preserved at the Institut Mittag-Leffler reveals that the competition was in fact beleaguered by difficulties throughout. In particular, it has emerged that only weeks before the prize-winning memoir was due to be published POINCARÉ discovered an error in his work which was of such grave consequence that he was forced to make very substantial changes. Indeed it was only as a result of correcting the error that he discovered the existence of what today are known as *homoclinic points.* As a result the memoir which eventually appeared in *Acta* was remarkably different from the one which had actually won the prize almost two years earlier.

The following is an account of the troubled history of the competition together with an explanation of the error in POINCARÉ's memoir.

The competition

By the late 19^{th} century, mathematical prize competitions were a well established method for seeking solutions to specific mathematical problems. These competitions usually emanated from the national Academies, notably Berlin and Paris, and although the prizes offered were generally financial in nature, they were valued far more in terms of academic prestige. Nevertheless, although the existence of a mathematical competition at this time was no novelty, this particular one was somewhat unusual in that its sponsor, King OSCAR II, did not associate it with an institution; he chose rather to link it to an academic journal, albeit one in which he had a personal interest. That King OSCAR should have sponsored such a competition was not so surprising since he had himself studied mathematics at the university and had then become an active patron of the subject, providing financial support for various publishing enterprises, including the founding of *Acta*[1], as well as making awards to individual mathematicians.

Organisation of the competition

From its beginnings in 1884, the competition was organised by GÖSTA MITTAG-LEFFLER, then professor of pure mathematics at the newly established Stockholm Högskola (later the University of Stockholm) and founder and editor-in-chief of *Acta*. MITTAG-LEFFLER had obtained his doctorate from the university of Uppsala in 1872, and later had studied under CHARLES HERMITE in Paris, ERNST SCHERING in Göttingen and KARL WEIERSTRASS in Berlin. He therefore had first-hand experience of life within the premier mathematical communities in Europe and this, together with his involvement with *Acta*, meant that he was well placed to promote the idea of an international competition.

Apart from being a talented mathematician, MITTAG-LEFFLER was also a skilled communicator. He assiduously cultivated and nurtured mathematical contacts at home and abroad, both travelling extensively and maintaining a vigorous correspondence[2]. He had already established a good relationship with King OSCAR through the founding of *Acta* and it is not clear whether the idea of holding the competition came from MITTAG-LEFFLER or from the King himself. However, since MITTAG-LEFFLER enthusiastically embraced any opportunity to raise the profile of Scandinavian mathematics or indeed to enhance his own reputation within the mathematical community, it seems likely that the competition emerged as a consequence of his initiative.

The first known reference to the competition occurs in a letter from MITTAG-LEFFLER to the Russian mathemetician SONYA KOVALEVSKAYA written in June 1884, whose contents show that the topic was already under discussion. As the following extract outlining its proposed form makes clear, the competition was intended to be one of pre-eminent importance in the field of mathematical analysis and was originally intended as a regular event[3]:

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[1] The first issue of *Acta Mathematica* appeared at the end of 1882. See DOMAR [3].

[2] MITTAG-LEFFLER's considerable correspondence is preserved at the Institut Mittag-Leffler and is described in GRATTAN-GUINNESS [4].

[3] Extract from a letter dated 7.6.1884, Institut Mittag-Leffler (tr. from Swedish by S. NORGAARD).

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

1. *The main editor of Acta Mathematica*
2. *A German or Austrian mathematician* – = WEIERSTRASS
3. *A French or Belgian mathematician* – = HERMITE
4. *An English or American mathematician* – = CAYLEY*? or* SYLVESTER
5. *A Russian or Italian mathematician* – = *the first time* BRIOSCHI *or* TSCHEBYCHEV, *the second time Mrs* KOVALEVSKAYA.

After each prize-giving two of the prize judges should leave the jury and new ones should be appointed by King OSCAR *as long as he is alive – he must be able to appoint [replacements] for both the leaving members. After King* OSCAR*'s death, the three remaining must appoint two new members but always in such a way as to fit the categories mentioned above.* ..."

In the event, MITTAG-LEFFLER was unable to fulfil any one of King OSCAR's requirements exactly. The difficulties he faced are well illustrated by KOVALEVSKAYA's reply written while on holiday in Berlin[4]:

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

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[4] Extract from an undated letter, Institut Mittag-Leffler (tr. from French by R. COOKE). The letter is reproduced in full (in translation) in *The Mathematics of Sonya Kovalevskaya*, R. COOKE, 1984, Springer-Verlag, New York.

While there was an element of melodrama in KOVALEVSKAYA's letter (HERMITE and WEIERSTRASS certainly had a healthy respect for each other), for the most part her presentiments proved to be well founded. The eventual outcome was a commission comprised of only three: HERMITE, WEIERSTRASS, and MITTAG-LEFFLER himself[5]. Although MITTAG-LEFFLER had abandoned the proposal for a commission of five members, he had managed to engage two of the leading analysts of the day, one from each of the premier mathematical nations, and, importantly, two mathematicians with whom he had already established warm and productive friendships[6]. Nevertheless, despite the reduction in the number of people involved, the commission did still present certain practical difficulties. Apart from the obvious problems arising from the different geographical locations involved, Berlin, Paris and Stockholm, there was an additional complication engendered by the lack of a common first language.

With regard to the competition itself, King OSCAR was keen that it should consist of a single question, but his view was not shared by the commission who felt that a single question would both be too restrictive and preclude work of an innovatory nature. After an intensive correspondence a format was finally agreed: the competition would consist of four questions together with the opportunity to submit an entry on a self-selected topic but with priority being given to entries attempting one of the proposed questions.

In mid-1885 the official announcement of the competition was published in both German and French in *Acta*,[7a] as well as in several other languages elsewhere. It gave details of the prize (a gold medal together with a sum of 2,500 Crowns[8]), named the commission, listed the questions and stipulated the conditions of entry.

Although MITTAG-LEFFLER had originally proposed that HERMITE and WEIERSTRASS should each set two questions, it seems that of the four questions eventually set, the first three were proposed by WEIERSTRASS and the fourth by HERMITE[9]. The first one addressed the well-known n-body problem, reflecting

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[5] Part of the WEIERSTRASS to MITTAG-LEFFLER correspondence is published in [5]; the HERMITE to MITTAG-LEFFLER correspondence 1884–1891 is published in [6].

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

[7] *Acta* 7, p. I–VI;

[7a] *Nature*, 30 July 1885 and nine others listed in *Acta* **10**, p. 382.

[8] For comparison: DOMAR [3] cites MITTAG-LEFFLER's annual salary in 1882 as a professor in Stockholm as 7,000 Crowns, and *Nature* (February 21, 1889, 396), in the announcement of the competition result, states that at that time it was equivalent to £160.

[9] Letters from MITTAG-LEFFLER to HERMITE, 20.2.1885 and 6.3.1885, Institut Mittag-Leffler Nos. 356 and 374.

WEIERSTRASS' longstanding interest in the problem[10]; the second required a detailed analysis of FUCHS' theory of differential equations; the third asked for further investigation into the first order non-linear differential equations studied by BRIOT and BOUQUET; and the last concerned the study of algebraic relations connecting POINCARÉ's Fuchsian functions which have the same automorphism group.

The entries were to be sent to the chief editor of *Acta* before June 1, 1888, and, as was customary in such competitions, they were to be sent in anonymously, identifiable only by a motto and accompanied by a sealed envelope bearing the motto and containing the author's name and address. The entries were not to have been previously published and notice was given that the winning entry would be published in *Acta*.

Kronecker's criticism

Unfortunately, the announcement did not meet with universal approval. It provoked an angry reaction from another professor at the University in Berlin: LEOPOLD KRONECKER. KRONECKER, apparently incensed at being left out of the commission, wrote to MITTAG-LEFFLER with a catalogue of complaints[11]. But since it was no secret that an intense rivalry existed between himself and WEIERSTRASS, it is likely that he was more angry about WEIERSTRASS' inclusion than he was about his own exclusion[12].

KRONECKER accused MITTAG-LEFFLER of using the competition as a vehicle for advertising the *Acta*. Why had the competition not been proposed by the Swedish Academy? It was an accusation MITTAG-LEFFLER could easily refute: the King wished the competition to be announced in *Acta*, not only because *Acta* could claim a wider mathematical readership than the transactions of the Swedish Academy, but also because of his personal interest in the journal. On being challenged on the choice of members for the commission, MITTAG-LEFFLER explained to KRONECKER that his instructions had been to choose a commission of three, consisting of a representative from each of the top mathematical nations, Germany and France, with a third member from Sweden.

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[10] In a letter dated 15.8.1878, WEIERSTRASS told KOVALEVSKAYA that he had constructed a formal series expansion for solutions to the problem but was unable to prove convergence and in 1880/81 he gave a seminar on the problems of perturbation theory in astronomy. Despite WEIERSTRASS' own difficulties with the problem, certain remarks made by DIRICHLET in 1858 had led him to believe that a complete solution was possible, and hence his choice of the problem as one of the competition questions. WEIERSTRASS' interest in the problem is chronicled in [5].

[11] The contents of KRONECKER's letter to MITTAG-LEFFLER have been reconstructed from MITTAG-LEFFLER's reply written in July 1885, a copy of which is at the Institut Mittag-Leffler.

[12] WEIERSTRASS believed that KRONECKER's avowed antipathy to the work of GEORG CANTOR reflected KRONECKER's opposition to his own work. See BIERMANN [7].

With regard to the German representative, he told him that it had been a straight choice between him and WEIERSTRASS, both being equally suitable, but WEIERSTRASS, being some 8 years the elder, had been chosen on the grounds of his "venerable" age. This may have mollified KRONECKER but it is doubtful whether WEIERSTRASS would have been impressed by this line of reasoning. However, KRONECKER levelled his most serious charge at Question 4, the question set by HERMITE. He maintained that he was the best person to judge algebraic questions of this type and that he had already proved that the results required to resolve this question were impossible to achieve, and he threatened to tell the King as much[13]. As a defence, MITTAG-LEFFLER could only plead ignorance on behalf of the commission and concluded his reply with a barrage of flattery well calculated to appeal to KRONECKER's vanity[14].

KRONECKER then let matters rest, but indefinitely. In 1888 he launched another assault but this time it was directed at the wording of Question 1. On this occasion he did not write to MITTAG-LEFFLER but instead made his complaint at a meeting of the Berlin Academy of Sciences [8]. Since this time he had chosen to make his views public and the object of his censure was one of WEIERSTRASS' questions, the attack appeared to be a further manifestation of the rivalry between the two Germans as opposed to a critique of the commission. Nevertheless, since KRONECKER steadfastly maintained that he did not know who had composed the question, it was difficult for the commission to know how best to respond to him. Should they do so collectively and in the name of the commission, or should WEIERSTRASS personally take on the responsibility? HERMITE made it quite plain that he did not wish to be involved in the dispute. Not only did he consider the matter to be an entirely German affair between the "two princes of analysis", but also he considered it his patriotic duty to avoid doing anything that could be construed as having a national connection. He was convinced that KRONECKER was a committed Francophobe and so felt there was nothing to be gained by his intervention.

WEIERSTRASS made it clear that although he would have had no difficulty in dealing with KRONECKER's complaints he was reluctant to do so on his own since he considered the task a joint responsibility. After much deliberation the commission decided against an immediate response in the belief that it would be better to wait until the judging of the competition had been completed and the winning paper(s) published. It turned out to be a wise decision. Not only did subsequent events overshadow the issue but the need to reply was obviated by KRONECKER's death in 1891, shortly after the publication of the winning memoirs.

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[13] The question made reference to solutions of equations of the 5th degree and it appears that this was what KRONECKER objected to. See the letter from MITTAG-LEFFLER to HERMITE, 8.9.1885, Institut Mittag-Leffler No. 481.

[14] Shortly afterwards HERMITE met KRONECKER and told him that he had proposed Question 4. He explained his intentions in setting the question, admitting that he had set it specifically with POINCARÉ in mind. HERMITE's explanation seems to have satisfied KRONECKER as he did not pursue the issue. Perhaps it was sufficient that WEIERSTRASS was not involved. See [6, pp. 108–111].

The entries in the competition

Despite the fact that the identity of entrants was supposed to be secret, all three members of the commission were well aware that POINCARÉ meant to enter. As early as July 1887 POINCARÉ had made clear his intentions to MITTAG-LEFFLER, explicitly mentioning Question 1[15]. In October of the same year HERMITE told MITTAG-LEFFLER that although he knew POINCARÉ was working on an entry for the competition, he did not know whether POINCARÉ would submit it and, in any case, he was not sure whether POINCARÉ was working on Question 1 or Question 4. MITTAG-LEFFLER, still scarred from KRONECKER'S original attack, admitted to POINCARÉ that he hoped he would provide an answer to Question 4. In fact the selection of topics for the competition was such that it would have been possible for POINCARÉ to have worked on any one of them. This raises the question: had they all been chosen with POINCARÉ in mind? HERMITE freely admitted that this was the case with his question and perhaps WEIERSTRASS too had designed his questions to appeal particularly to POINCARÉ. Certainly MITTAG-LEFFLER was an unquestionable champion of POINCARÉ'S work[16]. In the event, POINCARÉ chose to attempt Question 1, the most difficult of the four.

By the closing date twelve entries had been received. Shortly afterwards a list of their titles, numbered in date order of submission, was published in *Acta* with the authors identified solely by their respective mottos[17]. Five of the entrants had attempted Question 1, one had attempted Question 3, and the remaining six had chosen their own topics.

When POINCARÉ'S entry arrived it was clear that his reading of the regulations had been somewhat perfunctory. As required he had inscribed his memoir with an epigraph[18], but instead of enclosing a sealed envelope containing his name, he had written and signed a covering letter, and had also sent a personal note to MITTAG-LEFFLER. However, since he had already told MITTAG-LEFFLER and HERMITE of his intention to enter, and he knew that they would recognise his entry by its content – it was an explicit development of his earlier work on differential equations – as well as by his handwriting, it clearly was not a deliberate attempt to flout the procedures.

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[15] A selection of POINCARÉ'S letters to MITTAG-LEFFLER concerning the competition are published in [9].

[16] MITTAG-LEFFLER secured POINCARÉ'S support for the launch of *Acta*, publishing important papers by him in each of the first five volumes.

[17] See *Acta* **11** (1888), 401–402. Apart from POINCARÉ'S entry it has only been possible to identify positively three of the other contestants: PAUL APPELL (entry No 8), GUY DE LONGCHAMPS (4) and JEAN EŚCARY (10).

[18] *Nunquam præscriptos transibunt sidera fines* = Nothing exceeds the limits of the stars.

Judging the entries

A large part of the judging was done by correspondence. MITTAG-LEFFLER, having received the entries in Stockholm, appointed one of the editors of *Acta*, EDVARD PHRAGMÉN, to do the preliminary reading prior to having copies of the most significant entries made and sent to HERMITE and WEIERSTRASS. Within a fortnight of the closing date MITTAG-LEFFLER had decided that there were only three entries worth considering, two from former students of HERMITE – POINCARÉ and APPELL – and one from Heidelberg,[19] although none of the three had provided a complete solution to any of the given questions.

MITTAG-LEFFLER spent August in Germany with WEIERSTRASS so that they could study the memoirs together. The following month he wrote to HERMITE to tell him that they thought that POINCARÉ should win, with APPELL being given an honorable mention. He made the point that POINCARÉ had the advantage in as much as he had at least attempted one of the set questions whereas APPELL had chosen his own topic: POINCARÉ had limited his investigations to a particular form of the three body problem (now known as the restricted three body problem) rather than the n body problem as specified in the question, while APPELL had considered the expansion of Abelian functions by trigonometric series. Meanwhile HERMITE had also been studying POINCARÉ's memoir and was equally convinced of the importance of the work.

The commission had quickly reached a unanimous decision but the hard part of their work had not begun. It was one thing to recognise the quality of POINCARÉ's work but quite another to understand it. POINCARÉ's entry was not only extremely long (when printed for *Acta* it amounted to 158 pages) but it also contained many new ideas and results. Furthermore, as HERMITE freely admitted in a letter to MITTAG-LEFFLER, the difficulties of comprehension were compunded by POINCARÉ's customary lack of detail [6, p.147][20]

> "... *But it must be acknowledged that in this work as in almost all his researches,* POINCARÉ *shows the way and gives the signs, but leaves much to be done to fill the gaps and complete his work.* PICARD *has often asked him for enlightenment and explanations on very important points in his articles in the Comptes Rendus, without being able to obtain anything except the response: "it is so, it is like that", so that he seems like a seer to whom truths appear in a bright light, but mostly to him alone. ...*".

All three members of the commission struggled with various parts of the memoir, but it was MITTAG-LEFFLER who, determined that the version submitted to the King should be as complete as possible, entered into correspondence with POINCARÉ – notwithstanding the rules of the competition whereby he should have been ignorant of the memoir's authorship – appealing for

[19] Entry No 5.
[20] Tr. JB-G.

clarification on several issues. POINCARÉ responded by producing a series of *Notes* which, when printed, added a further 93 pages to the memoir.

MITTAG-LEFFLER may have had no qualms about his contact with POINCARÉ but WEIERSTRASS was less happy about it and made a point of asking MITTAG-LEFFLER not to mention that he knew that POINCARÉ had entered the competition. He told MITTAG-LEFFLER that it was almost an axiom in Germany for prize papers to be published exactly in the form in which they were submitted, although he personally considered that the proper time for additions and corrections was when the paper was being edited for publication, providing they were clearly acknowledged.

Having worked through POINCARÉ's and APPELL's memoirs, it now only remained for the commission to produce their reports. WEIERSTRASS had the responsibility for writing that on POINCARÉ's paper, HERMITE the responsibility for writing that on APPELL's and MITTAG-LEFFLER the responsibility for writing a general report on the competition as a whole.

On the 20th January 1889, the day before the King's 60th birthday, MITTAG-LEFFLER went to the palace (having belatedly obtained from POINCARÉ the necessary sealed envelope) and the result was officially approved. The general report was published in the newspaper *Posttidningen* and MITTAG-LEFFLER wrote to POINCARÉ to tell him that he would be receiving an official copy of the report via the Swedish ambassador in Paris[21]. Almost everything had been completed to the King's satisfaction, only WEIERSTRASS' report on POINCARÉ's memoir was outstanding. WEIERSTRASS, as MITTAG-LEFFLER had feared, had been too unwell to fulfil his obligation within the allotted time, although he gave assurances that the report would soon be finished.

A priority dispute?

Unfortunately for MITTAG-LEFFLER, the publication of the general report signalled the start of a distressing polemic between himself and the astronomer HUGO GYLDÉN, a fellow lecturer at the Stockholm Högskola and a member of the editorial board of *Acta*. Although the report only contained a cursory indication of POINCARÉ's results, it was enough to convince GYLDÉN that he had anticipated POINCARÉ in a paper of his own [10] published some two years earlier.

MITTAG-LEFFLER appears to have had some idea of GYLDÉN's views almost immediately because only a matter of days after the report was published he wrote to POINCARÉ to ask him for his opinion on a result in GYLDÉN's paper. This result appeared to conflict with something that POINCARÉ had written. The particular point at issue concerned the convergence of certain power series: GYLDÉN had claimed that the series were definitely convergent while POINCARÉ

[21] A French translation of the report is published in POINCARÉ *Œuvres* **11**, pp. 286–289.

had stated that the evidence for convergence was inconclusive. POINCARÉ responded swiftly to MITTAG-LEFFLER but he admitted that he had found GYLDÉN's paper extremely hard to read. In order to give a definitive answer to the convergence question he would have to make a much more detailed study of it, a task he was reluctant to undertake. He was therefore unable to say whether GYLDÉN's method led to a proof of either convergence or divergence, although he believed divergence more likely[22]. He was also unhappy with the fact that GYLDÉN's method did not allow the successive terms in the expansion to be deduced recurrently but involved making choices at each stage of the calculation, a feature which incorporated an element of chance into the process.

Meanwhile, MITTAG-LEFFLER, at the King's request, had been due to give a review of POINCARÉ's paper at the February meeting of the Swedish Academy of Sciences. In the event illness prevented him from attending, although he had expressed reluctance to talk publicly about POINCARÉ's work without the support of WEIERSTRASS' report. GYLDÉN, on the other hand, did attend the meeting and, moreover, did talk about POINCARÉ's memoir. He declared his own position on POINCARÉ's results and effectively claimed priority.

Once again MITTAG-LEFFLER was placed in an awkward position. The King made it plain that he expected him to reply to GYLDÉN at the meeting the following month. MITTAG-LEFFLER knew he could not rely on having WEIERSTRASS' report in time and so it became a matter of urgency to have detailed comments on GYLDÉN's paper from POINCARÉ.

On hearing from MITTAG-LEFFLER about GYLDÉN's position, POINCARÉ responded again and at length. He made the point that the dispute brought into sharp focus the difference between mathematicians and astronomers with regard to their interpretation of convergence. He reasoned in detail against the rigour of GYLDÉN's method, reiterating that he believed it to rely heavily on questions of judgement, and, in his final letter on the subject, showed clearly why he believed that GYLDÉN's argument actually led to divergent series.

Despite the critical appearance of POINCARÉ's side of the correspondence, POINCARÉ did in fact maintain a high regard for GYLDÉN's work, appreciating the flexibility and practical advantages of his methods. He had not intended to demolish GYLDÉN but rather to show how words such as *proof* and *convergence* take on different meanings depending on whether the user is a mathematician or an astronomer. Moreover, he was sensitive to the fact that GYLDÉN's approach was coloured by a practical interest in the problem which he himself did not share.

HERMITE and WEIERSTRASS were also drawn into the polemic. HERMITE, who had first heard about the dispute from KOVALEVSKAYA, thought GYLDÉN's series were asymptotic but carefully avoided drawing a direct comparison between the two memoirs. He had himself received a letter from GYLDÉN but since it was written in Swedish he had been unable to read it, although he had deduced that it concerned the convergence question.

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[22] POINCARÉ's side of the correspondence is published in [9].

Meanwhile, MITTAG-LEFFLER gave his talk at the Academy, and wrote in jubilation to WEIERSTRASS and POINCARÉ, certain that those who had heard him had been convinced that POINCARÉ deserved the prize. However MITTAG-LEFFLER's feeling of triumph was short lived. The academic community in Stockholm decided to weigh in on the side of GYLDÉN, and, despite the fact that POINCARÉ's memoir was not in the public domain, adopted the view that GYLDÉN had indeed published proofs of everything POINCARÉ had done. The consensus was that MITTAG-LEFFLER's denial of GYLDÉN's results had been motivated by jealousy; this idea was reinforced by the mathematician ALBERT BÄCKLUND who drew attention to the fact that GYLDÉN's memoir had recently been awarded the St. Petersburg prize.

Meanwhile GYLDÉN steadfastly maintained that the values of the constants for which his series diverged formed only a countable set and so it was infinitely unlikely that the series was actually divergent. MITTAG-LEFFLER continued to argue against him since, with POINCARÉ, he believed that the series were divergent not just for a countable set but for a perfect set in the neighbourhood of these constants. Moreover, he told WEIERSTRASS that he thought GYLDÉN not enough of a mathematician to understand.

With the publication of POINCARÉ's memoir not scheduled for several months, the controversy gradually died down. Nevertheless, when the memoir finally appeared, GYLDÉN did attempt to reopen the debate by writing directly to HERMITE. Possibly he thought he could count on HERMITE's support since HERMITE was known to share his interest in the applications of elliptic function theory in celestial mechanics. But HERMITE was not to be drawn. He stood by the judgement of the commission, declaring his loyalty to MITTAG-LEFFLER and WEIERSTRASS. As he indicated later to MITTAG-LEFFLER [6, pp. 195–196], he was not impressed by GYLDÉN's grasp of analysis, describing GYLDÉN as a ghost from a bygone age, who had been left behind as the world of analysis transformed about him[23].

Discovery of the error

MITTAG-LEFFLER's dispute with GYLDÉN paled into insignificance when compared with the problem which subsequently emerged. MITTAG-LEFFLER, having allowed time for editing, had hoped to have the volume of *Acta* containing the winning memoirs published by October 1889. Apart from WEIERSTRASS' report, for which he had continued to press, the actual printing was completed by the

[23] In the context of MITTAG-LEFFLER's problems over the competition, it is of interest to record that in May 1889 GYLDÉN met with KRONECKER in Berlin, a meeting which MITTAG-LEFFLER would surely have viewed with some misgiving. In any case, the occasion prompted MITTAG-LEFFLER to remark to WEIERSTRASS that although he had been led to believe that his two adversaries had understood each other perfectly, he suspected that GYLDÉN really understood as little of KRONECKER as KRONECKER understood of GYLDÉN.

end of November. But the volume did not appear until the end of the following year and when it did, it did not contain a replica of the memoir POINCARÉ had submitted to the competition.

The first glimmer that anything was awry occurred in July 1889. PHRAGMÉN, who was editing POINCARÉ's memoir for publication, alerted MITTAG-LEFFLER to some passages in it which seemed to him a little obscure. Thus prompted, MITTAG-LEFFLER wrote to POINCARÉ for yet further clarification. POINCARÉ, in the course of dealing with PHRAGMÉN's queries, realised that he had made a serious error in a different part of the paper. At the beginning of December, he wrote to MITTAG-LEFFLER. Making no attempt to conceal his distress, he told him that he had written to PHRAGMÉN to tell him of the error, the consequences of which were more far-reaching than he first thought, and as a result he was having to make substantial changes to the memoir[24].

This was most unwelcome news for MITTAG-LEFFLER for, although the volume of *Acta* had not been published, a limited number of printed copies of the memoir had been circulated. Once more MITTAG-LEFFLER's carefully nurtured mathematical reputation was in jeopardy. Despite his confidence in the overall quality of the memoir, he was only too conscious of the damage he personally might suffer should the scale of the error become public. Nevertheless, while total secrecy was impossible, he knew that if he could secure the return of the printed copies, then at least there would be no evidence to substantiate any rumours which might circulate. But while he knew to whom the copies had been sent, securing their return would not be easy since several had been dispatched to foreign destinations. Apart from those sent to HERMITE and WEIERSTRASS, the recipients included KOVALEVSKAYA, JORDAN, VON DYCK, GYLDÉN, LINDSTEDT, LIE and HILL[25].

MITTAG-LEFFLER also suggested to POINCARÉ that everything concerning the error should be kept between themselves, at least until publication of the new memoir. To safeguard himself still further, he gave POINCARÉ detailed instructions about what he wanted in the contents of the introduction to the reworked memoir in order to ensure that no details of the error were included. In addition, he also asked POINCARÉ to pay for the printing of the original version – a request to which POINCARÉ agreed without demur, despite the fact that the bill came to just over 3,500 Crowns, which was some 1,000 Crowns more than the prize he had won.

PHRAGMÉN's role in setting POINCARÉ on the trail of an error which had escaped the attention of the entire commission was certainly worthy of recognition. However, and characteristically, MITTAG-LEFFLER did not see it in his

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[24] The letter is given in full in the last section of this paper.

[25] These names are listed by PHRAGMÉN in a note to MITTAG-LEFFLER.

Despite the difficulties, MITTAG-LEFFLER appears to have been tireless in his efforts to retrieve these pre-publication copies of the memoir. In the library of the Institut Mittag-Leffler there is one inscribed in MITTAG-LEFFLER's hand with the phrase which in Swedish reads "*whole edition destroyed*".

interests to make a public acknowledgement of PHRAGMÉN's participation. Nevertheless, he did ask POINCARÉ for written support to help PHRAGMÉN in his attempt to secure the chair in mechanics at the university in Stockholm, and PHRAGMÉN was promoted to the editorial board of *Acta* in the following year. In November that year PHRAGMÉN himself revealed his interest in POINCARÉ's memoir by publishing a paper [11] which showed that some of POINCARÉ's results could be applied to dynamical problems other than the restricted three body problem.

Publication of the winning entries

By the beginning of January 1890 POINCARÉ had completed his reworking of the memoir and had sent a copy to PHRAGMÉN for editing. Not only had he made substantial alterations to accommodate the corrections arising from the error but also, where appropriate, he had incorporated the explanatory *Notes* into the paper itself. Thus in two quite distinct ways the memoir took on a significantly different appearance to that of its predecessor.

Although printing began at the end of April that year, a backlog of other work meant that it was not completed until the middle of November. When Volume 13 of *Acta* eventually appeared it contained both POINCARÉ's and APPELL's memoirs together with HERMITE's report on the latter.

WEIERSTRASS' report on POINCARÉ's revised memoir was promised for a future volume but it was never completed. Prior to the discovery of the error WEIERSTRASS had got as far as writing the introduction and in March 1889 had sent it to MITTAG-LEFFLER. However, it was only concerned with general issues connected with the question as set, and made no specific references to POINCARÉ's memoir[26].

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

Thus, over a year later than MITTAG-LEFFLER had originally planned, the climax to the competition, the publication of the winning entries in *Acta*, finally took place. More than six years had elapsed since MITTAG-LEFFLER had written optimistically to KOVALEVSKAYA with the original plans for the competition. Despite KOVALEVSKAYA's foreboding, MITTAG-LEFFLER could

[26] Since the introduction was not invalidated by the discovery of the error, MITTAG-LEFFLER later selected it to appear as part of an article [5] which focused on WEIERSTRASS' interest in the n body problem.

scarcely have foreseen the turbulent course of events which was to follow. Nevertheless, in the final analysis MITTAG-LEFFLER's considerable efforts were rewarded. Once the *Acta* volume was in circulation, the rumours of the error faded and the brilliance of POINCARÉ's memoir was freely acknowledged. MITTAG-LEFFLER's hope that the competition would result in some important new mathematics had been amply fulfilled. POINCARÉ's memoir had ensured that King OSCAR's 60th birthday celebration would not be forgotten.

The error in Poincaré's memoir

In the published memoir (henceforth referred to as [P2]; the first printed version being referred to as [P1]) POINCARÉ's only mention of the error is a passing reference in the introduction which, although including an acknowledgment to PHRAGMÉN, gives no indication of its nature nor of the extent of his alterations. Nevertheless, although POINCARÉ's original competition entry remains untraced, the discovery of his personally amended copy of [P1], which is preserved at the Institut Mittag-Leffler and which in its altered form corresponds almost exactly to [P2], has made it possible to follow completely the changes which POINCARÉ made.

In essence the memoir is the culmination of several strands of POINCARÉ's work from the previous decade. In it he brought together and applied a whole host of ideas and techniques which he had previously developed, many of which had originated in his pioneering research on the qualitative theory of differential equations [12]. Although in the first part of the memoir, which is concerned with the development of the underlying theory, his approach was essentially geometrical, he adopted a methodology which incorporated the consideration of complementary geometrical and analytical theory, with the result that the error too occurred in complementary forms: geometrically in the theory of invariant integrals and then analytically in the theory of asymptotic solutions. Its full implications become clear in the second part of the memoir when POINCARÉ deals with the application of the theory to systems with two degrees of freedom, such as those which describe the restricted three body problem.

Although the areas of the memoir in which the error occurred can be pin-pointed quite precisely, since there are in the heart of POINCARÉ's mathematical theory, a few preliminary remarks are helpful in order to put the error properly into context.

First, what was the actual problem POINCARÉ was trying to solve? The competition question had asked for a solution to the *n* body problem. That is, if *n* particles move in space under their mutual gravitational attraction and with known initial conditions, is it possible to determine their subsequent motion? Previous attempts to solve this problem had generally centered on the specific case of the three body problem since this was the first case which could not be solved exactly. The three body problem is in fact a complicated non-linear problem and has a long and well documented history of its own (see for example WHITTAKER [13] and MARCOLONGO [14]). Apart from its intrinsic appeal as a problem simple to state, a strong motivation for its study has been its relationship with the fundamental question of the stability of the solar system, a question in which POINCARÉ himself was particularly interested.

However, although POINCARÉ intended to tackle the general three-body problem, the inherent difficulties associated with it led him to focus his attention on a particular simplified form of the problem now known as the restricted three body problem[27]. In this case two of the bodies (the primaries) revolve around their centre of mass in circular orbits under the influence of their mutual gravitational attraction, while the third body (the planetoid), assumed massless with respect to the other two bodies, moves in the plane defined by the two primaries and, while being gravitationally influenced by them, exerts no influence of its own. The restricted problem is then to ascertain the motion of the planetoid.

In POINCARÉ's formulation of the problem the position of the planetoid in phase space is described by two linear and two angular variables, x_i and y_i respectively, y_i being periodic with period 2π, connected by the integral $F(x_1, x_2, y_1, y_2) = C$. He put the differential equations into Hamiltonian form,

$$\frac{dx_i}{dt} = \frac{\partial F}{\partial y_i}, \qquad \frac{dy_i}{dt} = -\frac{\partial F}{\partial x_i}, \qquad (i = 1, 2)$$

which, in accordance with his qualitative theory developed in [12], he regarded as defining flows on a three-dimensional surface.

His brilliant insight, introduced in [12], was to recognise that rather than considering the flow in the entire three-dimensional space, it was much more convenient to consider the first return map induced by the flow on a two-dimensional surface of section S transverse to the flow. This map is defined by choosing a point M on S at which S is intersected by a flow line, the image of M under the map is then the point M' at which that flow line first intersects S again. Thus in the three-dimensional space a periodic solution corresponds to a closed curve, but under the map a 2π periodic solution corresponds to a fixed point and a $2\pi k$ periodic solution corresponds to a cycle of period k.

Of particular importance in the memoir is the chapter in which POINCARÉ discusses his theory of periodic solutions and which includes his discovery of asymptotic solutions. These are solutions which slowly either approach or move away from an unstable (generating) periodic solution. He showed that in the three-dimensional solution space of the restricted problem, these asymptotic solutions generate families of curves which fill out surfaces and which asymptotically approach the curve representing the generating periodic solution, and that these surfaces correspond to curves in the transverse section. In order to try to gain an understanding of the behaviour of these asymptotic solutions, POINCARÉ investigated the nature of the curves on the transverse section; it was this investigation which required his theory of invariant integrals.

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[27] POINCARÉ was the first to use the term 'problème restreint' [2, III, p. 69].

With respect to the analytic part of POINCARÉ's theory, the parameter which he used to form the power series expansions of the solutions to the differential equations in the restricted problem was μ, the mass of the smaller of the two primaries. POINCARÉ chose μ as the parameter because when $\mu = 0$ the problem reduces to a pair of two body problems. This meant that he was able to employ the strategy of starting with a particular solution for which $\mu = 0$ and then vary μ analytically to see if solutions existed for very small values of μ.

Invariant integrals

Although POINCARÉ was not the first to recognise the existence and value of invariant integrals – they are earlier encountered in the work of both LIOUVILLE [15] and BOLTZMANN [16] – he was the first to formalise a theory centred on the concept. In an earlier paper [17] he had used the idea of a particular invariant integral within the context of a problem concerning the stability of the solutions of differential equations. In the memoir he considered the whole concept in a broader sense, developing a general theory which revealed that the existence of an invariant integral is a fundamental property of Hamiltonian systems of differential equations. In particular, he showed that it is a property of the system of differential equations which describes the restricted three body problem.

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

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

together with the incompressibility condition

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

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

He concluded his discussion of invariant integrals with a series of theorems characterized by their geometric nature. These theorems include one of his most celebrated results: the original formulation of his recurrence theorem [1, p. 314][28].

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[28] Briefly, POINCARÉ considered the case where there are three degrees of freedom and the flow is volume preserving, and he supposed that there was a moving point P which remained bounded under the flow. He then proved, albeit somewhat informally, that at some future time the point P will return arbitrarily close to its initial situation and will do so infinitely often.

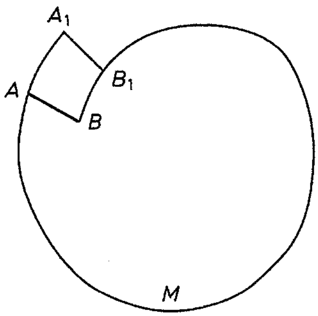

Figure 1.

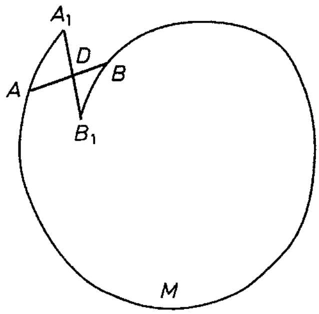

Figure 2.

This series of theorems provides the geometrical framework for the later analysis, the qualitative study giving an insight into the global behaviour of the system. It was in one of these theorems, the Corollary to Theorem III in [P1], that the fundamental error in his geometry occurred. In effect POINCARÉ failed to take proper account of the exact geometric nature of a particular curve.

Before discussing the relevant theorems, that is Theorem III [P1] and its Corollary and Theorem III [P2], one definition from [P1] (which does not appear in [P2]) in needed. In Theorem III [P1] POINCARÉ uses a term, *quasi-closed*, which is important with regard to the error, although unfortunately his definition is not altogether clear. He said that an n^{th} order curve, by which he meant a curve coincident with its n^{th} iterate, was quasi-closed if there were two points A and B on it which were separated by a finite arc but whose distance apart was very small of p^{th} order.

In Theorem III [P1] POINCARÉ proved that if an invariant curve C was quasi-closed such that the distance between the points A and B was also very small of n^{th} order (very small of n^{th} order being precisely defined) and there was an invariant integral, then the distance between the point A and its iterate A_1 and that between the point B and its iterate B_1 were also very small of n^{th} order. In proving the theorem POINCARÉ referred to Figures 1 and 2 and his argument, which hinged on an application of the triangle inequality, showed that the configuration given in Figure 2, as opposed to that given in Figure 1, was correct.

Then in the Corollary to Theorem III [P1] POINCARÉ claimed that in the case where an invariant curve C is thought to be quasi-closed because the distance AB is known to be very small of n^{th} order at least and the distance AA_1 is known to be finite or small of $(n-1)^{\text{th}}$ order *at most*, and there is an invariant integral, then in fact the curve C is not quasi-closed but it is actually *closed*. He gave no proof, simply observing that if the curve was only quasi-closed then the distance AA_1 would have to be of n^{th} order. What he did not explore was the possibility that the curve rather than being closed might be self-intersecting. This was where he made his mistake. In essence he failed to take into account the full range of possibilities consistent with the constraint of

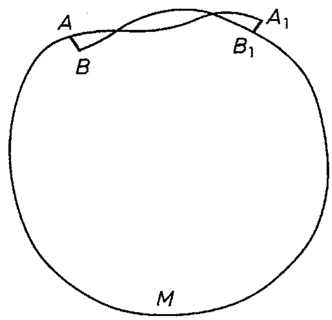

Figure 3.

area-preservation imposed by the existence of the invariant integral. Although he knew that the area inside the curve had to remain constant and independent of the iterative process, he focused on a single iteration and appears not to have investigated the possible outcomes engendered by the extension of the iterative process. As he later realised and showed in Theorem III [P2], the concept consistent with area preservation was not closure but self-intersection.

POINCARÉ set up Theorem III [P2] in essentially the same way as the Corollary to Theorem III [P1]. He then showed that of the possible hypotheses:

1. The two arcs AA_1 and BB_1 intersect each other.
2. The curvilinear quadrilateral AA_1B_1B is such that the four arcs which comprise its sides do not have a point in common except for the four corners A, A_1, B, B_1 (as in Figure 1).
3. The two arcs AB and A_1B_1 intersect each other at a point D (as in Figure 2).
4. One of the arcs AB or A_1B_1 intersects one of the arcs AA_1 or BB_1; but the arcs AA_1 and BB_1 do not intersect each other, neither do the arcs AB and A_1B_1.

He found that he could eliminate the second and the fourth hypotheses because they failed the condition of area preservation; in both cases the area AMB was not equal to the area A_1MB_1, although in order to prove the latter case he had to do some additional juggling with the arcs. He also found that the third hypothesis was unacceptable because it implied that the distance AA_1 must be a quantity of n^{th} order (as in Theorem III [P1]) and not a distance of $(n-1)^{\text{th}}$ order at most as specified. Thus he could conclude that the first hypothesis, that the curve was self-intersecting, was the only one possible.

POINCARÉ did not include a diagram, but the correct form of the curve is shown in Figure 3. The reason the correct curve has to take this slightly complicated shape with two crossing points is to allow for more than one iteration. If after one iteration the curve had only one crossing point, then any subsequent iteration would violate the condition of area preservation.

Asymptotic solutions

The error was then reflected in POINCARÉ's analytical description of asymptotic solutions which appears at the end of the chapter on periodic solutions. A major concern in this chapter is the stability of the solutions; particularly important in this connection is POINCARÉ's recognition of the significance of certain constants which he called *characteristic exponents.* POINCARÉ's insight was to realise that it was the form of the characteristic exponents which indicated the stability of the solutions. He saw that if the characteristic exponents were purely imaginary, then the solution was stable, otherwise it was unstable. However, as he discovered, the role of characteristic exponents in the generation of asymptotic solutions meant that they turned out to have special significance with regard to the error.

In [P1] when POINCARÉ calculated the series expansions for the asymptotic solutions, he claimed that when the differential equations depended on the parameter μ, the series could be expanded in powers of μ or $\sqrt{\mu}$, according to the circumstances, although nowhere did he prove that such expansions were actually possible. Furthermore, implicit in his claim was that these series were convergent, although again he gave no proof. Particularly significant with regard to the error is the fact that he did not distinguish between autonomous and nonautonomous systems of differential equations. He wrongly assumed that in both cases the series were convergent.

In [P2] POINCARÉ gave careful consideration to the convergence question, and found that in the case of the series for the asymptotic solutions there were certain necessary conditions for convergence, one of which was that when $\mu = 0$, all the characteristic exponents have to be distinct. But when he calculated the characteristic exponents as series expansions in powers of the parameter μ, he discovered that in the particular case of an autonomous Hamiltonian system, all the characteristic exponents are zero when $\mu = 0$, and furthermore, they cannot be expanded in integer powers of μ, but instead have to be expanded in powers of $\sqrt{\mu}$. This necessarily implied, contrary to what he had previously believed, that the series for the asymptotic solutions to the restricted three body problem were divergent, and furthermore that they were series in powers of $\sqrt{\mu}$ rather than μ[29]. He then went on to show that these divergent series belong to that special class of series which are now known as asymptotic series, and which he himself had earlier defined in [19].

[29] POINCARÉ was not the first to form series in powers of the square root of the parameter. As he acknowledged in his introduction to [P2], series of this type occur in BOHLIN's paper of 1888 [18], where they are used to overcome the problem of small divisors in planetary perturbation theory. POINCARÉ later made a detailed examination of BOHLIN's series in the second volume of his *Méthodes Nouvelles* [2].

The error's implications

In POINCARÉ's geometric representation of the restricted problem, a generating unstable periodic solution and its accompanying family of asymptotic solutions are represented in the three dimensional solution space by a closed curve and two asymptotic surfaces. In order to understand the behaviour of these asymptotic solutions, POINCARÉ sought the exact equations for these asymptotic surfaces. He first noted that it was possible to move from one surface to the other by changing the sign of the parameter μ in the equations for the surfaces. Thus by making such a sign change it is possible to generate the second surface from the first. Furthermore, since these two surfaces cut another, they can be considered together as two sides of the same surface. This surface will then have the special feature of a double curve and it is this double curve which identifies the particular series which satisfy the equations for the asymptotic surfaces.

The equations of these surfaces are of the form

$$\frac{x_2}{x_1} = f(y_1, y_2)$$

where x_1 and x_2 are given by the asymptotic series

$$x_1 = s_1(y_1, y_2, \sqrt{\mu}), \quad x_2 = s_2(y_1, y_2, \sqrt{\mu}) \ .$$

In order to calculate these equations exactly, POINCARÉ proceeded in three stages. In the first stage he calculated the first two coefficients which, since the series were in powers of $\sqrt{\mu}$, gave an approximation with an error of the order of μ. In the next stage he considered a larger, but finite, number of coefficients, which give an error of the order μ^p for any fixed p, no matter how large. In the final stage he calculated the exact equations.

He began by supposing that the series could be written

$$x_i = x_i^0 + \sqrt{\mu}x_i^1 + \mu x_i^2 + \ldots$$

where the x_i^j are functions of y_1 and y_2, which would give

$$\frac{x_2}{x_1} = \frac{x_2^0 + x_2^1\sqrt{\mu}}{x_1^0 + x_1^1\sqrt{\mu}}$$

as the first approximation for the equation of the surface trajectories.

Having calculated the coefficients, he found that this first approximation, which was of the form $a + b\sqrt{\mu}$, depended upon the choice of a certain constant, C_1 and a certain finite periodic function of y_2 which he labelled $[F_1]$.

In order to identify the particular surfaces described by the approximation POINCARÉ considered their intersections with the transverse section S defined by the surface $y_1 = 0$. Since the position of a point P on the surface S is defined by the coordinates (x_2/x_1) and y_2, which are analogous to polar coordinates, the curves $(x_2/x_1) = constant$ are closed concentric curves on the surface S and the position of a point P on S is unchanged when y_2 is increased by 2π.

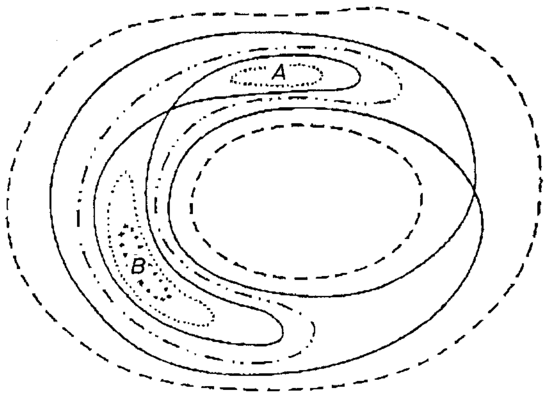

Figure 4.

He therefore constructed a set of curves for different values of the constant C_1, the values for C_1 having been determined by his choice for the function $[F_1]$. As he illustrated in Figure 4 [1, p. 419], since each one of these curves lies in the plane $y_1 = 0$, as y_1 is varied from 0 through to 2π, each curve sweeps out a surface. More precisely, if a line defined by the equations $y_2 = constant$, $(x_2/x_1) = constant$ is drawn through each point on an arbitrary one of these curves, then the set of all these lines constitutes a closed surface.

The choice for the function $[F_1]$ also determines the number of periodic solutions to the differential equations. In this instance, due to POINCARÉ's choice for the function, there are four, two stable and two unstable. On the surface S the stable solutions are represented by fixed points and the unstable by double curves, and it is therefore the latter which represent the first approximation. In Figure 4, the stable solutions correspond to the two fixed points A and B, and the unstable ones correspond to the two double curves represented by the unbroken lines.

The purpose of the second approximation, which only appeared in [P2], was to determine some arbitrary but finite number of coefficients of the series for the asymptotic solutions. Since POINCARÉ had originally believed the series to be convergent rather than asymptotic, there was no equivalent discussion in [P1]. However, most of the second approximation was in fact taken from *Note F* (entitled *Asymptotic Surfaces*) which POINCARÉ had added to [P1] because he had wanted to include an analytic description of the asymptotic surfaces to complement his geometric one. In [P1], since he believed that the asymptotic surfaces could be represented by series in $\sqrt{\mu}$ which were convergent for arbitrary values of y_1 and y_2, providing μ was sufficiently small, he thought that his calculations in *Note F* gave a full description of the entire series. In [P2] he wrote the approximate equations for the asymptotic surfaces as (divergent) asymptotic series.

However, it is in the final approximation – in which POINCARÉ constructed the asymptotic surfaces exactly, or rather their intersection with the transverse

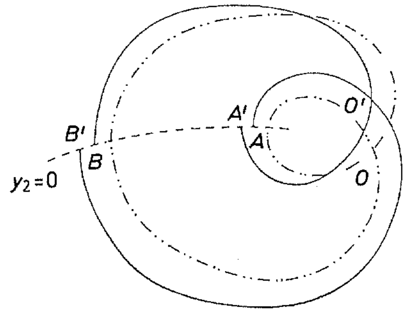

Figure 5.

section $y_1 = 0$ – that the differences between [P1] and [P2] are most dramatic. In both [P1] and [P2] he used Figure 5 [1, 438] to illustrate his construction.

In [P1] the plain lines $AO'B'$ and $A'O'B$ represent the two asymptotic surfaces which cut the surface $y_1 = 0$, and the dashed line represents the curve $y_1 = y_2 = 0$. The dotted and dashed line, which is a closed curve with a double point at O, represents the curves $y_1 = 0$, $(x_2/x_1) = a + b\sqrt{\mu}$. The generating (unstable) periodic solution is represented by a closed trajectory cutting the surface $y_1 = 0$ at the point O', and the distance OO' is of order μ.

POINCARÉ used his results from the end of the first approximation to infer, first, that the curve $BO'B'$ was quasi-closed and, second, that the distance of the point B to its iterate was of the order of $\sqrt{\mu}$. Appealing to the (invalid) Corollary to Theorem III he concluded (erroneously) that the curve $BO'B'$ was closed. Then, having invoked the same argument to conclude that the curve $AO'A'$ was closed, he was led to the mistaken result that the asymptotic surfaces were closed. Furthermore, inherent in this result was the implication of stability in the sense that the solutions remained confined to a given region of space.

Thus, in [P1] POINCARÉ believed he had proved that for sufficiently small values of the parameter μ there was, relative to a given unstable periodic solution, a set of asymptotic solutions which could be considered stable, that these solutions were well behaved, and that they could be completely understood. His analysis in [P2] led to a very different conclusion.

Although in [P2] he again referred to Figure 5, he now considered the dotted and dashed line to represent the curves with equations derived from the first p terms in the series for the asymptotic solutions. The question he then asked was whether the curves $AO'B'$ and $A'O'B$ were necessarily closed. He knew for certain that they would have been if the series for the asymptotic solutions had been convergent. In this case, the plain curves would have differed as little as required from the dotted and dashed curves, since the distance from a point on the former to a point on the latter would have tended to zero as p increased indefinitely. But were the curves $AO'B'$ and $A'O'B$ necessarily closed even though the series were divergent? Consideration of the specific example of a simple pendulum weakly coupled to a linear oscillator gave him the answer.

He found that in this particular case the curves were not closed. He had therefore shown that closure was not an inherent feature. Nevertheless, as he had learnt from his work on invariant integrals, lack of closure did not rule out the possibility of intersection. Thus the question he now asked was whether it was possible for the curves $O'B$ and $O'B'$ to intersect. For if this should occur, any trajectory which passed through the point of intersection would simultaneously belong to both sides of the asymptotic surface. In other words if C is the closed trajectory which passes through the point O' and represents the periodic solution, then a trajectory passing through the point of intersection would begin, when t is very large and negative, by being very close to the closed trajectory C, and it would then asymptotically move away, deviating greatly from C, before asymptotically reapproaching C as t becomes very large and positive. By showing that the system satisfied the conditions of Theorem III [P2], POINCARÉ was able to show that such trajectories, which he called *doubly asymptotic trajectories*, did indeed exist, and moreover that there were in fact an infinite number of them. POINCARÉ later called these trajectories *homoclinic trajectories* and the points of intersection are now known as *homoclinic points*[30].

This is arguably the first mathematical description of chaotic motion within a dynamical system. Although POINCARÉ drew little attention to the complexity of the behaviour he had discovered and made no attempt to draw a diagram, he was profoundly disturbed by his discovery as he revealed in a letter (postmarked 1.12.1889) to MITTAG-LEFFLER[31]:

"*I have written this morning to* M. PHRAGMÉN *to tell him of an error I have made and doubtless he has shown you my letter. But the consequences of this error are more serious than I first thought. It is not true that the asymptotic surfaces are closed, at least in the sense which I originally intended. What is true is that if both sides of this surface are considered (which I still believe are connected to each other) they intersect along an infinite number of asymptotic trajectories (and moreover that their distance becomes infinitely small of order higher than μ^p however great the order of p).*

I had thought all these asymptotic curves, having moved away from a closed curve representing a periodic solution, would then asymptotically approach the same closed curve. What is true is that there are an infinity which enjoy this property.

I will not conceal from you the distress this has caused me. In the first place, I do not know if you will still think that the results which remain, namely the existence of periodic solutions, the asymptotic solutions, the theory of characteristic exponents, the non-existence of single-valued integrals, and the divergence of LINDSTEDT'*s series, deserve the great reward you have given them.*

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30 POINCARÉ first used the word 'homocline' in [14, III, p. 384].

31 Tr. JB-G.

On the other hand, many changes have become necessary and I do not know if you can begin to print the memoir; I have telegraphed PHRAGMÉN.

In any case, I can do no more than to confess my confusion to a friend as loyal as you. I will write to you at length when I can see things more clearly."

Perhaps a further indication of POINCARÉ's concern and confusion at the discovery of the strange behaviour exhibited by these solutions can be detected in the fact that when he described them in [P2] he made very little comment about their complexity, nor did he draw attention to them in his introduction. Of course, this may well have been due to the fact that he felt unable to do so without mentioning the error (which MITTAG-LEFFLER had asked him not to do), or because he had had too little time in which to assess the implications of his discovery. Nevertheless, it is notable that an interval of almost ten years elapsed before the publication in 1899 of the third volume of his *Mécanique Céleste* [2] in which he reconsidered the question again, this time adding the now well known remark about these doubly asymptotic solutions [2, III, p. 389][32].

"*When one tries to depict the figure formed by these two curves and their infinity of intersections, each of which corresponds to a doubly asymptotic solution, these intersections form a kind of net, web, or infinitely tight mesh; neither of the two curves can ever intersect itself, but must fold back on itself in a very complex way in order to intersect all the links of the mesh infinitely often.*

One is struck by the complexity of this figure which I am not even attempting to draw. Nothing can give us a better idea of the complexity of the three-body problem and in general all the problems of dynamics where there is no single-valued integral and BOHLIN's *series diverge.*"

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