Chapter 3

1910 – 1919

1910-01-01

To D. Hilbert [⟨1⟩] **— 1.I.1910** **Paris**

Dear Geheimrat [Mein lieber Herr Geheimrat]

Warmest wishes to you and to your dear spouse for the new year, for your health and for your scientific work.

I am staying here during the winter holidays with my brother, the geologist. Unfortunately my wife couldn't accompany me. In the middle of January my lectures start again, and I will return.

The good relations with Mr. Schoenflies have been restored, I am certain, mostly through your intervention. I enclose his last two letters, to which I have answered that I am satisfied with his last version and that I consider the matter settled.

May I add a few remarks about the univocal [⟨2⟩] (not necessarily (1-1) [⟨3⟩]) continuous mapping of a sphere κ onto a sphere λ? If one imposes the condition that it is *both ways* continuous, then it is a (1-1) continuous image of a rational function of the complex variable. By the condition of continuity both ways, I mean that a closed Jordan curve around a point L of λ, that converges to L, for each point K of κ that has L as image will correspond to a closed Jordan curve around K that converges to K.

If we now have *two* of these maps satisfying these conditions from a sphere (or a more general closed surface) K to a sphere L and to a sphere

[⟨1⟩] No addressee; from the text it follows that the letter was addressed to Hilbert; see also Freudenthal's remark in *CW II*, p. 425. [⟨2⟩] single-valued; in letter *eindeutig*. [⟨3⟩] in letter *ein-eindeutig*.

D. van Dalen, *The Selected Correspondence of L.E.J. Brouwer*, Sources and Studies in the History of Mathematics and Physical Sciences, DOI 10.1007/978-0-85729-537-8_3,

M, then the question arises which additional conditions must be satisfied in order to conclude that the correspondence between L and M is a complex algebraic one in the sense of Analysis Situs. Returning to the general one way univocal and continuous correspondence between two spheres, for each of those a finite number n as its *degree* can be given, in such a way that all relations of the same degree, and only these, can be transformed continuously into each other. In particular, all correspondences of the n^{th} degree can be transformed continuously into rational functions of the n^{th} degree in the complex variable.

To define this degree we introduce homogeneous coordinates x, y, z on κ, and homogeneous coordinates ξ, η, ζ on λ and then we consider the injective mapping that is domain-wise determined by a correspondence

$$\xi : \eta : \zeta = f_1(x, y, z) : f_2(x, y, z) : f_3(x, y, z),$$

where f_1, f_2, f_3 are polynomials.

Next we assume a positive orientation on both spheres, and we choose in each point of κ this positive orientation, then each point in general position of κ occurs p times with positive orientation and q times with negative orientation. Then one can show that for each point of λ in general position $p - q$ is a constant, which we will call the *degree* of the mapping.

If the correspondence between x, y, z and ξ, η, ζ is not determined by polynomials, then one can approximate it by such polynomial correspondences, and it is easy to show that these approximating correspondences have a constant degree, which we can also assign to the limit correspondence. This degree is always a finite positive or negative number. In particular, a (1-1) ⟨4⟩ continuous transformation of the sphere into itself will have degree $+1$ if it doesn't change the orientation, and -1 otherwise.

Now you know my theorem that each (1-1) continuous transformation of the sphere into itself that does *not* change the orientation will always have at least one fixed point. This theorem can be extended in the following manner, namely that each *univocal* continuous transformation of the sphere into itself whose degree is *not* -1, will always have at least one fixed point.

And I have succeeded to extend the theorem in this form to the n-dimensional sphere. There it reads as follows: Each univocal continuous transformation of the n-dimensional sphere into itself has at least one fixed point. The exception is for odd n the transformations of degree $+1$, and for even n the transformations of degree -1.

⟨4⟩ Brouwer in the margin: 'stricter formulation'.

(1-1) transformations therefore have necessarily a fixed point, either for odd n and reverse orientation, or for even n and unchanged orientation. Even more general is the result for univocal continuous transformations of the interior of the n-dimensional sphere into itself, for these have anyway a fixed point.

Once more best wishes and greetings for you both

Your ever revering ⟨5⟩
L.E.J. Brouwer

[Autograph draft – in Brouwer; also in *CW II*, p. 420 ff.]

1910-01-04

To J. Hadamard — 4.I.1910 **Paris** ⟨6⟩
6 Rue de l'Abbé de l'Epée

Dear Sir [Cher Monsieur]

I can at present communicate to you several extensions of the fixed point theorem for (1-1) continuous transformation of the sphere. They concern univocal ⟨7⟩ continuous transformations of the sphere. To such a transformation one can assign a finite number n as its *degree*. Starting from a transformation of degree n, one can obtain by means of continuous variations each other transformation of degree n, but no others. In particular one can always obtain a rational transformation of degree n of the complex sphere.

To determine this *degree*, let us introduce homogeneous coordinates (in the double sense). Write x, y, z for the original sphere and ξ, η, ζ for the image, divide the sphere into a finite number of regions and first consider transformations defined by the relations:

$$\xi : \eta : \zeta = f_1(x, y, z) : f_2(x, y, z) : f_3(x, y, z),$$

where f_1, f_2, f_3 are polynomials, which might well be different for different regions of the sphere. Let us call this transformation a polynomial transformation. Let us define an indicatrix on the sphere: then every point P

⟨5⟩ *Ihres immer verehrenden.* ⟨6⟩ Draft without addressee. In view of earlier correspondence, and topic, Hadamard is clearly the recipient; letter also in *CWII*, pp. 426–427, with Freudenthal's comments. ⟨7⟩ single-valued.

of the image *in general position* occurs a number of r_p times with positive indicatrix, and a number of s_p times with negative indicatrix. One shows that $r_p - s_p$ is a constant: that is the degree of the polynomial transformation.

Let us return to the general univocal and continuous transformation. It can be approximated by a series of polynomial transformations; one shows that the latter all have the same degree. This is furthermore the degree of the limit transformation.

The degree is always a finite positive or negative integer. The degree of the (1-1) transformation is $+1$ if the indicator stays the same, and -1 if it is reversed.

Now the generalized fixed point theorem becomes the following: Each univocal and continuous transformation of the sphere into itself for which the degree is not -1 has at least one invariant point.

Moreover, I have extended this theorem to spheres of m dimensions. It is then stated in the following manner: Each univocal and continuous transformation of the m-dimensional sphere into itself contains at least one fixed point, except when *a*) m is odd and the degree n equals $+1$, *b*) when m is even and the degree n equals -1.

In particular when the transformation is (1-1) ⟨8⟩ there exists at least one fixed point a) if m is odd and the indicatrix is reversed and b) is m is even and the indicatrix is invariant.

For the volume of an m-dimensional sphere in the space of $m + 1$ dimensions (if we mean the sphere itself by it) I have lately succeeded in establishing a still more general result, to wit: every continuous univocal (not necessarily biunivocal) transformation of the volume of an m-dimensional sphere into itself possesses at least one fixed point.

On the general vector distributions of the sphere soon two more articles ⟨9⟩ by my hand will appear, where I study several questions connected with the principle of Dirichlet and with the decomposition of a field in a part that is 'source free' ⟨10⟩ and a part that is 'rotation free'. ⟨11⟩ For this I first establish the most general form that tangent curves (or characteristics, after Poincaré) can assume. As the main result of the first article one must consider the property that every characteristic that does not approach a singular point is a spiral whose two limit cycles are characteristics themselves. The property that the existence of at least one singular point is necessary, is basically nothing but an extra corollary, on which I would not have insisted,

⟨8⟩ Brouwer's remark in text: 'stricter formulation.' ⟨9⟩ [Brouwer 1910d, Brouwer 1910e].
⟨10⟩ *'quellenfrei'.* ⟨11⟩ *'wirbelfrei'.*

were it not that it was the first result that could be simply formulated, and also because to me there seems to be an close relation between this theorem and the one about the fixed point of the sphere, a relation that is nowhere clarified, except in your correspondence. In the second article I have inserted your beautiful direct and more complete proof of the existence of at least one singular point.

My address will be in Paris until January 15. Maybe there is an opportunity to meet you?

Sincerely yours, ⟨12⟩

L.E.J. Brouwer

[Signed draft/copy – in Brouwer]

1910-03-18

To D. Hilbert — 18.III.1910 **Amsterdam**

Dear Geheimrat, [`Sehr geehrter Herr Geheimrat`]

A few months ago I cited in an article that appeared in the Amsterdam Proceedings, ⟨13⟩ my small Annals note on 'Transformations of surfaces into themselves' ⟨14⟩ as to be found in volume 68. Meanwhile part 3 of this volume is already appearing now, so that probably my note will not get its turn in volume 68.

Could you perhaps arrange it so, without disturbing the regular course, that my note, which in print is only 4 to 5 pages, finds a place in part 4 of volume 68? Because of the above mentioned citation this please me very much, and I would be greatly indebted to you.

Just in case that you are away on holiday and this letter doesn't reach you in time, I am writing to Mr. Blumenthal in the same vein.

In July I hope to have the opportunity to come to Göttingen for a while.

Apart from a new group theoretic communication, ⟨15⟩ I am preparing an article to be submitted to the editors of the Annalen. In this article I solve the problem of invariance of dimension insofar that I prove that in any

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⟨12⟩ *Agréez, monsieur, mes salutations distinguées.* ⟨13⟩ *KNAW, Proceedings.* ⟨14⟩ *Über Transformationen von Flächen in sich* [Brouwer 1910g]. ⟨15⟩ [Brouwer 1910c].

case spaces of even and odd dimension cannot be continuously and one-one mapped onto each other.

In the Amsterdam communications [16] I continue my work on transformations of surfaces into themselves and on continuous vector fields.

Recently I studied your article about Dirichlet's principle and am very much interested in the sequel that you mentioned to me last year. [17]

Also on behalf of my wife I wish you all the best for the coming Easter, and a cordial 'auf Wiedersehen'

Ever your revering [18]
L.E.J. Brouwer.

[Signed autograph – in Hilbert; fragment 'Apart from . . . each other' also in *CWII*, p. 429]

1910-09-00

To D.J. Korteweg — late summer 1910 [19]

Now that the course is long enough behind me, and calm thinking back and complete consideration of the matter has been possible, I have come to the unshakable conviction that lecturing without having been invested with authority, as I have done in the past course (of which I have a wretched recollection). is tantamount to throwing my energy into a pit and I cannot and may not prolong that, and extending my duties, as implied by the proposal discussed recently with de Vries, is a fortiori completely excluded. Already before we started this experiment last year, I had, as you know, these negative expectations about it, but I thought that given your opinion, I had to suspend mine. However, now that the result has not vindicated you, I again carry the full responsibility for my position. This position is determined by the fact that I was only prepared to accept an appointment as privaatdocent if the point of departure was that the interest of the university demanded an expansion of mathematical teaching, and that I would undertake that task for free as long as the authorities wouldn't consent to

[16] *KNAW, proceedings.* [17] The discussions in October 1909 between Brouwer and Hilbert in Scheveningen, see [Van Dalen 1999], p. 128. [18] *Ihres immer verehrenden.* [19] Handwritten draft of a letter by Brouwer; the addressee has to be Korteweg; undated – sometime during, or after, the summer of 1910 is a fair guess; Brouwer had been a privaatdocent for one year.

the creation of a new post. In that case, however, there would be in the granting of this task a hint to the authorities, which would be fittingly emphasized by a compulsory examination [20] (which really would not violate the law any more than a compulsory examination by a professor), and this would be the more fitting because almost all other disciplines of our faculty have seen a substantial personnel expansion in the last years, even without there being a vacancy for the purpose. [21] I think that neither Schoorl, nor Cohen, nor de Meyere, nor Zeeman, at their first appearance as lecturer in Amsterdam, were appointed in an existing vacancy.

Thus I feel compelled to tighten up my attitude as follows: I am only *then* prepared to continue my activities at the university if the authorities are seriously urged to create a lecturer position for me, if need be with meagre pay, which automatically would carry with it compulsory examinations; and moreover if, in case of proven refusal of the authorities in this matter, you would make an examination with me compulsory. If you cannot cooperate in this, then *sans rancune*, but then I will end the sad enterprise that has disrupted the harmony of my life for a year.

[Handwritten draft – in Brouwer.]

Editorial supplement

[The following is a draft of a letter possibly written after a discussion with Korteweg following the above letter. It would be dated also sometime in October - November 1910.]

> A couple of the points, touched upon this afternoon, make me once more pick up the pen. First the remark that I don't learn anything from my lectures — on the contrary —, and that consequently the time spent on them is wasted as long as I have no guarantees that at least the students learn something from it. A remark of yours this afternoon forces me to reconsider the present situation from another point of view.

[20] The central courses had examinations at the end of term. These examinations were called *tentamens*, they did not have the same legal status the final exams had. A course without such a tentamen was not taken serious by students and staff. Hence Brouwer's insistence on this mark of recognition. [21] Scratched out by Brouwer: 'When Cohen as assistant of Bakhuis Roozeboom, Schoorl as assistant of de Bruyn, de Meyere as assistant of Sluyter was appointed; also when Zeeman was appointed as lecturer, there wasn't a vacancy, I believe.'

One of the reasons that has induced me to initiate the matter, was the hope that eventually the reinforcement of the mathematical teaching staff in our university will become possible and that an extraordinary professorship will come to me, and in your work; though it has become fairly clear to me, being passed over in Delft, that there is no prospect for me there if I do not show first what I can do as teacher. In that case I seem, in the present manner, to be putting the cart before the horse. For I can't expect that you come to hear and evaluate my lectures; so you will have to get your impressions of the quality of my teaching from the members of the audience, who because of their 'don't-work-for-it' attitude must think it abnormally difficult; if I understood you correctly this afternoon, such information has reached you already, and you seem to have attached some value to it.

In contrast to this, I would argue that those who have attended my lectures at my request, people with experience of lectures in mathematics, and who certainly would have held up to me the unvarnished truth if they had cause to, have stated that they never attended such clear lectures. But whatever is the case, 'show what I might be as teacher' I certainly cannot do in the present circumstances, I can only damage my reputation as such in an undesirable and wrong way.

This point of view could only be eliminated, if either you or de Vries or any other person who was completely competent in my eyes would come to my lectures under some kind of pretext to hear and judge.

For the rest, I'm still convinced that when a person of any scientific value is teaching badly, it is always because of indifference, never because of incompetence.

your
L.E.J. Brouwer

I enclose 2 reprints.

[Signed autograph draft – in Brouwer]

1911-00-00

To O. Blumenthal — 1911 [22]

Let us now imagine T_n to be a ring surface in three dimensional space, ϑ to be a contractible closed Jordan curve on T_n, A the simply connected domain determined by ϑ on T_n, B the complement in T_n. Then by the definition of linked varieties, [23] Γ will certainly intersect A (resp. A'), because ϑ can be contracted to a point inside A. Inside B however ϑ *cannot* be contracted to a single point. The existence of intersection points of Γ with B is consequently *not certain*. If then furthermore the invariance of dimension is *not* certain, Γ could be completely contained in A, and would then constitute only *one single domain* α. This domain α would possess *in that case no boundary* that one could approximate by *'point pairs'* T_p *that are linked with* T_n.

For the mentioned simple case one can by the way also easily prove the existence of intersection points of Γ with B (resp. B'). To achieve the same thing for arbitrary n and p and arbitrary T_n, one must prove that the closed polyhedral manifolds Γ always have an even number of intersections with $T_n = A' + B'$. But it seems to me that to carry out the proof of this 'evident' fact is extremely laborious. Actually a similar difficulty occurs already in the justification of the *definition* of the linked varieties for arbitrary p and n.

Hence my position about the second part of the note of Lebesgue is that he has quite correctly proved a very beautiful theorem for three dimensional space, but that for higher dimensional spaces he only has stated 'evident' extensions without proving anything. However, one precisely needs the invariance proof for the higher dimensional spaces; so in my opinion the mentioned part of Lebesgue's note doesn't contain anything at all pertinent to the invariance.

Best greeting

Your
L.E.J. Brouwer

[Autograph draft – in Brouwer; also reproduced as [Y5] in Brouwer *CW II*, p. 452]

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[22] Last page of a draft letter, part of the correspondence concerning the Lebesgue affair. Clearly O. Blumenthal is the addressee. In view of the available correspondence and the publications on the dimension invariance, the draft dates back to 1911. [23] In text everywhere: *variétés enlacées*.

1911-03-00a

L.E.J. Brouwer - *note on Lebesgue's proof* — **III.1911**[a] **Amsterdam**[24]

[Handwritten remark in the margin:] 'Accepted, Hilbert'.

Remark on the invariance proof of mr. Lebesgue
by L.E.J. Brouwer at Amsterdam

The proof of invariance of the dimension number, given by Mr. Lebesgue on page 166–168 of this volume, contains a gap on page 187, lines 6–13. Namely, from the property, that I_{h-1} extends from any manifold $X_i = X_i^0$ to any manifold $X_i = X_i^0 + 2l, (i = h, h+1, \ldots, n)$, one cannot immediately conclude that I_h extends from each manifold $X_i = X_i^o$ to each manifold $X_i = X_i^0 + 2l, (i = h+1, h+2, \ldots, n)$. Hence the existence of all I_p is not certain. In any case, considerable further considerations are required here.

Concerning the arguments of Mr. Baire, which Mr. Lebesgue used, the unproved theorems to which the problems are reduced there, lie deeper than the problem itself.

[Typescript – in Brouwer]

Editorial supplements

O. Blumenthal to D. Hilbert — 27.X.1910 *Aachen*[25]

> In the vacation we have made a very nice trip to Paris; however, I have unfortunately not seen mathematicians, they were all still on holiday. I did nonetheless get acquainted with Lebesgue, who happened to be in Paris. He is a very interesting man, and he told me that he is already for a long time in the possession of not one, but of several proofs of the invariance of the dimension number, which Brouwer has proved now in the Annalen.[26] He has sent me one of these proofs

[24] On this sheet a handwritten letter by H. Lebesgue, see *Lebesgue to Blumenthal III.1911.* See also the remark at the end of *Blumenthal to Brouwer, 28.III.1911.*
[25] Transcription, only of the part of the letter with relevance to Brouwer and Lebesgue is reproduced here. [26] Brouwer submitted the paper in June 1910, he lectured on the theorem in a meeting of the Dutch mathematics society in October 1910; the paper was published in 1911.

for the Annalen, which looks very clever. I have not scrutinized it in detail for the correctness of the proof, but only for the correctness of the idea. For the details one can trust such a sharp-witted man. But if you want to carry out a detailed check, the article is at your disposal.

[Signed typescript – in Hilbert]

O. Blumenthal to D. Hilbert — 14.III.1911 *Aachen*[27]

I find the matter Brouwer-Lebesgue highly unpleasant, and in fact I am completely on Lebesgue's side. That means the following: Lebesgue says explicitly that he accepts certain theorems as proven; these theorems refer to certain linear equations and inequalities, and these can certainly be proved; in other words, the problem seems to me not to lie in these equations, and the whole set-up of the proof of Lebesgue is in my opinion an altogether passable and beautiful road to reach the dimension proof. But anyone reading Brouwer's note will not get that idea at all; the note is in my opinion phrased in an unfriendly and unpleasant manner. Therefore I had planned to ask Brouwer to withdraw the note for the time being, particularly because volume 70 will be completed only in the middle of May, so there is no hurry at all. Moreover, I am personally (just like Lebesgue (according to an earlier communication)) not able to understand Brouwer's proof, and I consider it very well possible that there are several gaps there as well.

For these reasons I would think it best not to accept Brouwer's note for the time being, but to ask him to wait in any case till the end of the volume, and then also to phrase the note in a completely different tone. In the case of an emergency, i.e. if he doesn't accept this, the editors could add an objective note, just like Noether has done very successfully in the Sannia-Zinder conflict.

Please tell me your views. I am willing to negotiate with Brouwer, however I believe that an intervention from your side would carry more weight.

[Signed typescript – in Hilbert]

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[27] Transcription, only of the part of the letter related to Brouwer and Lebesgue.

1911-03-00b

H. Lebesgue to Blumenthal — III.1911[b] **Paris** [28]

If I understand the remark of Mr. Brouwer correctly, it amounts to this: I have announced that I was going to accept facts that I qualified as quite evident, and that doesn't replace a proof of these facts.

On this point I agree with Mr. Brouwer, I merely add that if I haven't written out my proof completely, it is only because I have promised already for some time an article on this topic to the Secretary of the Société Mathématique de France.

I willingly admit that my phrasing is quite poor, because Mr. Brouwer has been able to believe that I didn't see the necessity of proving all of it, and that he now thinks it useful to point out this necessity to other readers.

H. Lebesgue

[Signed autograph – in Brouwer]

1911-03-25

From O. Blumenthal — 25.III.1911 **Aachen**

Dear Mr. Brouwer! [`Sehr geehrter Herr Brouwer`]

Allow me a few words in the matter of your dispute with Lebesgue. To begin with, that I have informed Mr. Hilbert of the considerations that I will put to you, that he agrees with me and that he has asked me to negotiate with you in the name of the editors of the Annalen.

First, I can inform you that the last issue of the present volume of the Annalen will be published only towards the end of May. Hence there is no hurry for you to submit your note against Lebesgue, but you can take your

[28] No addressee. The letter is written on a typed document '*Bemerkung zu dem Invarianzbeweis des Herrn Lebesgue*', dated March 1911, which had been submitted to Blumenthal. The latter must have passed it on to Lebesgue for comments. Lebesgue returned the document with his evasive comments. It is reasonable to date it March 1911. See also the remark at the end of the letter *Blumenthal to Brouwer 28.III.1911*.

time for that until further correspondence with Lebesgue has cleared up the matter.

Indeed, your haste in publishing this remark raises the suspicion that you don't expect anything from further discussion with Lebesgue. This assumption would however be unjustified, for on the one hand it appears from the letter of Lebesgue to you that he had a clear conception of the proof of the provisionally assumed theorems, on the other hand he himself writes to me literally: 'Writing the proof in detail does not take very long and I am about do so, but really, it seems impossible to make my results come out piecemeal in this fashion, and I think that your readers, more generous than Mr. Brouwer, would be willing to give me credit until my definitive memoir appears.' ⟨29⟩

To me it seems to follow from this statement that it wouldn't be right to publish your note before you have ascertained that not only Lebesgue's short note in the Annalen, but really his *entire method of proof* is deficient. I am convinced that Lebesgue will make his manuscript, which he has finished, available to you for checking. If necessary I would be willing to mediate in this sense. I would in fact like to point out to you — and now I come to the core of my proposal — that *your note is phrased in a very rude form*, and that everybody will necessarily interpret it as saying that the gaps emphasized by you cannot be filled, which means that you consider the proof of Lebesgue *false*, because false and incomplete are in this case the same. In my opinion, however, you can only make this reproach to a man of Lebesgue's importance if you are entirely certain of your case.

So I would like to ask you again urgently to reconsider the matter concerning your remark once more. If you insist to publish it, I will of course do so, but then I will of course ask Lebesgue as well to send me his new manuscript, which I will then publish as soon as possible.

Finally, let me point out to you again something also stressed by Lebesgue, that *nobody doubts or contests your priority for this fundamental proof*. The priority belongs no doubt to the one who publishes first. But that your note was already there in print, when Lebesgue wrote his one, is clear from his text. So Lebesgue is in his own opinion and in that of the world not your rival, but your follower. So I think you should by all means leave him the time to present his proof in extenso.

By the way, Lebesgue has withdrawn the reply that he sent to you for the Annalen, and he has submitted to me another one, which I enclose. Please return it to me. I find this second text just as incomprehensible for

⟨29⟩ Lebesgue is quoted in French in the letter.

the uninitiated as the first one, and I would ask Lebesgue for a different formulation if it is comes to that.

Summarizing once more my view, it is as follows: in the way you have written your remark, it will generally be understood that you consider Lebesgue's proof irreparably false, more precisely that you believe that the theorems assumed to be true by Lebesgue constitute essentially the core of the whole proof. Publishing such a remark seems to me appropriate only then, if you are positively convinced that Lebesgue does not possess the missing completions. So I advise you to publish nothing about the matter for the time being. When, however, you have acquired that conviction, and when you can prove it, then the Annalen will of course be gladly at your disposition. Because then a warning for Lebesgue is ⟨30⟩ in the general interest.

I hope I have formulated this letter correctly in all its parts, and that it doesn't lead to misunderstandings, which can easily happen in a complicated situation. My aim is merely of a conciliatory nature, I don't favor one party, at the very least I want to guarantee factual correctness; but, if possible, I would like to spare the Annalen an unnecessary polemic, so I advise you to strike only when you are certain of the deficiency of Lebesgue's proof.

The correspondence that you have sent to Hilbert, I return hereby. Also the one copy of your note with the answer of Lebesgue. ⟨31⟩

Sincerely yours
O. Blumenthal

[Signed typescript – in Brouwer]

1911-03-27

To O. Blumenthal —27.III.1911 **Blaricum**

Dear Professor [`Sehr geehrter Herr Professor`]

Immediately after Mr. Lebesgue had informed me that he had prepared a complete version of his proof, I have informed him and Mr. Hilbert that I would withdraw my submitted remark; I have at the same time just asked

⟨30⟩ Blumenthal wrote first 'is' and then 'would be' but didn't cross out one of the two
⟨31⟩ See note *Brouwer, III.1911* and note *Lebesgue III.1911.*

Mr. Lebesgue to be so kind send me his elaborated version for my information.

In my opinion it would be most desirable that Mr. Lebesgue would publish the additions to his proof in the 70th volume of the Annalen, because every reader will be baffled at the place in question, and it seems to me that the deficiency of the argument is not sufficiently stressed by the footnote.

I very much appreciate the explanation in your letter; for the rest please believe me that the submission of my remarks was determined exclusively by scientific reasons, and not by any self-seeking motives. Also because the readers of the Annalen could have read something different in the text, I am glad to consider myself relieved from this disagreeable duty.

Sincerely yours,

L.E.J. Brouwer

[Signed autograph, copy – in Brouwer]

1911-03-31

To D. Hilbert — 31.III.1911 **Blaricum**

Göttingen dated 1.4.1911
To Blumenthal for [his] information, with the request to be so kind to send the letters back to Brouwer by registered mail. With best greetings Hilbert.

Dear Geheimrat, [`Lieber Herr Geheimrat`]

Enclosed I send you for your kind information the continuation of my exchange of letters with Mr. Lebesgue, in the course of which I have withdrawn my submitted remark.

This withdrawal pleased me very much, because the letters of Lebesgue (as well as those of Blumenthal later) showed me that my remark was interpreted by Lebesgue as a priority charge, which wasn't at all what I intended.

To me it remains inexplicable why Lebesgue doesn't want to bring the elaboration contained in his last letter (the contents of which, by the way, remained obscure to me after a first glance) to the notice of the readers of the Annalen.

Yesterday I conversed very pleasantly a couple of hours with Weyl; maybe I see him again today.

With the best greetings.

Your
L.E.J. Brouwer.

[Signed autograph – in Hilbert (signed draft in Brouwer)]

1911-05-09

To O. Blumenthal — 9.V.1911 **Blaricum** ⟨32⟩

Dear Professor [`Sehr geehrter Herr Professor`]

The situation with Mr. Lebesgue is now as follows:

1) It is impossible for me to discuss things further with him, because it is the second time now he has lost sight of politeness towards me.

2) In his latest letter he takes back his earlier statement that he had worked out a complete version of his proof in the Annalen. ⟨33⟩ The elaborations he announced to me earlier as such and which he sent to me, he now calls a 'hasty formulation' ⟨34⟩, for which he rejects every responsibility.

3) After the abandonment of this statement of Lebesgue, which was the ground for the withdrawal of my Annalen "Remark", I consider it my duty to resubmit my note.

4) The considerations that Lebesgue earlier called the 'complete version' are riddled with false conclusions, and they are beyond repair.

5) Recently Lebesgue has published in the Comptes Rendus a second proof which is likewise irreparably wrong. He still clings to the correctness of that, notwithstanding what I communicated with him (compare the paragraph of his letter that is marked in pencil).

6) I consider the submission of the enclosed "Remark", as I already remarked, as my duty, but as a disagreeable duty. When the editors would

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⟨32⟩ The handwriting is not Brouwer's. As Brouwer mentions a possible trip to Limburg (close to Aachen), the recipient must be Blumenthal. The '*Sehr geehrter Herr Professor*' indicates that Brouwer observed a measure of formality, called for by the content of the letter. Moreover, the letter would probably be passed on to Hilbert. ⟨33⟩ [Lebesgue 1911a]. ⟨34⟩ *rédaction hâtine.*

consider the publication not in the general interest, I would derive from this judgement the liberty to withdraw it.

7) My "remark" has been cast in a strictly objective formulation, and it remains silent about our exchange of letters; I would ask you to demand, in the possible answer from Lebesgue, the same from him.

I enclose the final part of the exchange of letters with Lebesgue.

I will probably come to Limburg within a few days; maybe I can see you? In that case I might learn from you what you think of my submitted article, better than in writing.

Sincerely yours,
(was signed) L.E.J. Brouwer

[Signed autograph, copy – in Brouwer]

Editorial supplement

[*The following document is most likely the Bemerkung that the above letter refers to. It is dated May 1911. The text is an expansion of the note of March 1911.*]

Bemerkung zu den Invarianzbeweisen des Herrn Lebesgue ⟨35⟩

von L.E.J. Brouwer in Amsterdam

In the derivation of the invariance of dimension, which Mr. Lebesgue communicated in Vol. 70 of the Mathematische Annalen (p. 166–168), certain facts are assumed as 'quite evident'. ⟨36⟩ I have to remark that the justification of these 'evident' properties constitute the kernel of the whole proof.

In the Comptes Rendus (vol. 152, p. 841, March 27, 1911) the same author has developed a second method, where he uses the following lemma: 'For every regular closed manifold T_n that lies in a R_{n+p+1}, there exists an arbitrary small T_p that is linked with T_n.' From this it is inferred that T_n is not everywhere dense in R_{n+p+1}.

⟨35⟩ A comment on the proofs of invariance of Mr. Lebesgue. ⟨36⟩ *bien évidents.*

Now there are two cases possible with respect to this argument.

Either R_{n+p+1} designates a Cartesian space; but then it is clear without any lemma, that T_n cannot fill R_{n+p+1} everywhere dense, because T_n is a closed and R_{n+p+1} an open manifold; consequently this trivial property is of no significance for the solution of the invariance problem.

Or R_{n+p+1} designates a regular space in the general sense, and we must understand the lemma as follows: 'In every regular closed manifold T_n that lies in a R_{n+p+1} there exists an arbitrary small manifold T_p, which is linked with T_n in a certain neighborhood.' But then the proof of Lebesgue of this lemma implicitly presupposes the invariance of dimension, because in the case that T_n fills R_{n+p+1} everywhere dense, there may not be such open domains α in Γ, whose boundaries are used.

Finally, at the end of Lebesgue's proof in the Annalen some elaborations of Mr. Baire are adduced, but contrary to what Mr. Lebesgue states, these do not essentially solve the problem, but merely elucidate its connection with deeper theorems.

[Autograph manuscript – in Brouwer]

1911-06-11

To O. Blumenthal [37] **— 11.VI.1911** **Amsterdam**

Dear Sir, [`Cher Monsieur`]

You have made clear to me that in the proof of invariance of dimension of a space I might have made the work for the reader lighter by prefacing the reasoning with a succinct explanation of the main ideas.

My proof is the rigorous elaboration of the following principles:

Let K be a q-dimensional cube, lying in a q-dimensional space E_q, with a center denoted by M, and a boundary denoted by F. If there would be a continuous (1-1) correspondence between E_q and a $(q+h)$-dimensional space E_{q+h}, such that K corresponds to a set k, and M to a point m of

[37] The document does not show the addressee, but from the content it is clear that it was written to Blumenthal. The letter was written in French, so that Blumenthal could forward it to Lebesgue.

the space E_{q+h}, each polyhedral set p of dimension q, resulting from a small continuous deformation of k, will be nowhere dense in E_{q+h}. Hence there exists in the space E_q a set P corresponding to p, which is nowhere dense in E_q, and which results from a small continuous deformation of K.

Next I show that each set π resulting from a small continuous deformation of K is everywhere dense in the neighborhood of M. First I show this for polyhedral sets π of dimension q; hence the same property ensues for more general sets π to which P belongs.

Preserving all details of this proof one can modify it slightly, considering the images f and $\digamma$ of F that belong to p and to π respectively, and then prove by that method that M is separated by $\digamma$ from infinity, whereas evidently m is not separated from infinity by f.

It seems to me that this is the modified proof that Mr. Lebesgue had in mind in the first part of his Note in the Comptes Rendus of March 27, 1911.

As for two other proofs published by Mr. Lebesgue, they hardly, in my opinion, merit that name.

In the one of the Mathematische Annalen, which you have published following mine, Mr. Lebesgue bases himself on certain facts said to be 'quite evident', suggesting that they are very simple properties whose proofs can be left to the reader. Mr. Lebesgue has as a matter of fact affirmed to me that this was indeed his idea, adding that it would suffice to project every I_p on the manifold $(x_2 = x_2^0, x_3 = x_3^0, \ldots, x_{p+1} = x_{p+1}^0)$, to deduce the existence of I_{p+1} from that of I_p.

I believe that Mr. Lebesgue is mistaken; that the proof of these facts constitutes a separate problem, which is more difficult than that of the invariance.

With respect to the second proof of the Comptes Rendus of March 27,1911, [38] it contains a vicious circle, in it the invariance is tacitly assumed as proven. Actually, if one isn't certain of invariance, it could happen that T_n fills Γ and that there exists no boundary at all of the domain α at a finite distance of T_n.

If E_{n+p+1} would refer exclusively to Cartesian spaces one could remedy this mistake and choose the manifold Γ in a special way. Indeed, if E_{n+p+1} is a Cartesian space, the reasoning that deduces from the lemma about the linked manifolds the theorem '*that T_n doesn't fill E_{n+p+1}*', and from there the invariance, doesn't make sense, because T_n is a closed manifold and E_{n+p+1} an open manifold, from which it follows immediately that T_n doesn't fill E_{n+p+1}, which is trivial and without importance for the invariance proof.

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[38] [Lebesgue 1911b].

Hence one must assume that E_{n+p+1} can be a closed manifold, but then the vicious circle cannot be cured.

I cannot finish this letter without expressing my regret that the correspondence with Mr. Lebesgue, as a result of his article in the Mathematische Annalen, could not get us to agree. But I don't see that I was in any way wrong in this matter. This is what happened: Mr. Lebesgue publishes a proof of invariance where he assumes certain facts as 'quite evident'; I find that I don't see the evidence, and I turn to the author, who—having written it—must be assumed to understand it, and who has the scientific duty to explain himself to the first reader who asks for it. However, when Mr. Lebesgue answered me, not only did he not say anything precise about the question, but he also admits that he never worked out the requested argument. So Mr. Lebesgue didn't have the right to use the term 'quite evident', and in the volume of the Mathematische Annalen containing his article, a rectification would be necessary indeed. To that purpose I proposed to Mr. Lebesgue a remark by me, by the way leaving him the choice if he preferred another form. Mr. Lebesgue, getting angry, informs me that he has no objection at all against my remark.

A little later Mr. Lebesgue starts corresponding again and declares formally that he now possesses a complete version of the proof of the properties in question. That changed everything completely, I withdraw my remark from the Mathematische Annalen, and I ask Mr. Lebesgue to send me this complete version. Mr. Lebesgue complies with my request, and I study his argument several times, but it remains obscure to me, and moreover contains nothing that anybody can't see right away. I ask for new explications: Mr. Lebesgue excuses himself by rejecting any responsibility for the version he sent me, qualifies it as a 'premature version', in short retracts his former statement that made me withdraw my remark for the Mathematische Annalen. Then I have found it impossible to continue the correspondence. Where in this whole story have I done something to reproach myself? I'm not conscious of such a thing.

Sincerely yours, ⟨39⟩

L.E.J. Brouwer

[Signed autograph copy – in Brouwer]

⟨39⟩ *Agréez, monsieur, l'expression de mes sentiments cordiaux.*

1911-06-14

From O. Blumenthal — 14.VI.1911 **Aachen**

Dear Mr. Brouwer! [`Lieber Herr Brouwer!`]

Thank you very much for your letter about Lebesgue. I believe that it is in principle quite felicitous, though you might also understand Lebesgue's wish for moderation of some terms. I have now, after your letter [arrived], studied once more the note of Lebesgue and I did it as precisely as I could; for my orientation I would like to ask you a few more questions.

First the article in the Annalen. The difficulty here is already in the construction of I_1, isn't it? At least it seems to me now, after I have grown suspicious, that the boundary of e_1 is already so complicated, that I don't see how Lebesgue works with that. As far as I remember, you thought that the problem only started at I_2. I'd like to ask you for information on this.

Now the Comptes Rendus. That you have recognized in the main proof a modification of your proof pleases me very much. Whether Lebesgue thinks that he gives another proof of the invariance of dimension in the note, or that he views the theorem about the linked manifolds as a further result, is not clear from the text. The title suggests that he considers the theorem as a result by itself, not as a lemma for another proof of the dimension theorem. It could also be possible that the final statement, that with these methods one can prove the dimension theorem in three different ways, was slipped in afterwards. In this respect I wouldn't be too hard on him. Now I would like to know: suppose that the dimension problem is solved, is then the theorem about the linked manifolds correct and rigorously proved? I don't dare to make a decision: it would be almost too beautiful if the theorem were true. I am especially suspicious because of the next-to-last theorem: A T_n does not fill $E_{n+p+1} (p \geq 0)$ and divides E_{n+p+1} into domains for $p = 0$ and only in this case. ⟨40⟩ That would be the reverse of the Jordan theorem for dimension n, and then, as far as I can see without the condition of reachability, hence a quite impossible result. Am I wrong there? Or did Lebesgue really goof so badly? That would surprise me very much.

Likewise, I would like to be informed about the theorem that Lebesgue uses as lemma in his Annalen proof, namely that $n+1$ domains always have a point in common. Can it be proved by your methods? Indeed, to me it seems that the theorem by itself is nice and important.

⟨40⟩ Blumenthal quotes this sentence in French.

Finally I have a question that doesn't relate to Lebesgue. I talked with you here about the generalization of the Jordan theorem to space. Afterwards it occurred to me that you misunderstood me, and that you thought of a more difficult theorem than I did. The theorem I imagined, and which I hoped would be provable, runs more or less as follows: Let G be a closed point set in R_3 that remains completely finite and let p, r_1 and r_2 be fixed numbers. It must be possible to isolate around every point P of G a subset g of G, which contains P containing only such points that have a distance $\leq p$ to P, and that can be mapped continuously and one-to-one onto a plane domain that completely contains a circle of radius r_1 and on the other hand lies completely in the interior of a circle of radius r_2. Then G divides the space into two parts.

I mean, such a thing should be provable by the invariance of dimension. Maybe it is this theorem that Lebesgue had in mind.

I will forward your letter to Lebesgue only when I have your answer, because I need the explications which I ask from you, for my accompanying letter, in which I will have to discuss matters quite in depth.

Your visit to us in Aachen is also for my wife and me a very pleasant memory. I hope very much we will soon meet again. That should be possible somehow. Please give my best greetings to Mrs. Brouwer. My wife's health is still the same. She greets you most cordially.

Your
O. Blumenthal

[Signed typescript with handwritten insertions. – in Brouwer]

1911-06-16a

From O. Blumenthal — 16.VI.1911[a] **Aachen**

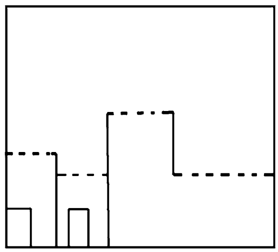
Dear Mr. Brouwer! [`Lieber Herr Brouwer!`]

Thank you very much for your letter. Your first explanations about I_1 I cannot understand. You must have expressed yourself not clearly. I understand you as follows: in the case of dimension 2, the totality of all polygons that intersect a line $x = 0$ must be bounded inside the cube by a line that contains the entire interval $0 < y < 2l$. It is clear that this can't be the theorem, but that the length of this interval must be distributed over several parallels $x = \lambda$. See the figure at the end. The interval I_1 would then be the totality of the dashed lines in my figure, in other words a discontinuous structure. The existence proof for such an I_1 can of course only be given by your indirect method. Now what is the problem with I_2? Is it that I_1 already consists of separated manifolds? Would you please explain that to me once more, and in French too, so I can send it to Lebesgue and afterwards incorporate it in the article for the Annalen.

I have thoroughly examined the theorem about the linked manifolds, also for three-dimensional space, and I found no errors. I am glad that it seems correct to you too. My objections regarding the deduction of the Jordan theorem are indeed resolved by the observation that Lebesgue only speaks about the easy part of the Jordan theorem.

I didn't know, by the way, that this part is easy to prove, I myself had only occasionally and superficially thought about it, and nothing came out of it. When I think that it would be valuable to prove the Jordan theorem for space, I mean of course the whole theorem, and not for the special purpose I have in mind, but for the general interest. The conditions I wrote down, should be merely the conditions that a point set is a one-to-one continuous image of a sphere. It is clear they are necessary but I wasn't clear about the sufficiency. You have put me at ease about this point.

Would you be so kind as to give me the explanation about I_1 and I_2 once more in extenso and in French, and especially the reason why the existence of I_2 can't be inferred with the same indirect reasoning as in the I_1 case. Please explain this very clearly, so the reader of the Annalen, and I too, really understand it, because if Lebesgue missed your objections and still doesn't understand them, you must assume that they are not right away evident.



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With best greetings

Your
O. Blumenthal

[Signed typescript – in Brouwer]

1911-06-19a

To O. Blumenthal — 19.VI.1911[a] **Amsterdam**

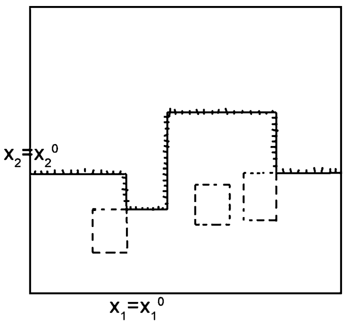
Dear professor, [`Lieber Herr Professor`]

You are asking for a more detailed exposition not only for yourself, but also for Lebesgue and for the readers of the Annalen. However, I would like to explain this first to you personally, if you don't mind. Because I have explained all of this to Mr. Lebesgue in letters, so extensively and repeatedly that nothing new can be added. Just because of that, an explanation for his attitude gradually forced itself upon me at last, namely that he saw his error right after my first letter, but that he was too vain to admit it, and that his further conduct was determined by the hope that he would perhaps later find a proof of the presupposed theorems and by the necessity to win time for that.

As far as the reader of the Annalen is concerned, in the sequel to the public French discussion they will naturally find the necessary explanations. For, I envision this sequel could be as follows: you urge Mr. Lebesgue in your accompanying letter to produce now the complete proof he announced both to me and to you he had ready. Then there are three cases possible: ⟨41⟩
first: he produces again the same so-called complete proof, of which he already sent a copy to me. This first case will occur of course if and only if Lebesgue has been honest until now. Then I send you a new French letter for the Annalen, ⟨42⟩ in which I reveal the irremediable errors of this proof and add a proof of my own.
second: he corrects the errors of the article of the Annalen and produces a completely new and correct proof, which he may have found in the meantime. Then he is obliged to apologize for his behavior, but for the rest the matter can be considered as finished.
third: he doesn't give any proof, and tries, as before, to back out of the public discussion. In this case as well I publish in the Annalen my own proof, starting with an explanation of the problems of Lebesgue's proof; I must add to this explanation that Lebesgue doesn't know how to solve these problems, which is clear from a correspondence with him! Thus the reader of the Annalen will in all cases be completely informed, if you agree to this plan, and he will get a complete and rigorous proof.

⟨41⟩For a similar list, see *Brouwer to Baire, 5.XI.1911* (draft). ⟨42⟩See *Brouwer to Blumenthal 11.VI.1911.*

I would like to ask you, by the way, not to tell Mr. Lebesgue for the moment about the existence of my proof. I myself have on purpose kept silent about my own considerations, in my correspondence with Lebesgue, as in my French letter, which I submitted to you. For I am of the opinion that first Lebesgue must have stated clearly and plainly his views on his own subjects, before my proof or its existence can come into play.

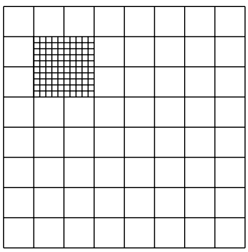


Now to business, and to begin with your plane figure.

In the case of two dimensions there is in the boundary of e_1, only *one single* (the line that dotted with small crossbars in the figure) one-dimensional space, which stretches from $x_2 = x_2^0$ to $x_2 = x_2^0 + 2l$; this we choose as I_1; the other one-dimensional spaces in the boundary of e_1, drawn as dashed lines, cannot extend from $x_2 = x_2^0$ until $x_2 = x_2^0 + 2l$.

Because I_1 is connected and stretches from $x_2 = x_2^0$ to l $x_2 = x_2^0 + 2l$, two subsets of I_1, of which the first contains the subinterval that borders $x_2 = x_2^0$, and the second one contains the subinterval that borders $x_2 = x_2^0 + 2l$, must have at least a point in common, so there exists certainly a point I_2, *hence for two dimensions there is no problem.*

For three dimensions it gets worse. For example, if we partition the edges of the main cube into 8 equal parts, then the cube is partitioned into 8^3 smaller cubes, and parallel to an arbitrary plane it can be split up in 8 layers of $[8^2]$ [43] small cubes each. I now assume that (as is possible) e_1 is composed of *in the first place* the whole first layer parallel to the plane $x_1 = x_1^0$, *secondly* of the second layer parallel to $x_1 = x_1^0$ the small cubes that are shaded in the figure here on the side; *in the third place* the entire third layer parallel to the plane $x_1 = x_1^0$, while e_2 and e_3 are composed of the two first layers parallel to respectively $x_2 = x_2^0$ and $x_3 = x_3^0$.



[43] 8^2 inserted by Freudenthal.

The boundary of e_1 inside the main cube is then composed of two connected two-dimensional spaces (one simply connected space ${}_\alpha I_1$ and one space ${}_\beta I_1$ that has the connectivity of a cylindric surface, which stretches both from $x_2 = x_2^0$ until $x_2 = x_2^0 + 2l$ and from $x_3 = x_3^0$ to $x_3 = x_3^0 + 2l$. *We must choose* one and only one of these spaces as I_1; keeping both together doesn't work, because all further conclusions rest on the *connectedness* of I_1; *which one* we must choose, Lebesgue doesn't say. Two spaces are considered here, but only one of them, namely ${}_\alpha I_1$ finally leads to an I_3; ${}_\beta I_1$ leads, as one easily sees, to a disconnected I_2, and I_3 doesn't exist at all. Still, ${}_\beta I_1$ possesses the property that is fundamental for Lebesgue, namely that it stretches from $x_\alpha = x_\alpha^0$ until $x_\alpha = x_\alpha^0 + 2l$ $(a = 2, \ldots, n)$. So this property is completely worthless for the argument, and gives no certainty for the n-dimensional space that among the different ${}_\nu I_1$ there always exists one that finally leads by a suitable choice of the successive I_α to an I_n. The choice of I_1 from the ${}_\nu I_1$ will have to be determined by the fact that I_1 represents the *outer boundary* of e_1, but for I_2, I_3 and so on the criterion fails, so that I don't believe that one can achieve something this way.

As I said already above, these explanations do not contain anything new for Mr. Lebesgue. I hope very much that I have expressed myself completely comprehensibly now, and finally, I would, once more, like now to hear from you which theorem of the Analysis Situs you mentioned to me as absolutely necessary for the continuity proof[44] of the existence of polymorphic functions on Riemann surfaces? From your last letter it seems that I must conclude that it is not the Jordan theorem.

With best greetings

[Autograph draft – in Brouwer; partly as in *CWII* pp. 446–447 (from 'Now to business' on), with Freudenthal's comments]

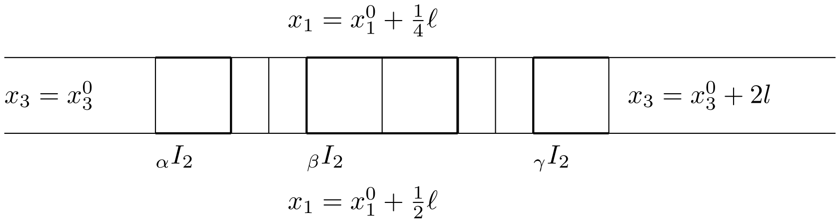
1911-06-20

To O. Blumenthal — 20.VI.1911 [45]

For a further elucidation of my letter of yesterday I add that if in my three-dimensional example ${}_\beta I_1$ is chosen as I_1, then we obtain as 'boundary

[44] Cf. [Van Dalen 1999] section 5.3. [45] No addressee – continuation of *Brouwer to Blumenthal 19.VI.1911*; see also Freudenthal's note in *CWII* p. 448.

of that part of I_1, which is contained in the elements of e_2 that do not completely belong to e_1' the following point set that lies in the plane $x_2 = x_2^0 + \frac{1}{4}l$:



Of the three connected subsets of this point set ${}_\alpha I_2$ is completely contained in '*the elements of* e_3 *that do not entirely belong to* e_1 *or* e_2'; ${}_\beta I_1$ and ${}_\gamma I_2$ lie however completely outside '*the elements of* e_3 *that do not entirely belong to* e_1 *or* e_2'.

One should have to choose therefore as I_3 the boundary between ${}_\alpha I_2$ and ${}_\beta I_2 +_\gamma I_2$. But this boundary doesn't exist, and *there is no* I_3.

[Autograph draft/copy – in Brouwer; also in *CWII*, p. 448]

1911-06-22

From O. Blumenthal — 22.VI.1911 **Aachen** [46]

Rütscherstrasse 48

Dear Mr. Brouwer! [`Lieber Herr Brouwer!`]

Please send me *immediately* your second proof copy. In March a batch of proofs that I sent to Teubner was lost, which I found out already in a different way. Probably I had put your proofs in the same package. So I ask you to send me quickly your second copy. — The proof that a closed surface divides the space in at least two parts is indeed very simple. I have figured that out already. For the purpose of function theory one needs *certainly* the division into only **2** subspaces. Whether one has to prove also reachability, I don't know yet, it is possible that one can do without. But

[46] Date and place - postmark.

even more important would be the reverse statement under the assumption of all possible reachability.

Many greetings,

your
O. Blumenthal

[Signed autograph, postcard – in Brouwer]

1911-07-02

To O. Blumenthal ⟨47⟩ **— 2.VII.1911** **Amsterdam**

Dear Sir, [`Cher Monsieur`]

The following is a proof of the 'evident' theorem on which Mr. Lebesgue based a proof of the invariance of dimension.

We start from the concepts of *n-dimensional element* and *two-sided n-dimensional manifold (open or closed)* that were introduced in my memoir 'Ueber Abbildung von Mannigfaltigkeiten' [1], and we mean by *two-sided n-dimensional system* a finite set of n-dimensional elements belonging tot to one or more two-sided n-dimensional manifolds.

By the *boundary* of such a system we will mean the $(n-1)$-dimensional sides belonging to a single element of the system. The points of the system not situated on the boundary form the *interior* of the system.

The boundary of the system is composed of a finite number of closed two-sided $(n-1)$-dimensional manifolds. It is true that several p-dimensional $(p < n-1)$ sides belonging to a single or several of these manifolds can overlap or coincide, but in the following we will abstract from this circumstance.

From the indices of the elements of the system we derive the indices of the elements of its boundary by a familiar method. [2]

Coming back to the article of Mr. Lebesgue, we designate by C_h the side of the interval I determined by the equations $x_p = x_p^0, (p = 1, 2, \dots, h)$.

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[1] Mathematische Annalen, vol. 71, p. 1, 2, 5 of the proofs. [2] ibid., vol. 71, p. 12 of the proofs.

⟨47⟩ Cf. *Blumenthal to Brouwer 16.VI.1911*: '[...] in extenso and in French [...]'

Let I_h be a two-sided $(n-h)$-dimensional system, represented '*simplicially*'[3] on C_h in such a way that the interior of I_h is represented on the interior of C_h and that the boundary of I_h is represented on the boundary of C_h. Then this representation possesses a certain *degree*,[4] which is an entire number that we will suppose to be equal to $+1$. It follows that the boundary of I_h is represented on the boundary of C_h with the same degree $+1$.

Those elements of the boundary of I_h whose interior is represented on the interior of C_{h+1} form a two-sided $(n-h-1)$-dimensional system S_{h+1}, whose interior is represented on the interior of C_{h+1}, and whose boundary is represented on the boundary of C_{h+1}. The degree of these representations is still equal to $+1$.

Let us destroy in I_h an element q_1, of which all image points have a coordinate x_{h+1} less than $x^0_{h+1}+l$; we are left with a two-sided $(n-h)$-dimensional system I'_h. Those elements of the boundary of I'_h whose interior is represented on the interior of C_{h+1} *or* on the interior of C_h form a two-sided $(n-h-1)$-dimensional system S'_{h+1}, whose image projected onto C_{h+1} gives a simplicial representation by virtue of which the interior of S'_{h+1} is represented on the interior of C_{h+1} and the boundary of S'_{h+1} is represented on the boundary of C_{h+1}. The degree of these representations still is $+1$, because there exist regions in the inside of C_{h+1} where the image of S'_{h+1} for the new representation is identical to the one of S_{h+1} for the original representation.

Let us now destroy in I_h a series of elements $q_1, q_2, q_3, \ldots, q_m$, one by one, who all have only image points with coordinates x_{h+1} less than $x^0_{h+1}+l$, and among whom are all the elements I_h whose image touches C_{h+1}; we are left with a two-sided $(n-1)$-dimensional system $I^{(m)}_h$. Those elements of the boundary of $I^{(m)}_h$ that possess for the original representation images whose interior lies in the interior of C_h form a two-sided $(n-h-1)$-dimensional system of $S^{(m)}_{h+1}$. Repeating for each q_α the reasoning applied to q_1 we will find that the image of $S^{(m)}_{h+1}$ for the original representation projected onto C_{h+1} gives a simplicial representation by virtue of which the interior of $S^{(m)}_{h+1}$ is represented with degree $+1$ on the interior of C_{h+1} and the boundary of $S^{(m)}_{h+1}$ is represented with a degree $+1$ on the boundary of C_{h+1}.

Let us denote $S^{(m)}_{h+1}$ by I_{h+1}; operating on I_{h+1} like on I_h, and so forth, we will not stop until we reach I_n, in other words to a system of points

[3]ibid., vol. 70, p. 162. [4]ibid., vol. 71, p. 7 of the proofs.

represented with degree +1 on the point C_n, hence consisting of at least one point.[5]

You see that the ideas and methods used in my proof of invariance reappear, all more complicated than the proof of the 'evident' theorem of Mr. Lebesgue, so that the Note of Mr. Lebesgue (and likewise the one of Mr. Baire about the same problem) does not have any other merit for invariance than reducing it to a more difficult problem.

Cordially yours

L.E.J. Brouwer

[Signed autograph draft/copy – in Brouwer]

1911-07-08

To O. Blumenthal — 8.VII.1911 Blaricum

Dear Professor [`Lieber Herr Professor`]

As a matter of fact I noticed yet another gap in the so-called third proof of Lebesgue. It is in the words: 'Let us reduce α in size in such a way that it is bounded by a finite set of polygonal manifolds T_p'.[⟨48⟩] But is, even for a domain in Cartesian space such a 'small reduction' possible? In three-dimensional space it is always possible, because there the boundary of a domain determined by a finite number of planes (as such the 'reduction' can of course always be constructed) is composed of a finite number of two-dimensional manifolds. This property vanishes already in four-dimensional space, as seen from the following example:

At the point O we place four mutually orthogonal three-dimensional coordinate spaces. They partition the neighborhood of O into 16 parts, which can be distinguished by the signs of the coordinates.From these 16 domains

[5]One might slightly modify the preceding reasoning and consider instead of the degree of the representation of I_h on C_h the parity of the number of points of the intersection of I_h with a plane manifold $x_p = x_p^0 + b, (p = h+1, h+2, \ldots, n; 0 < b < 2l)$. But this modification (analogous to the one that is contained in the Note of the Comptes Rendus of March 27, 1911, of Mr. Lesbesgue about my invariance proof) doesn't affect the basis of the argument.

[⟨48⟩]quote in French.

we select eight which consecutively have the following coordinate signs.

$$++++;+++-;++--;+---;----;---+;--++;-+++$$

The domain G composed of these eight subdomains determines a ring shaped domain on a sphere K around O, and this domain is bounded by a torus ρ; the boundary g of G will be found by projecting ρ from G. This point set g is *not* a three-dimensional manifold, and can also not be composed from a finite number of three-dimensional manifolds: one only has to consider a neighborhood of O in g. Perhaps one can prove that the boundary of a 'small reduction' of a such domain like G not only in this simple case, but also in complete generality, can be assembled from closed manifolds. But in any case that is a problem on its own. Maybe it is not hard, but I doubt that Lebesgue has been aware of this problem.
[crossed out part:] This new difficulty is of a quite different sort than the one that occurs in the basic definition of 'linked manifolds', and it would justify a criticism of the third proof in a form which differs form the original one. Or should one interpret the entire second part of the Comptes Rendus Note, that I can't give any definite interpretation in more than three dimensions, purely as a communication of an idea without any pretense of rigor?

As far as the Annalen article is concerned, such an interpretation is impossible.

One cannot subject a Note in the Comptes Rendus to the same requirements as an article in the Annalen.

Criticism on: 'it follows' etc.

[Handwritten draft – in Brouwer]

Editorial supplement

[Remarks by Brouwer in Dutch and German — notes jotted on top of the above letter. The middle one in German, the other two in Dutch.]

> In a multiply connected space E_{n+p+1} not every T_n can be contracted to a point. What happens then with the definition of 'linked manifolds'? (interpreting E_{n+p+1} exclusively as Cartesian won't do, for an earlier mentioned reason). And can we take each T_n as *some* boundary of a space of dimension $(n+1)$ in E_{n+p+1}?

Precisely because in this part of the Comptes Rendus-Note one cannot think of anything definite in more than three dimensions, it is so difficult to find in it a starting point for a constructive criticism, after I had to give up on the 'vicious circle' criticism.

For an even number of intersections of two closed spaces the proof is easy if one of the spaces is a line; because then on such a broken line we make each time a jump at one bend.

And we must check two kind of crossings, namely for a line interval with an $(n-2)$-edge, and of moving points with an $(n-1)$-edge.

But in general there are more kinds and much more difficult crossings to check.

1911-07-14b

To D. Hilbert — 14.VII.1911^b Amsterdam

Dear Mr. Geheimrat, [`Lieber Herr Geheimrat`]

I will spend a few weeks in the Harz, and will travel via of Göttingen, I will stay there a few days. I am very much looking forward getting acquainted with people and things there, and more in particular to see you and Mrs. Geheimrat again. I hope to travel on coming Monday or Tuesday from here to Göttingen.

Enclosed you find the tragic end of the correspondence with Lebesgue.

Together with this letter I send to you the proof of invariance of an n-dimensional domain, for publication in the Annalen.⟨49⟩ Immediately after my return I will prepare the proof of the Jordan theorem for space for publication in the Annalen.⟨50⟩

My wife regrets very much that this time she can't come with me, because of the pharmacy, and greets you most cordially.

Your
L.E.J. Brouwer.

[Signed autograph – in Hilbert]

⟨49⟩ [Brouwer 1911c]. ⟨50⟩ [Brouwer 1911d].

1911-08-19a

To O. Blumenthal — 19.VIII.1911[a] **Amsterdam**

Dear professor [Lieber Herr Professor]

As per our agreement I inform you that in the end of June or the beginning of July I received the proofs of the figures (without text) of my article 'Beweis des ebenen Translationssatzes', and, as indicated in these, I have sent them immediately back to Teubner, and since that time no proofs of either text or figures have arrived here.

I have thought more about the difficulties in the second part of the Note by Lebesgue in the Comptes Rendus, and I am now convinced that the justification of the (by the way undoubtedly correct) definition of *linked* manifolds T_n and T_p of Lebesgue is a very deep problem. I did succeed in determining a clarification for a more restricted concept, namely for *linked*, manifolds T_n and T_p, *measured in a certain way*; and because I restricted myself to this more narrow concept, I could reconstruct the course of Lebesgue's proof. The scope of the theorem of Lebesgue is then considerably restricted for arbitrary n and p, but for $p = 0$ it also says in the narrower version, that *in R_{n+1} a one-to-one continuous image of the n-dimensional sphere determines at least two domains*, i.e. the first part of the Jordan theorem in arbitrary dimensions.

I venture to communicate to you also my second proof of the theorem used by Lebesgue in his article in the Annalen. Neither the concept of degree of a mapping nor the sequence of the I_p are used. In the e_p I disregard the points that belong to the boundary of I. I denote by f_p the $(n-p)$-dimensional interval contained in the boundary of I that satisfies the equations $x_1 = x_1^0, x_2 = x_2^0, x_3 = x_3^0, \ldots, x_p = x_p^0$; I denote by g_p the $(n-1)$-dimensional interval contained in the boundary of I which satisfies the equation $x_p = x_p^0$; and h_p denotes the point set consisting of the $(n-1)$-dimensional intervals $x_{p+1} = x_{p+1}^0; x_{p+1} = x_{p+1}^0 + 2l; \ldots; x_n = x_n^0; x_n = x_n^0 + 2l$ contained in the boundary of I.

Then the boundary of e_1 is composed of a finite number of closed $(n-1)$-dimensional manifolds; we denote the one among these that contains f_1 by μ_1; μ_1 consists of f_1, of parts contained in h_1, and of parts contained in the interior of I.

The part of μ_1 contained in $g_2 + e_2$ is bounded by a finite number of closed $(n-2)$-dimensional manifolds; we denote the one among these that contains f_2 by μ_2; μ_2 is composed of f_2, of parts contained in h_2 and of parts contained in the interior of I.

The part of μ_2 that is contained in $g_3 + e_3$ is bounded by a finite number of closed $(n-3)$-dimensional manifolds; we denote the one among these that contains f_3 by μ_3; μ_3 is composed of f_3, of parts contained in h_3 and of parts contained in the interior of I.

Proceeding in this fashion we finally arrive at a point pair μ_n containing the point f_n, and as there is no h_n, the second point lies inside I. This point belongs as well to $e_1, e_2, \ldots, e_n$, as to an E_i that is not contained in any e_p, and this proves the Lebesgue theorem.

My complete Jordan proof⟨51⟩ is now also finished for n-dimensional space, and I hope to finish writing it up this month. Can I send the article then to Aachen⟨52⟩?

Will you get the extended memoir of Lebesgue for the Annalen?

Many greetings and goodbye for now!

Your
L.E.J. Brouwer

[Signed handwritten draft – in Brouwer]

1911-08-19b

To C.S. Adama van Scheltema⟨53⟩ — 19.VIII.1911[b] Amsterdam

Dear Carel [`Beste Carel`]

Coming home after protracted wanderings I find the Faust,⟨54⟩ which must have been here already for six weeks or so. I am glad it has finally appeared, and I believe that with this translation you have achieved the achievable; but what a mass of diligence, concentration, and dedication you have sacrificed on the altar of piety for your great predecessor!

⟨51⟩[Brouwer 1911d]. ⟨52⟩Blumenthal's hometown. ⟨53⟩Addressed - 'Bergmannstrasse 62^4, München'. ⟨54⟩Scheltema's Faust translation into Dutch, [Adama van Scheltema 1911].

I didn't know you had taken your task so conscientiously. From what sentiment do you draw the strength for it?

Your chum
Bertus

[Signed autograph, postcard – in Scheltema]

1911-08-26

From O. Blumenthal — 26.VIII.1911 **Aachen**

Dear Mr. Brouwer! [`Lieber Herr Brouwer!`]

Many thanks for the manuscript and the accompanying letter.[55] Apparently the proof that you have held out in prospect to me on the way back in Aachen, was the Lebesgue proof.

It is very simple and clear. We will see what Lebesgue does. I still think that he also has such a simple proof, which he has condensed so strongly in his manuscript that it is not possible anymore to read his true intentions in it. For what we at the time discussed in the Linzenshäuschen, was definitely not even the beginning of a proof. I have written now to Lebesgue and asked him to give me his 'Mémoire étendu'[56] for the Annalen, but as yet I have no answer.

I would like now to deal quickly with the automorphic functions. At the moment I'm not really up to date on the topic. Fricke is certainly much more competent than I. Altogether, it would surprise me if one could manage it with the simple Jordan theorem including reachability, but without any converse. At least with elliptic functions I always do the proof by using the reverse of the Jordan theorem, but it is possible that this is not necessary. But I believe that it would be easy for you to get information by yourself about this problem. In Klein's article in Mathematische Annalen 21[57] the problem is completely and clearly formulated from a set theoretic point of view, even though the answer given there doesn't satisfy the standard for rigor. I strongly advise you to go through the matter there, and *not* in the fat Fricke and Klein,[58] where one trips again and again over details that obscure the general idea.

[55] Probably [Brouwer 1911d]. [56] Extended memoir [57] [Klein 1882].
[58] [Fricke 1897, Fricke 1912].

Now as to your manuscript. I believe I have understood the proof itself in broad outline: before I get to a *real* understanding of things in the Analysis Situs always takes some time. But I have to say that I would have wished the exposition to be somewhat different. It is about a fundamentally important theorem, which will be used and read by many people. So I find it really awkward that for the reading of your paper there is so much cross reference to other papers, and not only to yours but also to the Comptes Rendus Note ⟨59⟩ of Lebesgue. I would thus strongly advise you to make a more complete version of this article, and explain in it as far as somehow possible all of the various concepts occurring in it, such as pseudo-manifold, net, fragment, and thus only for the *theorems* about these matters refer to your earlier papers; but in the very first place give the proof that the Jordan surface divides space into at least two parts in the article itself. This is all the easier, because the proof is so brief anyway. At least I had prepared once a proof that seemed perfect me to be and that could be given in a few lines. The citation of Lebesgue can of course remain, but then it will be a pleasant and courteous extra, and not an essential ingredient.

Please understand me correctly: if you don't want to make any changes, I will of course accept your work, also in its present form, but I believe that you would do yourself and your readers a great favor, if you complete your article in the way I indicated. If you are willing to make the change, I will send you back the manuscript.

Best greetings to you and Mrs. Brouwer.

Your
O. Blumenthal

[Signed typescript – in Brouwer]

1911-09-14

To D.J. Korteweg — 14.IX.1911 Blaricum

Professor,

I will tell my informants about the arrangement concerning the Proceedings. ⟨60⟩

⟨59⟩ [Lebesgue 1911b]. ⟨60⟩ *KNAW, Verslagen, Proceedings.*

Already about a week ago I had asked the Library to order Klein-Fricke for me from Delft. In the meantime I have as yet not received it; I have now written that I prefer the library to send me the copy of the Society.[61]

As far as the lectures in projective geometry are concerned, I am not convinced by you. By the 'authorities' I didn't mean the board[62] of the university (about whom I can indeed be assumed to know nothing) but the Mayor and Aldermen themselves,[63] who are not bound by the advice of the governors, and who bear full responsibility when they wish to perpetuate a shortage of teaching staff that has been pointed out to them; they who are so liberal with respect to other subjects of our faculty.

In the case of physics, chemistry, botany, and zoology they even arranged for a far more ample staffing than at other universities; for mathematics plus mechanics plus astronomy they still think two professors are sufficient. In Groningen and Utrecht those two are available for mathematics exclusively, which makes a big difference.

The rejection of your request by the Mayor and Aldermen was in my view an insult, not only for me and you, but even more so for our science, which should not be an appendix, but the crown of the faculty.

You say that mayor and aldermen have granted me the position of 'privaatdocent'. But although it is true that in other cases they really grant something substantial, namely an opportunity to get some visibility, or a means to collect fees from the students, in my case Mayor and Aldermen know after your request as well as you and I do, that it is I who grants the Mayor and Aldermen something, namely my assistance in teaching, and by their refusal they have qualified me as maybe the only municipal employee that isn't worth a wage.

Persons in governing positions are usually very far removed from our science, and as a consequence they are more or less insensitive to its needs. But to bow my head without protest for this insensitivity, doesn't seem to be my responsibility, even though the term 'insipid' in my previous letter was perhaps not quite well chosen.

With cordial greetings

Your
L.E.J. Brouwer

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[61] The Dutch Mathematical Society [62] *curators*. [63] The University of Amsterdam was a municipal university. The governors (members of the board) were called 'Curatoren', and the Mayor of Amsterdam was qualitate qua president of the board.

I already dropped by you, but this summer the Kostverloren Vaart ⟨64⟩ is more poisonous to me than ever, so I come to Amsterdam only for the most urgent administrative matters.

[Signed autograph – in Korteweg]

1911-10-08

From O. Blumenthal — 8.X.1911 **Aachen**

Dear Mr. Brouwer! [`Lieber Herr Brouwer!`]

I have the bad luck that in my function theory business I stumble each time upon geometric problems, which I don't trust myself to handle in full rigor, even when I have an outline of proof that seems reliable to me if done correctly. Usually I trust that these questions must seem very easy and childish to you. I need the following theorem for four-dimensional space. In my opinion the difficulty is the same for each dimension > 2.

Let a continuous closed manifold M be given that lies completely in a finite part of R_4. Now I consider the totality of all planes, i.e. three-dimensional linear manifolds, that have points in common with M. The totality of all these common points apparently is composed of continuous manifolds and single points, which form a nowhere dense set in the plane.

Theorem. There are always planes that have only nowhere dense points, but no continuous manifolds in common with M.

It would be even nicer when there were planes that only have a finite number of points, or even one point in common with M. But that is maybe hard to prove, and maybe not even correct at all.

Can you prove the theorem? Until now I don't quite see how one should do that. For the case of a three-dimensional analytic M in R_4 one can work out a simple proof with the indicatrix, but that is apparently a detour, because the only thing that matters is that the manifold is closed and lies in a finite part.

I would be very grateful for a speedy answer.

I was very sorry that I missed you in Karlsruhe. Bernstein told me that you argued with Koebe about the continuity proof. It is no pleasure to have

⟨64⟩ A canal in Amsterdam.

a discussion with him; but until now he has never made a mistake, therefore I am inclined to give him credit in this case,[6] especially because I believe that I can just about see what he can do with the deformation theorem ⟨65⟩. However, Bernstein ascribed opinions to him that would be very disputable. But I think this is a misunderstanding of Bernstein.

Many greetings to you and your wife from us both.

Your
O. Blumenthal

I have received your proofs. And I also thank you very much for your reprints.

[Signed typescript – in Brouwer]

1911-10-12

From O. Blumenthal — 12.X.1911 **Aachen** ⟨66⟩
Rütscherstrasse 48

Dear Mr. Brouwer! [`Lieber Herr Brouwer!`]

I must correct my latest letter insofar that I don't need the indicated theorem anymore, I'm pleased to say. My results could be obtained in a simpler way. Nonetheless I keep thinking the thing is right and also interesting by itself. I can't find a more or less clear proof. All the same you must be able to do it, because you know how to 'add dimensions'. If in three-dimensional space each support plane of arbitrary direction had a continuous structure in common with the continuous closed manifold, then one would have on the manifold at least ∞^1, ∞^2, ∞^3 points. That is of course no proof, because first of all one has difficulties with such lines that are common to a whole sheaf of support planes. That is however so far the way I got closest to approaching a proof. Another approach, wherein I wanted to prove that a closed surface with the property demanded in my theorem, must necessarily

[6] [handwritten remark] just like earlier Lebesgue, so maybe also unjustifiably.

⟨65⟩ *Verzerrungssatz*. ⟨66⟩ Date and place - postmark.

have a corner, was too hard for me to think through. I hope anyway that you will publish your uniformization of the automorphic functions in the Annalen?

Many greetings

Your
O. Blumenthal

[Signed autograph, postcard – in Brouwer]

1911-10-28

From R. Baire — 28.X.1911 **Dijon**

Université de Dijon,
Faculté des Sciences

Dear sir and colleague, [`Monsieur et cher collègue`]

I thank you cordially for sending me your publications, and I congratulate you very much with the progress you have made in the field of modern Analysis Situs.

As for me, I feel obliged for several reasons to postpone developing the methods that I had indicated in my publications of 1907. At that time I was too much occupied with working out my '*Leçons sur les théories générales de l'Analyse*',⟨67⟩ and after completion of that work I have unfortunately fallen ill, and for some time I had to leave aside my research.

Dear colleague, renewing my thanks to you, I hope you accept my best wishes for a beautiful scientific career.

René Baire

[Signed autograph – in Brouwer]

⟨67⟩ Lectures on the general theories of Analysis

1911-11-02

From R. Baire — 2.XI.1911 **Dijon**

Université de Dijon,
Faculté des Sciences

Dear colleague, [Mon cher Collègue]

For the reasons related to my health that I mentioned to you, I am at this moment not able to pay sufficiently sustained attention to the study of the questions that are raised in your letter. If I am not too indiscrete, permit me to ask you who are the authors about whom you complain? One is no doubt Lebesgue. It so happens that since several years I haven't had personal relations with him, for reasons that have nothing to do with pure science.

I haven't studied his proof in the Mathematische Annalen in depth, and his exposition was anyway too condensed.

As far as my method is concerned, there still was some work required to make it valid for n dimensions (I speak about the definition of the outside and the inside of a surface; the method was indicated by a phrase in the middle of my Note in the Comptes Rendus of 1907). I am convinced that there is in principle no difficulty, and no doubt that is what Lebesgue wanted to say.

I had hoped to improve these methods, and to give more complete theorems, but I didn't get around to it right away I had left that work provisionally aside, and since then I have been taken by surprise by the illness

I apologize that my great weakness in German and my ignorance of English don't allow me to follow your publications quickly enough. That is a deficiency of us French, that we have poor knowledge of other languages.

Cordially yours, dear colleague ⟨68⟩

René Baire
24 rue Andra

[Signed autograph – in Brouwer]

⟨68⟩ *Recevez, mon cher collègue, l'expression de mes sentiments les plus cordiaux.*

1911-11-05

To R. Baire — 5.XI.1911 **Amsterdam**
Overtoom 565

Dear colleague, [Mon cher Collègue]

I vividly enjoy continuing our correspondence.

The mathematicians that I think I have to complain about are Zoretti ⟨69⟩ and Lebesgue.

About your studies of 1907, they aim at the proof of the *invariance of the n-dimensional domain*, in other words the theorem that in the space E_n the (1-1) continuous image of a set without boundary points forms a set which itself also has no boundary points.

However, the equivalence of the invariance of the number of dimensions is the following theorem, which is much more restricted:

'In the (1-1) continuous image of a set that doesn't contain anything but non-boundary points, the non-boundary points form an everywhere dense set.' (See for this subject my note in volume 70 of the Mathematische Annalen, and the article of Mr. Fréchet in volume 68 of the same journal).

In my eyes the great merit of your studies of 1907 is that they show that the invariance of the n-dimensional domain can be deduced from the following theorem:

'In E_n the (1-1) continuous image of a closed manifold of $n-1$ dimensions determines *at least* two domains.'

This remark was a step forward in the solution of the extremely important problem of invariance of the n-dimensional domain, because its solution allows to use the continuity method in a perfectly rigorous manner for the uniformization of algebraic functions (see Poincaré, Acta Mathematica 4, p. 276–278).

Now, for the invariance of the number of dimensions, the theorem where you stopped, didn't lead to any progress, because the theorem is – in my view – much more difficult than the invariance of dimension. As I see it, the outline you give in the Comptes Rendus leaves the main difficulty untouched. For a long time I have been searching for a proof; for $n = 3$ it is easy, for arbitrary n I only have found it this summer, by means of a reasoning which

⟨69⟩Zoretti had in his review of Schoenflies' 1908 *Bericht* mentioned Baire, Lebesgue and Brouwer (in this order) as having made a decisive step forward in the matter of the dimension invariance.

I then rediscovered in the second part of the Note of Lebesgue (Comptes Rendus, March 27, 1911) [70], where by the way it is in a form that is almost completely incomprehensible, and inexact if one reads it literally.

This proof is complicated in a way differing completely from that of the one about the invariance of dimension, and it seems to me that one will not be able to simplify it.

Earlier I had succeeded in proving the invariance of the n-dimensional domain by means of the following lemma.

'In E_n the (1-1) continuous image of a closed part of closed manifold of $n-1$ dimensions determines only one domain.'

And afterwards I have completed the result of Lebesgue by proving that in E_n the (1-1) image of a closed $n-1$-dimensional manifold determines *precisely* two domains.

Concerning the Note of Lebesgue on pages 166–168 of volume 70 of the Mathematische Annalen, the characterization of the sequence $I_1, I_2, \ldots, I_n$ is very unsatisfactory, because it can happen that it already stops at I_3. And with this characterization the whole proof collapses.

This is what Lebesgue recognized immediately, when I pointed it out to him, and he has answered me by trying to complete the characterization of the I_p. Now, these additions turned out to be still insufficient; later Lebesgue has given a new proof of his lemma, in which the I_p did not play a rôle anymore. Neither I, nor Mr. Blumenthal (Editor of the Mathematische Annalen) have been able to understand this proof (taken literally it was wrong, but that was maybe because of an awkward formulation); well, Mr. Lebesgue refuses not only to give us new explanations, but he also doesn't want to come back to the subject in the Mathematische Annalen and correct the reasonings that he already has recognized to be wrong.

I myself have composed a proof of the lemma of Lebesgue, a few days after its publication, but I think I shouldn't publish it and leave Mr. Lebesgue the opportunity to acquit himself of his duty.

Sincerely yours, dear colleague

L.E.J. Brouwer

[Signed autograph – in Baire]

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[70] [Lebesgue 1911b]

Editorial supplement

[Private note in Dutch by Brouwer concerning the above letter]⟨71⟩

Letter to Baire 5.11.11

Explication of the 'lie deeper' than invariance. The continuity method of Poincaré.

Last summer I found the proof of the lemma of Baire, but then I recognized the proof in the 2nd part of the Comptes Rendus Note⟨72⟩ of Lesbesgue. *Before that* I had found my other 'Proof of the invariance of the n-dimensional domain'. *Later* the proof for the complete Jordan theorem. — *About the Annalen piece of Lebesgue.*⟨73⟩ I point out to Lebesgue the insufficiency of the characterization of the I_p. Lebesgue tries in his first letter to tidy up that characterization. For me they still are insufficient. Then Lebesgue tries to get there without the I_p. This last proof incomprehensible, for me and Blumenthal. However Lebesgue refuses 1°. further information 2°. to correct his error in the Annalen. I myself had proved the Lebesgue Annalen theorem a few days after it appeared, but I don't publish this proof, because I must give Lebesgue the opportunity to fulfill his duties.

The priority of my 'Invariance of domain' is not upset:

a. *publicly*, because with Baire-Lebesgue not yet everything has been published, but from Lebesgue one may expect supplementary arguments, as he *promised* them so emphatically, and hence doesn't seem to think they are trivial.
b. *privately*, because, when Lebesgue informed me that he could get half of Baire right with his 2nd Comptes Rendus, I already possessed my 'invariance of domain'.
c. *publicly and privately*, because Lebesgue didn't formulate 'invariance of domain', neither in Comptes Rendus, nor in his letters, and even Baire only mentioned 'invariance of domain sets'.

⟨71⟩ Cf. also *Brouwer to Blumenthal 19.VI.1911.* ⟨72⟩ [Lebesgue 1911b]. ⟨73⟩ [Lebesgue 1911b].

In the next letter to Baire point out that Lebesgue wrote in his remark in the Annalen 'because of Baire, whom he knew to be very neurasthenic', and that Lebesgue should have mentioned me when he corrected his Annalen article in the Comptes Rendus.

That I don't speak in my first of the 3 Annalen articles about the Baire-Lebesgue proof, is just because I understood this *formally erroneous* proof only after I had found it myself all over again; but then my first Annalen article was already submitted.

[Autograph draft – in Brouwer. English translation in *CWII* p. 441–442, with Freudenthal's comments.]

1911-11-07a

To C.S. Adama van Scheltema — 7.XI.1911[a] Amsterdam

Dear Carel, [`Beste Carel`]

Your report that I have not only foregone Annie's sauce, but also a fair with roller coaster, has of course aggravated my regret and remorse not a little.

But fortunately, this winter we can walk across the moor together and we can contrast our lonely lives: self-confidence, faith and creative power against universal denial, passive contemplation and a little vandalism.

Although I am nowadays rather fertile, and gradually have acquired some international fame and envy, don't get too serious an impression of my work. For I still harbor the intimate certainty that mathematical talent is analogous to an abnormal development of the nail of the big toe.

On congresses I perform for the popes of science the rôle of enthusiastic ensign, but when I sketch in spirited conversation 'mit flammender Begeisterung'⟨74⟩ the perspectives that are the soul of my work, my apparently absorbed gaze lavishes itself on the monomania of their expressions, and sees desolately trapped heroes in some, in others poison brewing goblins, and in the latter the anonymous torturers of the former. And while I am

⟨74⟩ With blazing enthusiasm.

physically imbued with the feeling of being in hell, my eyes radiate the sadistic lust of sympathy.

My productivity will never bring forth a grand creation, because it is only fertilised by the derisive analysis of existing things.

None of the colleagues, however, will ever fathom this, though some of them in the long run are feeling uneasy in my presence, and those then make the rounds calumniating.

Every now and then I talk to Bertha ⟨75⟩ in the train; she told me that you are going to publish your Italian diaries, ⟨76⟩ and she asked what it actually was that made Faust beautiful. She had asked others, but without result. It seems I have been somewhat successful, because the next time I saw her, she was engrossed in Faust.

Thanks for your letter, and your handshake across the German tankards — which always cheers me up, — and warm greetings to you and Annie, also from Lize,

Your friend Bertus

[Signed autograph – in Scheltema]

1911-11-21

To O. Blumenthal — 21.XI.1911 **Amsterdam**

Dear Professor [`Lieber Herr Professor`]

Can the enclosed Correction and Addendum ⟨77⟩ still be included in the last issue of Vol. 71? I would be most grateful for that.

The contents of the Addendum I had sent last week to the printer, to be included in a footnote. Unfortunately it was already too late, which I have regretted very much.

I owe you more information about the publication of my uniformization. Koebe claimed in Karlsruhe that he was already for a long time in possession of all arguments lectured about by me, except of the invariance of domain; and had partly stated these already in his articles, but he could not right

⟨75⟩ Scheltema's sister. ⟨76⟩ [Adama van Scheltema 1914]. ⟨77⟩ In text *Berichtigung* and *Nachtrag*; published as *Berichtigung* and *Bemerkung* in [Brouwer 1911b, Brouwer 1911a].

away name places. Therefore I can't make up my mind about publication; the invariance of domain appears now by itself; the remainder does not seem very profound to me, and I can very well imagine that it is completely trivial for the automorphic professional. In any case Koebe will probably refer to this in future publications as something completely self-evident; and that could then rob the, anyway not all too great, importance of my possibly available publication.

I have now sent my Karlsruhe talk ⟨78⟩ to Fricke, and I am very curious about his — indeed the most competent — opinion.

In the Bulletin des Sciences Mathématiques of October 1911 (S. 287) Baire (sic! ⟨79⟩), Lebesgue and I, in this order, are quoted as founders of the invariance of dimension. This matches exactly with the opinion which I thought I could read between the lines of Lebesgue's Note in the Annalen, ⟨80⟩ as I recently wrote to you. You can see from this how much to the point my critical footnote is, in more than one respect. Indeed, didn't in fact Lebesgue officially throw down the gauntlet for me with his Annalen Note? ⟨81⟩

Best greetings!

Your
L.E.J. Brouwer

[Signed autograph draft – in Brouwer]

1911-12-05

From R. Baire — 5.XII.1911 **Dijon**

Université de Dijon,
Faculté des Sciences

My dear colleague, [`Mon cher Collègue`]

I want to thank you immediately for sending your articles, though I can't promise you that I will study them right away.

⟨78⟩ [Brouwer 1912b]. The Letter to Fricke, [Brouwer 1912d], was dated 22.XII.1911. ⟨79⟩ inserted by Brouwer. ⟨80⟩ [Lebesgue 1911b]. ⟨81⟩ followed by the crossed out 'and this I must publicly take up.'

Formerly I have been a close friend of Lebesgue, my comrade at the École Normale. Our separation has come about by an act of his, as a consequence of malicious procedures he used against me, in matters of career, not of a scientific nature. That today he tries to give himself a beautiful rôle by praising my works out of 'pity' (?!), it's one more malicious procedure, especially in a letter addressed to a third party and a foreigner.

I don't ask him to advertise for me. I think that you will be the first to recognize that my very pronounced neurasthenia didn't stop me to push for clarity in my work at least as far as Lebesgue.

To return to the scientific question, I regret that unfortunate circumstances have prevented me from keeping the promise made in my notes of 1907. I still believe that by following the method that I indicated very *succinctly* in the Comptes Rendus, one can prove *without fundamental difficulties, but maybe requiring a longish exposition*, the propositions that I need. But on the other hand these propositions form a less complete set than your statements 1, 2, 3 of p. 314. ⟨82⟩

Cordially yours, ⟨83⟩

René Baire

[Signed autograph – in Brouwer]

1911-12-10a

To H. Poincaré — before 10.XII.1911 ⟨84⟩

I take the liberty to send you with this letter three small articles that recently appeared in the Mathematische Annalen, ⟨85⟩ as well as the unpublished text of a talk given by me at the German Congress in Karlsruhe on September 27, 1911. I can, however, not decide to publish this communication without asking you. ⟨86⟩

⟨82⟩ See [Brouwer 1911d]. ⟨83⟩ *Avec mes meilleurs sentiments de cordialité.* ⟨84⟩ As Brouwer questions the analyticity of the correspondence mentioned below, and Poincaré showed in his letter of 10.XII.1911 surprise at this, it is not too far fetched to date this letter before December 10. Furthermore, in view of the reprints Brouwer enclosed, the letter may be dated after November 16. ⟨85⟩ Probably [Brouwer 1911c, Brouwer 1911d, Brouwer 1911e], which appeared 16.XI.1911. ⟨86⟩ An insertion is missing here.

My 'Beweis der Invarianz des n-dimensionalen Gebiets' ⟨87⟩ has been inspired by the reading your 'Méthode de Continuité' in volume 4 of the Acta Mathematica. ⟨88⟩ It was in the course of this reading that I had the impression that on the one hand one didn't know in the general case if the one-one and continuous correspondence between the two $6p-6+2n$-dimensional varieties concerned, is analytic, and on the other hand that in order to be able to apply the method of continuity, one has to start by proving the absence of singular points in the variety of modules of Riemann surfaces of genus p; this last demonstration, incidentally, turns out to be fairly easy. Now after having read somewhere in an article by your hand (I believe about the equation $\Delta u = e^u$ in the Journal de Liouville) ⟨89⟩ that you considered your exposition of the method of continuity as perfectly rigorous and complete, I started to fear that I had poorly understood your memoirs in the Acta, and I have published my article 'Beweis der Invarianz des n-dimensionalen Gebiets' without indicating there the application to the method of continuity, restricting myself to an oral communication on this subject on September 27, 1911 at the Congress of the German mathematicians in Karlsruhe, of which communication I join the text to this letter. At the occasion of this talk Mr. Fricke has expressed to me his doubts to me that at the start I had formulated exactly the result of your arguments of pages 250–276 of the *Acta*. Meanwhile I continue to believe that I have interpreted you exactly.

In fact, if the conditions of this statement, in which the word 'uniformly' (uniformément) is the key word, are satisfied, the *reduced polygons of the sequence of groups converge also uniformly to the boundary of the* $(2n+6p-6)$-*dimensional cube*, and because of your arguments there exists at least a *reduced limit polygon* that only has parabolic angles on the fundamental circle, corresponding for that reason to a limit Riemann surface, for which either the genus is decreased, or the singular points have become coincident.

Would I ask too much of your benevolence and your precious time, asking you to be so kind as to convey briefly to me your opinion about the disputed points, to wit *1º* whether I have formulated the result of pages 250–276 of the Acta correctly and *2º* whether I was wrong saying on the first page of the attached communication that pages 276–278 of the *Acta* tacitly assume 'Theorem 1' and 'Theorem 2'?

⟨87⟩ Proof of invariance of the n-dimensional domain. ⟨88⟩ [Poincaré 1887]; Poincaré uses here the 'method of continuity' to solve the equation $\Delta u = e^{2u}$. ⟨89⟩ [Poincaré 1895].

I would be extremely obliged if you could thereby deliver me from my doubt on these points.

Yours deeply revering [90]

L.E.J. Brouwer

[Autograph draft – in Brouwer]

1911-12-10b

From H. Poincaré — 10.XII.1911^b [91]

Dear Colleague, [`Mon cher Collègue`]

Thank you very much for your letter; I don't see why you doubt that the correspondence between the two manifolds would be analytic; the modules of the Riemann surfaces can be analytically expressed as functions of the constants of Fuchsian groups; it is true that certain variables only can have real values, but the functions of those real variables preserve nonetheless the analytic character.

Now in your eyes the difficulty arises from the fact that one of these manifolds doesn't depend on the constants of the group but does depend on the invariants. If I recall correctly, I considered a manifold depending on the constants of the fundamental substitutions of the group; so to a group there will correspond a discrete infinity of points of this manifold; next I subdivided this manifold in partial manifolds, in such a fashion that to a group corresponds a single point of each partial manifold (in the same way as one decomposes the plane in parallelograms of the periods, or the fundamental circle in Fuchsian polygons). The analytic character of the correspondence doesn't seem to be altered to me.

With regard to the manifold of the Riemann surfaces one can get into problems if one considers those surfaces as Riemann did; one may for example wonder if the totality of these surfaces doesn't form *two* separate manifolds. The difficulty vanishes if one views these surfaces *from Mr. Klein's point of view*; the continuity, the absence of singularities, the possibility to go from one surface to the other in a continuous way become then almost intuitive truths.

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[90] *Agréez, monsieur, l'expression de ma profonde vénération.* [91] Postmark as mentioned by Mrs. C. Jongejan.

I apologize for the disjointed fashion and the disorder of my explications; I have no hope they are satisfactory to you, because I have presented them very poorly to you; but I think they will lead you to make the points that bother you more precise, so I can subsequently give you complete satisfaction. I am happy to have this opportunity to be in contact with a man of your merit.

Your devoted colleague, ⟨92⟩
Poincaré

[Signed autograph – in Brouwer; also in [Alexandrov 1972]. See also [Zorin 1972].]

1911-12-21

To A. Schoenflies — 21.XII.1911 **Amsterdam**
Overtoom 565

Dear Professor [`Sehr geehrter Herr Professor`]

When I was last summer with Mr. Fricke in Harzburg, ⟨93⟩ the conversation turned to the new edition of your Bericht ⟨94⟩, and we thought that you might not be averse to a little help in correction of the proofs, thereupon I said that I personally would be happy to collaborate in this way.

Just now I hear from Fricke than he has conveyed my offer to you and that you are in favor of it, So I have to the honor to inform you most obediently that I am at your service. I am glad to be able to express in this way how much I feel obliged to your Bericht. With cordial greetings

Your
L.E.J. Brouwer.

[Signed autograph – in Brouwer]

⟨92⟩ *Votre bien dévoué collègue.* ⟨93⟩ Brouwer regularly stayed in Harzburg, see [Van Dalen 1999], p. 306. ⟨94⟩ [Schoenflies 1900, Schoenflies 1908]; most of the corrections and revisions concerned the second part, which contained the basics of topology.

1911-12-22

To R. Fricke — 22.XII.1911 **Amsterdam**

Dear Geheimrat, [Hochgeehrter Herr Geheimrat]

With reference to our last conversation I inform you about some remarks related to the topological difficulties of the continuity proof, which I have presented at the meeting of Naturforscher in Karlsruhe.

Let κ be a class of discontinuous linear groups of genus p with n singular points and with a certain characteristic signature; for this class the *fundamental theorem of Klein* holds, if to every Riemann surface of genus p that is canonically cut and marked with n points there belongs *one and only one* canonical system of fundamental substitutions of a group of class κ.

In the continuity method, which Klein uses to deduce his fundamental theorem, the following six theorems are applied.

1. The class κ contains for every canonical system of fundamental substitutions that belongs to it *without exception* a neighborhood that can be represented one to one and continuously by $6p-6+2n$ real parameters.
2. During continuous change of the fundamental substitutions within the class κ the corresponding canonically cut Riemann surface ⟨95⟩ likewise changes continuously.
3. Two different canonical systems of fundamental substitutions of the class κ cannot correspond to the same cut Riemann surface. ⟨96⟩
4. When a sequence α of canonically cut Riemann surfaces with n designated points and genus p converges to a canonically cut Riemann surface with n designated points and genus p, and when each surface in the sequence α corresponds to a canonical system of fundamental substitutions of the class κ, then the limit surface likewise corresponds to a canonical system of fundamental substitutions of the class κ.
5. The manifold of cut Riemann surfaces contains for every surface belonging to it *without exception* a neighborhood that can be one to one and continuously represented by $6p-6+2n$ parameters. ⟨97⟩

⟨95⟩ Crossed out footnote of Brouwer's draft: 'For the sake of brevity I write 'covered Riemann surface' rather than 'a covering surface constructed with signature σ of a Riemann surface of genus p with n designated points.' ⟨96⟩ Crossed out footnote of Brouwer's draft: 'Two covered Riemann surfaces are considered identical, if and only if the corresponding uncovered surfaces can be mapped conformally onto each other such that corresponding return cuts and stigmata behave identically with respect to the formation of the covering surfaces.' ⟨97⟩ In the draft followed by a footnote number identical to that of the preceding footnote.

6. In the $(6p-6+2n)$-dimensional space the one to one continuous image of a $(6p-6+2n)$-dimensional domain is also a domain.

I am ignoring here theorems 1, 2, 3, 4. For the case of the boundary circle they have already been completely treated by Poincaré in Vol. 4 of the Acta Mathematica; for the most general case only theorems 3 and 4 await an exhaustive proof; in this matter also this gap will be filled in by Mr. Koebe in papers that are to appear soon.

Theorems 5 and 6 are those which constitute the topological difficulties of the continuity proof that are emphasized in your book about automorphic functions.[7] However, of these theorem 6 is settled by my recently published article 'Beweis der Invarianz des n-dimensionalen Gebiets',[8] whereas the application of Theorem 5 can be avoided by carrying out the continuity proof in the following modified form:

We choose $m > 2p-2$[9] and consider on the one hand the set M_g of automorphic functions belonging to the class κ that only have simple branching points and with m simple poles in the fundamental domain,[10] and on the other hand the set M_f of Riemann surfaces covering the surface, of genus p, with n signed points, and with m numbered leaves and with $2m+2p-2$ numbered simple branching points not at infinity, for which the sequential order of the leaves and the branching points correspond to the canonical relations in the sense of Lüroth-Clebsch.[⟨98⟩]

The set M_f constitutes a continuum, and possesses for each of its[⟨99⟩] corresponding surfaces *without exception* a neighborhood which is one to one and continuously representable by $4p-8+2n+4m$ real parameters.

For an arbitrary automorphic function φ belonging to M_g there exists in M_g a neighborhood u_φ which can determined by $4p-8+2n+4m$ real parameters; these parameters are the m complex places of the poles in the fundamental domain, the $m-p-1$ complex behaviors of the $m-p$ arbitrary pole residues, and the $6p-6+2n$ parameters of the canonical systems of fundamental substitutions. The value domain of the parameters belonging to u_φ constitutes a $(4p-8+2n+4m)$-dimensional domain w_φ.

[7] Cf. Vol. 2, p. 412, 413. [i.e. *Fricke-Klein, Theorie der automorphen Funktionen, Vol. 2, p. 413.*] [8] Mathematische Annalen 71, p. 305–313. Cf. also the articles of Baire and Lebesgue, quoted in the same volume p. 314. [9] In order to be more specific, we henceforth suppose $p > 1$. [10] Automorphic functions that only differ by an additive and multiplicative constant we consider as identical.

[⟨98⟩] Crossed out footnote of Brouwer: 'Two of these surfaces are considered identical if and only if the corresponding not-covered surfaces can be mapped so much similarly onto each other that corresponding return cuts and stigmata behave the same with respect to the construction of the covering surface.'. [⟨99⟩] Here Brouwer corrects a grammatical mistake related to the gender of the German word for 'set'.

With the function φ there corresponds a finite number of surfaces belonging to M_φ. Furthermore we conclude from the theorems 1, 2, 3 and the remark that possible birational transformations into itself not only for the single Riemann surface, but also for the totality of Riemann surfaces belonging to u_φ, cannot become arbitrarily small,[11] that with a sufficiently small w_φ in M_f there corresponds a finite number of one to one and continuous images, and hence because of Theorem 6 *a domain set*. However, *then the total set M_g in M_f corresponds with a domain set G_f too.*

Now we formulate Theorem 4 in the following form:

'When a sequence of canonically cut surfaces of M_f converges to a canonically cut surface of M_f and when each surface of the sequence corresponds to a canonical system of fundamental substitutions of the class κ, then the limit surface also corresponds to a canonical system of fundamental substitutions of the class κ.'[12]

This property immediately entails that the domain set G_f cannot be bounded in M_f, and *hence it must fill the whole manifold M_f*. This proves the fundamental theorem for every Riemann surface of genus p on which there exist algebraic functions with more than $2p-2$ simple poles and with exclusively simple branching points, i.e. just for any Riemann surface of genus p.

Sincerely yours,
L.E.J. Brouwer

[Autograph draft – in Brouwer]

[Brouwer's copy of the proofs of 22 February (returned 26 February) carries a few comments in Dutch:]

this note goes further than the one of the Jahresbericht 1) because of the completely different use of Theorem 4. 2) by virtue of the completely different way in which *here* is abstracted from the continuity of both 'sets' that are compared.

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[11] According to the treatise of Hurwitz in Vols. 32 and 41 of the Mathematische Annalen both the ordering of the periodicity and the number of fixed points must for these birational transformations remain below a certain finite bound, and hence the periodicity of a sufficiently small birational transformation must be transferred to the simply connected covering surface that winds aperidiodically around its fixed points. But then this covering surface would admit a periodical conformal transformation with fixed points into itself, which is a contradiction. The property used in the text can, by the way, probably also be understood in a much more direct way.

[12] As Mr. Koebe has expounded on the Naturforscherversammlung in Karlsruhe, this theorem can be most elegantly concluded from his deformation theorem (*Verzerrungssatz*).

For 'set' I use here everywhere the word 'Menge'; 'manifold' I use only where connectedness is implicitly 'alluded to'.

Editorial comment

> There are several manuscripts of the 'letter to Fricke'. The final version of the letter, up to corrections and additions to the proofs, is printed as 'Über die topologischen Schwierigkeiten des Kontinuitätsbeweises der Existenztheoreme eindeutig umkehrbarer polymorpher Funktionen auf Riemannschen Flächen, (Auszug aus einem Brief an R. Fricke). *Nachrichten von der koeniglichen Gesellschaft der Wissenschaften zu Göttingen*, (1912), pp. 603–606. The proofs of February 8, 1912 bear the different title 'Über den Kontinuitätsbeweis der . . . '. A typescript version of the letter in the Brouwer archive has the simple title 'Über die topologischen Schwierigkeiten des Kontinuitätsbeweises.' Freudenthal, in his notes to *CWII* pp. 577–580, gives a history of the various handwritten and typewritten manuscripts (*CWII* pp. 581–583). In particular the footnotes underwent drastic changes. The draft (with numerous corrections and insertions) is dated '22. Dezember 1911'. Brouwer's handwritten copy carries no date. The final version was dispatched to Fricke on 30.XII.1911.

1911-12-30b

To A. Hurwitz — 30.XII.1911[b] **Amsterdam**
Overtoom 565

Dear Professor [Hochgeehrter Herr Professor]

Please excuse me for taking the liberty to turn to you with a question. The fact is that I need the following theorem:

'A birational transformation τ of a Riemann surface of genus $p > 1$ into itself can never transform a canonical system s of cuts (consisting of p pairs of return cuts that are connected in a point C) into an *equivalent* canonical system s' of cuts.' (s and s' are called equivalent when they can be transformed into each other by a continuous motion of the surface.)

I have convinced myself of the correctness of this theorem in the following way:

'Let the transformation τ have n fixed points. We construct an extended canonical system of cuts S, consisting of s and n cuts from C to the fixed points, and which is taken by τ into S'. If now s' would be equivalent to s, then *by the periodicity of* τ S' would be equivalent to S. (S and S' are called equivalent, when they can be transformed into each other by a continuous motion of the surface *without moving the* n *fixed points*).'

Now we construct for this Riemann surface the simply connected covering surface, as is customary in the theory of automorphic functions, which winds aperiodically around the n fixed points, and which can be mapped conformally onto the interior of a circle, so that the n fixed points and their reproductions are moved to the circumference of the circle. The transformation τ then corresponds to a conformal transformation *without fixed points* of this circle interior into itself, which must be *periodic* because of the equivalence of S and S', which is a contradiction, because a periodic conformal transformation of the interior of a circle into itself always has a fixed point.

It seems to me very probable that the theorem in question can be grasped much more simply from the standpoint of the combinatorial construction of 'regular' Riemann surfaces, and even that it is an immediate consequence of your earlier investigations in this field.

Am I correct in conjecturing this? And has the theorem already been stated somewhere? I would be very grateful for your kind communication about this.

Sincerely yours

Your most obedient
L.E.J. Brouwer

[Signed autograph – in Hurwitz; also in *CW II* p. 616–617 (with Freudenthal's comments)]

1912-01-04a

From H. Poincaré — before 4.I.1912[a]

My dear colleague, [`Mon cher Collègue`]

Thank you very much for your successive letters; I will study the matter in detail as soon as I will have time. I still believe that the simplest way to prove the absence of a singular point would be to not use the Riemann surfaces in the form given by Riemann, in other words with stacked flat leaves and cuts, but in the form given by Klein; an arbitrary surface with a convenient connection and some law (with a representation that is or isn't conformal) for the correspondence of the points of this surface with the imaginary points of the curve $f(x, y) = 0$.

Already many years ago I have expounded my ideas about this point during a session of the Société Mathématique de France; but I didn't publish them, because Mr. Burkhardt, who was present at that session, told me then that Mr. Klein had already published them in his *autographically prepared* lecture notes ⟨100⟩; maybe you can avail yourself of these.

It all amounts to this. Let $f(x, y) = 0$ be a curve of genus p; to this curve I let correspond a Riemann-Klein surface S and a law L of correspondence between the real points of this surface and the complex points of the curve $f(x, y) = 0$. Next I consider surfaces S' and laws L' that differ infinitesimally ⟨101⟩ little from S and L. On first must prove that there are ∞^{6p-6} such surfaces S' (which are not considered distinct if they can be transformed into each other by birational transformations); and then one can always pass from an arbitrary S', L' to another arbitrary S', L', without moving too far from S, L and without passing **by** S, L.

Your devoted colleague,
Poincaré

[Signed autograph – in Brouwer]

1912-01-13

To F. Klein — before 13.I.1912[a] ⟨102⟩

I was very sorry to hear how you have completely overworked yourself by your indefatigable and unselfish efforts in the interest of science and the common good. Would that you take a bit more care of yourself in the future: we need you for a long time as our leader and master.

⟨100⟩ 'autographiées' in text; [Klein 1892] ⟨101⟩ In text: infin.^{t}. ⟨102⟩ Reproduced as [Y6] in *CW II* p. 584; with Freudenthal's comments on the dating and the addressee.

In correcting the galley proofs ⟨103⟩ I will be happy to take your remarks into account, and I will designate the relevant theorem as your '*general* fundamental theorem', and I will point out that my theorem 6 will be superfluous as soon as the analytical relation between the two manifolds has been shown ⟨104⟩ (unfortunately I have until now not succeeded in giving this proof). About your proof as presented by Poincaré in Acta Mathematica 7, of the finiteness of the birational transformations for $p > 1$, I would like to permit myself the remark that this proof presupposes the uniformization, so it cannot be applied in my line of thought. Moreover I don't need merely the finiteness of the transformations of a certain surface, but of *all* surfaces of genus p. (It would be a priori possible that indeed the smallest birational transformation of a given surface possesses a finite maximal deformation $\digamma$, but that this $\digamma$ can become arbitrarily small if the surface is varied. Noether recently informed me that also he didn't know a proof of the last theorem, independent of Hurwitz's). I would be very happy to compose a comprehensive version of the article for the Mathematische Annalen.

With best wishes for your speedy and complete recovery and with cordial greeting

your admiring ⟨105⟩

[Autograph draft/copy – in Brouwer]

1912-01-16

From O. Blumenthal — 16.I.1912 Aachen ⟨106⟩

Dear Mr. Brouwer! [`Lieber Herr Brouwer!`]

I would like very much to have the absolutely shortest and best proof of invariance of dimension in the Annalen. Therefore I promise you publication in the next issue, together with your translation theorem on the condition that the new proof doesn't exceed 3 pages. But I would like you to write it sufficiently elaborately so everybody can understand it. That will, I guess,

⟨103⟩ of [Brouwer 1912d]. ⟨104⟩ Page 2 footnote 4. ⟨105⟩ *Ihr verehrender.* ⟨106⟩ Date and place - postmark.

fit into three pages, as you think yourself that only one page is necessary. The issue appears in the beginning of March.

Thank you very much for your kind condolences at the death of my parents. I hope that everything is well at your end. Best greetings.

Your
O. Blumenthal

[Signed typescript, postcard – in Brouwer]

1912-01-21

To F. Engel — 21.I.1912 **Amsterdam**
Overtoom 565

Dear Professor [`Sehr geehrter Herr Professor`]

In your review on p. 194 of vol. 40 of 'Fortschritte der Mathematik' ⟨107⟩ you raise two kinds of objections against my paper. First, you think that I have imposed overly strong restrictions on the problem, and second, you find that even accepting these restrictions my exposition is not completely watertight.

With respect to the first point I would be very grateful, if you would be so kind as to inform me precisely which more comprehensive problems you envisage, as I have not succeeded to get a completely clear picture from your indications.

The phrasing of your review would roughly indicates that you wish the following assumptions:

'Suppose that an n-dimensional manifold μ carries a continuous *parametrizable set* of transformations containing the identity, which has in a neighborhood of the identity in the first place the group property, and which in the second place can be one-to-one and continuously represented by p real parameters.'

These conditions would certainly not be sufficient, because for every p-dimensional group g one can construct in many different ways a p-parameter

⟨107⟩F. Engel, review of [Brouwer 1909b] in *Jahrbuch über die Fortschritte der Mathematik* 40, p. 194.

set of transformations, which is identical with g in a certain neighborhood of the identity, but which is outside this neighborhood neither identical with g nor possesses the group properties at all. For in the set theoretic version of the problem, there is no possibility to infer under the assumption of the analytic dependence on the p parameters of the transformations, properties arbitrarily far away from the identity from properties close to the identity. Indeed, in the set theoretic version of the problem one doesn't have the possibility to conclude from properties close to the identity to properties arbitrarily far away because of analytic dependency of the p parameters of the transformations.

Hence in any case the conditions must be phrased in the following more restricted form:

'Assume that an n-dimensional manifold μ carries a continuous group ⟨108⟩ of transformations containing the identity, ⟨109⟩ which in a neighborhood of the identity can be one-to-one continuously represented by p real parameters.'

From these assumptions it follows immediately that the whole group can be mapped one-to-one and continuously onto a p-dimensional 'parameter manifold', and that the group consists of pairwise inverse transformations. This last property in its turn implies that on the n-dimensional manifold μ the transformations are *everywhere one-to-one*, so we have recovered all the conditions of my Annalen article.

But maybe I have in the above, completely misinterpreted the ideas of your review? For example, the meaning of your words 'unnecessary restriction that the group must be closed' has remained totally obscure to me.

For the second point of your criticism, namely the incompleteness of my exposition, you give two examples:

1) Certain obscurities in the formulation of the conditions in § 1. I would really like to know which obscurities or uncertainties you have found here, because I believed that I have satisfied every demand of set theoretic precision and exactness.

2) The 'lies certainly' on p. 255, l. 20. This is quite self evident indeed. For when a point x moves continuously from 0 to a, then the point $2x$ moves in the same direction from 0 to $2a$. The moment the point $2x$ reaches a, the point x is at the required point b.

⟨108⟩In the older literature the identity is not always included in the group definition.

⟨109⟩Note that the terminology in group theory had not been generally agreed on.

I would attach great importance to reach agreement on the above with a group theorist of your authority.

Sincerely yours [110]

L.E.J. Brouwer

[Signed autograph, draft – in Brouwer; also in *CWII* p. 141–142, with Freudenthal's comments]

1912-01-28

From F. Engel — 28.I.1912 [111] **Greifswald**
Arndtstr. 11 [112]

Dear Doctor! [Sehr geehrter Herr Dr.!]

I am very glad that you have directly contacted me with your letter of the 21st. Only, it's a pity that all these kinds of things are so difficult to discuss in writing. Verbally it would be much easier to come to an understanding.

To begin with, I would regret it if you would have read in my review any kind of disapproving judgement about your work. I didn't mean anything like that, but judged 'mit Bewunderung zweifelnd, mit Zweifel bewundernd', [113] as one should according to Lessing's scala judge when confronted with a Master.

With that I don't actually want to recognize you straight away as master, but just express my general esteem for your articles, although on the other hand I acknowledge you unconditionally as my master in set theory, the application of which is not my line at all.

I must maintain that the conditions indicated on p. 247 leave very much to be desired in clarity of formulation. Maybe they appear to be clear to a died in the wool set theorist [114] but I must say: 'the expressions of the system sound dark to uncircumcised ears.' [115]

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[110] *Mit ausgezeichneter Hochachtung bin ich – Ihr ergebenster.* [111] Letter in 3 parts: dated 28.I.1912, 30.I.1912, 31.I.1912; postmark 31.I.1912. [112] Envelope. [113] 'Admiring with doubt or doubting with admiration' – well-known quote of Gottfried Lessing about the correct attitude of critics with respect to masters. [114] in German '*eingefleischter Mengentheoretiker*'. [115] Allusion to Jeremiah 6:10.

On p. 247, l. 3–11 you have packed so much into one sentence, that already for that reason it is impossible to be clear, at least for me. Also l. 17–19 is much too succinctly worded to be clear.

As regards the other points, I cannot befriend myself with them,⟨116⟩ just as before.

[From Engel] — **30.I.1912** [Continuation of 28.I.1912]

that you assume from the beginning that the transformations are one-to-one and invertible throughout the whole manifold. I cannot see that the case where one has a group of analytic multi-valued transformations, can be reduced to the one-to-one case downright.

When I proposed to assume to begin with the one-to-one property of the transformations only in the neighborhood of a point, and likewise the group property only in the neighborhood of the identity transformation, then I meant of course: I have a parametrized family of transformations that are uniquely invertible in the neighborhood of a point, this family contains the identity transformation and two transformations of the family that lie in a certain neighborhood of the identity transformation produce again a transformation of the family.[13] Of course the transformations of the family must generate a group when they are arbitrarily often performed successively, and we only have to assume that this group doesn't contain other transformations than those in the original family.

In this way not only the original family of transformations will be extended, but at the same time one gets the original transformations defined outside of the range on which they were originally defined. So one gets an extension of the transformations also when one can't make use of the analytic continuation. Therein the tremendous power of the group concept manifests itself.

Now whether from these assumptions the ones that you made follow, I cannot judge, and I don't trust myself to say something about it. Should it be the case and should you already have checked that yourself, I would, to be honest, disapprove of your not saying so in your article. Then you would have omitted the reduction to the assumptions you made to the most simple and natural possible ones, or you would not have fathomed the true meaning of the group concept, namely that it by itself produces a principle

[13] I would for the sake of for simplicity also assume pairwise inverse transformations.

⟨116⟩ *ich kann mich nicht damit befreunden.*

for analytic continuation.

I hope that it is now clear to you what I meant when I said that one shouldn't assume at the outset that the group be closed; one shouldn't even think of the whole group but only of a piece of it. In all applications one finds oneself anyway in this case.

That the 'lies' on p. 255 is so self evident, I still can't see. I don't see that without further assumptions it amounts to the theorem that a continuous function that takes two values also takes every intermediate value. In whatever way one proves this theorem, it seems that a proof here is necessary, or the reduction to an earlier proven theorem. Maybe I am mistaken, but I have the feeling that a proof is necessary.

For the rest, I can't get around admitting that you have, by your article and your letter, perhaps unintentionally, brought me a new insight. For it now no longer appears practical to me, also when one considers analytic groups, to extend the equations that arise by analytic continuation in the usual way, but one should [14] only consider those analytic continuations that arise from the application of the group concept. In this manner one will in some cases not exhaust the entire domain to which the functions can be analytically continued, but instead of that one will not leave the domain in which the group transformations are all uniquely invertible.

[From Engel] — **31.I.1912** [Continuation of 30.I.1912]

This letter is written in several installments, because aside from the fact that I am nowadays fairly busy as a dean, I have also been selected by lottery for jury duty and that makes even more demands on my time, which is already so restricted.

Finally I would like to mention that, to be quite honest, I don't think that the profit of this kind of research does not quite match the effort spent and the necessary investment in acumen. You may consequently still think me a heretic. I would all the more be glad, if you now also would work on group theory itself, for there still is a lot to be done.

Sincerely yours

F. Engel

[Signed (on 31.I.1912) autograph – in Brouwer; also in *CWII* p. 144–146]

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[14]at least in the case of transitive groups

1912-02-03

From O. Blumenthal — 3.II.1912 ⟨117⟩ **Aachen**

Dear Mr. Brouwer! [`Lieber Herr Brouwer!`]

May I ask you for a service for the Annalen? I send you herewith an article by Lennes, 'Curves and Surfaces in non-metrical space',⟨118⟩ which seems to me closely related to your articles. More specifically I see a part of the Jordan theorem formulated there, and actually just what you have proved in detail in the Annalen. Hence I request your opinion on the article: whether it is correct, and in which relation it stands to yours, and whether you think it deserves to be published in the Annalen.

Many thanks in advance and many greetings!

Your
O. Blumenthal

[Signed typescript, postcard – in Brouwer; also in *CWII* p. 487]

1912-02-04b

From F. Engel — 4.II.1912^b **Greifswald**
Arndtstr. 11 ⟨119⟩

Answer to my short question, whether
according to Mr. E. the one-to-oneness
isn't a consequence of the pairwise
inverseness.⟨120⟩

Dear Dr.! [`Sehr geehrter Herr Dr.!`]

I think that an example will the best thing to explain to you what I mean.

⟨117⟩Date and place - postmark, Amsterdam postmark 4.II.1912. ⟨118⟩[Lennes 1911]. The paper was rejected by the *Mathematische Annalen*. The paper played a role in the later dimension discussion, where it served to show that Brouwer knew in 1911 the modern definition of connectedness, as it occurred in this paper of Lennes. Brouwer had independently formulated the notion in [Brouwer 1911c]; see Freudenthal's historical comments, *CWII* p. 486. ⟨119⟩From envelope. ⟨120⟩Brouwer's note.

I consider a one-parameter group, generated by a multivalued infinite transformation. For example if we have the infinite transformation

$$x' = x + \frac{1}{2}\sqrt{x}\delta t$$

then we obtain the one-parameter group of two-valued transformations

$$\sqrt{x'} = \sqrt{x} + t$$

or:

$$x' = x + 2t\sqrt{x} + t^2$$

Here every transformation is two-valued, but coincides with its inverse transformation, or perhaps better: it changes by analytic continuation into its inverse.

Now let $x_0 \neq 0$ and $\sqrt{x_0}$ be one of the two root values. We take our departure from the one-parameter set [121] of transformations:

$$x' = x + 2t \cdot \sqrt{x_0}\left(1 + \frac{x - x_0}{x_0}\right)^{\frac{1}{2}} + t^2$$

where $(1+\alpha)^{\frac{1}{2}} = 1 + \frac{1}{2}\alpha + \ldots$. This set is defined for every t and für $|x - x_0| < |x_0|$. Its transformations have the form:

$$x' - (t + \sqrt{x_0})^2 = (x - x_0)\left(1 + \frac{t}{\sqrt{x_0}}\right) + \ldots$$

where the omitted terms are of second and higher order in $x - x_0$.

Let us call these transformations of this set S_t, then the transformation $S_t^{-1} \cdot S_{\tau+t}$ defines the manner how the point into which x_0 is transformed by S_t, is transformed by S_t.

I choose $\tau = -2\sqrt{x_0}$, then:

$$S_{-2\sqrt{x_0}} : x' - x_0 = -(x - x_0) + \ldots,$$

hence this transformation takes the point x_0 back to x_0 again. But:

$$(S_{-2\sqrt{x_0}})^{-1} : x' - x_0 = -(x - x_0) + \ldots$$

$$S_{t-2\sqrt{x_0}} : x'' - (t - \sqrt{x_0}) = (x' - x_0)\left(\frac{t}{\sqrt{x_0}} - 1\right) + \ldots$$

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[121] '*Schar*'.

so it follows:

$$(S_{-2\sqrt{x_0}})^{-1} \cdot S_{t-2\sqrt{x_0}} :$$

$$x'' - (t - \sqrt{x_0})^2 = (x' - x_0)\left(1 - \frac{t}{\sqrt{x_0}}\right) + \ldots$$

i.e. this transformation is not S_t but S_{-t}. However, one obtains by group theoretic continuation from S_t the inverse transformation S_{-t}, just like when one makes an analytic continuation of S_t and goes once around the zero point.

This seems to prove clearly that the assumption of univaluedness of the transformations in the neighborhood of a point and the assumption that the group for each of these transformations also contains its inverse in the neighborhood of this point does not imply at all the univaluedness, let alone the unique invertibility in the domain which this point reaches under the transformations of the group.

Also when the infinite transformation is one-valued, the transformations of the one-parameter group can be multi-valued, e.g. $x' = x + e^{-x}\delta t$ gives

$$x' = lg(e^x + t)$$

But enough of this now. With best greetings

Yours truly
F. Engel

To my consternation I see just now, that I dispatched only half of my letter. ⟨122⟩

[Signed autograph – in Brouwer; also in *CWII* p. 147–148]

1912-02-12a

From O. Blumenthal — 12.II.1912[a] **Aachen** ⟨123⟩

Dear Mr. Brouwer! [`Lieber Herr Brouwer!`]

I have received Lennes' paper and your criticism. Thank you very much. I thank you especially for having justified your evaluation so thoroughly and

⟨122⟩Written upside down on top of first page. ⟨123⟩Date and place - postmark.

precisely. Consequently I have of course returned the paper to Mr. Lennes, with your comments enclosed, and I have moreover referred to your paper on the Jordan theorem, which he doesn't seem to know.

Again best thanks and many greetings

Your
O. Blumenthal

[Signed autograph, postcard – in Brouwer; also in *CWII*, p. 487, with Freudenthal's comments]

1912-02-12b

From P. Koebe — 12.II.1912[b] **Leipzig**

Dear Mr. Brouwer! [`Geehrter Herr Brouwer!`]

Ad A. Mention is lacking of the important indispensable proof item ⟨124⟩ that two canonically cut Riemann surfaces of genus p can always be continuously transformed into each other. (One can do without that only for the boundary circle theorem, but not for the other fundamental theorems). Klein's proof, Annalen, 21, ⟨125⟩ for this is not possible, the way it is done there, in an exact manner, because the regularity of the analytic boundary correspondence is interrupted in the corners. Therefore I map the surfaces that are cut open with p separated return cuts (as in Annalen 69) ⟨126⟩ onto a normal ⟨127⟩ region with altogether regular boundary correspondence. These normal regions form *one* continuum, from which it follows that not only the surfaces of genus p form *one* continuum, but that also the ones with p return cuts do so. From this it further follows t hat surfaces that are cut open in *any* fashion also are continua.

Ad B. Poincaré had certainly not planned to prove Theorem 4. Poincaré rather represents the interpretation of *closed* continua by adding *limits of polygons*, a viewpoint which you too, still constantly emphasized in Karlsruhe. Poincaré recently informed me in conversation that the continuity method cannot be used *at all* if one wants to prove the no-boundary-circle theorem, because these manifolds are *not closed*. Your way of presenting it

⟨124⟩ *Beweispunkt.* ⟨125⟩ [Klein 1882]. ⟨126⟩ [Koebe 1910]. ⟨127⟩ *schlicht.*

is therefore only an interpretation of Poincaré's views that afterwards was constructed under the impression of my communications in Karlsruhe, and in its kind a very original achievement. Also *Fricke-Klein's* 'Vorlesungen über automorphe Funktionen' has adopted extensively the view of Poincaré about *closedness*.

Ad C. More to the point and further taking into account the meaning of the achievement roughly as follows:

> while for the most general case in particular the theorems 3, 4 and A still lack an exact justification, which however according to his provisional communications in the Göttinger Nachrichten (see more in particular also the most recent communication 'Begründung der Kontinuitätsmethode im Gebiete der konformen Abbildung und Uniformisierung' (1912))⟨128⟩ has been achieved completely by Mr. Koebe, and which will be soon published in full extent in the Mathematische Annalen. The proofs found by Mr. Koebe extend to the case of boundary circle uniformization, the only oneconsidered by Poincaré, and imply a *life giving* advance, because of the liberation from the thoughts introduced by Poincaré and copied by Klein-Fricke about polygonal limits and closed continua, an advance which is at the same time a return to Klein's old standpoint of non-closed continua which was vigorously attacked by Poincaré. By the way, Koebe's continuity method represents also in relation to Klein a remarkable fundamenta advance because Koebe actually doesn't use Theorem 4, although this theorem can very well be proved, as Mr. Koebe told me in connection with his proof method, by enlisting the help of the 'choice-convergence theorem'.

NB: B and C can be best put in the form of a *footnote*, because it is not the text of a letter.⟨129⟩

Ad D. This goes indeed much better with the normal surface with $6p-6$ *parameters*, which is the Abelian integral of the first kind with $(p-1)$ modulo 6 and which gives a $2\pi i$, as I told you already in Karlsruhe.

⟨128⟩Foundation of the continuity method in the domain of conformal mapping and uniformization, [Koebe 1912] ⟨129⟩Note that Brouwer is requested to endorse a text that Koebe is withholding from Brouwer. For a discussion of the Brouwer-Koebe conflict see Freudenthal's comments in *CW II*, p. 572 ff. Furthermore [Van Dalen 1999] section 5.3, p. 189 ff. See also *Brouwer to Hilbert 9.III.1912*

Ad E. This footnote can be cancelled after the explanations ad C.

Ad F. Here A must be now considered.

I will soon send you my corrected page proofs. Please send me yours again.

With best greetings

P. Koebe

[Signed autograph – in Brouwer]

1912-02-14

To P. Koebe — 14.II.1912 **Amsterdam**
Overtoom 565

Copy

Dear Mr. Koebe [`Geehrter Herr Koebe`]

Fortunately I am still in possession of the abridged text of my Karlsruhe talk, which I enclose, so that you can no longer maintain that I used in Karlsruhe in the talk or in discussions the *'closed'* manifolds of Poincaré!

That you could make such a statement only proves that modern set theory must be absolutely unfamiliar to you. For, the elaborations of Poincaré who works with the so-called 'closed manifolds' are pure balderdash, and can only be excused by the fact that at the time of their formulation there was not yet any set theory.

That the proof of the "Weierstrass Theorem" (in Klein's terminology) and therefore the continuity proof for the case of the boundary circle can nonetheless be carried out on the basis of the other elaborations of Poincaré, was precisely the content of my lecture in Karlsruhe.

Through your communications I have acquired the further insight that by means of your deformation theorem my method can be carried over to the most general fundamental theorem.

What you recall from my lecture or from our conversation about the 'closed manifolds' used by me, refers to the following: I consider in the enclosed text automorphic functions as identical when they only differ by additive or multiplicative constants, and thereby I achieve that there corresponds to every internal point of the cube a *closed* manifold of functions with m poles. Only this justifies the word '*Alsdann*' ⟨130⟩ on p. 3, l.19 of the enclosed text, since only because of this closedness one can obtain certainty that a point sequence in M_π always will have a limit point that belongs to M_π, if the corresponding point sequence of the cube has a limit point inside the cube.

Please return the enclosed text to me after a few days.

With best greetings

L.E.J. Brouwer

Why don't you send me a copy of your manuscript, as I did, and as you promised me?

[Typescript copy – in Brouwer; also in *CW II* p. 585, with Freudenthal's comments; a signed autograph copy was attached to *Brouwer to Hilbert, 24.II.1912*]

1912-02-24

To D. Hilbert — 24.II.1912 **Amsterdam**

Dear Geheimrat, [`Lieber Herr Geheimrat!`]

I request your help and protection in a very disagreeable matter. ⟨131⟩ On January 2 I sent Koebe a copy of my letter to Mr. Fricke, which was sent in December and presented to the Göttinger Gesellschaft der Wissenschaften on January 13, and about one week later I received the enclosed postcard. This card was followed on February 14, *not* by the promised manuscript, but by the letter that is enclosed here together with my answer; in it I have marked in blue pencil the statement that my refutation refers to (all the rest is nonsense). ⟨132⟩

Koebe can however not really mean the statement concerned, just as little as anyone who has heard my talk in Karlsruhe. Hence I sense in Koebe's statement merely his intention to give in his next note the matter the appearance that my letter to Fricke contains certain thoughts that I have learned

⟨130⟩Consequently ⟨131⟩i.e. the Koebe affair. See [Van Dalen 1999] section 5.3. ⟨132⟩Letter *Brouwer to Koebe, 14.II.1912*.

in conversations with Koebe, while the true state of affairs in Karlsruhe was that I contributed to those conversations the complete continuity proof of the boundary circle case, whereas Koebe only contributed some inkling that his deformation theorem [133] could be used somehow in the continuity method. In fact he said in the session of September 27 at the end of my talk the following: 'Because on the basis of my deformation theorem *nothing can happen* during continuous change of modules, the achievements of Brouwer are in my train of thought dispensable.' To this I emphatically answered: 'The deformation theorem can only extend the boundary circle result obtained by Poincaré, and thereby also at the same time extend my continuity proof to the most general case; in this extension my contributions remain as before necessary in their full strength.' Then Koebe spoke the nonsensical words: 'What Mr. Brouwer has shown, I do with Poincaré sequences.' and then Klein closed the discussion.

Only after long private discussions, in which also Bieberbach, Bernstein and Rosenthal took part, Koebe learned subsequently from me between September 27 and 29 which partial result (incidentally formulated already by Klein in Annalen 21, and at that time called "Weierstrass Theorem" by me) can be obtained by means of his deformation theorem, and which remaining part remains to be treated by my contributions. And in those conversations I have, as the just mentioned gentlemen must know exactly, brought up *all* details of my present note.

However, several warning voices told me already at that time: 'All that you now are explaining to Koebe, you will only with the greatest difficulty be able to claim as your property, as soon as he will have understood it', and indeed certain symptoms in Koebe were visible that seemed to prove these voices right, so when I had returned home I wanted, in order to avoid an unpleasant fight with Koebe, to abandon any publication about this matter which is anyhow rather far removed from my interests and with which I had only occupied myself in passing on Klein's request. Only after Blumenthal had urged me and I moreover had heard that Klein would like to see a publication by me, it came to this note of January 13.

My request would now be the following: Just as I didn't receive Koebe's earlier promised manuscript, he will not, I believe, send me the now promised page proofs *before* my note is declared ready for printing, so that I will not for instance be able to adjust in time the text to refute Koebe's claims in advance. May I ask you now to arrange that I get the Koebe page proofs directly from the printer? And in case I then would find that they contain

[133] *Verzerrungssatz.*

the above mentioned or other falsehoods, to make him extirpate them so as to avoid unpleasant polemics?

I would be most grateful to you for that.

With best greetings
your

L.E.J. Brouwer.

On my enclosed letter of 2/14 I haven't heard anything more from Koebe. The text of my talk he hasn't returned to me either.

[Signed autograph – in Hilbert]

1912-02-27

To D. Hilbert — 27.II.1912 Blaricum

Dear Geheimrat [Lieber Herr Geheimrat]

For your better information I am sending also to you an abridged text of my lecture in Karlsruhe. I hope that you will be able to recall that, so to speak, every word of this text was also spoken in the lecture. In any case you must be able to call to mind that in my lecture I applied the continuity method neither to the Klein polygon continuum nor to the allegedly 'closed' group continuum of Poincaré (as Koebe claims) but to *the continuum of automorphic functions with m poles*, and that this indeed constituted the core of the matter.

Will the Wolfskehl Symposium about the foundations of mathematics go through, which you planned last summer for the Easter vacation?

Many greetings to both of you, also from my wife

Your
L.E.J. Brouwer.

[Signed autograph – in Hilbert]

1912-03-06a

To F. Engel — 6.III.1912[a] **Amsterdam**
Overtoom 565

Dear Professor, [Sehr geehrter Herr Professor!]

Absorbed by many activities, I was until today not able to reply to both of your extensive letters, which made your ideas now very clear to me. Now I address once more all points of my first letter.

The word 'obscurity' which you used in your first review must, according to your more detailed explanations, be interpreted as 'difficult to understand for the uninitiated'. About such a *subjective* view one can of course not argue, but many a reader will, contrary to your intention, have received from your words the impression that I have not defined my fundamental concepts with sufficient precision, which would be a very *objective* error, which I most emphatically must reject (the only purpose of the 'rectification' in the beginning of the second Correction in Bd. 69 ⟨134⟩ was not to exclude from the outset certain singular connectivity situations that do not occur in finite continuous groups).

The 'certainly lies' on p. 255 amounts, as you will now certainly see yourself, without further ado to the theorem that every continuous function ('continuous monotone function' would even suffice for this case) which takes two values, also takes every intermediate value; the reader of the Annalen hardly needs to be reminded of such a trivial theorem. Yet also here, as I believe, many a reader of your review will have got the impression, that my article contains *several objective gaps*, of which you pointed out the one just mentioned one merely as an example.

Therefore you would do me a great pleasure if you could decide to insert in a possibly forthcoming review of my second communication a remark to rehabilitate me.

As far as the inner foundation of my general assumptions is concerned, I believe that I can clarify in a most complete way by means of the following reflections.

Let us call a point set m *concatenated*, ⟨135⟩ if according to some law certain infinite point sequences f that belong to m are assigned certain points p_f that likewise belong to m and that are characterized as *limit points*

⟨134⟩ [Brouwer 1910a]. ⟨135⟩ *verkettet* in German, which is translated as 'bound together', 'connected', or 'concatenated'. We have opted for the last term.

of f in such a manner that for every point p_f there is always a *subsequence* of f that only has this single point as limit point, and that the limit points of a subsequence of f constitute a subset of the limit points of f, while finally the following property holds: if α_μ is the only limit point of the sequence $\alpha_{\mu\nu}$, and α the only limit point of the sequence α_μ, then every $\alpha_{\mu\nu}$ contains such a final segment $\alpha_{\mu\pi}$, that the sequence of these final segments *only* has α as limit point.

A point set that is not concatenated, i.e. a point set without limit points, is called *discrete*.

By a *neighborhood* of a point p that belongs to m we mean a subset of m that contains infinitely many points of every point sequence in m that has p as limit point.

(Let us now construct around every point p of m a neighborhood u_p, and let us choose two arbitrary points p_1 and p_2 in m. If, independently of the choice of u_p and of p_1 and p_2 it is possible to put a *finite* point sequence in m such that two consecutive points of this sequence belong to one and the same u_p, then m is called a *connected* point set.[136])

Let us call a map from one point set m to a point set m' *continuous*, when every limit point of a point sequence of m corresponds to a limit point of the corresponding point sequence of m'.

Let us call the point set m *homogeneous*, when for every neighborhood u_p of an arbitrary point p of m each other point of m has a neighborhood that can be mapped one to one and continuously onto u_p.

Now let an arbitrary point set m carry an arbitrary group γ containing the identity and pairwise (single- or many-valued) inverse elements. We then cover m with a point set μ such that any two coinciding points of m will also be considered identical in μ if and only if each transformation of γ will take them into two coinciding points of m. Furthermore in μ the point π will be considered to be a limit point of the sequence $\mathcal{F}$ if in the first place the point p in m corresponding to π is the limit point of the sequence f that corresponds to $\mathcal{F}$ in m, and in the second place this relation between p and f will be preserved by an arbitrary transformation of the group γ. Finally the transformation τ in γ will be considered as limit element of the infinite sequence φ of transformations, when every arbitrary point of μ is taken into such a point π by τ and by φ in such a point sequence $\mathcal{F}$, that π is a limit point of $\mathcal{F}$.

Consequently both the 'transformation manifold' μ and the 'parameter manifold' γ are *homogeneous point sets*, and in reference to μ the transforma-

[136] Here Brouwer defines the notion of 'connectedness'. In [Brouwer 1911c] he introduces the definition that is now universally accepted. For a discussion of the history of 'connected' see Freudenthal in *CW II* p. 486.

tions of γ are not only pairwise inverse, but also *one to one and continuous*, while the point set m now appears as a *folding* (i.e. as a single valued — not one to one — and continuous image) of μ.

Hence every group of (single- or many-valued) pairwise inverse transformations of an arbitrary point set *from a homogeneous group of one to one and continuous pairwise inverse transformations of a homogeneous transformation manifold* (a group of this latter kind will be called a *canonical group*, when both the transformation manifold ⟨137⟩ and the parameter manifold ⟨138⟩ are closed [15]) *be obtained by folding of the transformation manifold.*

Now only the following types of closed homogeneous point sets are until now known (and probably no others exist):

a) discrete point sets.
b) finite dimensional manifolds R_n according to my definition.
c) countably infinite dimensional manifolds R_ω (compare the relevant articles of Fréchet).
d) point sets of order type ζ of disconnected, nowhere dense, perfect point sets of R_n.
e) '*product sets*' constructed from sets of the four previous kinds (e.g. a discrete set of order types ζ of three-dimensional spaces).

And the most general canonical group, for which both the transformation manifold and the parameter manifold belong to type e), can be composed in a simple way from canonical groups for which both the transformation manifold and the parameter manifold belong to one of the types a), b), c), d), which therefore can be called *prime groups*.

Examples of prime groups are the finite substitution groups (parameter manifold and transformation manifold of type a)), the Fuchsian and Kleinian groups (transformation manifold of type b), parameter manifold of type a)), the finite continuous groups according to the definition of my Annalen article (parameter manifold and transformation manifold of type b)), the infinite continuous groups (transformation manifold of type b), parameter manifold of type c)), the ζ-groups to which I called attention in the Amsterdam Proceedings of April 1910 ⟨139⟩ (parameter manifold and transformation manifold of type d)).

In complete agreement with the above we get the example of your last letter (the group $\sqrt{x'} = \sqrt{x} + t$) from the translation group of the plane,

[15] We call a point set *closed* when there exists for every point a neighborhood in which every fundamental sequence has a limit point which also belongs to the point set.

⟨137⟩ In text: T.M. ⟨138⟩ in text: P.M. ⟨139⟩ [Brouwer 1910f].

i.e. from a finite continuous group in my definition, *through folding* (in this special case by a two-to-one mapping) *of the transformation manifold*.

The nature of this folding is, by the way, not subject to any limitation; because it is completely arbitrary, one can make it in specific cases also so complicated that the group cannot be expressed by analytic formulas. The related canonical group will however not be influenced by that; it remains a finite continuous group in my definition.

I would be very glad if the above has clarified why in my view the restriction of the problem formulated in my Annalen article, namely 'determine all finite continuous groups', is a completely natural one, and does not in the least entail an artificial restriction.

With best greetings ⟨140⟩

Sincerely yours
L.E.J. Brouwer

[Signed autograph, draft – in Brouwer; also in *CWII* p. 149–152 (with Freudenthal's comments)]

1912-03-06b

From P. Koebe — 6.III.1912^b ⟨141⟩

Dear Mr. Brouwer! [`Geehrter Herr Brouwer!`]

I am looking forward with interest to the publication of your talk in Karlsruhe. However, I cannot agree to the publication of parts of your letter to Fricke ⟨142⟩ because you have no right to the, so to speak arbitrational, presentation given there, and because the achievements of Poincaré and me appear there in an unworthy and incorrect light. Also, in view of the publication of your talk in the Jahresbericht the publication of the letter is anyway superfluous.

Yours truly ⟨143⟩
P. Koebe

[Signed autograph – in Hilbert]

⟨140⟩ *Ihr ganz ergebener.* ⟨141⟩ Date postmark. ⟨142⟩ [Brouwer 1912d]. ⟨143⟩ *Ergebenst.*

1912-03-07

To D. Hilbert — 7.III.1912 Blaricum [144]

Dear Geheimrat [Lieber Herr Geheimrat]

Let me add to my preceding letter that Koebe is in my opinion *obliged* to send me his proof sheets, and this for the following reason: When I believed in November I had to conclude from a letter from Fricke that Koebe was almost ready with a note for the Göttinger Nachrichten on the continuity proof, I proposed to Koebe to edit our notes in mutual agreement, and *only after Koebe had accepted this proposal*, I have sent him first my manuscript and then my proofs. When he now for his part sends me neither the one nor the other, he is guilty of the most outrageous faithlessness. Such a thing one does not have to accept! Moreover he stubbornly refuses to send back to me a manuscript of my lecture in Karlsruhe which three weeks ago I lent him for a few days. All of this is so mysterious to me! Or does Koebe's note perhaps not yet exist, and does he behave in this way only to gain time? In that case I would like ask you not to wait any longer for him, and to get my note now printed. Please, write me a line!

Best greetings! [145]

Your
Brouwer.

[Signed autograph, postcard – in Hilbert]

1912-03-09b

To D. Hilbert — 9.III.1912^b Blaricum

Dear Geheimrat [Lieber Herr Geheimrat]

After mailing my last letter to you I received the enclosed card from Koebe. It brings neither the recantation of his false statements about my talk in Karlsruhe that I desired, nor the promised page proofs of his note

[144] Postmark Blaricum. The address of the sender is given in handwriting: 'Overtoom 565, Amsterdam'. [145] *Schöne Grüsse.*

that he owes me. I'll now have to give up the hope that he will return to reason, and therefore I ask you to get my note for the Göttinger Nachrichten printed now. [16] In the meantime I set great store by rebutting here to you, the objections to my note that Koebe has raised in his letter ⟨146⟩ and on the enclosed postcard. ⟨147⟩

ad A) and E) of the letter. Koebe apparently doesn't know Fricke's cube theorem ⟨148⟩, otherwise he would understand that the premise that the cut up surfaces constitute a *single* continuum does not play a role in my proofs.

ad B) of the letter. The correctness of my quotation concerning Poincaré will be substantiated by the publication of my Karlsruhe talk.

ad C) of the letter. Here Koebe moves in a vicious circle, because on the one hand he demands from me that I extensively praise his paper which hasn't appeared yet, on the other hand he tries to prevent me from seeing this article.

I emphasize again that I don't know anything about Koebe's achievement except the vague idea he formulated in Karlsruhe, namely to use the deformation theorem ⟨149⟩ for the continuity method, and that I nonetheless quote therefore Koebe only in a very specific way, because I have justified for myself, in all detail, that Theorem 4 follows completely and generally from the deformation theorem.

Ad D) of the letter. Koebe apparently doesn't understand that a not one-to-one but continuous specification of a set by r real parameters doesn't guarantee at all that this set is an r-dimensional manifold without singularities.

To the statement on the card that of both publications in the Jahresbericht and in the Göttinger Nachrichten ⟨150⟩ one makes the other superfluous. The similarity of both notes is a purely superficial one; in their contents they supplement each other, and the role of the article in the Jahresbericht amounts to the justification of both footnotes 1) (p. 2) and 1) (p. 4) of the Göttingen note.

That the planned note of Koebe doesn't contain any falsehoods or insinuations concerning me, is, by the way, l more in Koebe's interest than in

[16] At the same time I send my second page proof to the printer, which contains a small subsequent change (insertion of the word 'recently' [*neulich*, ed.] on p. 2 l. 6 from below).

⟨146⟩ *Koebe to Brouwer 12.II.1912.* ⟨147⟩ *Koebe to Brouwer 6.III.1912.* ⟨148⟩ *Würfelsatz.* ⟨149⟩ *Verzerrungssatz.* ⟨150⟩ [Brouwer 1912d, Brouwer 1912c].

mine, because in my eventual refutation I will probably not be able to avoid to disgrace him irreparably.

With best greetings

your
L.E.J. Brouwer

[Signed autograph – in Hilbert]

1912-03-26

From F. Engel — 26.III.1912 **Greifswald**

Dear Dr.! [Sehr geehrter Herr Dr.!]

Thank you very much for your letter of the 6th of this month. I would have liked to answer a long time ago, but notwithstanding the vacation, I was all the time hampered.

In the review of your second communication, which I am just now preparing, I give an explanation of the sort you wish. I hope that you will be satisfied by it.

I agree fully with the considerations in your letter, and I freely admit that in this manner your restriction of the problem seems completely natural. But I miss in both of your articles any indication of the fact that thereby also a much more general problem is dealt with, and such a hint seems to me quite necessary, for which reader will figure that out by himself?

On the other hand I as yet lack the comprehensive view to see that thus now also the case that only a piece of the group is given, in the neighborhood of the identity transformation, and in the neighborhood of a point, is completely settled. Because in the groups one really meets, one actually always knows in advance only such a piece.

Furthermore even a group that one knows in its complete extension, can be given in such a form that, so it seems to me, difficulties arise.

For example, if one writes the general projective group homogeneously and with canonical parameters, [17] then the coefficients are everywhere convergent power series of the parameters, but ∞ many parameter systems give the same transformation. One should really always be on guard for some-

[17] I would be very grateful to you if you wouldn't use the word 'canonical' in yet another new meaning. That can cause confusion.

thing like that. Can you now always replace such a parameter manifold by one in which the relation between the points and the transformation is one to one? I shudder for the generality of such considerations and I cannot arm myself against the fear that it isn't feasible to exhaust all possibilities.

Anyway I wish very much to be able to speak to you in person at some time.

With best greetings

yours truly
F. Engel

[Signed autograph – in Brouwer; also in *CWII* p. 153]

1912-03-29

To F. Engel — 29.III.1912 **Blaricum**

Dear professor [`Sehr gelehrter Herr Professor`]

For your promise in your letter about your review [151] of my second communication I thank you most kindly.

As far as your example of a p-dimensional parameter manifold γ is concerned, in which infinitely many points correspond with the same transformation, it follows from my previous letter that this parameter manifold γ is from my point of view not the true parameter manifold, but that it changes into the true and likewise p-dimensional parameter manifold γ' only by identifying all points that correspond to the same transformation into a single point, so that γ' contains for every transformation only a single point. Naturally γ' will in general be quite differently connected than γ, more in particular, if γ has the simple connectivity of the p-dimensional *number space*, then this property will be generally lost for γ', so for purposes of calculation one will be often obliged to return to γ.

Now I come to the case mentioned by you, that initially only an n-dimensional piece of space τ in the neighborhood of a point P is given, which carries a p-dimensional set π, that lies in the neighborhood of the identity and that also contains it, and that consists of one-one and continuous and pairwise inverse transformations.

[151] [Engel 1913].

If we can speak of a group generated by this system (τ, π), then of course also a procedure $\mathcal{F}$ must be given by which the transformations of π also are meaningful for all points into which the points of τ are transformed by arbitrary repetitions of π; in the case of transformations given by power-series such a procedure $\mathcal{F}$ will naturally consist of analytic continuation.

Assuming this, the point set m which is generated from τ by arbitrary repetition of π is acted upon by a *group* γ consisting of these repetitions of π. This group of pairwise inverse (one- or many-valued) transformations is a so-called 'prime group' (cf. my previous letter) for the *homogenous* point set μ formed by the 'unfolding' of m, *and we will say that 'the system* (τ, π) *determines a finite continuous group' if and only if* μ *coincides with* τ *in a certain neighborhood of* P *and* γ *with* π *in a certain neighborhood of the identity.* (Indeed, if τ and π are assumed totally arbitrary, then one will generally find that μ and γ are manifolds of a *higher* dimension number than τ and π, mostly even of countably infinite dimension.)

At the same time it is clear that if one knows that the system (τ, μ) determines a finite continuous group, the procedure $\mathcal{F}$ is completely determined, for it must necessarily consist of the 'group theoretical continuation'.

With the best greetings, and also hoping for my part that I can meet you soon in person,

Your most truly [152]
L.E.J. Brouwer

[Signed autograph, draft – in Brouwer; also in *CWII* p. 154–155, with Freudenthal's comments]

1912-05-16

H. Weyl to F. Klein— 16.V.1912 Göttingen

Dear Geheimrat, [`Sehr geehrter Herr Geheimrat`]

About the *factual* differences between Koebe and Brouwer I am only very insufficiently informed. In carrying out the continuity proof three things play a role:

[152] *Ihr ergebenster.*

1) the group continuum,

2) the continuum of Riemann surfaces of genus p,

3) the mapping of both of these onto each other.

In 1) *Brouwer* relies throughout on earlier investigations (Klein, Fricke, Poincaré), that prove that one is dealing with a single connected continuum. *Koebe* takes up this part again and simplifies it substantially by using the deformation theorem ⟨153⟩ which relieves him from the investigation of all degeneracies (boundary parts of the continuum) and at the same time offers the possibility to expand the continuity theorem to all further cases (*Brouwer* only considers the boundary circle case). However I am not certain whether I assess the role of the Koebe deformation theorem correctly, because the course of the proof is completely unknown to me.

Ad 2): Here it seems that the tool of extension of the dimension number is necessary for Brouwer, and also a precise formulation of the circumstances under which two Riemann surfaces of the same genus can be held to be 'little different from each other' (precise formulation of the continuity concept in the manifold of Riemann surfaces). *Koebe* thinks that the extension of the dimension number is something of very secondary importance in the whole proof and claims (which *Brouwer* has disputed) that the theory of functions and integrals on the surface would yield $3p-3$ modules that correspond in the strict sense *one to one invertible* and continuous with the Riemann surfaces; e.g. one would obtain them with Riemann, as one maps the given surface by means of a suitable normed integral of the first kind.

Ad 3) That the theorem proved by Brouwer about the invariance of the n-dimensional domain is here the decisive argument is admitted without restrictions by *Koebe* too.

Koebe seems to present the matter to be that this, but also only this, is Brouwer's merit, namely that he has ascertained by this theorem the foundation of all continuity proofs, whereas he claims for himself: to have developed in a 'drastic' and 'plain' way those tools that in the specific case of the uniformization problem make the realization of the continuity proof possible. *Brouwer* for his part seems to attach great value to the *priority*; he disputes that Koebe was in Karlsruhe in possession of a proof without gaps, while he, Brouwer, at that time had completely proved along his own lines the matter for the case of the boundary circle.[18]

[18] About the exchange of letters between Brouwer and Koebe I don't know anything at all.

⟨153⟩ *Verzerrungssatz.*

That it has come to a conflict is not because of the matter itself, but the cause is rather the contrary characters that have collided here, Koebe's lack of concern about the claims of others, and Brouwer's irritability and passionate vehemence. [19]

Herr Geheimrat *Hilbert*, with whom I spoke today, and who sends you his best greetings, strongly rejected to exert influence whatsoever on either of the combatants; he didn't go into the matter at all and said only 'they are two adult persons, they must know themselves what they do'. It is not clear yet when Brouwer comes to Göttingen, anyway only *after* the Pentecost holidays.—

What I called in our discussions 'completeness of an axiom system' was in mathematized form nothing else but this: of every theorem, in which only are used *such* concepts, as are defined on the basis of those occurring in the axioms, it must be determined *on the basis of the axioms* whether they are true or false. And that is really the ultimate notion of 'completeness' that one can ask for. Each question that is *comprehensible* on the basis of the axioms, must be decidable with their help. If I leave for example the axiom of Eudoxos out of the axiom system for the real numbers, then the question 'Are there infinitely small magnitudes', i.e. is there a number $\varepsilon > 0$ for which every integral multiple $n\varepsilon < 1$, is comprehensible on the basis of these axioms {the concepts: 0, 1, entire number, multiplication, $<$ and $>$ occur in them; entire number $= 1 + 1 + \ldots + 1$}, *but is not decidable.*

About set theory, real variables and differential equations of mathematical physics I will try to collect some material for next time. If in fact, you, dear Herr Geheimrat, will lecture in the next semester about 'The development of mathematics in the 19th century' (and not about projective geometry), then I am of course willing to take part in the corresponding seminar; I will be able to learn there much myself.

Sunday evening I have returned again on foot to Goslar. On the journey home I met Hilb. ⟨154⟩

[19] See last page of this letter [ed. - Weyl had added an extensive footnote on a separate sheet:] At the end of last semester I received once from Brouwer a card with the content whether I wouldn't have so much influence on Koebe to help him, Brouwer, 'to get his property back'; Koebe had kept the manuscript that Brouwer wanted to compare with the page proofs for one or two weeks with him, notwithstanding Brouwer's request to send it back immediately. Altogether I have the impression that the present tension between Brouwer and Koebe is caused by such personal frictions, much more than by differences in content.

⟨154⟩ Probably E. Hilb.

I wholeheartedly hope, dear Herr Geheimrat, that your recovery will make further good and quick progress, so that you will not be confined for too long to the solitude in Hahnenklee.

With the most devoted greetings,
most respectfully yours[155]
Hermann Weyl

[Signed autograph – in Klein]

1912-05-22

From L. Bieberbach — 22.V.1912 Köningsberg

Dear Mr. Brouwer [`Lieber Herr Brouwer`]

Thank you very much for your kind letter. In order to give an as detailed as possible answer, permit me to repeat each time the individual sentences of your letter, and use this as a starting point for my answer.

1. 'To a certain system s of generating substitutions of a group of Schottky type belongs one (as far as the class is concerned) completely determined Riemann surface F equipped with p return cuts. When therefore there belong to the system s several fundamental domains[156] that cannot be transformed into each other by permitted modifications, then this can only be because the p return cuts of the surface in both cases are completed in a different way to a canonical system of p pairs of return cuts.'

The completion to pairs doesn't matter at all. The problem is rather that different fundamental domains belong to the same system of generators. These correspond then on the Riemann surfaces, that are uniquely determined by the group, to different systems of p return cuts, i.e. to two systems, that cannot by mere translation over the surface, be transformed into each other (cf. Dissertation[157] p. 22–23, p. 35–36). One sees this quickest in the case $p = 3$.

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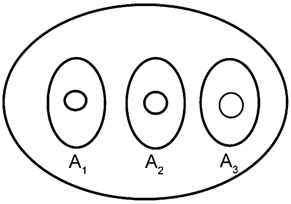
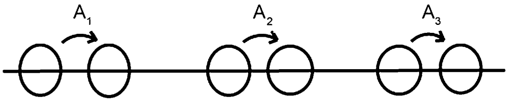
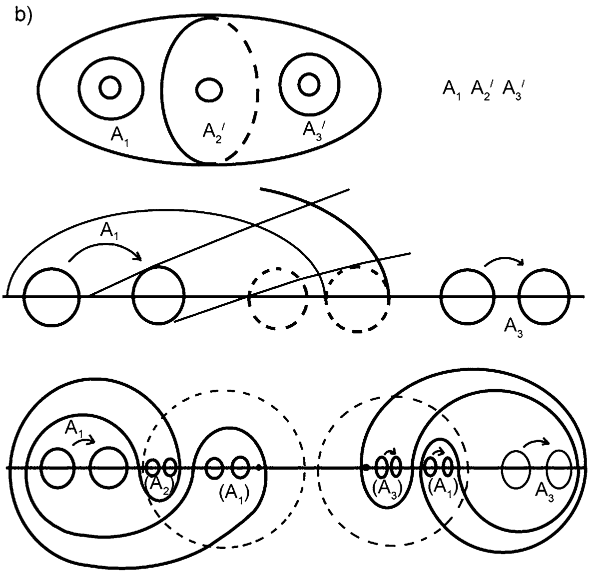
[155] *Mit den ergebensten Grüssen, Ihr Sie hochverehrender.* [156] '*FB*' in text, replaced throughout by '*Fundamentalbereich*'. [157] [Bieberbach 1910].



Ring shaped surface a)
Return cuts A_1 A_2 A_3

[..?..] Let the fundamental domain then be something like





Moreover, in the right punctured hole I imagine pictured the new fundamental domain, which one gets by applying A_2 once, and on the left side analogously. The boundary corresponding to A_2' is indicated.[158]

2. 'But then I don't understand how this non-uniqueness of the fundamental domain in the case of given generators can influence the determina-

[158] Brouwer had deciphered the poorly readable above lines, and inderted them at the bottom of the page.

tion of the group by its generators, in other words, the control of the group continuum by means of the invariants as parameters.

You seem to assume implicitly that by indicating the invariants of finitely many or also all substitutions of the group, the group itself is determined up to linear transformations. This theorem seems to me not self evident. Anyway, I can't prove it. In Fricke, the proof in the boundary circle case seems to rely completely on the fact that only one fundamental domain belongs to a system of generators. But that is not satisfied. Anyway, here one can make a change. If one takes for instance the p generators and assigns then three of the $3p$ coefficient relations special values by norming through a substitution, then one can take the other $3p-3$ as parameters, or every substitution has 3 [...?...] 2 fixed points and the multiplicator. If one takes 3 fixed points, there remain $3p-3$ parameters constrained by inequalities. The group is then determined uniquely by these, but not the fundamental domains. One has now either the multiply covered variability domains of the parameter, hence the 'Riemann' space of the fundamental domain or just the variability domain of the parameters themselves instead of the Fricke polygon continuum. If one wants to proceed geometrically, then one must first look for a geometrical normalization of a fundamental domain belonging to a parameter system, for example by means of the Fricke normal polyhedron, whose cut from the sphere *perhaps* always produces a fundamental domain which is bounded by p pairs of closed curves. — indeed, not every fundamental domain of a Schottky group is bounded by $2p$ closed curves.⟨159⟩ If one then has in this way given a geometric interpretation to the domain of the parameters, that is, one has assigned to every parameter system a fundamental domain, then one must go further to finding boundaries of the group continuum within the polygon continuum, i.e. to the determination of a fundamental domain of the group of modules.

Zusatz⟨160⟩ Now it remains to show that this variability domain is a continuum; this is handled in Fricke again on the basis of the geometric meaning of the invariants but in the final analysis on the basis of the unique determination of the fundamental domain by the invariants. To prove the analogue here seems not to have succeeded until now. Hence the advantages

⟨159⟩Here Bieberbach refers in a footnote to the '*Zusatz*' on the next sheet – 'cf. see other side'. ⟨160⟩Supplement of your new continuity proof, which in the present context operates only with neighborhoods, no longer with the full continuum.

———

Thus one cannot, on the basis of the existence of those parameters, conclude the existence of a group *continuum*, and therefore one cannot determine it by these means.

These things, which are certainly a bit vague in their being indefinite and unsettled, were floating in my mind in the case of my note. I did not at all want to write all of that down, just observe that already in the beginning there must be differences from the line of Fricke's proof.

It was not my wish at all to express myself in print about these things, in any case I didn't want to have printed anything about my remarks in Karlsruhe which were by themselves essentially superfluous. It happened only on Klein's explicit 'command'. Also in my dissertation I have restricted myself to what I could determine with certainty. I seems, now that the continuity proof is settled, rather unnecessary to continue on this road, unless it is for quite other purposes — convergence of the Schottky sequences.

At the same time, I send a copy of my dissertation. I believed that I had done so a long time ago. I therefore beg your pardon for this omission.

If you are of the opinion that my note needs an thorough textual change, I ask you for a brief communication.

I will not come to Cambridge. But in any case to Münster. So see you there and cordial greetings.

L. Bieberbach

K.i.Pr. ⟨161⟩ 22.V.1912

NB. On p. 33 of my dissertation I have made a really stupid mistake. The result is correct as can be seen much more easily, namely by showing that the changes of the [?] on p. 32 can always be fulfilled by new circular domains.

[Signed autograph – in Brouwer]

⟨161⟩ Königsberg in Preussen.

1912-05-31

To D. Hilbert — 31.V.1912 Blaricum

Dear Mr. Geheimrat! [`Lieber Herr Geheimrat!`]

I'll now come next Sunday to Göttingen via Löhne, Hameln, Elze, and I will arrive at 5.38 in the afternoon. My wife has decided to accompany me. We will stay in Hotel Gebhard, and will stay until early Wednesday. We will use the Wednesday and Thursday for a tour of the Brocken, and the night of Thursday to Friday is reserved for the return trip, because on Friday I will be busy in Amsterdam.

For a better preparation of our coming conversation, I enclose two letters which will answer for you the plausible question why I got mixed up at all with Koebe in connection with the publication of my continuity proof. Indeed, the contribution to the continuity proof I presented in Karlsruhe consisted of two parts, of which the first one (the 'invariance of domain') already was submitted in July for publication, whereas with respect to the second (the 'extension of the group set to the set of automorphic functions with m poles'), Koebe claims priority, according to the enclosed letter of Bernstein (cf. the part marked with pencil). Because, moreover, this part didn't seem very deep to me, I hesitated of course to publish it, even though Blumenthal urged me to do so. Finally I sent the manuscript early November to Fricke with the question whether he considered the contents new and worth publishing, and then I received the enclosed answer. The statement about Koebe therein[20] that is marked with pencil complicated the matter so much that I, when a short time later Blumenthal as well as Fricke and also Klein (namely indirectly through Fricke) requested me to publish, I could not possibly do so without, for the sake of more certainty and clarity about Koebe's achievements, getting in touch with Koebe himself, because otherwise there was the danger that Koebe would accuse my publication of being trivial and me of being a plagiarist. In the exchange of letters with Koebe I then received on my very specific questions again and again evasive answers; the only thing I got out of him was the mutual agreement to edit our notes about the continuity proof in mutual understanding. How he then later broke his word and the matter got dragged along, you know.

[20]The manuscript was at first incorrectly understood by Fricke, who hadn't been in Karlsruhe, hence the unfounded criticism contained in his letter of December 1.

Well, the rest we'll discuss next week. My wife eagerly looks forward to meeting you again, as I do, and we both greet you cordially.

Your
Egbertus Brouwer.

[Signed autograph – in Hilbert]

1912-11-07

From F. Bernstein — 7.XI.1912 **Göttingen**

Dear Friend! [`Lieber Freund`]

I have suffered the last two months from a severe depression and although I thought all the time of writing you again, I couldn't get myself to make a decision to do so. That has actually been weighing heavily on my mind — it becomes ever more difficult the longer one waits.

Now things are improving in every respect. I feel physically good again and I am happy to be able to do something again. The cure in Wildbad ⟨162⟩ ruined me so badly that I often had to stay in bed for days. Only in Halle I have completely recovered. I got rid of the rheumatism, so much good has at least come out of it.

It was quite a pity that we couldn't meet this vacation. I had so much counted on it.

At our lunch table there have been big changes. We are not in Gebhard anymore. There only a disagreeable physicist has remained, whom we were foolish enough to get stuck with last semester. I eat in the Ratskeller with Försterling, Mrs. Jalli, Paul Hertz and others that you don't known: Defregger, Schwartz, Rusitskya.

Weyl has — incomprehensibly — left us.

You must have received Rosenthal's Habilitation thesis.

Can you tell me perhaps what kind of an impression Borel's rejoinder in the Annalen ⟨163⟩ made on you? Blumenthal has nicely tricked me, because he showed me a totally different manuscript, but not at all the final version, like for example his comparison of our proofs. Now I don't know

⟨162⟩ A '*Kurort*', a 'Spa'. ⟨163⟩ [Borel 1912].

whether in my answer it is clear enough that I think the note of Borel in the Rendiconti too inexact to be counted as a source of a proof. Because I have still some publishable material about the subject, I could once more put his embellishments in the spotlight.

How are you and how is your family? Many greetings to your honored spouse and the young lady your daughter.

With best greetings

Yours truly
Felix Bernstein

[Signed autograph – in Brouwer]

1913-02-06

From L. Bieberbach — 6.II.1913 Königsberg (Preussen)

Dear Mr. Brouwer [`Lieber Herr Brouwer`]

Unfortunately I must trouble you with a probably silly question. Recently I noticed your proof of the Jordan curve theorem in Annalen 69 ⟨164⟩ (If you still have a sufficient number of reprints available, I would be grateful to obtain one, because I have reprints of all your articles except this one.). On page 172 of this you construct a polygon p. Of this you use the property that it has an interior and an exterior. Now it seems to me that to conclude this one must know the correctness of this statement for *every* polygon. For I don't see how for example on page 171 for the polygon π one can obtain evident information of this decomposition property of p by a suitable special choice. With a construction according to the π-recipe one only obtains polygons that necessarily also contain points of N_1.

In this conjecture of mine I was enhanced by the fact that I don't see at which other place the two-sidedness of the Cartesian plane is used, except in this polygon theorem. I miss the proof of the above, which anyway isn't difficult, in your article. I would be very grateful if you would so kind as to put me on the right road.

⟨164⟩ [Brouwer 1910b].

With cordial greetings

Bieberbach

[Signed autograph – in Brouwer]

1913-04-16

To D. Hilbert — 16.IV.1913 **Amsterdam**
Overtoom 565

Dear Mr. Geheimrat! [`Lieber Herr Geheimrat`]

It is perhaps known to you that I am occupying myself for some time now with the new edition of Schoenflies' Bericht on set theory. This came about as follows: For some years now I was repeatedly urged from various sides to write a book on set theory, because the existing books and encyclopedia articles about this subject are too unreliable and superficial. When in the summer of 1911 in Göttingen such exhortations were addressed to me again, and I at the same time learned that Schoenflies was preparing a new edition of his Bericht, I thought that the desired aim could be reached with relatively little loss of time on my side, if I was given the opportunity to check Schoenflies' book during the printing process, and if necessary to improve and complete it. The difficulty to bring Schoenflies to submit to my supervision was soon removed, when, Fricke, who knew Schoenflies personally, offered to mediate, on the occasion of visiting him in Harzburg. ⟨165⟩ Schoenflies then was even most pleased to accept this proposition that was put to him through Fricke (how Fricke formulated it is however unknown to me); as a consequence I am involved in a correspondence about the relevant galley proofs. Meanwhile it turned out that with respect to the method and intensity of my cooperation Schoenflies and I harbor fundamentally different tendencies: Schoenflies would like to restrict my influence if possible to improvement of the false theorems and proofs, while I of course aim in addition at accomplishing completions and more depth. In this struggle I am the weaker party, because Schoenflies possesses the right of the final decision, even though he occasionally does make certain concessions out of

⟨165⟩This refers probably to a visit of Brouwer to Fricke, see the correspondence with Fricke.

courtesy (or maybe also out of fear that I will desert him because he has seen what a tremendous amount ⟨166⟩ of errors I have picked out for him). The farther the handling of the proofs now proceeds, the more Schoenflies urges me to hurry and the less I can achieve with him, maybe also because he knows he is safe with respect to the cruder errors, so he feels gradually less dependent on me. Nowadays I almost feel the work, once undertaken and therefore to be completed, to be a Sisyphus task. For example, it is a small effort to compose an insertion; but to find then later that Schoenflies, who initially had left the editing work to me, nonetheless wants to 'improve' it later himself, i.e. to insert errors, and also not to be able to put him right, he, who is always in a hurry, and who is, as he admits himself, totally overworked — that is for me an intolerable situation! Also in this way a meager advantage will be reaped which by far doesn't match the efforts I spent, because the general foundation of the book becomes deficient, and difficult questions will remain undiscussed in the book.

Relief would only be possible when from the side of a third party gentle pressure would be exerted on Schoenflies. In this respect I don't want to ask you anything specific, but Schoenflies will next week be, as he writes to me, in Göttingen, and then he will probably spontaneously come to speak with you about his Bericht. Should this happen, then a certain suggestion would come from you to the effect that he should leave me as much freedom as possible, wouldn't it? Given the great respect of Schoenflies for you, such a suggestion would immediately turn out to be very effective, that I am sure of.

Well, I wanted after all just to inform you about the above mentioned state of affairs. Anyway, there is no harm in it that you know about it, and maybe this knowledge enables you to drop a few words in the next weeks, which might be of the greatest help — not to me personally, but to set theory, hence to mathematics, which we both love.

Cordial greetings to you both, also from my wife. When it is somehow possible, I will come myself next week for a few days.

Totally yours ⟨167⟩
L.E.J. Brouwer.

[Signed autograph – in Hilbert]

⟨166⟩ '*Unmenge*'. ⟨167⟩ *Ganz Ihr.*

1913-06-16

To D. Hilbert — 16.VI.1913 **The Hague**
Haag

Dear Geheimrat [Lieber Herr Geheimrat]

I beg you now for advice. I can become an full professor, and in fact both in *Groningen*, where on the one hand I will be completely *free* in my professional activities, but where on the other hand I will find a petty provincial town and probably fewer sympathetic colleagues, and in *Amsterdam*, i.e. in a lively big city, which always has been intimately connected with my life, and which is close to my cosy home in Blaricum and to the dunes, and where I also feel comfortable in the faculty, where however I will be mainly charged with teaching *Mechanics*. If my official duties were the same in both universities, then I would of course not hesitate to choose Amsterdam; but I cannot possibly envisage to what extent an unceasing involvement with applied mathematics would divert my research from its natural course, and hence, at the same time, would more or less paralyze it. What is your opinion in this matter? Should I risk to stay where I feel at home, and count on it that the harmony between my thinking and mechanics will establish itself completely automatically?

You know me after all, and you have such rich experiences as a researcher. I wouldn't know anybody whose advice I would seek more than yours, now that I carry on the most intense struggles of indecision The small-town social life and the pressure of convention must be terrible in Groningen, and there is no countryside at all.

Now for something different. Just recently I read that a fourth edition of your Grundlagen der Geometrie ⟨168⟩ will appear. Have the remarks about Appendix IV, that I communicated to you in the fall of 1909 (i.e. on the work from the Annalen 56 ⟨169⟩) been taken into account? I would anyhow be happy to help out with the correction of the paragraphs concerned, should you wish so, and if the authorization for printing ⟨170⟩ hasn't been given yet.

Unfortunately, the effect of your suggestions on Schoenflies hasn't been lasting. Just look at the enclosed letter. ⟨171⟩ You will understand how difficult it is to me to have to read, after all my efforts, words like those

⟨168⟩ Foundations of Geometry. ⟨169⟩ [Hilbert 1902]. *Brouwer to Hilbert 28.X.1909*, see *CWII* p. 102 ff. with Freudenthal's comments. ⟨170⟩ Imprimatur. ⟨171⟩ Refers to the revised Bericht.

marked with pencil in the enclosed letter of May 29. Much indeed, that after an endless exchange of letters, finally found its correct formulation, he now diligently starts to mess up again. He must be *very* overworked, because he makes mistakes any student should be ashamed of.

But I will stay on my post to the end, and patiently continue to teach him and try to save what can be saved at all.

Now many cordial greetings to you and your wife

Your
L.E.J. Brouwer.

Please return the enclosed letters of Schoenflies to me. At the moment I work as member of the examination committee of the Technical University in Delft, and live for the time being in The Hague. At the same time I am sending you a picture postcard that certainly will evoke good memories in you.

[Signed autograph – in Hilbert]

1913-07-04

To D. Hilbert — 4.VII.1913 **Amsterdam**
Overtoom 565

Dear Mr. Hilbert [`Lieber Herr Hilbert`]

I suddenly received an ultimatum from Groningen,⟨172⟩ and I have decided today to opt for Amsterdam and mechanics.⟨173⟩ Quod bonum felix faustumque sit!⟨174⟩

With Schoenflies things are getting ever worse. When there is not within a few days a complete change in his behavior, I will reach the point that I finally give up the whole enterprise for which I have suspended — in the general interest — all activity of my own for 8 months. I only hesitate

⟨172⟩Brouwer was offered Schoute's chair. ⟨173⟩Brouwer was an extraordinary professor in Amsterdam, so the full professorship in Groningen had its advantages. In July 1913 he was appointed full professor in Amsterdam, after Korteweg had given up his chair, and accepted an extraordinary chair. ⟨174⟩May it be good, fortunate and prosperous (Cicero).

because I can't stand the thought that I have wasted my energy for such a long period of time. Couldn't you write him once more a line?

With the best greetings,

Your Brouwer.

[Signed autograph, postcard – in Hilbert]

1913-08-16

To A. Schoenflies — 16.VIII.1913 Jungborn (Harz)

Dear Mr. Schoenflies, [`Lieber Herr Schoenflies`]

Enclosed, I send you your sheets back, and I add my own proof which I have written down in extenso on sheets 1), 2) and 3). It still seems to me the best thing that you just stick to replacing the considerations between the middle of p. 312 (starting from 'Sind also') and the middle of p. 314 ('Mit diesem Resultat'), by my proof. If this is done in the form proposed by me, i.e. introduced by the footnote on sheet 1 above, then we avoid on the one hand the edge with respect to Lebesgue, and on the other hand the reader will *not in any* respect get even a *whiff* of an impression as if at this point your force had failed in some way.

More specifically I have the following objections to your elaborations. In the first place you write in your letter that in the case of two sets one can deduce the theorem about the sum of sets also *directly* from relation (a) on sheet b. This would however only be the case when one has already *in advance* the certainty that the sum of two measurable sets is *again measurable.*

Secondly — and this is more important — you assume on sheet a that $\{\alpha'\}$ and $\{\beta'\}$ and likewise $\{\alpha\}$ and $\{\beta\}$ and $\{\gamma\}$ are *relations*;[175]. this is not the case; *the difference of two relations need not always to be a relation.*

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[175] *Beziehungen*

[*Ed. supplement*]

[*A sheet with some remarks has been preserved with the above draft. The remarks were apparently meant to be included in the 'Vorwort zur zweiten Auflage'.* [176]]

pointed out×

× the cooperation of Mr. Brouwer is the more valuable because Mr. Brouwer's personal views on the foundations of set theory are in many points in sharp contrast with the guiding principles of this report.

———*For the preface*———

(before: all in the text) referred, and this though his personal etc.

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also [177] the word 'auch' [178] must be omitted; for it gives the impression that the 'Besserungen und Richtigstellungen' [179] constitute only something 'nebensächliches' [180] in my 'Unterstützung' [181]; by leaving out 'auch' they will however appear as the 'wesentliche Inhalt' [182] of the 'Unterstützing' — and that is also the 'genaue Wahrheit.' [183]

1913-11-08

From É. Borel — 8.XI.1913

Paris
Université de Paris,
Ecole Normale Supérieure
45 rue d'Ulm

Dear Sir [`Cher Monsieur`]

I hope the letter that I have sent you yesterday to the University has reached you. Reading your first letter, I had interpreted your remarks in the

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[176] 'Preface to the second edition.' See also Freudenthal's notes in *CWII* p. 367–370, in particular note 9, p. 369–370. [177] The following is entirely in Dutch except for the quoted words. [178] 'also'. [179] 'improvements and corrections'. [180] 'of minor or secondary importance'. [181] 'support'. [182] 'essential contents'. [183] 'exact truth'.

sense of the explications you give me in the your second one. I hope that the publication of my course, if it is realized, will satisfy you.

As regards to reprints, I have unfortunately the very bad habit of leaving the packages mostly unopened for months or even years, because of lack of time. And when so much time has passed by, most of the time I have thought about them, and blushed about the corrections or simplifications or additions for which I plan a new publication, and I don't have the courage to dispatch the old and obsolete publication. That is why I haven't sent you that note of the Bulletin de la Societé Mathématique; also I have renounced completely from having reprints made of some publications, like the Comptes Rendus, that don't provide them for free.

I send you by the same mail a large package to repair my shortcomings to you.

Yours very truly ⟨184⟩
Emile Borel

[Signed autograph – in Brouwer]

1914-06-04

From D.J. Korteweg — 4.VI.1914 **Amsterdam**
Vondelstraat 104-F

Amice,

Furthermore, De Vries informed me how much Göttingen takes up your time, and I understand very well that you don't wish to take talks now upon you.

My request was in fact solely the consequence of my endeavor to raise the level of talks as high as possible, and I rather expected that this time you would ask to be excused.

Your outpouring was less expected by me.

I saddens me much that you like your professorship so little.

However I consider this to be a subjective phenomenon, indeed related to your great gifts, the way everything is more or less related in a certain person, but not as inseparable from such gifts.

⟨184⟩ *Votre bien dévoué.*

Methinks this is proved by our physicists who actually are members of foreign Academies, and yet for a long period had not less official worries than you (van der Waals, Lorentz who took over Onnes' [185] lectures for medical students).

Thus it is hard to believe that six lectures a week, partly of an elementary nature, a few examinations a month (and well over four months almost undisturbed vacation) should stop someone from doing scientific work, even of the highest order.

If this indeed is the case with you, then truly there is nothing for it than that you accept as soon as possible a German professorship, and that opportunity will not fail to appear, although I expect that also there inhibiting influences will occur, if you are really *that* sensitive to them.

Another question is whether you, if you can share with me the conviction that the problem must be found *in yourself*, can't do a thing or two to diminish the conflict.

For example, to prepare your lectures in the vacation, so that you are all the time well ahead, and that each time you only need a moment to prepare. That takes away much of the nervous and hurried aspects that are inherent to the teaching new material for the first time.

And then I believe that the more regularly, I almost would say more commonplace, one arranges one's *ex*ternal life such as accommodation, working hours etc., the more one's energy increases and the easier and less painful one's *in*ternal life develops.

I don't know whether you can or want to follow such advice, but you will understand that I feel obliged to give it to you after your poignant outpouring.

I very much hope that you won't blame me for it; if not I would be very sorry, but I felt not free to omit it.

With cordial greetings

Your
D.J. Korteweg

[Signed autograph – in Brouwer]

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[185] H. Kamerlingh Onnes.

1914-06-20

To G. Hamel — 20.VI.1914 **Amsterdam**

Dear Mr. Hamel, [`Sehr geehrter Herr Hamel`]

Blumenthal forwarded your letter of June 9 to me; allow me from now on to write to you directly, to thank you cordially for your interest and kind help. Your idea to reduce the treatment of practical stability to a 'slow' withdrawal from the equilibrium position has surprised me very much, but it seems germane to me, and I share your conviction that moreover the stability on the smooth rotating saddle must allow for an experimentally verification.

I think I can shorten the proof even more than in your letter. Let the general solution of the frictionless equations of motion be:

$$\begin{aligned}
x &= c_1e^{\lambda_1 t} + c_2e^{-\lambda_1 t} + c_3e^{\lambda_2 t} + c_4e^{-\lambda_2 t} \\
y &= k_1c_1e^{\lambda_1 t} - k_1c_2e^{-\lambda_1 t} + k_2c_3e^{\lambda_2 t} - k_2c_4e^{-\lambda_2 t} \\
\dot{x} &= \lambda_1c_1e^{\lambda_1 t} - \lambda_1c_2e^{-\lambda_1 t} + \lambda_2c_3e^{\lambda_2 t} - \lambda_2c_4e^{-\lambda_2 t} \\
\dot{y} &= k_1\lambda_1c_1e^{\lambda_1 t} + k_1\lambda_1c_2e^{-\lambda_1 t} + k_2\lambda_2c_3e^{\lambda_2 t} + k_2\lambda_2c_4e^{-\lambda_2 t}
\end{aligned} \qquad \textbf{(I)}$$

The state of motion of the material point can be defined on the one hand by the values of $x, y, \dot{x}, \dot{y}$, on the other hand by the four quantities $\gamma_1, \gamma_2, \gamma_3, \gamma_4$, by which we mean the corresponding values of c_1, c_2, c_3, c_4, if we let the given state of motion correspond to the zero point in time. The system of values $(x, y, \dot{x}, \dot{y})$ and $(\gamma_1, \gamma_2, \gamma_3, \gamma_4)$ are one to one and homogeneously linearly related. To one orbit there belongs the simply infinite manifold of value systems (by which we in the following mean a real orbit curve) $(\gamma_1, \gamma_2, \gamma_3, \gamma_4)$, and this has in the stable case the property that every (mod.γ_ν) is constant, and hence also $\sqrt{\Sigma(\text{mod. } \gamma_\nu)^2}$. For, one has $\gamma_1(t) = e^{\lambda_1 t}\gamma_1(0)$ and so on. Likewise $(\text{mod. } d\gamma_\nu)^2$ is constant for two adjacent (also with respect to time) orbits. For, one has again $d\gamma_1(t) = e^{\lambda_1 t} d\gamma_1(0)$, and so on.

From the equations (I) it furthermore follows that the ratio of

$$\sqrt{dx^2 + dy^2 + d\dot{x}^2 + d\dot{y}^2}$$

to the corresponding values of $\sqrt{\Sigma(\text{mod. } d\gamma_\nu)^2}$ varies between two fixed bounds, which are both different from 0 and ∞. The maximal value of $\sqrt{dx^2 + dy^2 + d\dot{x}^2 + d\dot{y}^2}$, measured from each point of the one to the 'closest' (i.e. yielding a minimal $dx^2 + dy^2 + d\dot{x}^2 + d\dot{y}^2$) point of the other orbit I call the path distance of the two neighboring orbits; let h be the maximum value in the whole system of orbits, which maximum certainly exists because of the above, of the ratio between the maximum and minimum value for any two neighboring orbits of the expression $\sqrt{dx^2 + dy^2 + d\dot{x}^2 + d\dot{y}^2}$ thus obtained.

Then, because of the sliding friction, the path distance covered during the time element dt is $< hk\,dt$. The increase of the 'shift' (i.e. the minimum of $\sqrt{dx^2 + dy^2 + d\dot{x}^2 + d\dot{y}^2}$ for the orbit hence likewise is $< hk\,dt$ and the increase of the shift between the times t_0 and t is $< hk(t - t_0)$, which proves your theorem.

Please allow me one more remark. You write in your letter the equations of motion in the following form:

$$\begin{aligned} \ddot{x} - 2\omega\dot{y} + ax &= 0 \\ \ddot{y} + 2\omega\dot{x} + by &= 0 \end{aligned}$$

and you state the *conjecture*, that in case $a, b > 0$ the friction causes convergence to a position of rest. The fact that standstill is reached already after a finite time follows from the existence of the energy integral of the frictionless motion:

$$H \equiv \frac{1}{2}(\dot{x}^2 + \dot{y}^2 + ax^2 + by^2) = c$$

In fact, for the changes of H caused by the friction the following holds:

$$\frac{dH}{dt} = -k\sqrt{\dot{x}^2 + \dot{y}^2}$$

Hence the positive definite form H can only decrease, which proves the stability of the motion with friction. Furthermore the total orbit length must be finite, because $dH/dt = -k$. Now if the end point of the orbit lies at a finite distance from the equilibrium point, then close to this end point only unboundedly decreasing velocities occur, the direction of which must approach the direction of the attracting force, because of the attractive force ($X = -ax, Y = -by$) and the friction force, and it cannot cross that direction, but on the other hand it must reach it, because otherwise the acceleration would converge to a finite limit, whose direction would be different from the limit of the velocity, so that the velocity could not be exhausted by the integral of the acceleration. Hence the velocity has in the end point the direction of the vector $-(a\bar{x}_x + b\bar{y}_y)$, so the point *approaches* during the end of the orbit the equilibrium position ($x = 0, y = 0$). Hence the limiting value of the attractive force can't be equal to the friction force, because otherwise the components of velocity and acceleration in the direction of the limiting tangent would have the same sign in the neighborhood of the end of the orbit!

Hence in the neighborhood of the end point the friction force dominates the attraction force with a finite surplus, so after a distance ε from the end point is reached, the end point itself will be reached within a time element of order ε.

But if the end point of the orbit lies in the equilibrium position itself, then $x, y, \dot{x}, \dot{y}$ will finally all become vanishingly small. At that moment however the friction force dominates, and the resting position is reached in a vanishingly small time.

Again thanking you, yours sincerely, ⟨186⟩

Your
L.E.J. Brouwer

[Signed autograph, draft – in Brouwer; also in *CW II* p. 684–686, with Freudenthal's comments]

1914-07-13

From D.J. Korteweg — 13.VII.1914 **Amsterdam**
Vondelstraat 104-F

Amice,

With reference to your letter one pragmatic remark.

Would you please postpone your official discharge as member of the prize contest committee until both problems that have been entered by you or with your cooperation, have been dealt with? ⟨187⟩

I will then delete them from the program of 1915; but the possibility exists that answers are submitted, and methinks you will see the reasonableness of my request, which amounts to you being then able to function as first reporter.

⟨186⟩ *Mit nochmaligen Dank, in grosser Hochachtung.* ⟨187⟩ The Dutch mathematical society offered regularly prize problems, that were judged by the above mentioned committee. These prize problems have generated some outstanding research.

Another proposal is that you *stay on as* a member and that I take on me the commitment to have you report only on the problems posed by yourself (and not as second or third for the others).

I think I can assume responsibility for this, taking into account the heavy duties that await you in your function as editor of the Annalen, ⟨188⟩ because I feel convinced that also the other committee members will prefer this over your full discharge. With respect to a possible successor as chairman, this letter can serve as a guarantee; that's the reason why I will write down the P.S. that I am adding on a separate sheet.

Please, a yes or a no on both these matters.

Greeting

Your
D.J. Korteweg

---

13 July '14

P.S. Whereas in different circumstances I would be very pleased with your prestigious appointment as editor of the world's foremost mathematical journal, my heart isn't in it for more than one reason.

First, I view the work, with which you are being flooded from Göttingen, as a very serious and *enduring* obstruction against continuing your own independent work, and yet *that* is what you will be judged by in the long run, also in Germany.

Second, I foresee that consequentially you will more and more withdraw from the life of the Dutch mathematical community, even though just the opposite attitude is expected from a Dutch professor, and rightly so in my opinion. That this is a great disappointment for me, is less relevant; however the fact itself would be very regrettable for the further development of this life.

Third, I fear that you will search for the cause of diminished fertility in a place where it is not, or only for a small part: in your *professorship*, and that you will consider this more and more to be a real nuisance.

At first I had planned to discuss this point in more detail, and raise, among other things, the obvious objections to your proportion. ⟨189⟩ but your comparison of a Dutch professorship with six hours of lectures, and

⟨188⟩ Brouwer was made associate editor (*Mitarbeiter*) of the *Mathematische Annalen* in the summer of 1914. ⟨189⟩ This is rather vague, it probably refers to the comparison below of the duties of the Dutch professor and of the family doctor in the country.

some of them of a very elementary nature (while the others leave you great freedom in the choice of subjects, plus four months of vacation) ⟨190⟩ to the position of a country physician with a busy practice, makes me, on further reflection, feel that this would be not be successful because in my view you have lost here all sense of proportion.

One thing I have to admit in order not to be unfair to you. Namely this, that your elementary lectures seem to be for *you* a great problem, because they make you impatient and seem to make you temporarily unfit for other work.

I wish I were younger and had more time ahead, to do in addition to the obligations I haven taken on, to which I have recently adapted my position, ⟨191⟩ something for you in this matter; but that is not possible and one cannot very well demand that from De Vries.

Moreover, I can't really understand that this problem should be in the long run insurmountable for you.

In my opinion you are just as grossly exaggerating, in calling the situation of mathematics in the universities deplorable, and in seeing in the Dutch environment an obstacle for the development of a gifted young mathematician.

I totally disagree with that. It seems to me that this Dutch environment, consisting of the Academy, ⟨192⟩ universities, and the Wiskundige Genootschap ⟨193⟩ (lectures; prize contests; the Revue ⟨194⟩ which, as it were, presents each beginning mathematician with the worldwide constantly developing mathematics, in which he will have to take part if he wants to accomplish anything; an almost complete journal collection) is by no means the deplorably insufficient environment you claim it is.

Methinks even that if you consult your own experience, you will have to recognize that you found in it many beneficial stimuli, a great freedom in the choice of your field of study, and for the rest nothing but recognition and encouragement.

In my opinion one should not overestimate the influence of the environment, neither in the positive, nor in the negative sense, because after all every mathematician of any importance has to take his *own* education

⟨190⟩In the original text there is punctuation instead of brackets. ⟨191⟩Korteweg, who was only four years away from his retirement, had exchanged his chair for an extraordinary professorship, so that Brouwer could become a full professor. ⟨192⟩KNAW. ⟨193⟩Dutch Mathematical Society. ⟨194⟩Revue semestrielle in hand and find his *own* way; but in the emergence of you, and recently Schouten, I see the proof that this environment is at any rate not unsuitable at all.

I really believe that every gifted mathematician has the opportunity to fulfill his capabilities, which naturally are rarely *very* extraordinary.

People of this kind are rare and therefore because of the laws of probability small in number, hence distributed irregularly over the different breeding grounds. In the Netherlands one may not expect to find always representatives of them in each field. I was overjoyed when it appeared that now mathematics got its turn, and I would be very sorry if in your case this would become a disappointment because of you being absorbed in editorial work, even though this is of the highest level.

Meanwhile, let us hope for the best now the matter is the way it is.

[Signed autograph – in Brouwer]

1915-06-11

From H.A. Lorentz — 11.VI.1915 **Haarlem**

Zijlweg 76

Amice,

When we strolled through Amsterdam after the last meeting of the Academy ⟨195⟩ and we came to speak about the mathematics vacancy in Leyden, it was on the tip of my tongue, that I would like nothing better than that you yourself could come to the decision to exchange Amsterdam for Leyden. I didn't mention it because the faculty hadn't met yet.

Now we have had a meeting and it turned out that it was the unanimous wish that you would, if possible, occupy the vacant post; we all consider this of the greatest importance for the flourishing of the faculty.

More in particular Kluyver, De Sitter, Ehrenfest and I would very much appreciate to be able to collaborate with you. You could be certain, that you would be welcomed warmly and with open arms.

Kluyver and I would like to come over sometime to discuss the matter, in order to explain the intentions of the faculty and answer questions from you to our ability. *Preferably* tomorrow, Saturday evening; we can be at your

⟨195⟩ KNAW.

place at about eight o'clock. We could also come, if that suits you better, on Sunday afternoon or Monday evening. Let me know, if you please, when you can receive us.

With friendly greeting

t.t.
H.A. Lorentz

[Signed autograph – in Brouwer]

Editorial supplement

[*How serious the Leyden offer was, is illustrated by the following letter. Apparently Ehrenfest was willing to make considerable sacrifices to attach Brouwer to Leiden.*]

H.A. Lorentz to P. Ehrenfest — 9.VI.1915 *Haarlem*
Zijlweg 76

Amice,

You make us a very generous offer, and I believe that we must accept it if there is no other means to relieve Brouwer from mechanics; that is, in the case that the Keesom plan would still meet too much resistance in the faculty, or the minister would after all not be inclined to satisfy our wishes. But I would regret it very much if this should have to be the solution. You have devoted yourself now with heart and soul for almost three years to theoretical physics and you had as a professor very good results; I would regret it if this fortuitous activity would suffer from an larger number of lectures.

On a few other points in your letter I must answer as follows.

a. Your plan to give Keesom only crystallography and in this manner humor M., I would think is very good, but I consider it unrealizable now.

Indeed, in present circumstances one can only obtain some money when it's a matter of great and urgent importance, like in the case of

Brouwer joining us. The problem of crystallography has no relation at all with that.

[..]

Now it seems to me that the best thing is to mention tomorrow your declaration of willingness, because it shows that in any case the mechanics course is provided for. We don't have to discuss it right now in more detail, but we can negotiate first with Brouwer. If he is prepared to do so, then we can resume the discussions in the faculty, and then I would not yet want to give up right away on the Keesom plan (i.e. the first plan).

With cordial greeting, t.t.

H.A. Lorentz.

[..]

[Signed autograph – in Ehrenfest]

1915-06-19

To P. Zeeman — 19.VI.1915 Blaricum [196]

Dear Colleague, [`Hooggeachte Collega`]

On behalf of the Faculty of Mathematics and Physics of the State University of Leyden I am invited to accept a chair in geometry there, with the promise that the Faculty will completely consent in my putting into practice the view that discharging the task of a professor consists more of dedication to one's own scientific work and being accessible for independently working students that look for guidance and information, than of regular lectures on routine theories that since long have been expounded clearly in books. The oral explication of the invitation was summarized by colleague Ehrenfest

[196] 'To Prof.Dr. P. Zeeman, Chairman of the Faculty of Mathematics and Physics of the University of Amsterdam.'

with the words: 'So materially nothing more is asked from you than being there.' ⟨197⟩

Now there are three circumstances that make the Leyden chair offered to me preferable over my present Amsterdam working environment, unless it would be possible to obtain certain encouraging assurances from the Curators.

First, I have since long experienced that the 'Leyden' interpretation, of the task of a professor, alluded to above, is not shared generally in the Amsterdam Faculty of Mathematics and Physics, which has become for me, who as youngest member of the Faculty doesn't feel strong enough to go my own way against other currents, the reason that I am, since my accession to office, handicapped in my scientific work in a very discouraging manner. Only from an encouragement from the Curators, I could derive the strength to carry out my task in full accordance with my own insights and conviction, which, incidentally, would be beneficial not only to my own scientific work but also to the interests of the students, and where there should be no fear at all that I would take the above quoted words of colleague Ehrenfest too literally as a guideline.

Second, the three mathematical sciences (mathematics, mathematical astronomy and theoretical mechanics) are represented in Amsterdam by a weaker teaching staff than in the State Universities, because we have for those subjects only two full and one extraordinary professor available, and in Groningen en Leyden one has three full professors for them. In Utrecht mathematical astronomy is joined with practical astronomy, and theoretical mechanics with theoretical physics, so that a precise comparison isn't possible; but because there one has two full professors for pure mathematics (without astronomy and mechanics), one must consider the strength there roughly equivalent to that of each of the two state universities.

Third, I, who cannot feel in good health for more than a few days, in the low-lying Dutch towns, and who has never been capable of intense mental work, while residing in such towns, would as a Leyden professor be able to live outside of the municipality without further formalities, for example in Noordwijk or Wassenaar, whereas as an Amsterdam professor I have to use two houses, one in Amsterdam because of the municipal ordinances, the other in Blaricum, where I am obliged, in the interest of my work and my health, to seek refuge for several days per week.

This disadvantage which is for me connected with Amsterdam could also be remedied, if I would be permitted to live outside the municipality. Disad-

⟨197⟩ Ehrenfest was quoted in German.

vantages for the regular course of teaching or my contact with the students should not be feared, because as a rule I am accessible at the university for students during the four days per week that I spend in Amsterdam for office activities, and for more extensive discussions people already now know very well how to find me in Blaricum, where my home has a telephone connection and is easily reached within an hour from Amsterdam.⟨198⟩

I already had a conversation on the above matter with our President Curator. He indicated to me that he personally was not unsympathetic to my viewpoints and wishes, and he said that he planned to bring up this matter next Friday, June 25, in the meeting of the Curators. It doesn't seem unlikely to me, that our President Curator would appreciate an explanation from your side before that time, and it is this consideration that has led to this letter to you.

Sincerely yours,

Your servant
L.E.J. Brouwer

[handwritten note on top of first page:] 'This letter back to P. Zeeman, please.'

[handwritten note (Zeeman) at the bottom of last page:] 'mathematical reading room!! + entry for books (f 500 per year).'

[Signed autograph – in Zeeman]

1915-09-18

To C.J. Snijders⟨199⟩ — 18.IX.1915 **Blaricum**
Loevesteyn⟨200⟩

Copy

Excellency [`Excellentie`]

In the conviction of acting in the national interest, I take the liberty to call Your Excellency's attention to a branch of practical mathematics,

⟨198⟩The distance is about 25 km. ⟨199⟩To 'His Excellency the Supreme Commander of Land and Sea Forces at 's Gravenhage', i.e. to general C.J. Snijders ⟨200⟩One of Brouwer's houses in Laren-Blaricum.

which recently found application in the armed forces of several countries, but which is, as far as I know, not applied in the Netherlands army; I mean *photogrammetry*. Especially, it is the flying service, the usefulness of which is considerably raised by connecting itself to a photogrammetric service. Indeed, only by means of photogrammetry is it possible to obtain exact maps and profiles of the recorded terrain from aerial photographs (which are strongly deformed, and about which moreover generally neither the correct location, nor the correct orientation of the aircraft during the shooting of the photo is known). And also, only by means of photogrammetry is it possible, if one possesses a map of the terrain in peacetime, to indicate on such a map the location of the means of war (such as batteries or trenches), observed and photographically recorded by the airmen, in order to fire at them with a chance of success, also if there is no opportunity to perform range shots. I am sending Your Excellency a brochure as an enclosure, in which are expounded the basics of photogrammetry and its methods in the developmental stage of 16 years ago. I will be glad to provide extensive and more recent literature, and I am also prepared to give oral comments on the above.

I have the more readily proceeded to writing this, because it seems to me that an efficient photogrammetric service can be established at fairly small costs and in a rather short time.

Hoping that Your Excellency will excuse my frankness as motivated by the national interest, I sign with due reverence

(sgd.) L.E.J. Brouwer,
Member of the Royal Academy of Sciences,
Professor of Mathematics in Amsterdam

Enclosed: Jahresber. der M.V. VI,
Heft 2, containing a photogram-
metric report of Finsterwaldes ⟨201⟩.

[Autograph, copy – in Zeeman]

⟨201⟩ [Finsterwalder 1899].

1915-10-12

From C.J. Snijders [202] **— 12.X.1915** **’s Gravenhage**
General Headquarter

Copy

To Professor L.E.J. Brouwer [`Aan den Hooggeleerden Heer L.E.J. Brouwer`]

Member of the Royal Academy of Sciences
Blaricum, house ‘Loevesteyn’.

Returning the *Jahresbericht, etc.* enclosed in your missive of last September 18, I have the honor to communicate to you the following.

In the department of aviation the need has until now not been felt to reduce images obtained of terrain to particular images, that are suitable for performing measurements upon.

By the use of our excellent topographic maps on a scale of 1:25,000 on which the minutest details are indicated, it is possible to use a photograph taken from an airplane for marking precisely each added fortification, trench, etc. on the map.

Nonetheless the commander of the aviation department has turned his attention to the study meant by you; the results obtained in this matter in Austria by Schimpfling are very encouraging indeed, so there will be no hesitation to proceed to the establishing of a photogrammetric service in the aviation department, if the need will be felt.

Meanwhile I thank you for the pains taken by you to draw attention to this matter.

The General,
(sgd.) Snijders

O.V.I. No. 3363
(Div. G.S. No. 9565
Attachments: a booklet,
Subject: Photogrammetry

[Autograph (Brouwer), copy – in Zeeman]

---

[202] General C.J. Snijders, commander in chief of the Dutch army.

1915-11-04

From W. Blaschke — 4.XI.1915 **Leipzig**

Fockestrasse 51

Dear colleague, [`Sehr verehrter Herr Kollege`]

I have recently spoken with Dr. Ackermann, an owner of the publishing house B.G. Teubner, and I approach you at his request. It concerns the following. Your fundamental geometric articles are not easily accessible. On the one hand they have appeared, scattered here and there, furthermore the Dutch language presents difficulties to some, and finally your articles are very succinct.

It would therefore be much in the interest of science, if you could decide to present your researches together in one book. Mr. Ackermann would be very glad if you would give preference to his publishing company, which can be considered to be the strongest mathematical publishing house next to Gauthiers-Villars, and with which I myself always have had the best experiences.

As far as I can recall some of your remarks, you are not really opposed to the idea of writing a book. Perhaps you could some time write to me or directly to Teubner, what you think about it. Let us hope, that the time will not be too far off, that there will also again be opportunity for peaceful meetings of mathematicians.

With best greetings

Your
W. Blaschke

[Signed autograph – in Brouwer]

1915-11-19

To W. Blaschke — 19.XI.1915 **Leipzig**

Fockestrasse 51

Dear Mr. Blaschke [`Lieber Herr Blaschke`]

A few years ago Blumenthal had already asked me to edit a book for his series appearing with Teubner: ‘Fortschritte der mathematischen Wissenschaften in Monografieen'. At that time I thought I could not yet promise it; now ⟨203⟩ my circumstances have changed somewhat, and I believe I can already make a promise. You can inform Mr. Ackermann on my behalf, but please point out to him that Blumenthal has prior rights to get the book for his series. The title would be something like: 'New investigations in topology'. ⟨204⟩ I intend to also incorporate the work of others (Tietze, Carathéodory, Lebesgue, Sierpiński).

Your letter suddenly reminded me that you have already asked me before Christmas of last year for a report for the Fortschritte ⟨205⟩ on my paper: *Eenige opmerkingen over het samenhangstype* η. ⟨206⟩ I am now really very sorry that I was so much occupied by exams, that I simply have forgotten to answer you. I wholeheartedly beg you to accept my apologies for that. It would have been, by the way, in my interest to answer you immediately essentially as follows:

The content of the article is given in the Revue Semestrielle des Publications Mathématiques ⟨207⟩ XXI 2, p. 99. The ⟨208⟩ essential point is contained in the theorem formulated there in the last three or four lines. This theorem was mentioned in a conversation in Cambridge ⟨209⟩ between Borel and me about sets of measure zero; neither of us had a proof at the time. Following that we each published independently and simultaneously a proof; I [did] in the article: 'Eenige opmerkingen enz.', ⟨210⟩ Borel in the paper: 'Les ensembles de mesure nulle', ⟨211⟩ which appeared in the Bulletin de la Societé Mathématique de France, 41 (1913), p. 6–14. But the Borel proof is not correct because on p. 9 he only ascertains that the ratio *of the dimensions of the successively constructed domains* lie between $(1-\varepsilon_1)\dots(1-\varepsilon_n)$ and $(1+\varepsilon_1)\dots(1+\varepsilon_n)$, but that the ratio *of the* ⟨212⟩ *corresponding ordinate differences of the corners of the domains* fail to do so; this entails that it can very well happen for a certain n, that the Borel conditions cannot be fulfilled, so that the construction fails. This is not just a gap in the proof that can be filled, but a real error which makes the whole proof collapse. I would really appreciate very much if in a report about both articles for the Fortschritte the above matter could be elucidated. Is that still possible? I

⟨203⟩ partially crossed out part: 'things have changed a little, and I would gladly follow Blumenthal's invitation'. ⟨204⟩ *Neue Untersuchungen über Analysis Situs.* ⟨205⟩ *Jahrbuch über die Fortschritte der Mathematik*; Author's review of [Brouwer 1913a] in JFd.M 44, p. 556 (also in *CW II* p. 405). ⟨206⟩ Some remarks about connectivity type η. [Brouwer 1913a, Brouwer 1913c]. ⟨207⟩ The Dutch reviewing periodical. ⟨208⟩ The draft is in telegram style – the sentence started with 'aber' ('but'). ⟨209⟩ International Congress of Mathematicians, August 1912 ⟨210⟩ [Brouwer 1913a]. ⟨211⟩ The sets of measure zero. ⟨212⟩ Words have been crossed out, so that the sentence is not quite clear.

have already explained the error to Borel himself, shortly after his article appeared.[213]

Did you receive any news from Weitzenböck? In the first year of the war I received a few cards from him, now, however, nothing more for several months.

At Pentecost I have visited Study.

Cordial greetings

from your
Brouwer

[Signed autograph draft/copy – in Brouwer; also in *CW II*, pp. 410–411, with Freudenthal's comments]

1916-02-07

To P. Ehrenfest — 7.II.1916 **Blaricum**

Dear Ehrenfest [`Waarde Ehrenfest`]

Your last letter still is on my desk, and only now it occurs to me that you wanted to ask me *a few more questions*, but that you wanted to await permission. Of course you can rest assured that you can put those questions to me, even though I can't say in advance whether I have time to study them.

I have searched the literature for the second question in your last letter, but I have not succeeded in finding the answer, which by now also for me is also *of the highest interest*, so if you find the solution somewhere, you would do me a great pleasure by telling it to me. I only can write you this about it:

Let $ds^2 = \sum_{h=1}^{n} \alpha_{hh} dx_h^2 + \sum_{h,k=1}^{n} 2\alpha_{hk} dx_h dx_k = \sum_{h=1}^{n} \beta_{hh} dy_h^2 + \sum_{h,k=1}^{n} 2\beta_{hk} dy_h dy_k.$

Substitute $dx_h = \sum_{k=1}^{n} \frac{\partial x_h}{\partial y_k} dy_k$, then it follows:

$$\beta_{hk} = \sum_{\mu=1}^{n} 2\alpha_{\mu\mu} \frac{\partial x_\mu}{\partial y_h} \cdot \frac{\partial x_\mu}{\partial y_k} + \sum_{\mu,\nu=1}^{n} 2\alpha_{\mu\nu} \left(\frac{\partial x_\mu}{\partial y_h} \cdot \frac{\partial x_\nu}{\partial y_k} + \frac{\partial x_\mu}{\partial y_k} \cdot \frac{\partial x_\nu}{\partial y_h} \right)$$

[213] *Brouwer to Borel 7.XI.1913*; see also Freudenthal's comments in *CW II* p. 407, 409.

i.e. for all values of $x_1 \ldots x_n$ *the points*

$$\left(\frac{\partial x_1}{\partial y_1}, \frac{\partial x_2}{\partial y_1}, \ldots, \frac{\partial x_n}{\partial y_1}\right), \left(\frac{\partial x_1}{\partial y_2}, \frac{\partial x_2}{\partial y_2}, \ldots, \frac{\partial x_n}{\partial y_2}\right), \ldots, \left(\frac{\partial x_1}{\partial y_n}, \frac{\partial x_2}{\partial y_n}, \ldots, \frac{\partial x_n}{\partial y_n}\right)$$

must form a polar simplex with respect to the manifold

$$\sum_{h=1}^{n} \alpha_{hh}\xi_h^2 + 2\sum_{h,k=1}^{n} \alpha_{h,k}\xi_h\xi_k = 0,$$

if all coefficients $\beta_{hk}(h \gtrless k)$ will cancel.

Which condition is equivalent to this, that the n points

$$\left(\frac{\partial y_1}{\partial x_1}, \frac{\partial y_1}{\partial x_2}, \ldots, \frac{\partial y_1}{\partial x_n}\right), \left(\frac{\partial y_2}{\partial x_1}, \frac{\partial y_2}{\partial x_2}, \ldots, \frac{\partial y_2}{\partial x_n}\right), \ldots, \left(\frac{\partial y_n}{\partial x_1}, \frac{\partial y_n}{\partial x_2}, \ldots, \frac{\partial y_n}{\partial x_n}\right)$$

must form a polar simplex with respect to the manifold

$$\sum_{h=1}^{n} A_{hh}\xi_h^2 + 2\sum_{h,k=1}^{n} A_{hk}\xi_h\xi_k = 0$$

For $n > 3$ the n functions y have to satisfy $\frac{n(n-1)}{2}$ hence *more than* n partial differential equations, which is generally not possible. However, for $n = 3$ the 3 functions y must satisfy 3 partial differential equations and *the existence proof of Cauchy works for these partial differential equations.*

Yet if we choose *for* $x_3 = 0$ $y_1, \frac{\partial y_1}{\partial x_1}, \frac{\partial y_1}{\partial x_2}, y_2, \frac{\partial y_2}{\partial x_1}, \frac{\partial y_2}{\partial x_2}, y_3, \frac{\partial y_3}{\partial x_1}$ en $\frac{\partial y_3}{\partial x_2}$ as arbitrary functions of x_1 en x_2, then for $\frac{\partial y_1}{\partial x_3}, \frac{\partial y_2}{\partial x_3}$ en $\frac{\partial y_3}{\partial x_3}$ can be found such functions for x_1 en x_2, that for $x_3 = 0$ the partial differential equation is satisfied (namely, it amounts to the determination of a polar triangle of a conic section, every vertex of which must lie on an arbitrary given straight line, and such a polar triangle can always be found).

So the problem is indeed possible for $n = 3$; If I have written on my last post card, written in haste, trusting my memory, that the problem is generally impossible, *also for* $n = 3$, be so kind as to return that card to me as it is discrediting for me.

Many greetings from home to home. How about the appointment of Van der Woude?

Brouwer

[Signed autograph – in Ehrenfest]

1916-05-06

To P. Ehrenfest — 6.V.1916 **Blaricum**

Dear Ehrenfest [`Waarde Ehrenfest`]

Hereby I return to you my letter of February 7. The post card with the incorrect information that preceded it I have destroyed, I am sorry if you think this narrow-minded and humorless, but after a certain experience with the German mathematician Koebe I have made it a firm *principle* for myself, firstly to be extremely careful with scientific correspondence and secondly to always try to get back into my possession any letters written by me from which scientific discredit might be extracted. This is a cool intellectual habit, which everyone who had an experience like mine would have adopted, and which is not accompanied by any mental affect of fear or remorse or such. I hope you will recognize the justification for such a habit in some cases, and that you will not have to withhold your respect for me on this account.

Many thanks for the bibliographic references concerning Einstein; since then I heard a talk in the Academy ⟨214⟩ by Lorentz on the subject, which deeply impressed me.

Furthermore I hope that you will accept my apologies for the delay in writing this letter; it was a consequence of being overloaded with correspondence. I had considered your last letter as 'not urgent' and consequently had put it on a pile where it had to wait its turn.

Cordial greetings also to your wife and from mine

t.t.
Brouwer

[Signed autograph – in Ehrenfest]

1916-09-16

To the Belgian Government — 16.IX.1916 ⟨215⟩

A QUESTION FOR THE BELGIAN GOVERNMENT

From conversations with Flemings residing hereabouts and belonging to various directions of domestic and foreign politics, it has become apparent

⟨214⟩KNAW. ⟨215⟩The present letter was published as an open letter in the Dutch weekly '*De Nieuwe Amsterdammer*'.

to me that among them there is well-nigh unanimity regarding the following facts that are in my view not at all generally known:

> *In Belgium there is no law regulating the official language used in university education.*

Hence a Belgian government that does not make Dutch the language for teaching in the University of Flanders and French in the Walloon university, cannot appeal to any law as an excuse for this violation of the natural rights of half the Belgian population, and bears personally full responsibility for this injustice. Hence the Flemings, who have been watching the German government violating Belgian justice for two years, cannot in the least be required to turn a blind eye to the eighty years of violation of Flemish rights by the Belgian government.

Furthermore *international law requires the German occupying force to maintain public life in Belgium to the best of its ability while respecting the national laws.* Because the national laws remain silent about the official language of higher education, the German authorities are not at all obliged to imitate the violation of justice committed by the successive Belgian governments, and according to international law it would even be *obligatory* to make the Ghent university Flemish, were it not that . . . because of a decision of the Belgian government at the beginning of the war, higher education in Belgium has been *suspended*, and its reinstatement by the German occupying force would only be legitimate if the interest of public order made this mandatory, which can be doubted with good reason.

Also the Flemings whose political attitudes are not foremost dominated by indignation about the German invasion, seem to have to refrain from any support of the German authorities in their efforts to make the Ghent university Flemish. Why do nonetheless many feel strongly inclined to give such support? *Because they distrust the Belgian government*, and they fear that after the war it will swiftly forget that the army defending Belgium was four fifths Flemish, and that it will violate Flemish rights as before.

Such a distrust may be insulting for the Belgian government; but it cannot deny that its past gives some cause for it, because when a bill was proposed (not by the government itself, which would have been proper, but by the members of Parliament Franck, Huysmans and Van Cauwelaert) to regulate the official language of higher education (a bill which in fact only had nothing but a negative tenor, namely to forbid *by law* further violations of Flemish rights in the future), it found before the war unconditional support with – if I'm right – only two of the ten members of the government.

And therefore the question: *Why doesn't the Belgian government make it easy for the Flemish to determine their position with respect to the German authorities in the matter of making the Ghent university Flemish, which touches so intimately upon their existence as a people, by openly declaring that the Flemish rights will not be violated again after the war if they can help it; namely that they have unanimously decided to take up the Franck-Huysmans-Van Cauwelaert bill after the reinstatement of Belgium?*

Thus it would provide the proof, to the satisfaction of all the Flemings, that it has the moral courage to refuse unconditionally not only to deliver the whole of the Belgian people to the German urge for expansion, but also to refuse to deliver half the Belgian people to the French urge for expansion.

L.E.J. Brouwer

[Printed – in 'De Nieuwe Amsterdammer'.]

1917-04-16

From A. Schoenflies — 16.IV.1917 **Mönichkirchen**
Hotel Windbichler

Dear Mr. Brouwer [`Lieber Herr Brouwer`]

On your recommendation we are reading here (my wife and I) the new novel by Meyrink, The green face! ⟨216⟩ After the first chapters I wanted to ask you whether you recommend me to train myself in time for the profession of magician and swindler, so I can find a reliable livelihood in The Hague in the new time after the war — now I know that I would be broken, atomized and blown away horizontally in the great cyclone. Unless you might in the meantime have learned vigilance, and I would have enjoyed the same undeserved fate as your friend, like Pfeill as friend of Hauberrisser. ⟨217⟩

By the way, did you read Gerhart Hauptmann's *Emanuel Quint, der Narr in Christo*? ⟨218⟩ It touches in part on Meyrink's novel, but is much more of value and to be taken seriously, compared to Meyrink's mixture of

⟨216⟩ Gustav Meyrink, *Das grüne Gesicht* (1916). This novel plays in post-war decadent Amsterdam. ⟨217⟩ Pfeill and Hauberrisser are characters in *Das grüne Gesicht*. ⟨218⟩ Gerhart Hauptmann was awarded the 1912 Nobel Prize for all of his work, but mostly for his novel *Der Narr in Christo Emanuel Quint* (1910) (The Fool in Christ: Emanuel Quint).

talmudic and Hegelian wisdom, of besotted dialectics, and a little understanding of his own!

Cordial greetings

your
A. Schoenflies.

[Upside down at top of the letter:] Cordial greetings
your Emma Schoenflies

[Signed autograph, postcard – in Brouwer]

1917-06-09

To G. Mannoury — 9.VI.1917 **Laren**

Dear chap [`Beste Kerel`]

Thank you for your history of mathematics and for the specification of your hours. To exclude misunderstandings (for it has become clear that Korteweg obtained a wrong idea from your letter about this) I repeat once more that if a mechanics course by you is realized next semester, you will only get the third year as listeners; maybe you can take in the second year too, when it has advanced far enough in analysis in the course of De Vries, which was also the case with my mechanics course that I started in 1915.

Enclosed I send you 8 times 12 copies in four languages of the manifesto plus the statutes of the International Academy of Practical Philosophy and Sociology.⟨219⟩ Since the time of drawing up the manifesto the Board of Directors for which at the time only four members were designated, is extended with L.S. Ornstein (professor of physics in Utrecht) and G. Mannoury. You declared yourself willing to do so, didn't you? That we have never invited you for another meeting, was only because we were certain that you were incapable of attending. We hope that this will change after the summer, and that you may perhaps now already find time to send some copies of the circular to Dutch or foreign acquaintances, so as to get letters of approval, and as a preparation for the appointment of representatives in

⟨219⟩Note the expanded name, cf. [Schmitz 1990] p. 223.

other countries. In that case add the names of Ornstein and Mannoury in ink, both in the statutes and to the signatures of the manifesto, and correct the sole remaining printing error in the German manifesto, where an umlaut was left in 'beinflusst'.

Your
Bertus Brouwer.

[Signed autograph – in Mannoury]

1917-10-01

From F.M. Jaeger — 1.X.1917 **Groningen**

Amice,

Many thanks for your letter and the effort spent. Today I have informed Schoute ⟨220⟩ about the matter, and I now let you know in the name of both of us that we would very much appreciate it if you would go ahead and talk to Zeeman about the matter, and if you would for instance tell him that we (and you as well) would be pleased to confer with him and Lorentz sometime at the end of October about that matter, if convenient before the general session ⟨221⟩ in a separate meeting. Personally I think that Mr. Korteweg's objections will rather be purely theoretical. Probably he doesn't know the military environment and has a much too exalted opinion of the amount of initiative among military authorities. For three years now the directorate of the army has had the chance to improve the army by means of the adjoined intellect. The result has been nil, simply because of the total lack of initiative. About the boundless bigotry in those circles I could tell you far worse stories.

Hence: *nothing* can be expected from common sense or initiative of the army administration, a fortiori not in a time of panic. So it *has* to come from our side. Wouldn't it perhaps be good to call Lorentz and Zeeman and also Mr. Lely ⟨222⟩ in conclave? He very much detests the military muddle, and he knows its spirit, or rather the total lack of any spirit, and maybe

⟨220⟩the Groningen meteorologist. ⟨221⟩KNAW. ⟨222⟩Minister of public works (1913–1918), see *Brouwer to Lorentz, 16.II.1918.*

he could achieve something for us with Cort van der Linden, ⟨223⟩ or in the Cabinet.

In any case, to me too, it seems that right now the Academy really can achieve something *good* in this matter, and that the best thing is to take action as soon as possible. —

In November my Lectures on the Principle of Symmetry will appear. I shall honor you with a copy. For, although it is not in the first place a mathematical work, it will probably, —if its mathematical shortcomings are kindly passed over,— give you some pleasure in a related field, and it would give me pleasure, if you would think well enough of it to introduce it to your younger pupils as something that might be if some use for their general education. In pure mathematics I am just a plebeian; possibly there is something informative for them in the Applications of the theory. If the gentlemen want a meeting still before the end of October, perhaps it is best to have that on Saturday morning.

With friendly greetings
tt
Jaeger

[Signed autograph – in Brouwer]

1918-01-09

To J.A. Schouten — 9.I.1918 **Laren**

Copy

Dear Sir [`Weledele Heer`]

I have informed you at the time about my view that the mental attitudes of the two of us are not suited for mutual understanding. At the same time I asked you only for a message, whether you wanted your duplicate manuscript ⟨224⟩ back from my archive on legal grounds.

The letter that was subsequently received would have been opened in the Christmas vacation, were it not that I heard from my friend Ornstein

⟨223⟩ Liberal prime minister (1913–1918). ⟨224⟩ See *Brouwer to Klein 19.IX.1919*.

(from whom you have earlier tried to find out my more intimate feelings in a manner that, as I assume, is permitted according to your morals, but that I find highly improper) that you have again taken a step with him in this matter 'to avoid squabbling' (!) Consequently the opening of your letter has been left undone, and neither will the letter received today from you, be read by me.

As I have meanwhile ascertained not to have a strong legal position in the matter of retaining your duplicate manuscript, I will now have a copy made at my own expense, and then I will return to you the copy belonging to you.

And now I urgently request you to leave me alone in the future. In my capacity of member of the Academy, Member of the board of the Dutch Mathematical Society ⟨225⟩ and Editor of the Annalen I always have felt obliged to reserve a large part of my time in the interest of young mathematicians at the beginning of their career, and you have profited amply from this. In return I demand no gratitude or apologies for the efforts made(even though words of to this effect from others never were entirely absent, when they took up my time in the same manner as done by you), but I do demand the strictest possible respect for the method that I consider correct in discharging this demanding task. And by your failing in this respect — also after the hint given to you — you have automatically put an end to any availability of my time for you (even for reading your, to me incomprehensible, letters).

Sincerely yours ⟨226⟩
(w.g.) L.E.J. Brouwer

[Signed autograph, copy – in Brouwer]

1918-02-04a

To M. Buber 4.II.1918[a] **Laren**

Dear Sir, [`Hochgeehrter Herr`]

The executive committee of the *Internationales Institut für Philosophie* has instructed me to answer your letter to our member Mr. Borel ⟨227⟩ of

⟨225⟩ *Wiskundig Genootschap.* ⟨226⟩ *Met verschuldigde gevoelens.* ⟨227⟩ Henri Borel, the sinologist.

March 17, 1917, in which you raise a fundamental objection to our manifesto. Hence I beg you to take into account the following:

In several cases the occidental word has indeed in addition to its material value also a spiritual value, but the latter is always subordinate to the former, and while the first has attained a certain and lasting orienting effect on the activity of the community in the sense that it stimulates the separate individuals to hinder each other as little as possible in their pursuit of physical certainty and material comfort, and possibly also even to support each other, the latter lacks any influence on the legal relationships (except insofar it is abused for deviously committing injustices); consequently its effects are weak, temporary and localized.

Words that have an exclusively spiritual value and that are suitable for orienting the community towards inhaling and exhaling the world spirit ⟨228⟩ and towards observing the Tao, don't exist in the occidental languages; should these exist, their effect would be paralyzed by the mutual physical hatred of people that live too close to each other, which has roots in the mutual distrust of the purity of their birth, and which obstructs the pursuit of material comfort of the separate individuals only to a small degree, but to a high degree obstruct the inhaling and exhaling the world spirit. The introduction of the first word with exclusively spiritual value into the general human understanding will as phenomenon be inseparably connected with the insight that this physical hatred is intolerable, and will immediately give rise to legal rules about human procreation.

But a possibility for this introduction will only be created, when the 'mystery of the emergence' of this word has taken place not in the isolated individual, but in the mutual understanding of a *community* of clear feeling and acutely thinking people that furthermore are materially not too close to each other.

Yours truly, ⟨229⟩

Prof. Dr. L.E.J. Brouwer

[Printed text – in *Comm. of the Intern. Inst. for Phil.* **1**, 1918; cf. [Brouwer 1918c]]

⟨228⟩ *Weltgeist.* ⟨229⟩ *Mit vorzüglicher Hochachtung.*

Editorial supplement

[*The following argument is at the heart of Buber's objection to the signific enterprise*⟨230⟩, see [Brouwer 1918c]:]

> Word creation, the making of a word, is for me one of the most mysterious events of spiritual life, indeed I admit that in my view there exists no *essential* difference between what I here call word creation and that which has been called the appearance of the Logos. The emergence of a word is a mystery, which takes place in the inflamed and receptive soul of man who is poetically creating, discovering the world. Only such a word that has been begotten in the spirit, can originate in man. Therefore, in my view, it cannot be the task of a community to make it. It rather seems to me that a society, such as the one planned by you and your friends, may only aim at *purifying* the word. The abuse of the great old words can be fought, the use of new ones can not be taught.

1918-02-15

From H.A. Lorentz — 15.II.1918 **Haarlem**

Amice,

After our last conversation we have considered in the board-to-be of the 'Scientific Committee' ⟨231⟩ in more detail how it can operate, and more in particular which subcommittees will have to be formed from ordinary and extraordinary members. You will recall that the committee will have the right to co-opt extraordinary members, a form that has been chosen because it seemed undesirable that one should have to ask for a decision of the Minister, each time when the need was felt for cooperation of experts in some field. I hardly need to add that the activity of the extraordinary members will be appreciated as much as those of the ordinary members. For the question which persons were to be proposed as ordinary members and which not, the crucial factor was mainly the size of the task that we had in mind for them; also most of the proposed ordinary members are

⟨230⟩ *Buber to Borel 17.III.1917.* ⟨231⟩ Wetenschappelijke Commissie van Advies en Onderzoek; Scientific Committee of Advice and Research experimentalists or technologists, i.e. people that have at their disposal the resources of a laboratory, or of factory, that they control.

It is not necessary that I give you the full list of subcommittees (for nutrition, clothing, fuels and minerals, agriculture, animal food, etc.), but I think that I can inform you already now that it seems to us that there should be one for survey-photographs obtained by airplanes; naturally we put our hopes on you in this respect. Moreover, that we thought of this point, we owe to what you have done already in this matter, the importance of which I have emphasized right away in my conversation with the Minister.

So we would appreciate very much if we could include you as extraordinary member in the committee and also if you would take a seat in the mentioned subcommittee. To this subcommittee would furthermore belong Dr. Schoute of the Meteorological Institute, who has already often been in the air, and myself. I am not informing Schoute yet, but with you it is of course a different case.

I myself would be very pleased if I could be of use to you in your work in any way and if I could contribute, so as to help that justice is done to it. Reading what you have written already about the subject gave me the impression that I would be in the right place in this subcommittee, whereas in the main committee with its experimentalists and technologists, I probably will have the feeling not quite to fit in. I would have stayed entirely out of the matter if I were not chairman of the Academy ⟨232⟩ and if the Minister hadn't explicitly insisted that the function of Executive Committee would be in the hands of some members of the Academy.

In view of this I could not shirk my responsibilities and naturally I have had a great part in the preparation, so there is much for which I am responsible. From the outset I had in mind an arrangement such as we now are going to get and of course I immediately have been considering who might be the ordinary and extraordinary members. The task that we would like to see you take on, is the one I had intended for you from the beginning. I imagined that you could accept that and so fulfill your duties vis-à-vis the nation, without too much disruption in your scientific work.

With cordial greetings from house to house

t.t.
H.A. Lorentz

[Signed autograph – in Brouwer]

⟨232⟩ KNAW.

1918-02-16

To H.A. Lorentz — 16.II.1918 **Laren**

Dear Mr. Lorentz, [`Hooggeachte Heer Lorentz`]

In the past weeks I have sincerely tried to acquiesce in my non-apppointment in the Scientific Advisory Committee,⟨233⟩ but I can't succeed. And on the contrary, this incident brings me more and more out of balance. Therefore I cannot act otherwise than asking you kindly to look at the affair from my point of view, based on the following exposition.

Since more than two years I have the ambition to establish a photogrammetric service in the army, and the fact that my original motives were to be found mostly in the danger, considered rather great by me, that I would yet be called up (and I know from experience what this means to me); and that this danger now has been reduced to minute proportions, in no way diminishes my wish to continue the work, once I had initiated it, in this direction, until my goal has been reached. In view of this I have started on a series of articles about photogrammetry in the 'Aeronautical journal',⟨234⟩ and in the meantime it becomes ever more urgent to carry out of experiments; so for some months now I have been looking for an opportunity to have these carried out under my direction, where in the first place I recalled to my mind that in foreign countries members of the Academy regularly receive commissions, also from the Ministry of War, and in the second place I have kept in mind Article 2a of the rules of our own Academy. My wishes and aims in this matter I have mentioned for the first time in a conversation with Jaeger at the Academy meeting of September 1917. For me, and as I think, also for Jaeger, the main issue was that because of Article 2a of the Rules members of the Academy who wished so, should be given the opportunity to give directions to the Government in the interest of national defense, and have experiments performed in the interest of the fruitfulness of these directions. Moreover we spoke as a side issue about the desirability of exempting the members of the Academy from ordinary home guard duties.

As the Board had already indicated at an earlier occasion that I shouldn't bring up important matters directly in the plenary Academy meeting, but that I should do so first with the Board, I thought I should act thus in this matter as well, and I turned to colleague Zeeman in the first days of October

⟨233⟩Wetenschappelijke Commissie van Advies en Onderzoek, Scientific Committee for Advice and Research. ⟨234⟩*Luchtvaartorgaan.*

1917; he expressed his full agreement with my plans. In the interest of the Academy's prestige he found it, in view of earlier experiences, necessary indeed that the occasion were used to point out to the Government the rights and the place of the Academy, and he was quite prepared to pass my expositions on to the other Board members, and to ask their cooperation in the fulfillment of my wishes; I did not hide from him my fear of obstruction by the Vice-Chairman,[235] where it concerned a proposal originating from a proposal of mine.

A few days later already, I heard from Zeeman that you completely agreed with us (with the only exception, that in the side issue you rather wished the exemption from home guard duties to be extended to all professors), because no opposition from the side of the Vice-Chairman had been noted.

Subsequently the meeting of the Academy Board with Minister Lely,[236] Jaeger, Schoute and myself took place, and neither there nor in the Extraordinary Meeting of the Academy in November 1917 anything whatsoever happened, nor was any word spoken that could give me reason to suspect that you or Zeeman had changed your opinion in any respect, and hence, in accordance with all unwritten laws of human relations, would not do so; further that the initiator, whose ambition it was to be adopted in the meanwhile conceived Committee of Advice, would indeed be included.

And that, if any obstruction was met, you would warn him for the purpose of designing a joint plan of resistance, the more so where you had in the memoir that you submitted to the minister (as appears from the reading of it at the meeting) specifically mentioned, as an example, the field where I could in particular be active in the committee.

Instead of this, and without any prior warning, I am told two months later by Zeeman, casually, and without any accompanying clarification, that the Committee is all set and that I am not included. And when I protested to you after receiving this staggering message, I got from you no other consolation than the suggestion of the possibility that for the purpose of proposing the mentioned activities, I could be placed in a subcommittee.

Apart from the order of the probability that this possibility becomes reality, and apart from the question whether I could do any productive work in this position (what certainly would *not* be the case if Korteweg is the only mathematical member of the Committee itself, and if therefore my work would more or less fall under his responsibility). Finally, apart from all personal paternity rights, the dignity of the Academy doesn't tolerate in my

[235] Korteweg. [236] Minister of public works (1913–1918), see also Jaeger to Brouwer, 1.X.1917.

opinion that in a Committee established on the initiative of the Academy itself, the mission of which is part of the regular task of the Academy, a member of the Academy should have to forego a place to which he aspired, and to withdraw to the second rank for the benefit of an outsider.

I have elaborated extensively, but I wanted to be clear and complete. I hope that I have succeeded, that you won't hold my frankness against me, and that I can look forward to an answer from you. If you want to allow me in this matter to have a conversation with you, then I would gladly use the opportunity.

Sincerely yours

Your
L.E.J. Brouwer.

[Signed autograph, draft – in Brouwer]

Editorial supplement

[*sheet with Brouwer's handwritten remarks*] ⟨237⟩

> If the *cause* as mentioned by Lorentz (in letter of 15.2.18) for the 'form' of the organization (consisting of ordinary and extraordinary members), and the *criterion* stated by him for ordinary and extraordinary members is correct, the extraordinary members should have at least an advisory vote in the main committee.

1918-05-23

From C. Carathéodory — 23.V.1918 **Göttingen**
Friedländerweg 31

Dear Mr. Brouwer, [`Lieber Herr Brouwer`]

Many thanks for your letter, as well as for sending me your article ⟨238⟩ and also the article of Van der Corput. Concerning the latter, there is a

⟨237⟩ Both sheets in Brouwer Archive. ⟨238⟩ In view of the topic (Lebesgue measure), probably [Brouwer 1918b].

series of reasons why we won't print it in the present form. Part of these reasons you will find in the enclosed letter of Landau. If even Landau, who has spent the last five years almost exclusively on these problems, can't understand the article in spite of his great diligence, then something must be wrong. The second reason is purely formal: already for several decennia the Annalen have the fundamental rule not to print dissertations (I believe that the only dissertation that has appeared in the Annalen was the one of Hurwitz). Whereas parts of dissertations have very often been printed (e.g. Erhard Schmidt's investigations on integral equations). The third reason, which has to do with paper shortage, is purely personal. About three months ago Noether sent a long article by R. König which he has accepted, and of which you probably will have seen the galley proofs. Then six weeks ago a second article by the same author, which he also accepted and which was even longer. I protested against that on purely formal grounds, namely that we now have per year only 26 sheets available and that it is impossible that we spend almost one third of that for one author without harming the other authors. Hilbert and Klein supported me and I am now expecting any day that Köning will withdraw his article. It would now be an insult to Noether, when we immediately would accept such a long article as the one of Van der Corput. The solution that Landau proposes, that part of the article appears in the Liechtenstein journal ⟨239⟩ and the rest in the Annalen, seems to me one that should satisfy all parties concerned, and I hope that you also agree with it, or that you make another suggestion. We can reserve up to 40 pages for Van der Corput, I think. However, in the present size I estimate it to be over 100 pages — that is more than the number of pages that all your own discoveries have demanded.—Three weeks ago a small article of four pages arrived that I found *very amusing*, entitled 'on Brouwer's fix point theorems' by an unknown Hungarian. ⟨240⟩ I wrote to him that he might add the proofs of a few theorems that he only stated and that we would probably accept the article. Now it turns out that it is a fourth semester student. Isn't that amusing?

With many greetings

Yours truly ⟨241⟩
C. Carathéodory

⟨239⟩ *Mathematische Zeitschrift*; Lichtenstein was the editor in chief of the journal. ⟨240⟩ *Über die Brouwerschen Fixpunktsätze*; [Kerékjártó 1919]. ⟨241⟩ *Mit vielen Grüssen – Ihr sehr ergebener.*

P.S. Klein would like to have articles about the theory of gravitation in the Annalen also. Maybe you can stimulate some Dutchman who does this kind of thing (for example De Sitter) to produce something. Of course it should not be too long.

[Signed autograph – in Brouwer]

1918-11-25a

To D. Hilbert — 25.XI.1918[a] ⟨242⟩

Dear Mr. Hilbert, [Lieber Herr Hilbert]

May the hale heart of your fatherland overcome the present crisis; and may the German lands soon blossom in exceptional ways in a world of justice. ⟨243⟩

That wishes you

Your Brouwer.

[Signed autograph – in Hilbert]

1918-11-28

From A. Denjoy — 28.XI.1918 **Utrecht**

Stationsstraat 12[bis]

Dear Mr. Brouwer, [Cher Monsieur Brouwer]

Infinite thanks for your kind idea to congratulate me with the great events the history of my country is going through now. Our joy is made by

⟨242⟩ Identical message to Klein (in Klein Archive). ⟨243⟩ November was a fateful month for Germany; after the armistice (11.XI.1918) the emperor abdicated and fled to Holland. Revolution was in the air, etc. The future was bleak and uncertain.

all we have suffered, by all we have feared and by realizing now that those sad days and the threat of shameful slavery seem to be over.

Yours cordially, A. Denjoy

A. Denjoy.

[Signed autograph – in Brouwer]

1919-02-16

From J. Noordhoff — 16.II.1919 **Groningen**

N.V. Erven P. Noordhoff's,
Boekhandel en Uitgeverszaak
Oude Boteringestraat 12

Dear Professor, [Hooggeleerde Heer]

In reply to your letter about taking over your work '*Grondslagen der Wiskunde*',[244] I am pleased to confirm the preliminary promise that Mr. Wijdenes has made to you in Amsterdam, that I appreciate it very much to undertake the marketing of the available copies of the work and that I would even more be pleased if I could succeed to sell a great part of the stock of this work by sufficient advertising, so that you can be found willing to work on the manuscript of a second printing of the '*Grondslagen*' in order to have it published in the series of Mathematical books which is published by me, after consulting Mr. Wijdenes. At first it was Wijdenes' idea that if the stocks of your '*Grondslagen*' were small, the copies could be put aside and a new printing could be undertaken right away. But now that it turns out that the now available copies are 240 in number, it is in this expensive time of paper and printing, a pity to make the available copies worthless by printing a new edition right away.

I would like to suggest that you henceforth commission me with the selling of the available copies of your '*Grondslagen*'. I will try to see to it that by good advertising the sales of your work increase and I propose that you let me henceforth do the accounting on the following conditions:

You receive each year in the month of January a statement with the available number of copies. The sold copies will be credited to you for half

[244] *Foundations of Mathematics*, Brouwer's 1907 dissertation.

the price. As the price is f 2,90, as I believe, you will receive f 1,45 per copy sold. As soon as it appears that by good advertising, the stock has greatly diminished, we can confer further about the manner and time of publishing a second printing. If you think it is desirable to have a second edition printed sooner, I would be glad to talk things over. You will receive a fee for the reprint of f 40,- per sheet of 16 pages, in the format and type of the works of my series, known to you. This fee of f 40,- per sheet is paid when a part or the entire work is published. Of course I leave the possible publication of the second printing entirely up to you, but I do inform you that it is my opinion that the sale of a new book always is better than that of a book that is already a few years old.

Sincerely your obedient servant ⟨245⟩
Noordhoff

[Signed typescript – in Brouwer]

1919-02-26

From F.M. Jaeger — 26.II.1919 **Groningen** ⟨246⟩

Amice,

If the Board *explicitly* wants to stipulate in its proposal that we *remain member of the existing Association internationale*; that we will *not* become a member of the interallied firm, and keep our complete freedom to act; and if furthermore the notorious ‘justified feelings’ would disappear from the document,— then I could agree with the proposal, at least in the essentials, even though I think that in that case, that League of Nations in the background is rather superfluous. The statutes of the interallied confederation are a faithful *copy* of the now published project of the so-called League of Nations; the leitmotif of both is how the victors play the boss. It seems to me that on such a monstrosity we cannot base a missive, as required in this

⟨245⟩ *Hoogachtend Uw dienstwillige dienaar.* ⟨246⟩ In this letter Jaeger discusses the issue of joining the Conseil Internationale de Recherche; its secretary, A. Schuster had invited the KNAW in a letter of 19.IX.1919. The sentiments in the Academy were mixed. Brouwer and the Groningen group led the opposition. For more information, see [Van Dalen 1999, Van Dalen 2005] section 9.1, 13.4; [Otterspeer and Schuller tot Peursum-Meijer 1997].

case — exactly because that foundation has been, as you quite correctly have remarked, *condemned already beforehand*! The gentlemen in Paris and London have disgraced themselves severely vis-à-vis Science, and if we co-operate with their plans, we disgrace ourselves *with* them, and even more so, because for us there isn't even the 'excuse' that we are in a state of war psychosis...

Now I am quite certain that the Board of the Academy will *not* approve of the conditions mentioned in the beginning of this letter. Indeed the odds are 99:1 that the interallied will emphatically reject a proposal in which we state that we remain member of the old Association. I believe that the chances for such a proposal will be even less than for the Groningen project,— although I don't swear obstinately by the latter, and would be pleased to give it up for something better. But the proposal of the Board *is* not something better, but in *my* view it is something much worse, namely a petition based on the veneration of success, to those that have committed an injustice, and a document that will make us lose face *and* in the eyes of the Allied *and* of the Central Powers.

This matter,— as so many,— concerns for a large part issues of instinctive feelings about morality; and precisely for *that* reason we said last Saturday that in *our* opinion a 'compromise' between the two viewpoints wasn't possible. I still believe so,— unless Lorentz or one of the other gentlemen can convince me that his or their point of view is morally superior to ours. As far as mixing politics with pure scientific matters is concerned, I still cannot see that.

Meanwhile,— we still have time to let our thoughts mature in this difficult business; *before* anything else, one should try to make op one's own mind about the value of the moral motives that will have to determine our position in this.

With friendly greetings ⟨247⟩

tt
Jaeger

[Signed autograph – in Brouwer]

⟨247⟩ *Met vriendschappelijke groet.*

1919-06-10

To A. Hurwitz — 10.VI.1919 Laren

Dear colleague [Hochgeehrter Herr Kollege]

May I ask you a favor? I would like next month to make a trip to Switzerland. Now I hear in the Swiss General Consulate in Amsterdam that I would have the best prospect to get permission for this journey, if I would indicate as the purpose for the trip 'discussion of scientific interest' and if at the same time my entrance request would be supported by some Swiss colleague in the same field by a letter directly to the Federal Center of the Aliens Police ⟨248⟩ in Bern. Would you be willing to lend me your support and, perhaps get also another colleague from Zürich to sign? If Weyl would still be in Zürich, he would be the most suitable person to cosign, because I actually do have scientific topics I want to discuss with him. ⟨249⟩

I thank you cordially in advance for any help. Also, I thank you once more for your message a couple of months ago concerning regular Riemann surfaces, which led me quickly to the result that the enumeration in question hasn't been explicitly carried out for any genus except zero. Subsequently I have devoted two Comptes Rendus notes to the question for genus one (Sessions of March 31 and April 28), ⟨250⟩ but unfortunately I have not yet received any reprints.

If my trip comes about, it will be a great pleasure for me to meet you.

With warm greeting

Yours truly ⟨251⟩
L.E.J. Brouwer

[Signed autograph – in Hurwitz]

⟨248⟩ Eidgenössische Zentralstelle für Fremdenpolizei ⟨249⟩ The foundational discussion of 1919 in the Engadin resulted in Weyl's conversion to Brouwer's view point, see [Van Dalen 1999], section 8.6. ⟨250⟩ [Brouwer 1919c, Brouwer 1919b]. ⟨251⟩ *Ihr ganz ergebener.*

1919-06-28

To D. Hilbert — 28.VI.1919

Dear Mr. Hilbert! [`Lieber Herr Hilbert`]

I don't know whether these lines can bring you any consolation, but I set great store by declaring to you on the day of the signing of the Peace Treaty ⟨252⟩ that, seen from Holland, the Allied Powers have, through the peace extorted today, taken upon themselves a guilt, that is certainly not less than the combined guilt of those (whoever they actually were!), that started this war.— My sincere thanks for your letter from Switzerland. How glad would I be to meet you again soon, if it were somehow possible.— At the end of the day, we scholars are after all in a fortunate situation, because such a large part of our realm of thoughts is completely independent of political nonsense.

Cordial greetings from house to house

Faithfully yours, your ⟨253⟩
Egbertus Brouwer.

[Signed autograph, picture postcard – in Hilbert]

1919-09-08

From F. Klein — 8.IX.1919 Göttingen

Dear colleague, [`Sehr geehrter Herr Kollege`]

Your letter of September 5 arrived just now. We have not missed anything yet, and everything can be arranged somehow.

There was an inadvertence in the procedure of Teubner – who had the proofs of Kerékjártó and the following ones typeset till the end of the issue – in which Cararathéodory possibly had been involved. Probably the opinion was that from your side only a few words would have to be corrected. Now there are larger changes and these, of course, cause under the present circumstances out of proportion large costs. To what extent, I cannot guess

⟨252⟩ Peace treaty of Versailles (28.VI.1919). ⟨253⟩ *In Treue Ihr.*

for the time being. Anyway, I want to ask you and the other editors that in the future you send only manuscripts to Teubner that are completely ready to print, so that more extensive corrections will be avoided.

Carathéodory wrote to me from The Hague that you have doubts as to the acceptance of Schouten's paper. Fortunately I haven't yet undertaken anything definite in this respect. I have only generally voiced the opinion that it would be more practical if articles that relate to Lorentz and Einstein would not be given such a prominence in the Annalen as was done so far. Indeed the constraints in the printer's shop have become less, even though they have not been overcome. When you could do something in this direction, I would be grateful.

For the rest Cara ⟨254⟩ will have also spoken to you about the vague plans regarding the long term future of the Annalen, that float around. Scientific publishing in Germany now has to deal with quite different conditions than before (where the only thing that is so worrying is that nobody can say whether the reshaping in the external circumstances already have come to an end). In about a month an extensive conference with Teubner (more specifically also because of the Encyclopedia) will take place here in Göttingen. Let's hope we really can find a solid foundation!

Yours truly ⟨255⟩
Klein

[Signed autograph – in Brouwer]

1919-09-19

To F. Klein — 19.IX.1919 **Laren (near Amsterdam)**

Dear Geheimrat [`Hochgeehrter Herr Geheimrat`]

In reply to your letter and your card, I first of all inform you that I recently have rejected a large article by Schouten about the application of his 'direct analysis' to the theory of relativity for the Annalen, in the first place because the author doesn't understand the art of presentation and in the second place (which is more important) because, in short, his achievements

⟨254⟩ The generally adopted abbreviation of 'Carathéodory'. ⟨255⟩ *Ihr ganz ergebener.*

consist of wrapping up results already found by inventive authors into a new (but thick and opaque) attire. In addition, the quotations are very complete in inessential points, but very incomplete in the essential points, so that the superficial reader of these articles gets a wholly false impression of their value. What is lacking in Mr. Schouten is, by the way, not talent but erudition and moderation, so I don't exclude at all the possibility that in the future he will turn into a good mathematical author.

Because I don't consider myself a prominent expert in this field, the rejection of Schouten's article (which certainly has also been recorded in the editorial archive of the Annalen) has only taken place after I had sent the manuscript to Study and obtained his advice. To his negative assessment for my information, Study has added, among other things, the following words with respect to the author: 'I don't expect that a factual discussion with such a muddled head would be of any advantage to him.' Weitzenböck too, whom I see as the second representative authority in these matters, completely shares the unfavorable judgment about the publications of Schouten, and refers to the latter's '*Grundlagen des Vektor- und Affinoranalysis*' ⟨256⟩ as 'that horrible book that he has committed'. [21]

I myself am, by the way, to be blamed to a certain degree, that at the time I have prematurely called the attention of the Annalen editors to Schouten, because in the summer of 1913 I sent the article 'On the classification of the associative number systems' ⟨257⟩ (since then published in vol. 76) to Blumenthal, with the recommendation to publish in the Annalen after checking the salient point of the contents, namely the 'principle of continuation of self-isomorphism' for novelty, because this novelty (which I could not judge myself) was crucial for the value of the article. I believe that Blumenthal then sent the manuscript to Hölder, who gave the definitive approval, and only later it turned out that the mentioned principle had been much earlier explained by Cartan, and in much more transparent form.

As far as the misunderstanding (fortunately with no serious consequences) with respect to the printing of the article of Kerékjártó is concerned, I had pointed out when I sent this article to Carathéodory that, apart from the final corrections by me concerning the content, it needed a drastic reworking of the language, and that I was willing to do this myself, if necessary, but that I rather left this to a German to get a perfect result. Only from the proof sheets that I received in Switzerland, I learned that until now no such

[21] Please consider the information about these words of Study and Weitzenböck as confidential.

⟨256⟩ Foundations of vector and affinor analysis. ⟨257⟩ *Zu Klassifizierung der assoziativen Zahlensysteme.*

rewriting had been done, so I have taken it on myself. However, in this matter Carathéodory (with whom I have been spending several very nice and cozy days in my house) is probably not to be blamed either, because from certain signs of incoherence in the correspondence of the two of us, it appears that some letters or cards must have been lost.

As far as the dating of my article that appears in vol. 80 issue 1 is concerned, as a matter of principle I never date my publications (apart from very special exceptional cases), and in this case I even have (for reasons that are by no means secret but somewhat laborious to describe) a special objection to it; hence I would like to ask you to agree that at the end of the article I leave out place and date.

The Bernstein quoted in the introduction is indeed Felix Bernstein of Göttingen; I have nothing against inserting an F or the entire first name at the relevant place.

With cordial greetings, also from Carathéodory

your revering ⟨258⟩
L.E.J. Brouwer

[Signed autograph – in Klein; part of draft in Brouwer]

1919-10-18

From J. Nielsen — 18.X.1919 **Hamburg**
Abendrothsweg 50 II

Dear professor, [`Sehr geehrter Herr Professor`]

It has pleased me very much, that you approve of the contents of my article. With respect to the required changes I await your communications.

The justification of the theorem in question in my dissertation — § 4 can be read without connection with the preceding — is of course not sufficient. I realized that at the time (1912), but had to finish my dissertation quickly and after that I was so absorbed by other work that I didn't come back to it. Therefore, when this summer in Göttingen the problem surfaced again at the occasion of a discussion about the paper by Mr. von Kerékjártó, I was all the more eager to use the opportunity to put the proof in order.

⟨258⟩ *Ihr verehrender.*

The presentation in the dissertation is now perhaps useful as a convenient illustration for the topological core of the idea of the present proof.

Allow me to enclose on this occasion two reprints from the Mathematische Annalen of last year that deal with infinite groups. Now I am most of all involved in making group theoretical principles useful for topology. More specifically I have been trying already for a long time to find the group of mapping types for a surface of genus $p > 1$. The solution of this problem will also give a necessary condition for the solution of the fixed point problem in the most general case. At the moment I am making some progress. When I would be allowed to submit to you at some later moment a communication, I would owe you my warmest thanks

Sincerely yours ⟨259⟩

J. Nielsen

[Signed autograph – in Brouwer]

1919-10-21b

To F. Klein — 21.X.1919[b] **Laren**

Dear Geheimrat, [`Hochgeehrter Herr Geheimrat`]

Carathéodory has not returned here yet. He should be back on the 15th, but he informed me at the last moment that because of a sudden trip of Venizelos ⟨260⟩ to London, he is forced to stay in Paris until Venizelos will have returned.

Enclosed I send you the note by Mr. Wolff, for the Annalen, about which I wrote to you recently.

I have started my discussion with Nielsen; it may possibly have to take quite some time. If this should realize its aim (that is the establishing of a flawless and a best possible direct proof) in a satisfactory manner, then I must be able to count with certainty on it that the author is not at the same time going to negotiate with the managing editors about the publication of his article; only because this principle of Carathéodory was strictly maintained with respect to Kerékjártó, I was at last able to get

⟨259⟩ *Ihr sehr ergebener*. ⟨260⟩ Eleuthérios Venizélos, Greek statesman and diplomat, at the time Greek representative at the Paris Peace Conference.

something good out of that young Hungarian; and only because Blumenthal, when dealing with Juel, was in this respect too tolerant, a load of confused nonsense by the latter author has been published in the Annalen.

It is only because in the present case the author has, as I believe, close personal relations with Göttingen, that it would be for me, for certainty's sake, most welcome to have your guarantee that the article, that was sent to me for refereeing will anyway be accepted only by me, in order to preclude in advance any possibility of vain effort.

Please accept my apology for my frankness and thank you very much in advance for your possible assurance.

As always, your devoted ⟨261⟩

L.E.J. Brouwer.

[Signed autograph – in Klein]

1919-11-09

From F. Klein — 9.XI.1919 **Göttingen** ⟨262⟩

Dear professor! [`Hochgeehrter Herr`]

The difficulties that publication of the Mathematische Annalen with Teubner recently have met, and that became so clearly visible to all, because of the competition of the journal of Springer, now have culminated in a crisis.

On September 30 an elaborate discussion took place here in Göttingen, in the presence of Mr. Ackermann, ⟨263⟩ between Giesecke representing Teubner, and von Dyck, Hilbert and me. In particular Hilbert has emphasized that we must insist on publishing one volume per year in peacetime strength; through attracting more mathematical physics the business can very well hold its own next to the mathematische Zeitschrift. The next day von Dyck made me the proposal, that he would withdraw from the board of chief editors in favor of a representative from mathematical physics,— I have answered him that we only can accept this offer if he would join the ranks

⟨261⟩ *stets Ihr verehrender.* ⟨262⟩ Letter to the editors. ⟨263⟩ of Teubner.

of associate editors, [264], this already for the reason, that no semblance of dissension within the editorial board could come up in the eyes of the public.

Meanwhile volume 80, number 1, which Carathéodory concluded before his travel abroad, has been finished under my supervision by Teubner. Over and above this, we still have a few short manuscripts:

1.) Sternberg, Asymptotische Integration gewöhnlicher Differentialgleichungen — objected to.
2.) Bögel, Stetigkeit von Funktionen mehrerer Veränderlicher — under revision.
3.) Rademacher, Ueber partielle und totale Differenzierbarkeit.
4.) Ostrowski, Existenz einer endlich Basis bei Systemen.
5.) Nielsen, Fixpunkte bei topologischen Abbildungen. For refereeing with Brouwer.
6.) Frl. Noether, Zur Reihenentwicklung in der Formentheorie.

    Typesetting of this manuscript has not yet commenced.

So far the matter seemed to proceed in a normal fashion, until I received a letter, dated October 27, from the publishing company, saying that Teubner couldn't bear the exceptional expenses which were demanded by publishing the Annalen in the yearly extent requested by us, and that he left it to us to look for a new publisher. Indeed, at the meeting of September 30 it had been mentioned that Springer, as stated orally at one time or another, might be willing to take over publication of the Annalen from Teubner.

In essential agreement with von Dyck, and, as soon turned out, Blumenthal, Hilbert and I have subsequently written to Springer in this sense and from him we immediately received a telegraphic and a written commitment that left nothing to be wished.

> 'Thanking you for your letter, I wish to express my special joy about the trust that can be read from your proposal. I am very happy to take over the publishing of the Mathematische Annalen. In this willingness of course is included the wish to do everything to make it possible that this journal, famous of old, can be successfully continued. I commit myself explicitly to agree with a size of the journal that allows the publication of all eligible articles ... '

[264] The Mathematische Annalen had a board of chief editors, called *Herausgeber* and a board of associate editors, called *Mitwirkenden*.

Thus the transfer of the Annalen to Springer's publishing company may be considered concluded, and I only have to ask the gentlemen of the editorial board that weren't part of the two negotiations to remain faithful to the Annalen; when no cancellation is received by Hilbert or me within fourteen days, we will assume the agreement of each of the gentlemen.

Details can only be negotiated orally with Springer. For this we have in mind November 26, because then Blumenthal will be here while passing on his way through. Our plan is that Blumenthal gets from now on a position such as Liechtenstein has at the Mathematische Zeitschrift, where we assume that in the long run the occupation of Aachen ⟨265⟩ will no longer hinder the necessary business traffic with the rest of Germany.

Finally it must be noted that Mr. Ackermann writes to me in an long letter that he had heard only afterwards about the letter of October 27 of the Teubner firm to me, and that he regrets very much the course of the events.— Further also, that the lines above have been written in full agreement with Hilbert, whom I have furthermore asked to initiate all steps that are conducive to the further organization of the Annalen.

Your truly ⟨266⟩
(signed) Klein.

[Typescript, (copy) – in Brouwer]

1919-11-10

From A. Schoenflies — 10.XI.1919 **Frankfurt a. Main**

Dear Mr. Brouwer [`Lieber Herr Brouwer`]

Because this year your trip through Frankfurt a/M could not materialize, we hope all the more for the next year. Then you can certainly activate your affection again for our Harz forests!— Though I don't know whether the Engadin ⟨267⟩ will attract you even more. Anyway I also hope to see you one of these days.— Today one more thing. I still have an elaboration of the note in the Göttinger Nachrichten (1912, p. 605), which you have entrusted to me. Should I perhaps send it back to you, or do you wish me still to keep

⟨265⟩ by the French. ⟨266⟩ *Ganz ergebenst.* ⟨267⟩ Switzerland.

it? The reason that I write is partly that you may perhaps not think of it.— After having survived the anniversary of the revolution (November 9, 1918), I hope for a steady improvement; if only the Entente, in fact La France, doesn't aggravate it too much. But it is indeed one of my axioms that I believe in the victory of common sense, which is inherent *in the things*; this alleviates the difficult time for me.

With cordial greetings from house to house

Your
A. Schoenflies.

[Signed autograph, postcard – in Brouwer]

1919-12-04a

From B.G. Teubner — 4.XII.1919[a] **Leipzig**
Poststrasse 3

Dear sir! [`Sehr geehrter Herr`]

From your kind reaction in writing of November 22, I see that Herr Geheimrat Klein has already informed you about the matter of the Annalen, which is for myself exceedingly unpleasant. The gentlemen of the editorial board have demanded from me that I increase the extent even over what it was before the war; and this would go with an annual subsidy from the publisher of 15-20,000 Mark. He is not able to accept this for a Journal; taking into account the economic situation in which he has found himself, because of the circumstances caused by the war and the revolution, and especially in consideration of the fact that I, not only for the Annalen, but also for other mathematical enterprises, have in the course of many decennia, made sacrifices, running into hundreds of thousands, it is most regrettable that the mentioned gentlemen of the editorial board, did not take this into consideration, and that no venue was sought to enable the continuation of the Annalen in my publishing house where it now has appeared for 50 years. Rather, after my statement that I could not be required to increase the subsidy substantially in the present circumstances, which also wasn't done for the editors of other scientific journals, they have seen fit to contact without further ado the Springer firm, which of course sees in the takeover of the Journal a special advertising object for the expansion of its mathematical publishing. Your request provided me with an occasion to reveal to you the reasons that lie at the basis of the discontinuation of the publishing of the Annalen by my publishing house, because it is of course of importance to me that outsiders don't get the wrong ideas about this. In any case I will also find an occasion to make these reasons public.

Issues 1-6 of the 28th volume of the Jahresbericht der Deutschen Mathematiker-Vereinigung has already appeared and they were dispatched to you on the 13th of last month. I hope that meanwhile they have come into your possession.

The first correction of your article [268] was sent to you on the first of the month, because it wasn't for the first issue but for the next one.

Sincerely yours [269]

Ackermann

[Signed typescript – in Brouwer]

1919-12-29

To A. Schoenflies — 29.XII.1919 **Berlin** [270]

Christliches Hospiz St. Michael
Wilhelmstrasse 34

Dear Mr. Schoenflies [`Lieber Herr Schoenflies`]

You will be surprised to get from here a letter from me. It so happens that I simultaneously received an offer from Göttingen and from Berlin (or more precisely both faculties have put me first on their list of possible candidates), and for that reason I am now here to confer with the ministry. The decision to come to Germany or stay at home will be a very difficult one; in itself I would be most happy to come, indeed, the university facilities in Berlin and Göttingen are tremendously better than in Amsterdam, and here I could expect a much wider scope and also much more frequent stimulation, on the other hand I am afraid that under present conditions I would have to go back considerably in pecuniary respect, because it seems to me that

[268] [Brouwer 1919d] [269] *Ganz ergebenst.* [270] In the margin of the letter Brouwer had made in pencil a list of expenses; hence the document may well be a draft.

a university professor without financial means can hardly subsist here with a family. Therefore I should in any case for the time being have to give up the idea of investing my small capital of eight to ten thousand guilders in a villa in the German countryside; because I will definitely need the interest of that for my cost of living, as this would produce almost 10,000 marks at the present low exchange rate for the German currency. In addition I hope to be able to negotiate a fixed salary of 25,000 mark, so together with the tuition fee I can get up to a total annual income of almost 40,000 mark, from which, by the way, about 5000 mark taxes and 2500 mark rent must furthermore be subtracted. Do you think that a family of four persons (five years ago we have in fact adopted a friend of my daughter as foster daughter ⟨271⟩) on this basis can exist in Berlin without having to worry about food, and so that I can still also buy the necessary books? Your advice would be valuable for me, because the colleagues here will understandably depict the circumstances rather too favorable, than too unfavorable, just because they like to get me immediately. I will certainly stay here for another eight to ten days; and I would appreciate very much to get an answer from you to Berlin; for the rest it appeals strongly to me, if the railway situation makes it possible without too much discomfort, to make the return trip via Frankfurt. I didn't hear from you about your Swiss debt after my letter to that effect; I am curious whether the method I proposed to you suited you and whether you have used it.

Meanwhile wishes you and your family a happy New Year

Your
L.E.J. Brouwer.

My address is as is printed at the head of this letter.

[Signed autograph (draft?) – in Brouwer]

⟨271⟩ Cor Jongejan.