Chapter 2

1900 − 1909

1901-12-15

From C.S. Adama van Scheltema — 15.XII.1901   Amsterdam

Dear Bertus [Beste Bertus]

Your letter [1] is something of a small event on this first cold winter's day. I had asked Huet [2] whether I was allowed to visit you, [3] but he preferred to postpone that until you would ask for it by yourself,— and now your letter, which doesn't really ask for it, arrived, but which does show that you are recovering,— and also: *a new will to live.* Only with that too you will in fact be able to return to life, that just happens to push down the most brilliant man, when he primarily doesn't know how to live *intellectually* in a conscious direction. And when you really and consciously start all over again using your renewed vitality, a treasure will be open for your desires, and you will experience again the gratitude to live in just *this* wild and wonderful time,— but also a will to take part in it!

Sometimes I yearn for the quiet and endlessness of the country side, but I will have yet to remain for some time in the oppressing narrowness of city life, to breathe for a moment in the sad sphere of the time, to submerge for just a moment as 'die Wildente' [4] and after that to fly better and safer,— and foremost also to search for the simplest connection between the real and the ideal.— This summer I did in fact make something beautiful,— I hope

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[1] *Brouwer to Scheltema, 5.XII.1901.*   [2] Brouwer's family doctor   [3] Brouwer invariably suffered from nervous breakdowns, complete with physical side effects, as a result of his military service. The effects often lasted for some time. In fact it seriously delayed his study.   [4] Refers to Ibsen's play *The Wild Duck*

to publish in April a new collection, ⟨5⟩ but fame won't yet be captured thus, I guess. Here I am sterile,— it is hard for me to reserve a couple of hours each day to lift my head above 'die Sumpfluft' ⟨6⟩ and . . . there are no great minds around,—

returning to you I hope to find a smaller soul:— it's sometimes also a kind of art and sometimes it is courageous indeed to be a bit smaller,— for us men,— women are like that by nature;— more marrow is required to bear greatness.

Write to me then when you have recovered so far that you can see me, I long for that — the day is rather cold and so is my room, and hence maybe my letter, but all the warmer is my desire for your return, for your appearance, for your words,— if it is really to be a *revival* (— and you must want that) it can also be the *birth* of a new and closer relation:— I sometimes feel myself dying from the small desires of life,— the great ones I keep — because it is beautiful to *be* human,— I hope that you will have *become* so.

The addresses are:

Heelsum (Hupkes) f. 2,50 per day.

Ruurloo (H. v.d. Mey (baker)) f. 1,75

Doorn (Miss H.W. Boeschoten) f. 3,=

Bergen (near Alkmaar, this is very beautiful, the name I don't have near at hand, but if you want to go there I can easily send it, f. 3.=)

For the rest Huet will know others.— Send me then your new address.—

Your friend Carel

[Signed autograph – in Scheltema]

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1903-05-23

To C.S. Adama van Scheltema — 23.V.1903   Amsterdam

'Il faut savoir se séparer' ⟨7⟩ (La Rochefoucauld).
'C'est le privilege des grands esprits, de ne pouvoir se brouiller' ⟨8⟩ (Voltaire).

Carel, my rich poet, I have finished your book, but now listen: In no realm there are two Kings, each must live in his own country of commoners;

⟨5⟩of poetry.   ⟨6⟩the swamp air.   ⟨7⟩One must know to separate   ⟨8⟩It is the privilege of the great minds that they cannot quarrel

that solitude without peers, that is what they are Kings for. But once a year they visit each other, and behold their great contrasts, with nothing in common except their joint feeling of both being king, both standing in the direct grace of God; their intercourse cannot consist of anything but showing each other the powers and splendors of each other's realms.— And the only during charm that they can receive themselves from the knowledge of both being King, consists in showing each other prescribed courtesies and in the reporting of the outward *appearances* of their person and kingdom.

Carel, your realm is more summery than mine, and your people more pacified — both our countries are endowed by God with wondrous beauties.

Well, after our talk of Thursday evening I believe that you are right, but also that the best interpretation of the fact that we have to live apart is what I write here.

Let us get together every year on Ascension Day, and together solemnly bathe ourselves in the cool spring sunshine, and sup together and exchange what the past year has brought, and, by the way indiscernible for each other, feel ourselves joined by 'knowing each other to be King'.

So, brother, dost thou assent to this?
Then, hail to Thee and Thine Kingdom.— Until 1904.

Bertus

To be deposited for posteriority in the 'Archive of the Holy Twin Alliance', established Ascension Day 1903.

[Signed autograph – in Scheltema]

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1903-08-09

To C.S. Adama van Scheltema — 9.VIII.1903   **Haarlem**

Brother and King, [`Broeder en Koning`]

Be of good courage, be of good courage! - for your task, which is busy unwinding. It's a pity that also a King has a body, just as his subjects, a vestige indeed from an earlier stage of development, but he does have one

and he has to guide it decently to the grave. That doesn't make the royal task any lighter; the subjects must not know worries of his body.

And this bodily concern weighs much heavier on us, yes is almost impossible. That body with its life of passion in a muddled brain needs the warmth of the soul, and man's soul warmth flows down; who gives some of it to the King?

That is the misery of purification, the warning misery which restrains the great soul that is rich at the bosom of its mother, the fair earth, and together with mankind, the laboring mankind, the freedom, seeking mankind, the naive tower building mankind at his feet — from too high a flight, and in the fear of God, of God whom he has to serve in the guarding of his children, asking nothing for himself — we are the elected — not in the world for our pleasure — we are the prophets, who, messengers between God and mankind, lead its development, its work, its growth and its blossoming and we inspire it with the dewdrops flowing from our fingers — you stride solemnly and stately through your garden, and you disperse them with a steady and knowledgeable gesture: I fly through my jungle, and they roll without me knowing — few though who find them, but for them all the more valuable.

But we may only turn a look of sadness to our mother, to nature, or to each other — not to the people — because if our tears fell there, they would burn themselves and they wouldn't understand. But if we direct our eyes upward, our tear will not fall, and it will eat into our face and leave the scars of ascesis.

That is our sad inner discord; that is the tragedy of the lonely resignation that we have known: it is heart-rending when among the hard workers beneath us there is one happening to look upward who then discerns his King sometimes even, clairvoyantly, sees his royal sadness, and wants to comfort him, but the King shakes his head: no comfort can reach him.

Brother, write, write every now and then, warmth doesn't flow horizontally either, but, to be seen, stimulates our vigor, even though it doesn't make our task more joyful.

Last week I talked a bit with your brother: [9] he is not at all like a poet, and he doesn't understand your divine side at all — but as a socialist he's one of the brightest that I have met, even if he has not made everything

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[9] Frits Adama van Scheltema.

explicit to himself, he isn't trapped anywhere. I quite liked him. Addio, happiness and strength.

your friend

Bertus

[Signed autograph – in Scheltema]

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1903-11-15a

To C.S. Adama van Scheltema — 15.XI.1903^a **Amsterdam**
Nieuwe Tolstraat 39

Dear Carel, [Beste Carel,]

Of course you haven't minded that I have been silent for so long. I was busy: to return after two years of absence to my subject required some dedication, especially where all love for that subject was lacking. But now I have succeeded bit by bit and I am rowing in steady strokes to my 'doctoraal'. ⟨10⟩ My work proceeds without illusions, but with a feeling of cheerfulness about the activity as such. And that is growing big, broad and comprehensive. Always working on, reading and thinking — and harmonizing one's life ever more, borne by resignation and trust in God — that's bliss down here. My house is homely ⟨11⟩, striking and modestly cheering. And sacred to me — I could do no harm there nor have evil thoughts — here I am even friendly to everybody. If a boring person comes to me — in my house I don't find him annoying — and when I am out of doors the next day, I don't comprehend how I could have managed to tolerate him.

That ultimate harmonization of our lives seems to be hardest and slowest and most cumbersome for people of our kind. It seems that in the line of progress of generations the eldest child of each parental couple must not be sent out into the big stream of 'strives and mates', but has to be offered as a opening sacrifice — as a flower that sprouts sideways without partaking in the upward striving of the stem — to the Gods of consciousness, a consciousness that is infertile in the worldly motions — and then the Gods forsake by way of compensation their rights to the other children. Let there-

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⟨10⟩Final examination, comparable to 'masters'.   ⟨11⟩Brouwer used the English term 'homely' (i.e. 'cosy') here.

fore those holy sacrificial animals be conscious of their role, and let them not be jealous of the coarse rye bread of the animals of the herd.

Ah, well, my dear companion and comrade in arms, the purest in ourselves, the resignation and dedication to our task, it lives best in our solitude. Associating with people is necessary, but as different from the jewels of our hearts, as rye bread – which is necessary as well, from ambrosia.

With someone else we descend from our holy tower, and we live in lower regions, which also have their own demands, which also want to be part of life. The two of us do not have such lower regions in common; blessed be the moment we have seen that.

But I do not value you less because of that — because you are far away I can see you better in your lonely splendor, of which I got the sensation the first time I saw you — the first moment I saw a person for whom I felt something. And that splendor of yours I have not observed in anyone else, but to perceive it, it's best to stand far away. Therefore, stay afar, know a warm friendship for you in me, and let us work with glad acquiescence on our task in this life, knowing that all sorrow is given by God and part of that task, and see each other

> sub specie aeternitatis.

If you can maintain yourself on that level, and thus view the presently living herd animals, — and I believe you can — then you can indeed move on to tragedies. In the end of Dusseldorp [12] I saw an announcement of that — I am longing for their birth.

With a handshake from Christian King to Christian King

your Bertus

[Signed autograph – in Scheltema]

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1904-01-18

To C.S. Adama van Scheltema — 18.I.1904   **Amsterdam**

Dear Carel, [Beste Carel,]

Passing your house, I saw the green shelves of your book case being lowered down, and with this I associated the world, — which makes you

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[12] [Adama van Scheltema 1903].

look in vain for a quiet corner, where you might serve it without disturbance, wherein your rich soul is allowed to blossom. Say, do write me now and then, how you are, how you feel, and if you can find quietude in the dedication to your duty, leaving it to the Gods to give you the time to complete that duty.

I spent the Christmas holiday on the heath of the Gooi, [13] and sat there in wind and frost between the pines, and wept tears about the transience, when I for a moment remembered in a flash how ecstatic I used to be about such things five years ago. That dead life is worth crystal tears, and flowers on its grave.

Would that memory be alive in yet another person, but in my barren man's soul? If so, it would be in yours. Do some day hold a requiem mass for the dead too. What a lack of tenderness, artlessness, of dedication in the words I write; I know it, I would be ashamed of myself if I met myself as I was five years ago; but just as one cannot stop one's beard from growing, neither can one stop the growth of the philistine tissue in the soul. Then let me be great as philistine! And unfeelingly go my way through the dead stones, alone to the splendid End. And so leave my trace on the melancholic earth. That is, Ambition is born in me – perhaps. But in any case one that knows how to restrain itself, and to collect quietly building materials until its time has come! I will have to be obscure for some more years, and then my grasp shall be felt. Exactly because I feel the insignificance of all worldly affairs, no other ambition or fear will disturb my course.

My short lived socialist inclinations, of two years ago, have thus turned out not to be viable. And even yet I believe that in you they don't belong to your proper substance. Read that last volume of Gorter, [14] then you will also feel nothing but revulsion, I know you too well for that. In you too introversion is the main theme; take care that an overhasty ambition will not lead us, out of yearning for quick success, to assimilation and to consorting with low company.

I heard from a nobody, who had heard you read Dusseldorp, [15] and found it beautiful, and who apparently thought that there had to be a lot of serious stuff behind it; please don't do such things, or maybe you need that kind of caresses, you would know best; in that case, do.

Wednesday Riechers and Waldemar [16] play in the Paleis; [17] maybe they can soften my philistine tendencies; you will of course also go there; I remember Die Wildente as if it were yesterday evening.

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[13] At the time a poor farming area, some 25 km southeast of Amsterdam. It had already been discovered by artists. [14] Socialist poet. [15] [Adama van Scheltema 1903]. [16] actors. [17] *Paleis van Volksvlijt* (Palace of the People's Industriousness) in Amsterdam, the Dutch version of Chrystal Palace.

Please write to me your new address. I hope you will find there solitary quiet; surround yourself with books of your equals [18] and people you find sympathetic. I live with Pascal, Emerson, Madame Gimon and Montaigne.

And if you don't know it yet, do me a favor, read 'Journal de Marie Bachkirtoff' (Paris, Charpentier). She resembles us both, she is halfway between us. [19]

Carel, my writing is tough, I am nowadays hardened, but underneath these words I feel my long soft hand of yore.

Your friend Bertus

[Signed autograph – in Scheltema]

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1904-07-04

To C.S. Adama van Scheltema — 4.VII.1904

Sunday evening

Dear Carel, [Beste Carel,]

I haven't left yet; the long duration of the vibrations of restlessness, which always precede my departure, may indicate that my absence will be long and far. Anyway, I need it very much, not so much because of my physical health, which doesn't leave much to be desired, but rather for regaining the pure relation in which I have to place myself with respect to the various people and institutions within my narrow social horizon, so as not to be distracted from nurturing my abilities and the development of my clairvoyance in the service of God.— Finding the purities of human relations is rather a comprehensive task — nowadays there is more interaction between the unconscious lives of me and my environment than before. But even if the equilibrium isn't reached, work in that direction is the happy task in our lives.

Coming winter I will be in Blaricum — where a small house is being built for me [20] — working on a philosophical confession, which is going to

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[18] In text 'Ebenbürtige' ('of equal birth').   [19] Marie Bachkirtoff (Bashkirtseff) was a much admired genius artist-intellectual, who died prematurely   [20] This is the so-called 'hut', designed by his friend Ru Mauve.

be the prologue of my work, or in London, in the great British Library, for
my dissertation: 'The value of Mathematics' with the motto

$$Οὐδεις \quad ἀγεομετρικος \quad εἰσιτω \,^{(21)}$$

I thank you for your well-meant admonishment to me at the gate of the
paradise of freedom. If I were looking for kingship on earth, it might be good
to wall myself in mathematics, and have myself crowned like a pope in the
Vatican, a prisoner on his throne. But I covet a Kingship in better regions,
where not the goal but the motive of the heart is of primary importance.

We are not on earth for our pleasure, but with a mission that we have
to render account for. And a small kingdom by the Grace of God is better
than a large one by the will of the people.

But all this are tough thoughts of the heavy work of steering the ship,
which after all floats on the clear lakes, protected by sun and lucky stars.
And this heavy steering work is the direct punishment for a lack of confidence
to surrender oneself to unknown powers that after all would the best for us
to sail by.

Anyway, such a trust is the essential thing, and whether one directs
oneself in good faith or allow oneself to be directed is more a matter of a
name, which mainly depends on the nature of your own character, which is
after all what you are trustingly relying on.

In this manner we both are looking for what is strongest and permanent
in ourselves, hence true, and we want to make ourselves independent of
the weak, the changeable, what is false, where we will never find support.
However — we must wittingly every now and then let our hair down, because
without paying every once in a while a small tribute, we cannot get rid of the
devil. Letting our hair down we do with the necessary contempt, because
we don't want to become chummy with the devil.

Carel, I hope that you can read under all these words a current which is
the eternal content of the letter.

Greetings from your friend,

Bertus

[Signed autograph – in Scheltema]

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(21) Let no one who is not a geometer enter.

1905-05-13

From D.J. Korteweg — 13.V.1905   **Hilversum**

Dear Brouwer [`Waarde Brouwer`]

You are certainly not mistaken that I take much interest in you, and
therefore I appreciate you sending me your booklet. [22] Whether I will read
it? I leafed through it, but it is not the kind of reading I wish for, or that is
good for me. It is true that right next to us there are those unfathomable
abysses, but I don't like walking along their edges. It makes me dizzy and
less able for what I have to do. Whether it is good for *you*, I doubt. That
much is certain, that I rather like seeing you walk different paths, even if it is
difficult to follow you there as well, where you dig so deep into fundamental
matters.

With cordial greetings

Your
D.J. Korteweg

[Signed autograph – in Brouwer]

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1906-09-07a

To D.J. Korteweg — 7.IX.1906[a]   **Blaricum**
                                                                         Torenlaan

Professor,

For some time I have been in Blaricum now; where I can more easily
devote all my time to my work. I have stopped reading others, and I am
now busy ordering my notes and arranging them into chapters.

I feel the more strengthened in my convictions, now that I observe that
I can fully stand by my notes of roughly two years ago, even now, after all
my reading of the intermediate period.

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[22] [Brouwer 1905A], *Leven, Kunst en Mystiek/Art, life, and Mysticism.*

But now I can support them better with mathematical elaborations than at the time. I already have a publisher, [23] and to coerce myself, I have agreed with him that he can start printing in the beginning of October. Before that time I'll drop by you, to hear whether you wouldn't rather see the copy before it's printed; then I can still change as much as I want; and once they are page proofs, I am of course much more restricted.

Hoping you have had an agreeable summer, and looking forward to see you again soon.

With polite greetings

L.E.J. Brouwer

[Signed autograph – in Korteweg]

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1906-09-07b

To C.S. Adama van Scheltema — 7.IX.1906^b Blaricum

Dear Carel, [Beste Carel]

For quite some time we haven't heard from each other; Sunday afternoon I was in town, and saw you at half past 4 at the Leidscheplein [24] in the streetcar; I whistled, but in vain; I'm working hard on my dissertation; but for 5 weeks I have been suffering from a terrible root canal-toothache, from which I'm free only the last 14 days; it was almost unbearable; the last sign of you were those German professors eating coconuts on an island; [25] the island is very good, the food is still wrong, and those Germans will probably eat a lot too. Enclosed here I send you two English translations, [26] by the end of September there is again a new thing in the Academy, [27] and by the end of October the dissertation, many 'deeds', isn't it, speaking in your language. Oh, if they knew how little energy I have, and how afraid I am of all those doers of deeds.

Life is a magic garden. With wondrous softly shining flowers, but between the flowers there are the little gnomes, they frighten me so much, they stand on their heads, and the worst is, they call out to me that I should also stand on my head, every once in a while I try, and I die of embarrassment;

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[23] Maas & van Suchtelen.   [24] Square in front of the City Playhouse in Amsterdam.
[25] A newspaper cutting.   [26] [Brouwer 1906A2], [Brouwer 1906b].   [27] [Brouwer 1906c].

but sometimes the gnomes shout that I am doing very well, and that I'm indeed a real gnome myself after all. But on no account I will ever fall for that.

Would you like tomorrow (Saturday) to go swimming with me in the sea? Not in Obelt [28]? Then I will arrive about ten o'clock in the morning at your place, and we walk there from Overveen all the way through the dunes. If you don't join me, that's all right too, but by all means you should do it.

Bye, old chap,

Bertus

[Signed autograph – in Scheltema]

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1906-10-16

To D.J. Korteweg — 16.X.1906   Blaricum

Professor,

I have subdivided the material for the dissertation, as it is now before me, into 6 chapters.

   1°) The construction [29] of mathematics.

   2°) Its genesis in relation to experience.

   3°) Its philosophical meaning.

   4°) Its founding on axioms.

   5°) Its value for society.

   6°) Its value for the individual.

The survey in the first chapter I sent you serves mainly as a support for the next chapters, and to be able to refer to it, moreover to display various investigations in the foundations of mathematics from one single point of view, namely that of their meaning for the constructing mathematics. [30] A couple of things I have worked out a bit further, for instance the research of Hamel on the straight line as a minimal curve, because I need that in

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[28] Public swimming pool in Amsterdam.   [29] Brouwer used a more colorful terminology here "opbouw", i.e. "erection" or "building".   [30] Brouwer uses the term "building mathematics". To translate this with "constructive mathematics" would twist Brouwer's intentions. He indeed referred to a mathematics that constructs its own objects.

principle in chapter 3 for opposing Russell; furthermore the construction of the group of fundamental operations on the continuum, because I want to present the construction of groups independent of differentiability as an essential part in the construction of mathematics; and then the deduction of the non-Euclidean arc element by means of the calculus of variations because I can't find that anywhere, and because it seems to me the only way to make that arc element *also for n dimensions* appear directly from what has been deduced for 2 dimensions. (The ordinary way is on the basis of formulas for geodetic curvatures according to the investigations of Christoffel and Lipschitz in Crelle 1870 and the following years, which become very complicated in n dimensions.)

I plan to come to Amsterdam tomorrow or the day after tomorrow in the morning, and to hear what you think of it.

Perhaps the chapter can be typeset at the end of the week, after I have gone through it once more.

Meanwhile I'm working on the next part, and I hope to send it to you as soon as possible.

With polite greetings,

L.E.J. Brouwer

[Signed autograph – in Korteweg]

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1906-11-05a

To D.J. Korteweg — 5.XI.1906[a] [31]

Allow me to send you the enclosed volume of the Göttinger Nachrichten, in which Hilbert's Paris lecture, 'Mathematical Problems' is printed. Then you will see that in the first chapter of my dissertation I have given a complete treatment of n°. 1, ('Cantor's problem of the power of the continuum'), [32] and indeed by going back to the intuitive construction that has to exist for all of mathematics.

N°. 2 ('Consistency of the axioms of arithmetic') [33] is discussed in the last chapter, in so far there the solution of Hilbert himself, as given at the Heidelberg Congress [34] is rejected, and that for the one and only solution

[31] Letter without salutation.   [32] Cantor's *Problem von der Mächtigkeit des Continuums.*   [33] *Widerspruchslosigkeit der arithmetischen Axiomen.*   [34] Heidelberg Congress

one is referred back to the construction of the arithmetic on the continuum, as it is given in the first chapter by characterizing addition and multiplication as *the* twofold group.

I have also solved n°. 5 (Lie's notion of continuous transformation groups without the assumption of differentiability') [35] for a simple case (the two parameter linear group). Hilbert himself has treated another case (the three parameter group of plane motions) in the Mathematische Annalen 56. [36]

I am sending you this book, because I seemed to note that you doubted somewhat whether the subjects in my dissertation were really worth the effort.

Then, regarding your remark that the name of Kant doesn't belong in a mathematical dissertation: you will see that the 'Foundations' of Russell deal repeatedly with Kant, and that 'The principles of mathematics' [37] of Couturat are completed with an Appendix of over 100 pages about Kant. And when you compare the Transcendental Aesthetics [38] of Kant to these, you'll see that he speaks about exactly the same things as Russell and Couturat. And Poincaré points out that the present struggle about the foundations is a continuation of the old mathematical-philosophical controversy between Kant and Leibniz.

Even though the name of Kant can be avoided here, his subjects are touched upon; is it then necessary to avoid his name because he is known as a philosopher? You can't really qualify the books of Russell and Couturat as outside of mathematics? Virtually all mathematical periodicals with a bibliographic section have always reviewed them.

As to my words that you find so absurd, namely that astronomy is nothing but a convenient summary of causal sequences of readings on our measuring instruments; Poincaré says something of comparable intent (even though I haven't copied him) in 'Science et Hypothèse'. There we find: 'The earth rotates' has no other meaning than: 'To order several phenomena in a convenient way, it is very useful to assume that the earth rotates.' And I think that such a thing is far from being absurd, on the contrary it immediately convinces anybody who happens to read it. The system of celestial bodies is indeed nothing but a mathematical system freely built by ourselves; of which people are so proud, only because it serves to control the phenomena.

And also such propositions belong indeed to the subject, at least nobody will deny 'La Science et l'Hypothèse' a place within the faculty of mathe-

[35] *Lie's Begriff der continuirlichen Transformationsgruppen ohne Annahme der Differenzierbarkeit.*   [36] [Hilbert 1902].   [37] *Les Principes des Mathématiques.* [38] *Tranzendentale Esthetik.*

matics and physics. Incidentally, various congress talks of Klein, Cantor, Boltzmann, and others treat this kind of subjects.

Finally, Sunday you said you weren't sure at all that I had studied Kant thoroughly enough to be able to make a judgment. Of course I cannot give you such certainty, but I can tell you that I read the 'Kritik der reinen Vernunft' [39] in its entirety, and that I have studied many parts (among which those that bear on my dissertation) repeatedly and seriously.

That my work is unclear, and its structure unpolished and that it shows traces of having been edited in haste, will probably be true, and also that there are here and there inaccuracies, but that the thoughts in it are vague and that the preparatory study has been superficial, I emphatically deny.

I would like so much that it will not end up bargaining between you and me about what can remain and what must go, but that you would rather sense and acknowledge the fundamental ideas; in other words more the general than the specific what's written between the lines as it were, even though your fundamental thoughts are different, though you find mine absurd,— because I am a child of a different epoch than you are.

You will recall that when two years ago I chose my subject it was not because I wasn't able to handle a more 'ordinary' one, but only because I felt an urge to take on this subject: it evolved spontaneously in me. You agreed, 'if there remained enough mathematics in it', probably suspecting that it would drive me strongly into philosophy, which it did, to the extent that I sometimes lost sight of mathematics altogether. But what I brought you now treats exclusively *how mathematics is rooted in life* and *how the starting points of the theory therefore ought to be*, and all special subjects in it receive their meaning in relation to that fundamental proposition. Taken by themselves, some of these subjects remain of value (for example the solution to the three problems of Hilbert mentioned above), but others become, when torn from their context, rather trivial, for example the survey of physics.

For me the essential part of the work is the general spirit. That is why I would like to send it into the world as a dissertation, which is fitting because of old a dissertation has the character of taking a position. The doctor's degree would give me satisfaction only if that spirit will be appreciated by my thesis advisor. With polite greetings,

L.E.J. Brouwer

[Signed autograph – in Korteweg]

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[39] *Critique of Pure Reason.*

1906-11-06

To D.J. Korteweg — 6.XI.1906

Professor,

I will be then with you on Saturday morning at about 10 o'clock, but I don't want to wait so long in answering your letter.

When I had left you on Sunday, November 4, I did not feel upset at all, but in the next days the particulars of our conversation were ever stronger brought back to me, and they brought me more and more into a state of dejection. I believe that it was mainly the recollection of that paragraph, which you thought so absurd that you even cut off my words that tried to give a further explanation. Besides, in my imagination the parts that you wanted to delete were perhaps larger than they were in reality. — for I really was under the impression that I wasn't allowed to speak about Kant, because I thought I recalled you saying that you weren't sure that I had sufficiently acquainted myself with the literature about Kant. But the main thing was the first one; if you found something too absurd even to discuss it at all with me, then I probably have consciously associated with this the idea that you doubted the earnestness of the writing, and that therefore you doubted how the honesty and the thoroughness of the reflections that had led to it. That probably made me defend myself in that respect in my last letter, which originally was intended to consist of only a few words to go with the book I sent to you, but which, under the influence of the thoughts that haunted me those days, involuntarily expanded into what it became.

Even now I still would appreciate it that you would not think that paragraph to be 'too absurd for words'; so allow me to elaborate on it for just a moment. To be rehabilitated in your eyes matters more to me than to keep it at all in my dissertation, if it can be removed completely without damage to the whole — and in that respect I certainly believe it can.

You think (this in reference to that what I mistakenly thought to remember as having been judged absurd) that the general law of attraction has very little to do with the instruments that led to its discovery; but are laws anything but inductive summaries of phenomena, means to control the phenomena, and existing nowhere but in the human mind? Taken by itself the law of attraction only exists in reference to Euclidean space, and the latter only exists by a suitable but arbitrary extension of the domain of motion of solid bodies here on earth. Without solid bodies on earth the law of attraction couldn't exist, and the connection between the two is made

by astronomical measuring tools. The law of attraction exists with respect to astronomical phenomena in the same way as molecules with respect to the state equation; both turn out to be suitable for summarizing a group of phenomena, and to be effective as a means of prediction; but the law of attraction just prevails over the molecular theory with respect to simplicity. But once more: the law of attraction is a hypothesis; the distance from earth to the sun is just as well a hypothesis.

Now I would like to say something about the main issue, namely that a similarity of laws in physics is to be expected on the basis of a similarity of the instruments used, and I would like to start with the remark:

Projected on our measuring instruments, there is no distinction between the electromagnetic field of a Leclancher element and a Daniell element; but if we look at it with an open mind, we must expect that both fields differ as much as copper sulphate and ammonium chloride; [40] only on our counting- and measuring instinct, working with certain selected instruments, they act identically there it appears that the same mathematical system can be applied to both, but it is merely the lack of suitable instruments that has so far stopped us from finding other mathematical systems that can be applied to one field but not to the other.

In each phase of the development of physics the measuring instruments that 'have been found suitable' remain a restricted collection, with respect to the totality of measuring instruments that 'might be found suitable to control all kinds of other yet unknown phenomena'; parallel with this 'the mathematical systems that have already been applied to nature' form a restricted whole compared to the totality of mathematics which 'would be applicable to nature if only physics would have expanded sufficiently.'— And since every group of mathematical systems has its invariants, it is to be expected that every restricted group of phenomena of nature has its invariants, precisely because of those restrictions, namely in the form of laws or principles that are valid for all phenomena of that group.

Now someone could say: 'But why should we expect invariants for the whole of present day physics; as this physics doesn't make any *specific* restriction at all, but it arbitrarily includes the most heterogeneous things in its scope?'

This could be answered as follows: 'There actually is a *specific* restriction, because, after all, the mathematical laws that have been observed in nature don't express anything but relations between measures, which all are

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[40] The electrolytes in these two types of batteries.

taken from the group of rigid motions; only the *influences* to which those rigid measures are exposed, are freely varied. The other physical quantities are only auxiliary quantities that are suitably chosen for certain influences of the measures, and which through their introduction as coordinates, give a simple form to the equations of state. The physical quantities are indeed never measured themselves, only the rigid measures are, in the fictive context by means of which they have been introduced; for example, one didn't measure magnetic forces and currents, but torsion angles of silk threads, and the angle-measure is based on the group of rigid motions.— And also: speaking about equivalent things, or about circumstances without influence, we always mean: with respect to our readings on measuring instruments. is only one thing that can be stated as an empirical truth by itself, namely: the group of motions of solid bodies has roughly such and such properties, and those remain roughly constant in time.'

— 'But we do measure after all things other than rigid measures; for example amounts of electricity; can't we for example give a conductor consecutively equal charges, by discharging a charged globule on it that has been charged twice in exactly the same manner, and don't we know then that the charge after the second discharge is double the amount after the first discharge?'

— 'No; because to what extent can we speak about *quantities of electricity*, in other words, to what extent can the effects of consecutively applied equal charges be superposed as equal effects? For example to the extent that they give cumulative effects on the torsion balance of Coulomb. But in how far can we superposition forces that give equal torsions there? In as far as they balance equal copper weights. But to what extent can we superpose the weights of equal pieces of copper? To the extent that the accelerations that they give rise to in the same body(for example in the Atwood machine) can be superposed. But those accelerations are only observed in solid bodies; for, both velocities and accelerations are observed in the rigid group. And this remains the case for weights of fluids, we measure them either by volume — and that is measured on the basis of the rigid group — or the weights are transferred as forces to a solid body, for example a balance or a piston.'

In this manner every physical measurement is in the end reduced to a measurement in the rigid group; and in fact *the laws of these measurements are examined in all kinds of different circumstances.* So we can really expect a *specific* restriction on physically applicable mathematical systems, and the existence of invariant principles shouldn't surprise us. Just as an

organ pipe refuses to resonate with other than specific notes, we may expect that the rigid group refuses to *resonate* with other phenomena than those which satisfy the principles of energy, action and thermodynamics.— The more generally unknown, which lies outside, could still manifest itself in the physical laws as all kinds of 'contingent' constants, as unexplained atomic weights, dielectric constants, frequencies, specific weights etc., and also the 'accidental' fact that the laws are the way they are and not different.

Maybe you find in these arguments a weak spot, but in any case, they show that my statement is more than a vague feeling, and not merely founded in a pessimistic outlook. — To conclude, please regard this letter, and also the previous one, as inspired only by the apprehension that I might have to give up the empathy with you with respect to the subject, and by the deeply felt wish to preserve that as much as possible, also in all its parts.

With polite greetings,

L.E.J. Brouwer

[Signed autograph – in Korteweg; draft in Brouwer]

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1906-11-11

From D.J. Korteweg — 11.XI.1906 ⟨41⟩

Dear Brouwer, [Waarde Brouwer]

I have now also made myself acquainted with your third chapter. The result is very satisfactory. I find a lot of beautiful things in it. I would prefer rather that some things would be expressed a bit less crudely, as this can only bring a note of passion where it doesn't belong and that a few statements be expressed somewhat less absolute.

For example it seems to me that you can't object that strongly against the logical figures by themselves as attempts, outside of mathematics, to analyze and classify the way people reason, and that, if you do, you are going beyond your subject. But all of that concerns just a few sentences or even words. For the rest see later on about the final part.

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⟨41⟩ The archive contains some drafts and notes for this letter. One carries a note 'eene andere redactie, waarschijnlijk meer overeenkomend met het verzondenen.' (another version, probably more conform the letter that was sent).

Concerning the first chapter 'the construction', you know that I would wish here only clarification, which you have declared yourself to be prepared to make an effort.

So only the second chapter remains.

After receiving your letter I have again considered whether I could accept it as it is before me. But really Brouwer, this won't do. A kind of pessimistic and mystic philosophy of life has been woven into it, that is no longer mathematics, and has also nothing to do with the foundations of mathematics. It may here and there have coalesced in your mind with mathematics, but that is wholly subjective. One can in *that* respect totally differ with you, and yet completely share your views on the foundations of mathematics. I am convinced that every supervisor, young or old, sharing or not sharing your philosophy of life, would object to its incorporation in a mathematical dissertation.

In my opinion your dissertation can only gain by removing it. It now gives it a character of bizarreness which can only harm it. It doesn't come back in the third chapter, except on a single page at the end, which therefore of course also should be deleted because it wouldn't remain comprehensible any longer.

I have tried to indicate how it could be removed from chapter 2. Take this in at your leisure, and try along these lines to make something out of it that *you* too find worth preserving.

I would regret it if this were impossible, because I find much that is good and to the point in some of your expositions and in your treatment of Russell's book, including in the conclusions you draw about Kant's considerations about the aprioristic in mathematics.

I guess that you now better understand my objection. Your last letter was a great disappointment because it shows all kinds of misunderstandings. This pained me all the more, as I was under the impression that we understood each other quite well last Sunday.

You inform me of all sorts of matters which could not possibly be unknown to me, as a regular reviewer [42] of the Revue de Métaphysique et de Morale, as if they were things that I would not know. You thought to have understood that you were not allowed to use the name of Kant, even where it concerned opinions of Kant on mathematics, and you thought that I found the view 'that astronomy is nothing but a convenient summary of causal sequences in the reading of our measuring instruments' absurd. No, not *that* view; I admit that one can present the matter in that way, although in my

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[42] for the *Revue semestrielle des publications mathématiques.*

opinion the general law of gravity has indeed little to do with our measuring instruments which led to its discovery, than that these make measurement possible at all; but that the similarity of the laws which are valid in very different parts of physics would find its origin in the similarity of the used instruments; it was that claim that appeared absurd to me.

You also thought that I suspected that your preparatory studies had been superficial. This can only be caused by the explanations I asked from you (and which I most likely will ask every now and then). But the aim of these were, apart from enlightening myself, that I would be able to state (and that might become necessary) that I have repeatedly asked you for explanations, and that I have found each time that you had solid. Personally I don't doubt that at all.

Enough now! I am very busy this week and I prefer to see you on coming Saturday November 17. I'll keep the entire morning free for you, and I want to ask you some more particulars. Hopefully your revision of the first part is ready then.

Greeting,

Your
D.J. Korteweg

[Signed autograph – in Brouwer.]

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1907-01-10a

**From D.J. Korteweg — 10.I.1907[a]**   **Amsterdam**

Dear Brouwer,   [Waarde Brouwer]

On the enclosed sheet I have only a couple of very insignificant corrections to propose, as you will see.

Meanwhile the question came up with me, can the proof on page 86 be given for higher differential quotients?

For example, let φ be the coordinate of an ordinary Weierstrass curve without differential quotient $\varphi = C + \sum_0^\infty b_n cos(a^n x\pi)$, then C can be taken such that φ is always positive, which makes it easier for me.
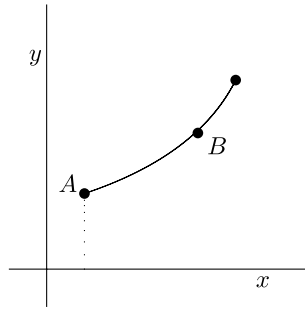
Now consider the curve that starts from an arbitrary point A and for which $\frac{dy}{dx} = \int \varphi dx$. That integral *exists*, at least Klein says that every 'continuous function'[43] admits such an integral. The curve that is thus obtained in this fashion certainly has certainly no second differential quotient, but doesn't it satisfy your requirement? In my opinion it does, because the differential quotient steadily increases within boundaries that become smaller as one considers a smaller part of the curve. In my view one has here indeed for nearby argument points (like A and B) approximately equal values for all [first][44] differential quotients because they all 'continuously' approximate the differential quotients[45] when the increase becomes smaller.

But maybe I haven't understood you well.

Greetings,

Your
D.J. Korteweg

[Signed autograph – in Brouwer]

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1907-01-10b

To D.J. Korteweg — 10.I.1907[b]

Professor,

The curve you indicate is really one for which the proof of page 86ff. can be given with respect to the first differential quotient, and not with

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[43] '*stetige Function*' in letter.   [44] Korteweg's brackets.   [45] '*stetig*' in letter.

respect to the higher differential quotients. But it does not fully satisfy the requirements I formulated.

'In nearby argument points roughly equal behavior' means that all functions of the independent variable determined by the curve should be *continuous* [46] (this is indeed expressed unambiguously by my words); now I know about a continuous curve that its difference quotients *exist*, they are functions that are determined by the curve, hence by my assumption *continuous.*

The proof of page 86ff. shows that the first differential quotient *exists*, hence it is a function determined by the original curve and therefore it is (by my assumption) *continuous*, hence is has *existing* difference quotients, which now are also functions determined by the original curve, hence (by my assumption) continuous, and according to the proof of page 86ff. the *existence* of the *second* differential quotient is deduced. We can continue in this way; first it follows from the *assumption* on page 86 that the second differential quotient and its difference quotients are *continuous.* Then from the *proof* of page 86ff. the third differential quotient *exists*, etc.

The function you indicate doesn't satisfy my postulate; for there *exist* functions determined by the curve (difference quotients of the differential quotient) that are not continuous. If these functions were continuous, then the second differential quotient would exist too.

With polite greetings

L.E.J. Brouwer

[Signed autograph – in Korteweg]

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1907-01-11

To D.J. Korteweg — 11.I.1907 (in the morning)

Professor,

It occurs to me that my letter to you of last evening was somewhat incomplete, and that the question could be raised why I formulated my requirement on page 86 as 'in nearby points roughly equal behavior' and not as 'continuity of functions determined by the curve'.

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[46] Everywhere in this letter Brouwer uses '*stetig*' for continuous.

The former does mean the latter, but one thinks then only of functions determined by physical measurements (or continuous operations on the results of those measurements), so *observed* functions. To these belong among others difference quotients and the various differential quotients, which, if they exist, can be approximated by measurements of $\Delta f, \Delta^2 f, \Delta^3 f$, etc.

However, in the latter phrasing one might include all kinds of arbitrary *mathematical* functions that I might construct from the ordinate in my mathematical imagination, and of course in that sense the postulate can never be satisfied. *Each mathematical* curve certainly determines *mathematical* discontinuous functions. I thought I expressed correctly what I meant with the word *behavior* of physically observed quantities, without the need for further elaborations.

All the more, because it was a vague feeling in people, that I pointed out, of which they have not made for themselves an outline of the precise mathematical purport I use in my proof will *indeed* lie inside that not sharply delineated domain.

Of course much can be said in addition to the short indication I gave; this is also true for quite a few other subjects that are treated in the second chapter. Maybe the reason is that in my head they were only accessory offshoots of a unifying fundamental idea (which isn't in the dissertation anymore), hence they only had secondary importance.

After their sudden appearance on the foreground as replacement of their former leader, it wasn't on the spur of the moment possible to dress them all up so that, left to themselves, could together save the whole performance.

At least that is what I occasionally feel when I take a look at the chapter. On the other hand, I more and more understand that thoughts, in the form I wrote them down at first, would have completely interfered with the mathematical tone in a mathematical dissertation, and I have tried to write about the connection between mathematics and experience as thoroughly and non-trivially as possible, while excluding these thoughts completely.

Now this letter has become somewhat longer than I thought.

The printer seems to be dawdling again; the last few days I haven't received anything. But maybe the printing of the first sheets takes up his time.

With polite greetings,

L.E.J. Brouwer

[Signed autograph – in Korteweg]

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1907-01-18

To D.J. Korteweg — 18.I.1907

Professor,

After consideration of your remarks about page 128, I also think that there the mathematical tone is obstructed, and I have deleted the sentences that implied a 'judgement'.

Also my remark that theoretical logic is not oriented towards the external world wasn't felicitous; by saying 'directed to the external world, to control or to oppose it' I roughly meant 'having practical applications in the external world', but one does not gather that from it.

From your characterization of theoretical logic as part of psychology I gathered that I had expressed myself rather vaguely, because it was actually my intention to show that theoretical logic on no account has a psychological meaning, even though it is a science.

I have therefore provided a few additions to page 128, and also reworked the last lines of page 127, so in the end a completely new and more extensive presentation of the subject resulted, and I will send you the proof again when I get it back, together with the proof of sheet 9. As I sent it yesterday evening to Nijmegen, I expect to have it back again tomorrow night, so I hope you will have it on Sunday morning.

The matter of differentiability of physical functions on sheet 6 has received an – adequate, I hope – supplement.

With polite greetings

L.E.J. Brouwer

[Signed autograph – in Korteweg]

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1907-01-23

To D.J. Korteweg — 23.I.1907

Professor,

In order to lose no more time, I will, with your permission, make no more changes in the text, but I would just like to answer your objections

and try to bring you a little more on my side in the hitherto questionable subject.

In the beginning of the chapter [47] I show that mathematical reasoning is *not* logical reasoning, and that it is just out of poverty of language that mathematical reasoning uses the connectives of logical reasoning; and thereby perhaps will keep linguistic accompaniment of the logical arguments alive, long after the human intellect will have outgrown logical reasoning. Far from it being 'queer folk' that does not argue logically, I do believe that it's just a phenomenon of inertia that the corresponding words still exist in modern languages. Pure usage of those words only rarely occurs, and impurely they are used in daily life, where they have led to all kinds of misunderstandings and dogmatism, and in mathematics to the misconceptions of set theory. [48] Those misconceptions did *not* arise through insufficient mathematical insight, but because mathematics, lacking a pure language, *has to make do with the language of logical reasonings*; whereas *the thoughts of mathematics don't reason logically, but mathematically*, which is something completely different.

The theorem: *If a triangle is isosceles, it is acute-angled*[49] is used as a logical theorem — the predicate *isosceles* is considered to *imply* for triangles the predicate acute-angled, in other words, one imagines all triangles (of a flat surface [50] for example) pictured as points of an R_6, and then one sees that the region of R_6 that represents the isosceles triangles lies inside the region that represents the acute-angled ones. This is in this case really true, so the logical formulations of the logical language can be safely used here.

But the mathematician who, because of the poverty of language, phrases the above mentioned theorem as a logical theorem, thinks something different from the logical interpretation just mentioned. He imagines that he starts to construct an isosceles triangle, and then that after the construction either the angles will turn out to be acute, or the construction doesn't *succeed* if a right or obtuse angle is postulated. In other words, he gives the theorem in his mind a mathematical, not a logical interpretation. It is precisely the main content of the 3rd chapter to show that the naive use of a logical language rather than a mathematical one has led parts of mathematics astray.

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[47] Chapter 3.   [48] '*Mengenlehre*' in the letter.   [49] To be precise: the angles opposite the equal sides are acute. Perhaps Brouwer had equilateral in mind. For the following the distinction is not important.   [50] The Euclidean plane will do.

Now let me briefly elucidate why I believe that logical language is obsolete; by the way, we discussed that the day before yesterday. The mathematical systems that are applied to the world, and that are thus the only ones that qualify for representation in language, will have to teach us something practical through their mathematical theory. But *the mathematics of whole and part* doesn't teach us through its theory anything new for applications. Once the system is applied to a part of the observational world, even a very mediocre intellect can immediately read off all consequences: no intermediary logical reasoning is necessary. Nowadays one knows very well that if one deduces something about the external world by logical reasoning that wasn't immediately clear a priori, then exactly because of that it is totally unreliable; because one doesn't believe anymore in the postulate on which it is based, that the world consists of an admittedly very large, but finite number of atoms, and that hence each word represents a (therefore also finite) group or group of groups of atoms. In other words, one knows very well that the world is not a logical system and one cannot argue logically about it; one knows very well that in the end every debate is hogwash and only can be decided for mathematical problems, but then *not by logical arguments* (even if it seems so in a deficient language; how false that appearance is, is shown in the case of the axiomatic foundations and the transfinite numbers), *but by mathematical arguments.*

Theoretical logic doesn't teach anything in the present day world, and people know this, at least the sensible people; it serves only lawyers and demagogues, not to instruct other people but to deceive them, and that is because the vulgar herd unconsciously reasons: that language with its logical figures is there, so it will be useful and so they meekly let themselves be deceived; just as I heard several people defend their habit of gin drinking with the words: 'What else is gin for?' Whoever has the illusion to improve the world, may just as well agitate against the language of logical reasoning as against alcohol; and just as little it would be a 'queer folk' that doesn't drink alcohol, it would be a 'queer folk' that doesn't argue logically; I believe though, that maybe no abuse is rooted deeper than what has grown together with the most popular parts of the language.

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Your question about the word *exists* on page 141, line 5 from below is maybe answered with the example that is given 4 lines down. In the conditions is comprised that it is a *finite* number, i.e. a well known mathematical

thing that can be searched for; but it isn't certain that the conditions can be fulfilled, in other words, that the mathematical thing *exists.*

Maybe I have after all stated my intentions in a clearer way in this somewhat wildly written letter than I succeeded in doing in my modest text. But perhaps after this letter the text will be seen in a different light. That would please me very much. With friendly greetings,

Sincerely,
L.E.J. Brouwer

[Signed autograph – in Korteweg]

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1908-02-21

To C.S. Adama van Scheltema — 21.II.1908   **Blaricum**

Dear Carel, [Beste Carel]

There is always such a considerateness in you, to write to me on the anniversary of what may have been my last passage under the yoke of society. [51] But independent of that, I was happy with your call across the misty sea of life where our view is so limited, but where we feel so clearly the climate change, and see our lodestar ever more clearly and follow its guidance more patiently. Yet we will meet enough surprises, and the end and afterimage will remain exciting enough. What a marvelous sensation it will be to survey the whole episode in one's dying moment and to understand it as the purest novel. Sometimes I really long for it, when I've lost my ambition and mood for a while.

A month ago I thought almost that the time had come. I had skated to Rotterdam against the wind over bad ice, and back the next day. Three days afterwards I got a fever as I have never had in my life before. After a week it was gone, just like that. Meanwhile I was weakened so much that I'm still in bed the whole day, and I have no wish to get out of it ever. I also had my last will drawn up, but my state of mind was mainly affected by the impression of the pitiful lackey's role of the notary public in the whole social theatre, the only one of the players who isn't human, but a

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[51] The public defense of the dissertation - 19.II.1907.

mere acting machine. The way he sat at my bedside gave me tears in my
eyes.

But now I believe that I shall and may have to behold and experience
various things, before I can say: 'at evening time it shall be light.' [52]

Do you think it a sign of weakness, that you can't maintain your mask?
What if that was caused by your continual urge to be honest, forced upon
you by your work?

What do you think of this little poem by De Génestet [53] for people who
want to be part of the movements of society

> Let each grow glad and quiet
> His happiness in the world
> If the rose adorns itself, it
> adorns the garden too. [54]

The 'Penseur' [55] is a beautiful thing. Only it is certain that a person
who sits like that will forever lose the balance in his head and never find
truth. He is much to active for that and he has too little self-confidence. He
has almost tumbled from the 'sublime' into the 'ridiculous'. He arrived here
when I was sick already, therefore you didn't hear anything about it. I also
got that crazy (but not disgustingly crazy) thing in Brusse's advertisement
booklet.

And the promise? [56] Well, Carel, I gave you my word that you will see
its fulfillment before you need it, and foi de gentilhomme, [57] I will keep it.
I don't need to be reminded of promises, didn't you ever notice that?

About or to you wife I have been silent, but now she has become such a
dear to you, all parts of the letter that are hers will flow to her. Bye now,
je t'aime toujours, [58]

Your Bertus

[Signed autograph – in Scheltema]

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[52] '*Als het avond wordt, wordt het licht*'.   Zechariah 14:7.   [53] P.A. de Génestet
(1829–1861), popular Dutch poet.   [54] *Rückert's egoïsme.*   [55] Rodin's Thinker. Prob-
ably a picture postcard sent by Scheltema.   [56] Cf. *Scheltema to Brouwer 11.I.1908*;
Scheltema, traumatized by the suffering of his father, feared a slow, lingering death; he
had asked Brouwer for a pill. See [Van Dalen 1999], p. 202.   [57] My word as a gentleman.
[58] I always love you.

1908-05-00b

To D.J. Korteweg — V.1908^b [59]

Professor

Do you have by any chance

U. Dini 'Grundzüge der theorie der Funktionen einer veränderlichen reellen Grösze'. [60]

I would very much like to consult it and probably quote it for the article for the Academy. [61] I used to get it from the library, but now it has been lent, and the man at the desk is very strict about the lending secrecy.

If you don't have it, could you, maybe Monday or so when you pass by the library, ask in your capacity of Librarian of the Society [62] who has got the book? Then I could ask that person, if we know him, to lend me the book for a day.

I received the issues of Rennes; the next week Tuesday or Wednesday I will have finished these, if nothing comes up. [63]

With polite greetings

L.E.J. Brouwer

[Signed autograph – in Korteweg]

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1908-06-08

To D.J. Korteweg — 8.VI.1908   **Blaricum** [64]

Professor,

After your last letter I started to hesitate dispatching the application and finally I didn't do it at all, when I thought I had clearly understood that there was no need whatsoever for me as a teacher, and that my being

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[59] Undated; the month May is plausible as Brouwer needed the book for a paper that was to be submitted on 30.V.   [60] Fundamentals of the theory of functions of one real variable. At the time the basic text on the subject.   [61] [Brouwer 1908a], p. 59.   [62] Dutch Mathematical Society.   [63] Concerns Brouwer's reviewing for the Revue semestrielle.   [64] The letter is erroneously dated 1907.

a privaatdocent $^{\langle 65\rangle}$ would be of service to nobody, it would only dissipate my strength. For, where I have dedicated myself to our mathematics, to achieve the best that my abilities will enable me, I have too much a sense of responsibility to be diverted from my work, when I cannot really be of any *service*. That I will, however, at all times be prepared to do the latter — I hope you will remain convinced of that.

I always have considered privaatdocents in optional subjects to be nothing but careerists, $^{\langle 66\rangle}$ and perhaps you know me well enough to know that I don't care about a social position; if you didn't know that side of me, I would set store by telling you so explicitly.

I'm sorry that you didn't get this impression about me right away this winter, but I have never had anything else in mind; and I also understood from you that it really was the intention to charge me with a few parts that actually belonged to the curriculum but that weren't done enough justice, as formerly De Vries and Coelingh. After the appointment of Van Laar I thought my privaatdocent position had certainly become superfluous, and I wondered about it when you subsequently brought it up again.

As regards my personal desires, I hope that I can stay away from teaching, at least as long as I am still such an immature mathematician, as I am now, and also afterwards if there is no need for me. Anyhow I will never be able to give courses, where I am dependent on the pleasure of the audience. I have consecutively attended such courses held by Van der Waals Jr., Mannoury and Kohnstamm, and I have always clearly felt incapable of something like that.

Of course I am well aware that you had nothing but my best interests in mind with your proposal, and I am grateful for that, but it would push me in the direction of a career that is alien to my desires; I sincerely hope that you won't hold my point of view against me, and that your interest in, and friendship for me will not be less on this account.

L.E.J. Brouwer.

[Signed autograph – in Korteweg]

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$^{\langle 65\rangle}$ A *privaatdocent* was an academic teacher (as a rule with a PhD), who is allowed to give courses at the university for a nominal salary. It was usually a position for young, promising scholars, but it could equally well be used to get teachers for routine lectures. The German equivalent, *Privatdozent*, required a Habilitation, and as a rule had more status. $^{\langle 66\rangle}$ '*Strebers*' in the text.

1908-06-24

To C.S. Adama van Scheltema — 24.VI.1908   Blaricum

Dear Carel, [Beste Carel]

Being about to send you a mathematical reprint, I would at the same
time just like to shake your hand. My trip to Paris was impossible until
now because of various activities and worries that already now are tying me
down in the world of my profession: looking back I notice how I am grad-
ually encapsulated by them until I tear myself loose with a vigorous pull.
Korteweg [67] and De Vries [68] want to make me privaatdocent after the va-
cation, I want to escape, I resist, I make conditions . . . and in the end I'll give
in perhaps: I love that subject, and why not serve it in society as well; what's
a God without altars on earth? And if I might be more of a philosopher
than a mathematician, it will break some day also out of that straightjacket.

The review that I will write about you in the Journal for Philosophy [69]
is settled. I will get some eight pages, but it will depend on circumstances
whether I will make it that long. The first weeks I will have no time for
it: I didn't even finish your book yet. The main point is that I think it so
beautiful and necessary of you to have written that book; what should a
man be but a growing philosophy of life, as a tree full of lifeblood, that in
the fall drops some fruits every now and then — his work. For the rest I
completely agree with the main tenor, and I think it is a badly needed purge
of the market, healthily simple and yet never said before. But you reject too
much the doubting poetry, and what do you mean in your foreword where
you identify specialized investigations with scientific investigation? You may
choose your terms any way you wish, but really you don't want to accuse
scientific instincts such as those of Goethe, Schopenhauer and Helmholtz,
which mine, I believe, resembles, of a tendency to specialization or being
subject to specialization, don't you? [70]

Maybe I can come now in about 14 days — an issue of a journal that I
have to do quite a lot of paperwork for [71] must first appear—: I sometimes
long so much for a summer evening with a chilly wind from the north in a
Paris suburb, just cold enough for the girls in their white blouses to quicken

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[67] Brouwer's Ph.D. advisor.   [68] Hk. de Vries, mathematics professor in Amsterdam.
[69] *Tijdschrift voor Wijsbegeerte*; refers to [Adama van Scheltema 1907].   [70] Brouwer
strongly objected to what he called "verbijzondering", specializing, in the sense that the
attention was fixed on isolated details and phenomena, instead of keeping the full image
in mind.   [71] Brouwer assisted Korteweg in editing the 'Revue semestrielle'.

their step and get color on their cheeks. Also I long to see the Louvre again, and I hope to see a couple of decent statues there; did I tell you that Rome and Naples have almost only bad statues? And that it all was compensated by the Greek temple in Paestum, and that I wept there because of nostalgia, as when I read Aeschylus? Rome never had any sentiment. One can see that already from the landscape: in such a place only dogmatism and sensuality can grow, whereas in Tuscany a quiet humanity breathes, as in the past it must have been only in Attica; I am terribly curious whether *that* country will give me that intimate hallowed feeling if I get there later.

I hope I will find you strong of mind; your book sufficiently indicates so; I very much long to see you in your home with your wife who understands you and supports you. For the rest life is difficult, dreamlike and happy. Let us live it to the end in joy.

Bye Carel, and greetings to your wife too,

from Bertus

[Signed autograph – in Scheltema]

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1908-11-08

From D.J. Korteweg — 8.XI.1908

Amice

Thank you for showing me your correspondence with Schoute. [72] He told me more about his plan and you certainly have not committed an indiscretion.

I don't want at all to disapprove of your decision. What you had to take upon yourself is of such magnitude indeed, that it would interrupt your present study completely and it would draw you completely out of your present way of life. Whether this would contribute to your happiness or be of advantage to science, is impossible to judge.

There is just one aspect of your considerations I would like to react to. It concerns what you write about the 'loftiness' of the position of a professor. In my view the position of a professor is neither higher, or lower than any other. Just as for any other job, one has to ask for a man that will best

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[72] Professor in mathematics in Groningen.

fill the role. It is clear that scientific merits and scientific insight are to be considered in the first place.

But that's not the *only* thing.

If we had just one large university instead of four, then matters would have been different. Then one would enjoy a luxury of professors where every highly qualified scientific man — because there aren't that many— can find his place.

Now we enjoy a luxury in a different respect, that is, there are four universities at relatively close distance, where similar courses are given.

And almost every professorship in mathematics has, which you correctly point out, a scope and meaning for all kinds of students and for the regular routines of the university, which forces [the university (ed.)] to make also other demands.

I am not saying that you would not satisfy those demands. But that they will be made seems unavoidable. I myself would think that one were to fail one's duty if one didn't make them.

Anyway, professor or not, a person's value depends on what one is as a human being and as a scientific man.

Greetings

Your
D.J. Korteweg

[Signed autograph – in Brouwer]

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1909-03-01

To C.S. Adama van Scheltema — 1.III.1909   **Amsterdam**

Dear Carel, [Beste Carel]

Forgive me, that I forgot your birthday. I also forgot mine. ⟨73⟩ But I don't forget you, and I will not forget you; you have been too much a person in my life for that. Also my two promises to you are still in my head.

A while ago I stood at a crossroads: I was asked to take temporarily the place of a Groningen professor who had died; I did not qualify for a permanent position, as I never had taught. Thank God I had the strength to say no, which meant that at least temporarily I have almost cut off myself

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⟨73⟩birthday Scheltema: 26 February; birthday Brouwer: 27 February.

from a social career. Actually, it wasn't a very attractive offer: after the course they would probably appoint a Delft professor in Groningen, and then I could have gone to Delft, where as professor you are something as a supervisor of drawing lessons.

Please remember that soundings for professors vacancies are confidential: except for my wife and brother you are the only one who knows this from me.

Lily's accident ⟨74⟩ has deeply moved me, she pulls through well, and she has gone to Paris with my brother; he is doing mineralogical research there, and when this is finished, they'll probably marry soon. ⟨75⟩

Bye now, dear chap; live in happiness with your wife and your work; build up your view of life as a Cathedral; I can't, but on day I like to sit on its steps like a roaming pilgrim.

Will you inform me of your address, when you leave? Warmest greetings to Annie.

Your Bertus

[Signed autograph – in Scheltema]

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1909-03-16

To D.J. Korteweg — 16.III.1909

Professor,

May I once more ask for space in the Proceedings for the enclosed short article? ⟨76⟩

I have been for a long time in doubt, whether it would be desirable to add figures; but as the text is now, I believe it doesn't need support, and in that case figures would make the ideas unnecessarily specific.

With respectful greetings

L.E.J. Brouwer

[Signed autograph – in Korteweg]

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⟨74⟩ The fiancee of Brouwer's brother Aldert lost both her legs in an accident. ⟨75⟩ Marriage of Aldert Brouwer and Lily van der Spil, 2.VI.1909. ⟨76⟩ Probably [Brouwer 1909e, Brouwer 1909d], the paper does contain a figure.

1909-05-27

From A. Schoenflies — 27.V.1909   **Königsberg**

Haarbrückerstrasse 12

Dear Doctor, [Sehr geehrter Herr Dr]$^{\langle 77\rangle}$

First of all I would like to thank you most cordially for sending me a copy of your article submitted to the editors of the Annalen.$^{\langle 78\rangle}$ Please accept my apologies that I do so only now, I first wanted to think matters over thoroughly. I hardly need say that I am highly interested in your important results.

My joy that you have studied my article so thoroughly is however not without a bitter taste. Because I see that I overlooked one of the possible shapes, namely of domains that in my terminology do not have a closed curve as boundary. The error in the proof is on page 123, where I work with the set $\mathfrak{T}_{gh}$. This can be empty — as indeed it is in your Figure 1 — and then the conclusion that is based on that set fails.

I have addressed the question to what extent my results and methods therefore must be modified, in particular the presentation in § 3ff of Chapter V. With a few changes in the arrangement and the proofs they remain valid in the following manner:

1) First the theorems in § 3 must, as you stress yourself, be restricted to such closed curves etc. that can be decomposed into arcs without common points (except for the end points). It is even sufficient to prove them first for *polygons* etc. In that way I define the concept 'proper arc' — which is the only thing I need — from the outset in such a way that its complement is a single domain. Only that will be considered for my treatment.

I have changed it in the following way, by which it remains applicable to all cases.

$^{\langle 77\rangle}$The letter looks more like a draft than a letter. There are many insertions, corrections, crossed out passages. The fact that a newcomer had found serious gaps and errors in the first exposition of point-set theory – topology, we would say – by the leading expert, must have had a devastating effect on Schoenflies. The document even lacks the obligatory polite closing sentences and a signature. In view of the fact that a man with Schoenflies' reputation would hardly send a letter of this sort, one might consider the possibility that these sheets were indeed his draft pages, and that for some reason Brouwer later obtained them from Schoenflies. This would not be a quite satisfactory explanation, for then Brouwer would doubtlessly have asked for (copies of) his own letters, which are not extant. Brouwer had sent the original letter of Schoenflies to Korteweg (see *Brouwer to Korteweg 18.VI.1909*); unfortunately it is not in the Korteweg archive. Both Brouwer and Schoenflies repeatedly appealed to Hilbert, sending him the letters of the other party; it is not clear what exactly happened to their letters. $^{\langle 78\rangle}$*Mathematische Annalen.*

One constructs in the domain $\mathfrak{H}'$ a polygon $\mathfrak{P}'$ that approximates its boundary $\mathfrak{T}'$ to within a distance ε. This divides the domain $\mathfrak{H}'$ into a ring shaped domain $\mathfrak{R}'$ and a complementary domain $\mathfrak{H}''$.

Then one concludes again that by continuity one can chose the polygon P so near to the given curve $\mathfrak{C}$ — which I assume to satisfy the more restricted sense — that the image set P' is completely contained in the ring domain $\mathfrak{R}'$. Now we only have to distinguish two cases, namely the remainder domain $\mathfrak{G}''$ is separated from $\mathfrak{T}'$ by P' or not.

In the first case the proof can proceed as in the article.

In the second case one obtains the contradiction by working with *two* points a'; for the sake of simplicity I use the images of two such points a_1 and a_2 of $\mathfrak{A}_1$ which are near to two points c_1 and c_2 of $\mathfrak{C}$ whose distance $\rho(c_1, c_2)$ is a maximum, and moreover I choose them such that 1) also the lines $a_1 c_1$ and $a_2 c_2$ are completely inside $\mathfrak{A}$, which fits the assumptions of § 2, and also 2) the image sets C_1' and C_2' of these lines have no point in common — which is possible because of continuity. That done, one chooses again ε such that both a_1' and a_2'' belong to the remainder domain $\mathfrak{H}''$.$^{\langle 79 \rangle}$ The contradiction is then obtained, because both image sets C_1' and C_2' must on the one hand penetrate into the ring domain $\mathfrak{R}'$, but on the other hand they have no point in common; and they also may not contain a point of P'. From this it would follow that P' couldn't be a continuum. This remains true also when $\mathfrak{H}''$ is separated from $\mathfrak{T}'$ by P'.

The precise proof can anyway proceed by assuming first a_1 and a_2, then choosing ε, thereby determining the ring domain $\mathfrak{R}'$, and then one chooses P such that P' is in the ring domain, and then one argues as before. So one doesn't have to distinguish two cases at all.

The proofs of Theorems 1 and 2 on § 3 then still work — by the way, I see that on page 159 line 13/14 from below it should say: ... 'that C_1' and C_2' must be identical to, *or one a subset of* the other.'

However, Theorem V on page 160 can not be inferred at this point.

On the other hand, Theorem VI, which expresses the invariance of the order, still is valid, and also the general course of its proof. First a general remark about this proof. On page 160/161 it would be better to state the conclusion of the proof — to which you have objected — at the outset; then the basic idea of it will be clearer. Indeed, one always starts from a *specific* point, and then one chooses the quantities σ etc. namely as follows. (Line 3). Then immediately line 10ff.

$^{\langle 79 \rangle}$[In the margin, with the comment '*left out*']: 'i.e. $\varepsilon < \frac{\sigma_1}{3}$ and $\varepsilon < \frac{\sigma_2}{3}$ in case $\rho(a_1', \mathfrak{T}') = \sigma_1$ and $\rho(a_2', \mathfrak{T}') = \sigma_2$.'

This proof can be transferred without any problems to the more general interpretation that one has to give then to the image $\mathfrak{C}'$ of the curve $\mathfrak{C}$. Because from the auxiliary theorems of § 3 it doesn't only follow that $\mathfrak{C}'$ is a set that decomposes the plane $\mathfrak{E}'$ into *at least* two domains; but more isn't necessary for § 4. One of these is the exterior and one the interior of what is bounded by the border $\mathfrak{T}'$. One doesn't need to suppose or know more for the discussions of § 4. Of course Theorem VI only holds insofar as it relates to the respective single domains.

The theorem in § 5 can be proved as before, with the small addition indicated by you, to exclude that in the image plane there are domains that stay disjoint from the boundary curves $P_1', P_2' \ldots$.

With the invariance of the domain we have the basis for the rest, also for Theorem V which now can be concluded.

As you see, essentially one only has to avoid temporarily the general concept of a closed curve, and a few rearrangements suffice to preserve the building. The main thing, the actual conceptual construction, remains.

Concerning your further expositions and your own results, I permit myself today to make two remarks.

1) I don't understand how you get on the basis of your Figure 3 the division of domains where just the one 'curve' is simultaneously the boundary of three domains. Namely, the text and the picture have different letters. A more detailed explanation would be most welcome, and I kindly ask you for it.

2) Your admonishments at § 15/16 concerning reachability I don't understand at all; also I cannot interpret the figures. Maybe you overlooked that my theorems only refer to *simple* curves or arcs and their images. Also concerning this I would like to ask you for more detailed expositions.

I will send you shortly the above exposition about the changes to be made in §§ 3–5 of your article in the Annalen.

[Autograph – in Brouwer]

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1909-06-18

To D.J. Korteweg —   18.VI.1909   **Amsterdam**

Professor,

Allow me to submit the enclosed article for the next Academy meeting. I am in town until tomorrow afternoon 3 o'clock. Then I will go with Lize to

pension 'Sunny Home'
nearby Ede (Gelderland)
bus stop Doesburgerbuurt.

where I'll be until July 5.

So if next week you want to speak me about the article, write to that address.

Schoenflies has rather extensively gone into my latest article for the Annalen, of which I had sent him a copy. I actually pressed him hard, and my success is probably only due to that; all the same I'm glad that I at last had a 'bite', and something more than just a friendly card concerning my work.

I enclose the letter of Schoenflies, which I have answered just as extensively, convinced as I am that you like to stay informed about my scientific situation.

After your last post card saying that the next issue of the Nieuw Archief voor de Wiskunde is filled up already, I have postponed the French translation a bit. It seems to me that September 1 would be early enough for submissions? Maybe I would be allowed to append a translation of the enclosed article, which is closely related?

With polite greetings

L.E.J. Brouwer

[Signed autograph – in Korteweg; enclosures not extant.]

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1909-07-08

To D.J. Korteweg — 1 or 8.VII.1909   **Blaricum** [80]
Thursday evening

Professor,

I have heard that the position of curator of Teyler's physical Cabinet [81] and editor of the Archives du Musée Teyler is free. I would like to ask you:

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[80] Undated, in view of *Brouwer to Korteweg 10.VII.1909* to be dated July 1 or July 8. Since the mentioned curator was still alive on June 24, the two Thursdays 1 and 8 July qualify.   [81] Part of the *Teylers Museum* in Haarlem (the oldest museum in the Netherlands).

would I overestimate myself to apply for that, and might I count on some support from you?

As a solution for my livelihood it would be an almost too beautiful piece of luck. From my earlier stay in Haarlem I seem to remember that Mr. v.d. Ven ⟨82⟩ had almost all his time available for his scientific work.

I didn't receive proofs of the little note I sent some time ago to the Nieuw Archief; do you know the reason for that? ⟨83⟩

At the election bureau ⟨84⟩ there was very little for me to do.

with respectful greetings ⟨85⟩
L.E.J. Brouwer

[Signed autograph – in Korteweg.]

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1909-07-26

To D. Hilbert — 26.VII.1909   **Amsterdam**
                                                           Overtoom 565

Dear Mr. Geheimrat, [Sehr geehrter Herr Geheimrat]

I am sending you, as an enclosure, my note about Analysis Situs in slightly modified form. ⟨86⟩ May I kindly ask you to print it in this form, and to stick to the old figures?

I hope I have achieved in this way that Mr. Schoenflies (to whom I already sent a copy) can now approve of it in every respect.

The modifications consist of the following:

1°. I have given in to Mr. Schoenflies in so far, that I have removed the admonishments that only refer to the presentation, not to the proper contents of his theory. They actually only occupied a few

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⟨82⟩dr. Elisa van der Ven, October 5, 1833 - June 27, 1909, curator since 1868.
⟨83⟩[Brouwer 1909f]. Korteweg must have taken action, for the editor Kluyver informed Korteweg that Brouwer would soon receive his proofs (Kluyver to Korteweg 12.VI.1909).
⟨84⟩The parliamentary election of June 11, 1909. Brouwer had offered his support to Korteweg.   ⟨85⟩*Met beleefde groeten.*   ⟨86⟩[Brouwer 1910h]; also in *CWII*, 352–366; cf. Freudenthal's remark's in *CWII*, 367–368.

lines, and maybe they were somewhat out of place in an article in the Annalen.

2°. At the end of the introduction I have inserted a note so that also for the reader my admiration for the achievements of Mr. Schoenflies will be beyond all doubt.

3°. My remark about Chapter V § 15 of Schoenflies' Report [87] is formulated a little differently, because of a communication of Mr. Schoenflies, according to which I here partially misunderstood the meaning of his text.

4°. At the end of the whole article I have added a summary for the sake of a better overview.

The date I have left the same, because nothing has been changed in the scientific contents of the article.

I have once more explained to Mr. Schoenflies in a letter why his deduction of the invariance of the closed curve and the ordering remains invalid even after the modifications he proposes in his addendum.

I expect now a copy of the definitive form of his addendum; we both hope that with that the matter can be considered as closed.

I will submit my second communication about finite continuous groups [88] to you in August. It will make use of a note in the Amsterdam Communications, [89] which I will dispatch to you today.

Most sincerely yours [90]
L.E.J. Brouwer.

P.S. Just now I receive your card; have you perhaps not received my letter of June 23 about the addendum that Mr. Schoenflies was going to send to you? That was the letter I referred to in my card of July 19.

In any case I will send my letter now by registered mail.

LEJ Brouwer

[Signed autograph – in Hilbert]

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[87] Usually simply quoted as the *Bericht*, [Schoenflies 1913].   [88] [Brouwer 1910c].
[89] [Brouwer 1909g, Brouwer 1909a].   [90] *Mit ausgezeichneter Hochachtung.*

1909-08-08

**From A. Schoenflies — 8.VIII.1909**   **Königsberg**
                                                             Haarbrückerstr. 12

Dear Doctor, [Sehr geehrter Herr Dr]

Possible shapes, as contained in your letter, are available for *arbitrary* polygons; that is not the case with approximating polygons. I ventured to point this out already once before. Originally I did it as you did, I worked with polygons of arbitrary shape, but I soon saw that then the conclusion became [..?..], and have then subsequently, when I was editing the *Bericht* [91] deduced a series of theorems about the shape an approximating polygon can have.

As an example, forms as those reproduced here etc. are excluded, in which the shaded part of the plane represents the polygon and the horizontal edge has precisely the length ε, hence is a side of a square. It follows immediately that for the *plane domain* of $\mathfrak{P}_\nu$ there are only such square parts that themselves [..?..] next to the 8 [..?..] of $\mathfrak{T}$.
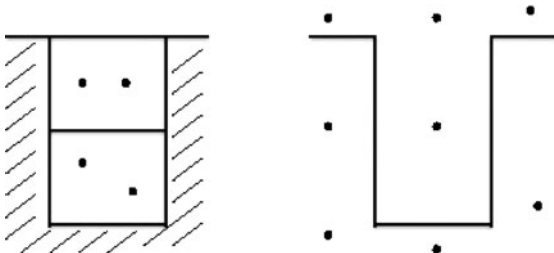
In the end I have left them out too, in the conviction that I could save the trouble of the proofs of everything related to polygons, except the little bit in Chapter IV, § 1.

Now as concerns your objection, please consider the following. Let p_m be the *last* intersection point of the path with $\mathfrak{P}_m$, then it is impossible that $\mathfrak{P}_m$ — as in your figure — penetrates once more into the circle, because

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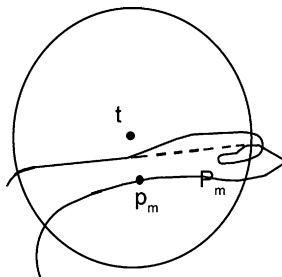
[91] [Schoenflies 1908].

I have drawn the circle around t in such a fashion that the whole part of the path from the circle around t is drawn in such a way that the whole part of the path from p_m to t is *inside* the circle. Hence, as the dotted line separates t from P_m, it will intersect $\mathfrak{P}_m$ once more.

Furthermore, as far as $\mathfrak{P}_{m+1}$ is concerned, your drawing is impossible for this polygon because of the theorems above. Also I refer to them on top of page 106 in the article. There I say that one should construct the polygons starting from a *fixed* division into squares. Now observe how as a consequence, the polygon $\mathfrak{P}_{m+1}$ is formed. It is best if one considers polygonal pieces that consist of a finite number of squares. Then the plane region of $\mathfrak{P}_{m+1}$ is formed by excluding from $\mathfrak{P}_m$ a finite number of smaller pieces of the plane.


You convince yourself that also in this way, that what you assumed about the possibility of $\mathfrak{P}_{m+1}$, is excluded.

The second point is similar. Surely one *can* to a given P and $\mathfrak{Q}'$, $\mathfrak{Q}''$, possibly choose the region H'_μ such that it joins to H'; it is impossible to do that for all regions H'_μ, as follows from my last proof. Two polygons $\mathfrak{Q}'$ and $\mathfrak{Q}''$ as I considered them, determine always a region in which not *each* of the available H'_μ can be joined to H'. Maybe I will explain this in more detail in my MS.

Now I readily agree, also today, that I could have inserted all of that in the article.

It would be useful to clear this up this in a coherent context. I intend therefore to let one my students work this out in detail, for of a dissertation. Some of this can be transferred immediately to 3-dimensional space, for example the theorem that a polygon is a *simple curve* is easy to prove as in Chapter V § 3.—

Your own proof for the paths I still don't understand. I refer you to the earlier mentioned example. Take a point set that looks like the curve $y = \sin\frac{1}{x}$.
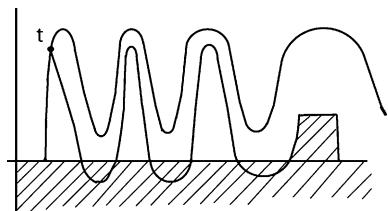
As far as I understand your notation [.?.] that part of $\mathfrak{P}_\nu$ whose distance from l is not smaller than $\frac{3}{2}\varepsilon_\nu$, assigns to the path l its *exterior* rather than its interior, and the maximal change will than necessarily be larger than $2\varepsilon_\nu$. For sufficiently large ν it is of course different, but from your argumentation that can't follow, because that refers to *every* ν.

Would you please be so kind as to write to me as soon as possible; then I will send you as soon as possible my MS.

Sincerely

Affectionately yours [92]
A. Schoenflies

[Signed autograph – in Brouwer]

1909-11-09a

To C.S. Adama van Scheltema [93] — 9.XI.1909^a   **Blaricum**

Dear Carel, [Beste Carel]

Yesterday I heard from Bertha, [94] that you are still in Florence at your piano III. Why didn't you drop me a note about that? I send you now my inaugural address on the off chance. [95] Did you, by any chance, not get my last card? Your postal system is hopeless, I still remember that from the last time. I wish that we could go round together. Lucas van Leyden's Christ in the Tribuna, the self-portraits of Bellini and Michelangelo in the Uffizi, the drawings of Da Vinci, and the Spanish Chapel, the glory of dogmatism! Are you still not intrigued by those?

This summer the world's foremost mathematician [96] was in Scheveningen; through my work I had already been in contact with him, now I have again and again walked with him, and talked as a young apostle with a prophet. He was 46, [97] but youthful in soul and body, a strong swimmer who eagerly climbed over walls and barbed wire fences.

It was a beautiful new ray of light through my life. Now do write me a few words right away.

Your Bertus

[Signed autograph, postcard – in Scheltema]

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[92] *Hochachtungsvoll – Ihr ergebener.*   [93] To 'Sig. C.S. A. v. Scheltema (all indirizzo della Sign.a. Rossi) Lugarno Vespucci 32 (piano III) Firenze.'   [94] Scheltema's sister.   [95] [Brouwer 1909c].   [96] David Hilbert.   [97] 47, in fact.

1909-12-15

From H.A. Lorentz — 15.XII.1909   Leiden

Dear Mr. Brouwer, [Zeer geachte Heer Brouwer]

I still have to thank to you for so kindly sending me the 'Public Lecture'[98] with which you began your activity as a privaatdocent.[99] Insofar as my ignorance of several of the subjects discussed did not prevent me from following you, I have read your lecture with much interest and pleasure, and enjoyed the wealth of ideas by which it distinguishes itself.

Of course I have paid special attention to the part referring to questions related to the principle of relativity. I do understand the point of view you embrace, but it still takes me some effort to share it. In fact, I still have the idea (or like to fancy that I do) that space and time can be completely separated, and in relation to that, that there is just *one* time, and that hence the proposition that two facts occurring at different places are simultaneous, can have only *one* meaning; it remains of course a question whether one can succeed in observing that simultaneity. Under this point of view one arrives at considering *one* system x, y, z, t as the 'true' one; the introduction of another x', y', z', t' is nothing but a mathematical transformation. Even when I would, by way of precaution, not base myself on this idea, and so do not assign a special meaning to *one* system x, y, z, t with respect to all the other just mentioned systems, I think it would be advisable, for clarity's sake to agree once and for all that by coordinates x, y, z we will mean those that are measured with measuring rods which are at rest with respect to the ether, and that t will mean the time measured by clocks that are in the same situation and that are synchronized by means of light signals. In that case we can call this *the* time, and (x, y, z) *the* space; again x', y', z', t' will be mathematical basic quantities.

This point of view presupposes that one assigns the ether a kind of substantiality, in so far that one can speak about being at rest or being in motion with respect to the ether, where again it doesn't matter whether we can *determine* whether a system moves with respect to the ether. In favor of this 'substantiality' of the ether is the circumstance that it can be the carrier of electromagnetic states and the accompanying energy.

If one frees oneself completely from the old conception of the ether as a 'substance', then we loose the tool that we just used to assign a special

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[98] I.e. the inaugural address of a privaatdocent, called – '*Openbare les*'.
[99] [Brouwer 1909c], 12.X.1909.

meaning to *one* system (x, y, z, t) out of many others, and one is forced
to suppose that one system (x, y, z, t) is as good as the other (x', y', z', t'),
and we hardly have another way out than to drop the sharp distinction
between space and time. This point of view has the advantage that we
can see immediately that the phenomena in a system can't change by a
translation imparted to the system (so we bring the relativity principle to
the foreground); in the old representation it always remains a somewhat
'arbitrary' result that a translation never has any influence on observable
phenomena.

Please, don't hold this exposition against me, and allow me to wish you
all the best for your future activity.

With kind greetings, sincerely

Obediently yours $^{\langle 100 \rangle}$
H.A. Lorentz

[Signed autograph – in Brouwer]

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1909-12-19

From A. Schoenflies — 19.XII.1909   **Königsberg**
                                                            Haarbrückerstr. 12

Partially changed $^{\langle 101 \rangle}$

Dear doctor, [`Sehr geehrter Herr Dr`]

If you think that you have 'more or less a right to request the deletion
of the words you objected to' and if you furthermore suggest that I would
have violated our agreement, then I quite firmly must resist that. I cannot
acknowledge this in any way.

First I remind you that I expressly reserved the right of a free hand in
relation to Chapter IV § 12. Second, our agreement referred to the revi-
sions, and that only means that changes that the one thinks necessary will
be submitted to the other for inspection and for information and for com-
ment, before the imprimatur is given. That one can consider such changes
necessary, seems self-evident to me.

$^{\langle 100 \rangle}$*Met vriendelijken groet hoogachtend – Uw dienstwillige.* $^{\langle 101 \rangle}$Note on top of page.

At least it happens to me, and as far as I can see, to many others, that only after reading the printed version of the manuscript one can reach a correct judgement whether one has found the right formulation for what one wanted to say.

Precisely this I have left open for myself by our agreement. This doesn't mean at all that you are obliged to endorse my changes. But I must definitely reject the implicit reproach in your words. I think I acted in fact almost with exceeding correctness by pointing out to you in my letter everything that I changed or added. So much about things in general.

On page 1, line 7 I will be happy to replace the words 'für die ersten Paragraphen' [102] by the words 'den allgemeinen Aufbau', [103] in line with your wishes. Instead of 'in ihnen' [104] I say then 'in den ersten Paragraphen' [105]. Moreover, I enclose the corrected page.

About the end of page 2, allow me the following remark. To decide which facts are to be presented in my article, is up to my own judgement, and not a stranger's. That is after all what this is all about. When I consider it necessary that the actual changes that must be made to theorems XIII and XV should be mentioned in more detail in § 1 and tentatively in the introduction, I have every right to do so; and I will give you the motivation, though I'm not obliged to. I have done my best to give a positive turn to my article, by showing that my line of thought in Chapter V can be retained, namely by 1) stressing the reachability and the simple curves and 2) replacing the erroneous Theorem XIII in Chapter IV by a more restricted one, which however suffices for the proof. The latter I could have been inserted already in the second paragraph of page 2 on the place marked ⊗; that it is at the end of page 2 instead, is indeed not important. You yourself acknowledged this interpretation of the exterior boundary in the letter I quoted. I referred to these words only, to marshal your own words against the opinion expressed in your letter of 8/12, in which you label the changes in Theorem XV formal and trivial. I already had for various reasons the plan to treat the outer boundary [106] in more detail in § 1 and in the introduction, especially also in connection with the letters exchanged earlier with you. You will have to admit that this concerns new facts, and that the question where and how I insert them into my article only depends on me. You are entitled to expect that I will refrain from any polemic or restrictive remarks against your article — and that is what I did. To let this come out even more clearly, I have made yet another change which you can see on the

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[102] for the first sections.   [103] the general structure.   [104] in these.   [105] in the first sections.   [106] *Aussenrand.*

enclosed page. However, I cannot forego an explicit mention of the contents of the end of page 2.

I hope and wish that this concludes our exchange of letters on these matters. Anyway, it is not possible for me to adopt further material changes.

Sincerely yours $^{\langle 107\rangle}$
A. Schoenflies

Anyway, I don't believe that anyone could think that I want to belittle your article or lessen my errors. In my opinion the opposite is the case.   D.O. $^{\langle 108\rangle}$

Because you have sent the revision already to Mr. Blumenthal, I have sent the printer, in order to speed up things, a page with the same additional corrections as the enclosed page. You will receive then the final revision from me. $^{\langle 109\rangle}$

[Signed autograph – in Brouwer]

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1909-12-24a

From J. Hadamard — before 24.XII.1909^{a} $^{\langle 110\rangle}$

Dear colleague, [`Mon cher collègue`]

Thank you very much for your two articles. $^{\langle 111\rangle}$

The following is a very simple proof of the invariance of a point under a univocal $^{\langle 112\rangle}$ transformation of the sphere. It suffices to apply the proposition contained in your first note $^{\langle 113\rangle}$ as applied to the vector distribution, $^{\langle 114\rangle}$ taking for the vector at any point M the tangent to the great circle arc connecting this point to its corresponding point M'.

Such a vector must become indeterminate for (at least) one position of M: this means that for such a position either M' will coincide with M, or M' will be diametrically opposite to M.

$^{\langle 107\rangle}$ *Hochachtungsvoll und ganz ergebenst.* $^{\langle 108\rangle}$ *der Obige* = the above. $^{\langle 109\rangle}$ Note by Schoenflies in the margin of the last page. $^{\langle 110\rangle}$ Undated. As *Brouwer to Hadamard, 24.XII.1909* is a reply to this letter (see also *Brouwer to Korteweg, 24.XII.1909^{b}*), the 'before' is evident. $^{\langle 111\rangle}$ Most likely [Brouwer 1909a] and [Brouwer 1909d]. $^{\langle 112\rangle}$ single-valued. $^{\langle 113\rangle}$ *brochure*. See Freudenthal's comments in Brouwer, Collected Works II, p. 428. $^{\langle 114\rangle}$ vector field.

The choice between these two hypotheses doesn't need the *indicatrix*. To obtain that, it is convenient to phrase your proposition on vector distributions not in the form given by you, but in that given by Poincaré in his investigations of curves defined by differential equations (Journal de Mathématiques, 1881, first Memoir, I think). From the results of Poincaré (or, if you wish, from Euler's theorem) it follows that at least one of the singularities of the vector distribution (say M_0) must be such that when a point M describes a small circle around M_0, the corresponding vector will make one turn (in total) in the same sense as M. For this to be the case the point M_0' corresponding to M_0 has to coincide with M_0 if the transformation preserves the indicatrix, and is just opposite it in the other case.

Sincerely yours, [115]

J. Hadamard

[Signed autograph – in Brouwer]

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1909-12-24b

To D.J. Korteweg — 24.XII.1909[b]   **Amsterdam**

Professor

I have become convinced that the proof of Hadamard cannot be salvaged, not even by further developing his train of thought. But the treatise of Poincaré, which gave him the idea, has in a different way suggested to me a good proof, which is quite a bit simpler than the one I gave last year. However, the result appears by surprise, while in my original proof I gradually build up the transformation, and I see myself gradually being forced to admit the invariant point.

Enclosed the copy of the letter to Hadamard, which I dispatch at the same time as this one. [116]

Sincerely yours

L.E.J. Brouwer

[Signed autograph – in Korteweg]

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[115] *Croyez, je vous prie, à mes meilleurs sentiments.*   [116] Cf. [Johnson 1981] p. 139 ff.

1909-12-24c

To J. Hadamard — 24.XII.1909^c **Amsterdam**$^{\langle 117\rangle}$

Dear Sir, [Cher Monsieur]

Thank you very much for having pointed me to out Mr. Poincaré's memoirs on algebraic vector distributions. But concerning your proof, could it be that you are mistaken? You say that in a univocal$^{\langle 118\rangle}$ transformation that preserves the indicatrix (to begin with, for a univocal but not biunivocal$^{\langle 119\rangle}$ this expression, 'preserving the indicatrix', does not always have a meaning, so let us replace 'univocal' by 'biunivocal') the vector of a point M that describes a small circle around M_0 will rotate in the same sense if M_0' coincides with M_0, and in the opposite sense if M_0' is diametrically opposed to M_0. But for a general biunivocal and continuous transformation neither of these properties exists. As far as I can see, these tangents to arcs of great circles, which I have tried as vectors myself (cf. p. 8 of my note$^{\langle 120\rangle}$), do not succeed in attaining our objective. It is only in the case of the elliptic plane that they are sufficient (cf. p. 9 of my note).

Reading the memoirs of Mr. Poincaré quoted by you, I have had another idea. First we remark that if we adapt the concept of an 'index' (quoted from the first memoir, p. 400) to general continuous vector distributions, corollary I of page 405 becomes the following: '*If the singular points are finite in number, each of them has a finite index and the algebraic sum of all the indices is equal to 2.*'

Let us now assume a biunivocal and continuous transformation of the sphere into it itself. Let A be a point that is not invariant, B its image, and C the image of B. Then let M be a variable point of the sphere and M' its image. To define the vector at the point M, we draw a small circle through M, M' and B, and as vector in M we choose the tangent to the arc MM' of that circle that doesn't contain B. The vector distribution has then as singular points 1°. the invariant points of the transformation, 2°. the points A and B. If the former don't exist, we will only have the points A and B,

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$^{\langle 117\rangle}$This letter is an reply to the undated letter *Hadamard to Brouwer before 24.XII,1909^a*). See also the draft *Brouwer to Hadamard, 4.I.1910*, (Paris), also in *CWII*, pp. 426–427, [Y17]; Freudenthal's comments p. 422 ff. Place of dispatch – from *Brouwer to Korteweg 24.XII.1909*. $^{\langle 118\rangle}$single-valued. $^{\langle 119\rangle}$bijective. $^{\langle 120\rangle}$[Brouwer 1909a].

and because the index of A equals -1, and the one of B is $+1$, the sum of the indices will be zero, which is impossible.

Sincerely yours,

L.E.J. Brouwer.

[Signed autograph, draft – in Brouwer; copy in Korteweg]

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