

# THE SCHRÖDER-PEIRCE CORRESPONDENCE

*Introduced and edited by*

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Ernst Schröder and Charles S. Peirce were members of the small circle of 19th century logicians cum mathematicians—or vice versa—who founded modern symbolic logic. Schröder and Peirce each made important contributions to the theory and notation of modern logic and, by developing and extending the researches of Boole and De Morgan, they played a large role in setting the stage for 20th century advances. They are often thought to have carried algebraic logic in the Boolean tradition to its pinnacle, at least until the much more recent work of Tarski and his followers. (See Anellis & Houser 1991) But while the importance of the work of these two men is not doubted, knowledge of their contributions is at best sketchy, and next to nothing is known about their relations with each other.

The other contributors to this issue of *Modern Logic* have done as much as anyone to fill in this important and interesting chapter of the history of logic, especially as it pertains to Schröder. But given the meager legacy from Schröder's manuscripts and letters that has been left to us, any remaining unpublished professional letters give hope for new insights into the man and his work. This is indeed true in the case of an exchange of letters between Schröder and a colleague of Peirce's stature. Therefore, it seems appropriate that what remains of the hitherto unpublished correspondence between Schröder and Peirce should be printed here.

Schröder and Peirce probably began their correspondence in 1879; in any case, by then they had begun to exchange publications. In an 1879 note in *Repertorium der literarischen Arbeiten aus dem Gebiete der reinen und angewandten Mathematik*, Schröder remarked on having recently received "from their author" copies of two publications: *Three papers on logic* and "Description of a notation for the logic of relatives." After reading these papers, Schröder felt obliged to acknowledge Peirce's priority with respect to some of the results given in *Der Operationskreis des Logikkalküls*. (Houser 1985 & 1987) Around the same time Peirce must have become familiar with Schröder's work, for his 1879 "On the Algebraic Principles of Formal Logic"—which would soon evolve into his famous "On the Algebra of Logic" which appeared in the spring of 1880 in J. J. Sylvester's *The American Journal of Mathematics*—is replete with references to Schröder's *Operationskreis*. By 1882 Peirce had adopted the *Operationskreis*, along with De Morgan's *Syllabus*, as a text for his Johns Hopkins logic course, and by 1885 Peirce would refer to Schröder, in print, as "my friend."

No letters survive from this first period of correspondence. In the earliest letter we have, dated 1 February 1890, Schröder tells Peirce that up to 1884 "I rejoiced in receiving your communications." Schröder apparently suspected that he had somehow hurt Peirce's feelings—perhaps he thought it was because he had openly challenged Peirce's claim that the full law of distribution could be proved in his 1880 algebra (see Dipert 1978 and Houser 1985, 1991)—and that that was the

cause of the five year hiatus in their correspondence; but it is far more likely that Peirce's silence was merely the result of the tumult that followed his forced resignation from Johns Hopkins in 1884 and his unsettled life which ensued for the following five or six years, until he had settled in Milford, Pennsylvania and had resigned from his demanding assistantship with the U. S. Coast and Geodetic Survey. This is supported by the following remark of Peirce's from a draft letter to Christine Ladd-Franklin dated 29 August 1891: ". . . if Schröder's *manner* seems a little harsh toward me, that is more than excused by the manner in which I have neglected to write to him. He does not know, and nobody can begin to imagine, the difficulties under which I have labored." (MS L 237: Peirce Papers, Harvard).

The letters that remain are part of the Peirce collection at Harvard University and are from a period of just over a dozen years, beginning with Schröder's 1 February 1890 letter to Peirce, in which he announced that the first volume of his *Vorlesungen über die Algebra der Logik* was about to be published, and ending with Peirce's 23 July 1902 request for support for his application for a grant from the Carnegie Institution. In his final letter, Peirce urged Schröder to write without delay and to say that he qualified as an "exceptional man" in a department of study that is useful to science. Peirce then assured Schröder: "I shall for my part, be warmly in favor of some of the money being employed to aid the completion of your great work." But Schröder had already died before Peirce's letter arrived, so it was returned to Milford. It is the only finished letter from Peirce that remains, but it is not really part of the dialogue which unifies the rest of the collection and has for that reason not been transcribed for inclusion below. Of the remaining eleven letters (including a post card), eight are from Schröder and three are from Peirce. All eleven are included here in their entirety. At least two of the Peirce letters are drafts, the finished letters having been lost with the Schröder papers, but remarks in Schröder's letters suggest that much that is in Peirce's letter drafts carried over to the finished letters.

It is difficult to tell how many letters passed between Schröder and Peirce. In the surviving letters there is no indication how many were exchanged up to 1884 and there is no precise accounting for the later period, 1890-1902. However, it is evident from references and remarks in the letters we have that there are at least six missing letters from Peirce (7 October 1890; 20 March 1895; 7 November 1895; 29 April 1896; n. d. but later in 1896; 19 November 1898) and two missing letters from Schröder (October 1890; January 1892). The first two letters from Peirce transcribed below are drafts of his two missing 1896 letters. It is to be expected, of course, that the Peirce letters that remain with his papers would be drafts. Peirce's letter dated 7 April 1897, in reply to Schröder's letter of 2/22 March 1897, also appears to be a draft, but it seems that the finished letter never reached Schröder, who therefore assumed that his March '97 letter had been lost. There may have been other letters sent after 1890 which are not specifically referred in the ones that survive. On a number of occasions books and papers—and photographs—were sent (both ways) and letters, or at least notes, may have been included. Depending, then, on how many letters were written before 1884, and how many that we have no knowledge of were written after 1890, the letters below may represent either the majority or only a small part of the total correspondence. A safe guess might be that these letters constitute about half of the full set of letters exchanged, but, as is

to be expected, obviously the weight of the surviving correspondence is heavily on the side of Schröder.

I will say very little about what can be learned from these letters, but will leave it to the readers of *Modern Logic* to discover what is of importance. It may surprise some readers that Schröder and Peirce gave so much attention to quantifiers and seem to have been so interested in Peano and Cantor. It is evident, though, that they were confident that they had embarked on a superior course, Schröder going so far as to assure Peirce that his was "the logic of the future." And Schröder did not speak from ignorance of other logical theories. But there were criticisms of each other as well, and these provide one significant contribution of these letters. For example, in his letter of 7 March 1892 Schröder points out rather forcefully "certain inconsistencies" in Peirce's system although he tones down his criticism in his subsequent letter of 12 April 1893. Peirce was equally ready to tell Schröder where he had gone wrong. He took special pains to understand why Schröder insisted that the class calculus and the statement calculus are *fundamentally* incongruous and concluded that Schröder had missed the point. Peirce's 7 April 1897 letter, the letter Schröder never received, gives the best treatment of this topic.

What is probably most valuable about these letters is the light they shed on the friendship of Schröder and Peirce, and on the interests and concerns that occupied their correspondence. Notwithstanding some criticisms of Peirce, Schröder gave him high praise by revealing that he had written to Paul Carus, editor of *The Monist*, that Peirce's "fame would shine like that of Leibniz or Aristoteles into all the thousands of years to come." But it may well be a greater compliment that Schröder invited Peirce—who at first appears to have invited himself—to contribute to the third volume of his great *Algebra der Logik*. Of course, except for the details, this should not come as a great surprise. Schröder's writings abound with acknowledgements of Peirce's importance and with evidence of his influence.

Peirce's criticisms of Schröder were perhaps a little more biting, and his compliments a little more restrained, yet Peirce clearly thought well of Schröder—it is a shame that we have so few of his letters and cannot know for sure what he revealed to Schröder. Peirce's opinion of Schröder's work may be more fully discovered in his writings and in letters to other correspondents, such as Christine Ladd-Franklin or Frances C. Russell (who worked for Carus). It is true that Peirce sometimes speaks disdainfully of Schröder's achievements, yet this is mainly because of limitations Peirce sees, and because Schröder sometimes loses his logic to mathematics. A typical characterization of Schröder is recorded in Peirce's second Harvard Lecture of 1903 (MS 302).

I value Schröder's work highly and he was a highly sympathetic man whom it was impossible to know and not to like even more than the great merit of his work justified. But he was too mathematical, not enough of the logician in him. The most striking thing in his first volume is a fallacy. His mode of presentation rests on a mistake and his second volume which defends it is largely retracted in his third [and] is one big blunder. There are some very fine things in his third volume and his posthumous volume I hope will contain still better. He was a growing man.



Yet, overall, Peirce held Schröder in high esteem. In 1891 he had written to Ladd-Franklin: "I feel that we are all greatly in Professor Schröder's debt, and I above all; and I think the way he conducts the discussion highly conducive to its purpose" (MS L237). In 1896, apparently supposing that he was near the end of his life, Peirce instructed Frances C. Russell that he and Schröder were to have his MSS, save for his *Arithmetic* which he had given to his wife. Schröder was to get Peirce's logic. Even with his faults, Schröder was the one logician Peirce trusted to understand the significance of his own writings. A final indication of the importance Peirce attributed to Schröder is the great frequency with which Schröder appears in Peirce's work. Peirce's later writings, after 1890, are replete with references to Schröder, and often one finds extended discussions of Schröder's ideas. Peirce thought of Schröder as an important leader in the field of exact logic and regarded his *Algebra der Logik* as *the* work to be reckoned with: "Prof. Schröder's work is, and must for years remain, the standard treatise upon exact logic." (MS 521).

Although it would have been a service to the reader to provide detailed commentary on the letters, that was not possible here. I *have* included a bibliography of all works that are specifically referred to in the letters. (Works preceded by an asterisk are mentioned in this introduction or are mentioned only indirectly in the letters.) Some writers are mentioned with familiarity but no specific works are indicated, e.g., George Bruce Halsted, Georg Cantor, and Cesare Burali-Forti. Works of these men, and of others who are not included in my list of references, may be found in the bibliography of Schröder's *Algebra der Logik* or in any standard historical bibliography of logic, such as that of Alonzo Church (Providence: American Math. Soc. 1984). Finally, to avoid possible confusion—for want of information—I will remark on two obscure passages.

In his letter of 7 March 1892, Schröder refers to Ladd-Franklin's "maiden investigation on the constitution of the Universe" which he compares to "Peano's number." "On the Constitution of the Universe" is a section of Ladd-Franklin's "On the Algebra of Logic," her contribution to *Studies in Logic* (Peirce 1883). Ladd-Franklin gives a table of "the sixteen possible combinations of the universe with respect to two terms" which, with a twist of interpretation, amounts to what may be the first publication of the full truth-table for the sixteen binary connectives. Ladd-Franklin makes an interesting connection between two-valued logic and a binary-based number system. I am not certain what connection Schröder sees between Ladd-Franklin and Peano.

A second remark that I hope to clarify was made by Schröder in his letter of 16 February 1896. In that letter Schröder discussed his dealings with Paul Carus about who would review the first part of the third volume of his *Algebra der Logik* for *The Monist*. Schröder wrote to Peirce that nothing had reached him from Peirce's "Chicago friend" and he mentions a "secret." The Chicago friend was Frances C. Russell and the secret was that Peirce, then on bad terms with Carus, would write a review of Schröder which would be attributed to Russell. Considerable work was expended toward fulfilling this plan, evident from the discussion of Schröder's Vol. 1 in the Peirce-Russell correspondence, but in the end Carus decided that the review should appear over its true author's name. (See Peirce 1896 and, also, Peirce 1897.)

Readers of these letters should be aware that in order to have them included in this issue of *Modern Logic* it became necessary to prepare the transcriptions in some haste from photocopies, and it was not possible to check the results against the original letters at Harvard. The ellipsis toward the end of Schröder's 16 Feb. 1896 represents illegible text which might have been made out from the original. Thus it cannot be claimed that this is truly a critical edition. Yet considerable effort has been made to insure that the reader cannot be led far astray from original intentions. Some stylistic liberties, primarily with formatting, have been taken, but the general style and layout of the letters has been maintained. Most editorial intervention is made explicit by the use of three kinds of italic brackets, distinguished by small font subscripts 1, 2, or 3. Subscript 1 brackets mark text (words or formulas) that has been corrected or altered. Subscript 2 brackets mark characters, letters, words, and so forth, supplied by the editor to stand for missing or illegible text. Subscript 3 brackets mark words or formulas suggested by the editor as what may have been intended by the preceding words or formulas. A few unproblematic emendations were made silently such as the deletion of unintentionally repeated words and the supplying of a few missing periods. Alterations (cross-outs, overwrites, etc.) have not been recorded, but the reader can take some consolation in knowing that the Peirce Edition Project, which is producing a critical edition of the *Writings of Charles S. Peirce*, plans to publish volumes of critically edited correspondence, which will include the Schröder-Peirce letters.

All of the letters were written in English, and all except for the last one were handwritten. The final letter (Schröder's 7 December 1898) was typewritten. I have attempted to represent special signs and characters more or less as they appear in the letters, and hope that they will be recognized. Peirce's claw ( $\text{—}\langle$ ) corresponds to Schröder's " $\notin$ ". For "the Universe", instead of Schröder's "1" (or, sometimes, "1" for propositional logic), Peirce used a sign for a positive logarithmic infinity. Initially Peirce used the standard infinity sign ( $\infty$ ) for this purpose, which is what Schröder took as Peirce's sign. But in these letters Peirce used a stylized zodiacal sign for Aries, which looks like the standard infinity sign with the loops cut at the bottom and pushed upwards a bit. I have not attempted to reproduce this sign. The only other special character that may require comment is Peirce's " $\Psi$ ", which is his sign for aggregation, what today we often call non-exclusive disjunction. In his letters—reflecting his growing concern about the ethics of terminology—Peirce reserves the plus sign for Boole's logical addition (exclusive disjunction). Other special characters are explained, or can, I believe, be understood from the context. These letters are reproduced with the permission of the Department of Philosophy of Harvard University. The letters are part of the Peirce Papers which are housed in Harvard's Houghton Library.

I wish to express my gratitude to Cathy Clark, who provided considerable assistance with inputting and formatting, and to the editors of this issue of *Modern Logic* for many helpful and important suggestions.

*THE LETTERS*

(Mr. Charles S. Peirce; Washington)

Dear Sir,

Herewith I am trying to ascertain, whether your address continues to be the same that I have known some years ago: The fact is that in about a month the print of the first volume of my "Vorlesungen über die Algebra der Logik (exakte Logik)" will at last be finished—the same containing over 600 pages—and I purpose to order my publisher to forward it to you immediately when accomplished.

Besides I am striving not to neglect anything for rendering my index of [literature]<sub>1</sub> (which will cover sheets [a full printer's sheet of pages]<sub>3</sub> nearly) as complete as possible, and therefore beg to excuse my feeling compelled to ask you a few questions.

Mr. Venn in his "Symbolic Logic" quotes as yours "Three papers" in the *Journal of speculative philosophy* from 1868, whilst I owe to your kindness the "Three papers on logic etc.,"<sub>2</sub> in the *Proceedings of the American Academy of arts and sciences* from 1867. Are both identical, the one quotation failing to be a correct one? (Neither of the two periodicals is here accessible to me; at least I have no facility in getting at them and should perhaps be obliged to order them from Berlin or München.)

I believe to be well informed with respect to all recent publications of Mr. Bruce Halsted, Mr. Macfarlane and Mrs. [Ladd-Franklin]<sub>1</sub>. Not quite so with regard to yours and those, if there be, of Mr. Mitchell or perhaps others of your adepts yet.

I could not help feeling a little sorry for failing to receive your second memoir on the Algebra of logic in the 7<sup>th</sup> Vol. of the *American Journal of Math.*, and struggled against the presumption, that either it had been lost on the way, or that by some reason unknown you had got angry or tired with me.

I hope that you did receive formerly my "Lehrbuch der Arithmetik und Algebra, 1873," which my publisher had been ordered to deliver unto your address some seven or eight years ago, as well as a photograph of Boole, that I had [dispatched]<sub>1</sub> myself (I should be very glad, by the by, to have yours; my colleague, Prof. Valentiner, whom perhaps you will remember to have seen on the astronomical congress of 1884 having told me besides, that you were willing to send or exchange it) etc.; then more recently two papers from 1887 (the one "Über Algorithmen und Kalkuln"). But henceforth I only forwarded some small notes from the Report of the British Association or a Tagblatt der deutsche Naturforscherversammlung—myself being [too]<sub>1</sub> thoroughly preoccupied with composing the larger work for being able to indulge in other treatises.

Up to 1884 also I rejoiced in receiving your communications.

Dr. Ventura Reyes Prósper (Madrid) wrote me something about "Contributions to formal logic" being recently printed at Johns Hopkins University, but not for sale: For the benefit of the aforesaid index I should be glad to know further particulars.—

As to my book, I trust, that for the open way in which, as far as I am aware, I freely acknowledge your merits, you will—I dare not say "forgive"—but kindly accept, nay welcome, the several criticisms. However, a final verdict, concerning that book, ought rather to be postponed till after the coming out of the second volume—the first one limiting itself entirely to the "absolute terms" and propositions of the "universal" character, the very narrowness of which scope will prove justified, I hope, by Goethe's "In der Beschränkung zeigt sich der Meister."

The second vol. is already written and secured, save a gap, concerning your logic of relatives, about my understanding of which I do not feel quite at ease yet. But owing to the large extent, that volume also has gained, the putting in print will require months again.—

Whenever my lines, dear Sir, should anyhow hurt your feelings, I beg you always to insinuate the very best of all the possible meanings, kindly taking into account how very difficult it is to tactfully write in a foreign tongue, whenever matters of but a slightly delicate nature were to be skimmed.

Believe me.

yours very truly  
Dr. Ernst Schröder,  
Prof. at the Technische Hochschule.

Karlsruhe in Baden,  
1. February 1890.

(Address: Gottesauerstrasse 9)

P.S. "Gottesae", formerly a monastery, is now a casern of artillery.



(Charles S. Peirce Esqu., Milford Pa. U.S.)

Dear Sir,

Having received your letter of Oct. 7, 1890, I had answered it (though not to its full content) immediately, and since then I have also written you a letter about the middle of January of the year instant, whereon [hitherto], I have not been favored with an answer. However I hope that my two letters have duly reached you, as also the first part of my vol. 2, that I dispatched in August 1891.

In both letters I did not enter into a certain topic advanced in your former letter, and thereon I believe to be owing you an explanation yet:



You had the kindness to propose sending me your important "*unprinted* additions to the logic of relatives," regretting that I had not been in possession already of some of your (unprinted) examples, to insert them.

Fully as I appreciated from the first the confidence you honored me with in making the said proposal and however highly I should have taxed your further contributions intended for my book, I did nevertheless abstain from (as might have been expected) eagerly availing myself of the valuable opportunity offered—from several reasons, that I will try to lay bare before you.

1°) I am knowing from friends having accomplished editions of posthume works that were left behind in manuscript—[maybe]<sub>1</sub> in an unfinished condition—by defunct investigators of highest merits (especially in mathematics), how *heavily* they have invariably been tasked by the enterprise, and how they felt their share or role in the matter to be a comparatively ungrateful one, the reward (quite apart from mercenary considerations) never being adequate with the painstaking. Now as your *printed* papers promised to give me considerable trouble already, when going to incorporate the theory laid down therein, I believed to have weighty reasons for apprehending [being apprehensive]<sub>3</sub>, lest I should share a similar fate. Nevertheless I should for the benefit of the cause have courageously undergone every sacrifice, if sacrifice there be, but for the other reasons to be [mentioned]<sub>1</sub>.

2°) Of course the manuscript of the 1600 or more pages of my two vols. did not lay quite finished and ready before me, when the printing of the first chapters began (and it does not even now!). At the time, when your thankworthy proposal reached me, I felt so heavily engaged and entirely absorbed by the task of accomplishing not a few of the earlier chapters of my book and overcoming the various difficulties therein, that I should not have been in a position to make *any* use of your contributions for a large time to come—*indeed so until quite of late*—in case of your having realized your offer. These were reasons for at least postponing a decision with respect to the latter. And

3°) last but not least you are to know that I am continually overtasked *beyond description* even no account taken of the directorial duties I have been charged with and am now happily released of. There are still remnants of my directorial year to be got rid of, as for instance my being in retard with most of the private correspondence heaped up during that year, which I had forcibly laid aside for the official one. I ought to have given you these reasons earlier indeed; but—there you see the vicious circle—was prevented from doing so by the reasons themselves. It is very likely that I shall continue being overtasked until several books will have come out, but first of all my vol. 2. Now offer a man, who is so much harrassed, that morally speaking he is almost never getting rid of the foam before his mouth, to add a burden more (though attractive in itself) to the many he is already carrying



on his shoulders! And such was the state of things during the year mentioned.—

What I wrote you in my first (hasty) letter concerning the probability of no essential additions being likely to be made by me to your theory, has been true for the time: when I knew the latter only from a superficial perusal of your printed papers (the one in the proceedings of the American Academy for 1875 I could not even procure and see as yet). It may be not quite so today. *Then* I intended only to give in my book a brief historical sketch—so to say—or procure the reader but a general idea of your logic of relatives. But since, my mscpt has [developed]<sub>1</sub> to an aspiration of laying a thorough foundation thereof in the tract under consideration. And I believe now, in my exposition of it, your theory (so far as it has been printed) to receive several improvements indeed—

first: by my filling up several gaps, as for instance the explanation why, or proof that, a *substitution* is *at once* the *identical sum* (logical aggregate) and the *relative product* of its cycles (having no element in common), and others

second, and especially, by the theory being cleared from *a pair of "inconsistencies"* (however slight), [hitherto]<sub>1</sub> concealed therein, which only your tact or genius did not allow to become fatal, but which cannot fail to become so for most beginners, or rather, by deterring the would-be beginners, will cause them to refrain from the study. Having said so much, I feel compelled—for not being misjudged, nor perhaps only thereby exciting your anger—to [characterize]<sub>1</sub> these "inconsistencies": The heavier one is capable of being summed up to my stating, that it is absolutely indispensable—for the sake of *reconciling* your definition of the individual (say dual or binary) relative *as signifying* (or meaning) *the relate*, with the main theory of dual relatives: *to distinguish from the outset different realms or universes* ("Denkbereiche"), which I denote by  $1^1, 1^2, 1^3, \dots 1^\infty$  (rejecting your  $\infty$ , and replacing your relative moduli  $\aleph$  and 1 by 0' and 1'). *For*: a good deal of propositions, true "*sub 1<sup>2</sup>*" cease to be so, or are essentially to be modified, when the relatives are reduced to their said meaning ("sub 1<sup>1</sup>"). Etc.

The minor "inconsistency" relies on the circumstance that since  $(a \neq 0)$   $(b \neq 0)$  is not  $= (ab \neq 0)$ , but only

$$(ab \neq 0) \notin (a \neq 0)(b \neq 0)$$

I cannot find it perfectly legitimate or "unbedenklich" (unhesitatingly to be allowed, because of the danger, adduced thereby, of misleading the students into blunders from confusion) the " $\neq 0$ " with you to be *suppressed throughout*, as a fact mentally to be suggested everywhere. Of course so only with the *relatives a, b themselves* (they being class-terms), whilst with their *coefficients a<sub>ij</sub>, b<sub>ij</sub>* (being statements, or playing exactly the part of such) there can indeed be no objection to the procedure.

I feel sure, that with you I need not enlarge any more on these points.—

In sparing the best for the last (or end)—Das Beste für zuletzt aufsparend—I had to postpone any seriously approaching the logic of relatives until the remaining parts of my book were in the main nearly all of them accomplished.

Thus *it is only quite recently* that I began to *understand* the main features of your beautiful theory (though having not succeeded so with *every* particular as yet). By the by I do not know of any man on earth, understanding or pretending to understand your theory besides; at least neither Mr. Venn nor Macfarlane, nor Mrs. Ladd-Franklin does—nor Signor Peano (the latter so far as may be judged from appearances). I have grown an [enthusiastical]<sub>1</sub> admirer of the theory and your merits (Ihrer "Leistung") since then and in fact believe it to be "*the logic of the future (part superior)*."

No doubt I *could* avail myself of your generous offer *now*. But having not accepted in time, I must declare you to be under no obligation.

Best I should like to see your additions printed in time for being taken into account by my book. If during the long interval since 1890 you have—as I hope—forwarded them to a periodical, my wish might easily be fulfilled; perhaps it even might, if you did so only now. (It has been as yet impossible for me to ascertain, what novelties did appear in literature since then, especially as we have not the proceedings nor the memoirs of the American Academy in our Karlsruhe libraries).

If working on independently, I feel certain now that we shall coincide in many points (apperceptions or discoveries) and that I shall be anticipated in most of them—or *be so already*. I clearly see the direction in which further progress has carried you on (and more or less will do the same with me). For instance I can write the definition of a (onesidedly) determinative correspondence (eindeutige Zuordnung, Abbildung) with no more than *five* signs, and that of a "one to one correspondence" *c* with *eight* (repetitions counted over again, and everything included, but counting  $\bar{c}$  as one sign—no indices nor  $\Sigma$  and  $\Pi$ 's being used)—with much less signs, then, than the number of letters the word contains! I shall have to do considerable work doubly, or in vain. Notwithstanding these drawbacks I should feel in a clearer position outwardly, whilst referring to your additions printed, than when they were confided to me in mscpt.

However I do not object to receiving them at all nor henceforth, and, relying on the "*qui tacet videtur consentire*", protest against ever having objected to.

There is another drawback still, which I am to point out to you. Were I but allowed to write out a third volume just as bulky in spc [space]<sub>3</sub> as (apart from the 24<sup>th</sup> and 25<sup>th</sup> lecture) the two other ones will be! Actually I am sorry to find myself restricted to the space of little more than one lecture only. This is sure to be one of the most extended already—and by far the most important—, if I do limit myself to only expounding with a solid foundation the very elements of your theory with now and then a critical remark. Happily the views I have gained through your theory have

enforced on me the necessity of rewriting the nearly finished lecture on Prof. Dedekind's investigations ("Was sind und was sollen die Zahlen?")—the space *won*, to be dedicated to the continuation of the logic of relatives. Nevertheless I may be constrained to abstain from adding *many* further or higher developments, however beautiful and important they may prove—the same with examples. I cannot promise then, that every contribution of yours, however worthy in itself, will be incorporated in the book.—

As to the [hitherto]<sub>1</sub> outcome [published]<sub>3</sub> parts of my work I by no means believe them to have turned out a perfect opus. Especially the second vol. part may be said to have decidedly suffered not a little through the intervention of my directorial duties; and I am aware of a series of imperfections already. Allow me to hint at a few of these. I owe Mrs. Ladd-Franklin a (third!) retribution yet for not having seen the identity in essence of her maiden investigation "on the constitution of the Universe" (very misleading title!) with Peano's number. The retributions will be made prominent by referring the reader in the short preface to the pages in the text (Haupttext, and not footnotes only). In the introduction I have gone too far in declining Mill's distinction of connotative and nonconnotative names. The desideratum expressed on p. XI vol. 2 [Schröder 1966, vol. 1, p. 352b]<sub>3</sub>, concerning an early proof of  $(a \notin b) = (b \notin a)$  is easily to be fulfilled by a little simplifying the one given by you in vol. III Am. Journ. of Math.—the latter I had great pains to arrive at an understanding. The restriction to statements on p. 72 is unnecessary and, instead of stating that  $(a \notin b) = (a = 0) + (b = 1)$  possesses the "engere Geltung" only, I had better stated that  $(a \notin b) = (1 \notin a + b)$  enjoys of the "weitere"—and others. But happily these do not bear on the plan as a whole and will be capable of remedy by means of some brief remarks. Other improvements I must leave for a new edition.—

Because of the uncertainty of your address having remained unaltered I must beg you to excuse me if I shall not dispatch part II before learning the first part to have reached you.

Believe me, dear Sir,

yours very truly  
Dr. Ernst Schröder,  
Professor.

Karlsruhe in Baden,

$\frac{7}{3}$  92.

(Gottesauerstrasse 9)

P.S. It may be of some interest to you, to know that I adopt in the main your (latest) notations and terminology, submitting also to the horizontal negation line and employing your suffices [suffixes]<sub>3</sub>. My deviations (besides my sticking to the  $\notin$  and  $\neq$  and) besides the  $1^2$  (for  $\infty$ ),  $0'$  and  $1'$  already mentioned, are the following. I continue to denote the 

logical
identical

 product with  $ab$  or  $a \bullet b$ , deeming your Komma in  $a, b$  *unacceptable* from



reasons given vol. 1, p. 193/194. For the relative product dually corresponding with your  $a \uplus b$  I write  $a ; b$  ( $a$  <sup>of</sup> von  $b$ ). Of course your 8 [reduced]<sub>2</sub> to four operations in vol. III Am. Journ. are rendered superfluous by the two:  $\uplus$  and  $;$ . Last I *exchange* your denominations of "aliorelatives" and "opponents" from reasons to be given:—



(Charles S. Peirce, Esqu. Milford U.A.)

My dear Sir,

I have been some weeks ago most [agreeably]<sub>1</sub> surprised by receiving your well done and sympathetic [likeness]<sub>1</sub> in Sun and Shade. I did enjoy it especially as a token of your not having entirely forgotten me, as might else have been conjectured from their remaining unanswered three consecutive letters separated by long intervals.

My joy would have been a double one—had I not over a month before anticipated nearly half of it by succeeding to procure the same likeness of yours from another source—whence you may conclude that I am a bit tenax propositi.

However I greatly congratulate you on the way you proved able to at last comply with my request. I shall never be in a position to take a revenge in the same fashion, with returning a likeness of myself as published in a contemporary review of my Vaterland. Safe De Morgan and Jevons (Dedekind and Peano I am sure to acquire) my list of logicians/'<sub>2</sub> likenesses is already complete.

With respect to the two "inconsistencies" imputed to your theory in my last letter I must own [admit]<sub>3</sub> as to the second, that you were not guilty of it at all—at most attracting the suspicion of it on yourself by not using more freely the sign of inequality whilst as to the first I ought to have put the matter quite otherwise.

I have been constrained to cut out of my vol. 2, part II the contingent chapters of the relative logic on account of the size they swelled to, and am greatly occupied with framing them into a third volume, though not without bringing pecuniary sacrifices for the purpose of getting the latter printed. I am afraid it will not come out before the end of the year inst., but I venture to hope you will find interesting parts in it when accomplished.

With many thanks

very truly yours  
Dr. Ernst Schröder.

Karlsruhe in Baden  
April 12<sup>th</sup> 1893.

(Gottesauerstrasse 9)



(Professor Charles S. Peirce, Milford, P<sup>a</sup>, U.S.)

My dear Sir,

and I hope to be justified in continuing to say: friend.

I have had the pleasure of dispatching unto your address, as last given me, a paper: "Note über die Algebra der binären Relative" of 15 pages, that is opening\*) the vol. 46, 1895 of the *Mathematische Annalen*, and precursory to the coming out of my vol. 3 part I, of which 400 pages are already printed and somewhat over a hundred are to be printed yet. End of vol. 2 is to follow it a little later.

For diminishing the risk of sending the voluminous book itself, I should be grateful if you were so kind as to acknowledge\*\*) by a postcard the having received the present letter and the aforesaid paper to which I intend to add also a specimen of my "Anzeige" (advertisement) of the vol. 3 in Teubner's *Literarische Mitteilungen*—for the sake of putting you entirely "au fait".

In the book, if not in the paper, I hope you will find several things of interest even to yourself, and at any rate gather therefrom that I have struggled very hard and to the best of my ability to thoroughly understand, expose and promote your important, fundamental theory. Perhaps then, when vol. 3, I and 2, II is accomplished, the time will have come and be appropriate, for you: kindly to reconsider, whether you would not for the completion of my vol. 3 supply me with your contributions, as once formerly hinted. For the more logical than algebraical parts of the book, especially the interpretation by words of the algebraical symbols as well as for examples and applications of the theory, I should indeed be glad for some help. In case of such a favor—which I only beg to confer on me by means of *registered* letters—I would of course scrupulously acknowledge your contributions. You may, of course continue employing therewith your own notations, as wonted, which I am capable to read (now), notwithstanding their divergence from mine in several respects, as for instance with regard to brackets.

[As to examples I have [hitherto], besides Macfarlane/']<sub>2</sub> relationships, only prepared some applications to the theory of numbers.]

If in the mean time you have not forwarded some publication on the subject as yet unknown to me, I feel pretty certain that my book (vol. 3 part I) will contain not a few groups of formulae as well as elementary or important theorems, that are already known to you. And, as to these, you will—at least in publication—have been anticipated by me. If, when in possession of the book, you would kindly supply me with a list of them, or of those out of their number, you mainly lay stress upon, I should rejoice

[mentioning]<sub>1</sub> them in the continuation (part II) of the volume, and to point out the results wherein we had met—so to say independently. With the words "so to say" I meant to imply: save the fact of my having been initiated by dint of your publications into your theory in general.

Notwithstanding my tendency towards thoroughness I am already aware, that I have not been spared to outlive several misconceptions of mine—that will be set aright by and by in the continuation of vols. 2 and 3. As to those, I should not be aware of yet, you could oblige me by some hint, perhaps.

Much convinced as I am of what it means to encroach on your time, and though I do not seldom feel very heavily tasked myself by the necessity of entering into some private correspondence of any kind, I venture to say that, as there are at any rate not many persons writing a book to that extent (of over 2000 pages) on your ideas, or such as you have laid down the germ of, and [developing]<sub>1</sub> them farther, as I am striving—in view of that I believe to have a moral claim, slight as it may be, to learn the view you take therefrom, or to be informed of your "Stellungnahme" against it, with respect to the points also where we might be disagreeing. And I begin cherishing the hope, that you might a little more abandon your [hitherto]<sub>1</sub> reserve in that respect.

Please consider the present as a "ballon d'essai" whether the book would reach you, and believe me

very faithfully yours  
Dr. Ernst Schröder,  
Prof.

Karlsruhe in Baden,  
28. Febr. 1895

(Gottesauerstr. 9)

P.S. With but small trouble you could easily oblige me by an information: *where* your paper quoted as "6)" in the literary index of my vol. 1, p. 710, entitled: "Brief description of the algebra of relatives", has appeared?? You'll find the question "(wo?)" behind. I have been no less than 3 times at Strassburg <sup>i</sup>/Els. spending the whole day in the library of the University and fumbling all its American periodicals, etc., without hitting on such a contribution anywhere, and I should despair of making it out myself, since I am not allowed the leisure to go to München or Berlin for the purpose. The specimen I am in possession of, I am owing to your kindness.

The same.

\*) Not exactly—the numbers of pagination having (since) eventually changed against the corrections, when they were the foremost ones.

\*\*\*) Sufficient: to confirm or renew your address.





(Prof. Charles S. Peirce, Esqu. Milford Pa. U.S.)

My dear friend,

In possession of your kind letter of Nov. 7, 1895 I am first to apologize, on account of an excess of work, for not having answered it earlier.

I am so sorry that your means of subsistence have grown such scarce ones and very much regret not to be in a position to help you substantially. I am nothing less than a capitalist, and besides had to bleed very heavily quite of late for my Vol. 3, I in the shape of irretrievable contributions to the expenses of its being put to press, & must be or get prepared to do so again for Vol. 3, II.

You must allow me however to range with the subscribers of your contributions mentioned in the cutting that accompanied your letter, accepting the twenty Mark as my subscription for the last and the present year, that I shall post unto your Milford address at once with this letter. Anything, you may have said or say on the topic, is too interesting and valuable for me, not to secure a right on its possession! Please to post your contributions, when finished as a whole or in a few portions by and by as you deem fit, unto my address, repeated below—registered, if feasible.

From your (unknown to me) "Chicago friend" nothing has reached me [hitherto]<sub>1</sub>. Possibly he has not sufficiently guarded the secret you mentioned, of your standing behind him, for it is scarcely a week—"entre nous" be it said—that Dr. Carus tried to secure my help for a criticism of my own work in his periodical. I don't know from whence the ill-feeling arises that I could not but be aware of, when he visited me a couple of years ago. Suffice to say, that I flatly, though politely of course, refused (sending him only my "Anzeige" which I believe he was already in possession of). In a six pages letter I exhausted my eloquence trying to dissuade him from undertaking such a criticism himself, and to reconcile him (if possible) with you. I most seriously assured him, that however ungrateful your countrymen and contemporaneans might prove, your fame would shine like that of Leibniz or Aristoteles into all the thousands of years to come, and that he could do no better than openly to join your banner (however: difference of opinion as to necessitarianism allowed); pointing you out as the one mostly, if not only, appropriate for undertaking the task of the criticism, and laying stress on the fact that *your* doing so would evidently lie in the interest of the Monist itself; advised him to ask, and offer you a good salary for the contribution. Should he therefore (as I hope, but with regret cannot help doubting somewhat) come to meet you, then build him a golden bridge!

As to me, these confidences ought not to make you refrain from blaming whatever you find worthy of it in my book—in case of the happy occurrence I am longing for. A blame from your side I should [cherish]<sub>1</sub>

more than any praise from persons whose business it cannot be to undertake such a criticism at all. In case of my *not* having succeeded, I must leave things going as they might, having tried all I could to better them.—

In order to somewhat accelerate the recognition of the algebraic logic, so as to experience some of it during our lifetimes (that we may live yet to see some of it) it will be indispensable that the discipline soon shows its force by deciding burning questions among the celebrated mathematicians. I am therefore presently working hard with proving several theorems of Prof. *Georg Cantor* on one to one correspondence and "Gleichmächtigkeit von Mengen", [hitherto]<sub>1</sub> unproved, and have indeed succeeded with a good deal of them (not for all as yet). I hope to send you the papers when I get them printed. This appearing to me of still greater importance (not to [lose]<sub>1</sub> such an opportunity), I [gave]<sub>2</sub> it purposely the preference to finishing my . . . so much extra work, retarded (I shall be content if it is accomplished before the lapse of this year).

There are some points in your letter of [March]<sub>1</sub> 20, 1895, which I intended to skirt in mine, but have not done so as far as I know. Whitney's [dictionary]<sub>1</sub> of the English language is not within my reach, so that I am sorry not to be able consulting your divers articles on relation etc. there. I cannot order such volumes from Berlin for instance, and at present I am neither likely to go there for a visit in the library, nor to München—supposing it to be found there, etc. Had I more leisure, I should circulate a quest to ascertain where it is to be seen. But with my actual overtaskedness such designs are remaining "fromme Wünsche" (innocent wishes).

You wrote that you were working out a general cromology. Does that mean—in consequence of a slip of the pen perhaps—chromology = *Farbenlehre*, a theory of colour-[seeing]<sub>1</sub>? Night advancing I must conclude. With best wishes      very faithfully yours

Dr. Ernst Schröder, Prof.

Karlsruhe in Baden,  
16. Februar 1896.

(Gottesauerstrasse 9)



My dear Friend:

I have just concluded writing a review of your logic. I sincerely hope you will not find its tone supercilious. But I must tell you that for 30 years I have been constantly writing for a literary journal and the editors do everything to make me write *caustic, sharp, impertinent, quarterly-review*, things; so that I have insensibly fallen into a habit from which I cannot free myself because I am unconscious of it. The present review is chiefly about philosophical points. I could not say much about the substance of the last volume. But *that* I intend to review for the Bulletin of the American

Mathematical Society. I have been making an investigation aided by the logical calculus into the multitudes of collections. I begin by establishing a doctrine of logical possibility. It is not sufficient to say that whatever is not self-contradictory is logically possible. We must distinguish *essential*, *informational*, and *substantive* possibility. (By the way, why is it that my important paper of 1868 on Logical Extension & Comprehension is not on your list?) When we speak of logical possibility we usually mean *essential* possibility which is the possibility of all which is not known to be false by a mind which knows nothing but logic. I maintain that there is a *substantive* logical possibility. The *substantively* possible is simply that which is admitted into a hypothesis, that hypothesis being definite. In this sense, of two contradictory propositions one is possible, the other impossible. In this kind of logical possibility, it is impossible [that]<sub>2</sub> the universe should consist of any limited multitude of objects; so that anything which would be possible were anything supposed to exist *is* possible. Next whatever character any object might have it is possible for all objects to have, unless a contradiction thereby arises. Next, any objects whatever form a collection distinguished by some marks from all other objects. We have to distinguish in the universe of substantive logical possibility the *hecceities*, the *first intentions*, and the *second intentions*. Let indices of the Italic alphabet denote hecceities, those of the Greek alphabet first intentions, and those that are accents and other such not alphabetic characters second intentions. Then I say that no matter what the description of  $m$ ,  $\Sigma_i m_i$  is possible unless it be self-contradictory, so that  $m \text{---} < 0$  by logical rules. Hence,  $\Pi_i m_i$  is impossible unless by the rules of logic  $\infty \text{---} < m$ . So  $\Sigma_i \Sigma_j l_{ij}$  is possible under the same limitation. But what is possible for one object is equally possible for every other. Hence  $\Pi_i \Sigma_j l_{ij}$  is possible under the same conditions. Consequently  $\Sigma_i \Pi_j l_{ij}$  (since this is the negative of  $\Pi_i \Sigma_j l_{ij}$  with another [negated]<sub>3</sub>  $l$ ) is impossible. In like manner  $\Sigma_i \Sigma_j \Sigma_k g_{ijk}$  is possible unless self contradictory. Hence  $\Pi_i \Sigma_i \Sigma_j l_{ij}$  is possible under the same limitation.



My dear Professor Schröder

Relative to the problem from  $a \text{---} < x\bar{x}$  to determine  $x$ , if the number of hecceities in the universe exceeds 3  $a$  can be eliminated.

Thus, let A, B, C, D, be four individuals and  $i$  any fifth. Let

$$x \text{---} A:B \vee A:D \vee B:A \vee B:C \vee C:A \vee C:C \vee D:B \vee D:D \vee i:A \vee i:B$$

$$\bar{x} \text{---} A:A \vee A:C \vee B:B \vee B:D \vee C:B \vee C:D \vee D:A \vee D:C \vee A:i \vee B:i$$



It makes no difference about  $i:C$ ,  $i:D$ ,  $i:i$ ,  $C:i$ ,  $D:i$ .

You will see then that  $\infty \rightarrow x\bar{x}$ , so that  $a$  is eliminated.

I have sent a long and tolerably careful review to Carus. You know what kind of a man he is. You eat a bushel of salt with him and you have not reached the conception of what twists he is capable. *But* what Carus leaves unprinted I shall print in the Bulletin of the American Mathematical Society,—a most respectable journal & the  $[n]_2$  the two I shall circulate together. I fear that Carus will pick my article to pieces & print it so that my real opinion may not be seen. *But if so* (it is mere surmise,—I don't like those Pomeranians) the correctorium fratris Pauli is in my hands.

My wife is dreadfully ill. The doctor says to me brutally: "We think the thing to be done is to extirpate womb & ovaries." A pretty speech!



(Charles S. Peirce, Esqu., Milford Pa. U.S.)

My dear Sir,

Being in possession of your two letters of last year (29<sup>th</sup> April, and the second undated) I can only now express my deep regret that they have so very long remained unanswered by me, especially the second one, which called me to your succour in a certain sense.

But, since, every day has brought its own particular exactions to such an extent, that I have never been left with sufficient energy to meet the demands of my private correspondence. I simply *could not* answer you before now. It would lead me too far, were I to attempt to enumerate the multifarious obligations laid upon me. Apart from my calling I have never been spared worries of various kinds; sometimes also I have felt the effect of overwork and been forced to take my health into consideration. Already my official duties are heavy and with examinations, sessions, certifications, correspondence pertaining, appointments of fresh professors, and so on, by far exceed what can be guessed from our program of my lectures. Moreover the publication of my volumes has drawn upon me a large amount of correspondence domestic and foreign (in different languages!), that makes me regret the want of a secretary, whom I am not in a position to provide myself with. Indeed I experience all the disadvantages of fame without its emoluments.

As one instance I should like to draw your attention to the pasigraphic movement *in Italy*. Have you ever noticed the 5 vols. of Peano's Rivista di Matematica together with his "Formulario" and so many papers of Burali-Forti, Peano, Pieri, de Amicis, Vivanti, Vailati, etc. therein, as well as in the reports of the Academia di Torino and in other Italian periodicals (also in the German Mathematische Annalen)?

By the bye you had better not [emphasize]<sub>1</sub> the comparatively trifling divergencies of our systems of notation in view of the contrast with the latter of the one, unanimously employed by those most active Italian investigators, which is, at least with regard to relative notions, so very inferior to ours. I have so to say to stand out nearly alone against them all; whereby "the Good" again and again proves to be an enemy of "the Better"—as is averred by the [German]<sub>1</sub> proverb: Das Gute ist des Bessern Feind.—

I thankfully acknowledge having received your article on the Regenerated Logic in the October Monist. From a remark in the first of your letters I should guess this to be the third or fourth review on my book you have published already. I am exceedingly sorry not to have seen, nor indeed to possess, your previous reviews. I should so very much like to enjoy your "caustic" wit, were it even at my expense. There is no danger of my feeling offended. Please at least so kindly let me know where they may be procured.

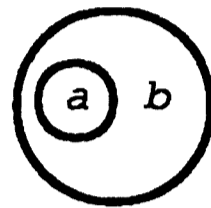
Before coming to the present [review]<sub>1</sub> let me briefly deal with your letters.

The first requires deeper study than I have been enabled hitherto to bestow upon it. Your notion of "hecceities" does not at first sight commend itself to my mind and I have not yet overcome its deterring effect. As to your assumption of an absolute solution of the problem  $a \notin x ; \bar{x}$  I as yet fail to see its exhaustiveness for an unlimited universe, if not its conclusiveness for a universe concerning which there is no presupposition (voraussetzungsloser Denkbereich).

The main point of your second letter, where you sought my aid which was not given when requested because of sheer overwork, appears to me to depend on some misunderstanding, if not a mental idiosyncrasy almost incomprehensible to those who do not share it. (Mental idiosyncrasies I had frequently and to my great astonishment discovered—though in a different form—in Mrs. [Ladd-Franklin]<sub>1</sub>, and in other investigators of the first rank as well. Thus I am much afraid and scarcely doubt, that I have my own. They are resembling the punctum coccum, or a lot of such, but are situated differently in every other pair of eyes).

The very simple fact being: That every  $\left\{ \begin{array}{l} \text{theorem} \\ \text{proposition} \end{array} \right.$  holding good in the class-calculus also holds in the statement-calculus. *But not vice versa.* There are many formulae holding in the latter, but not in the former. This is to be shown *by exemplification*, and one single instance would be sufficient as proof. Now in my section §45 (NB. see further on) vol. 2, I have given dozens of such instances, all the formulae marked with an asterisk belonging to the category (of those holding good in the statement calculus, whilst not holding in the class calculus). One need but examine any one of them with sufficient thoroughness. For your benefit I will put a few examples before you. The simplest and at once fundamental example is:

$(a \notin b) = \bar{a} + b = (a = 0) + (b = 1)$ . This formula, rightly so much [emphasized]<sub>1</sub> by you, holds with hypotheticals and *in the statement-calculus*, [i.e. 0, 1]<sub>2</sub> meaning propositions. It does *not hold* with *classes*, as the figure



shows, where  $a$  is  $\notin b$ , but neither  $a = 0$  nor  $b = 1$ .

Another example is furnished by your own formula (vol. 2, p. 270)  $(a \notin b) \notin (c \notin a)$ , to which I added a "pendant" l.c. They rigorously are valid in the statement calculus—the premise of yours implying that  $a = 1$ —but they do not with the classes:



A third example would be the propositions holding for statements only:  $(\Pi a = 0) = \Sigma(a = 0)$ , and so on.

*NB.* The §45 is *not* the section §28 first quoted, and as you say referred to by me in vol. 1 (I don't find where, just now). But that does not matter, inasmuch the reader is there prepared (cf. vol. 2, p. 13 the words: "ein Stück weit, *aber nicht durchaus*"—I am sorry not to have underlined) to refer the proper "Aussagenkalkul" *only* to the statements of constant meaning, and thence restricted to the values 0 & 1, wherefrom the §45 will prove a necessary consequence. At any rate the sections following §28 and mostly §45 ought to show, what I intended to call "Aussagenkalkul".

Your assertion that both calculusses are one—considered in the light of your own theory of (binary) relatives—would amount to saying: that your "*absolute terms*" or *classes*, also called by me *Gebiete* or *Systeme*, and logically defined by the relation  $a; 1 \notin a$ , were throughout exactly following the rules and formal laws of those relatives which I have originally termed "*ausgezeichnete Relative*" (distinguished?, *prominent* relatives) being capable only of the values 0 and 1. And such relatives indeed are all the "statements"! (You are wrong in denoting those relatives, when not vanishing, by  $\infty$ , and the statement coefficients, in the same case, by 1—thus preventing their very useful identification, cf. vol. 3, page 423, that, by the bye, I wish you might not overlook).

I should feel sorry, if the cause of your misapprehension or not having clearly understood the point, were in any way to be ascribed to the deficiencies of my exposition—imperfect though it be.

Now with this I hope that at least one point of your [review]<sub>1</sub>, here you find me unsatisfactory (page 34 to 36) is settled—at least as between us privately, its settlement before the public only remaining yet unaccomplished.

Some minor points:

p. 32, line 17 from the top: where do I hold the said doctrine to be "very erroneous"?

p. 34, concerning the alleged ignoratio [elenchi]<sub>2</sub> and the dragging in of time, you appear not to have taken into account Boole's Laws of Thought (!)—see chapter XI, p. 163 and following.

p. 38, line 15 from the bottom I am unaware to what part, or anything I have said in my vols. you there refer.

On p. 37, first line, you almost seem to confound myself with Mrs. Ladd-Franklin: see her "most unfortunate restriction" in her (pretended) review in Mind (New Series, vol. 1, p. 129, 1892) of my *vol. 1*. My ideas differ widely from hers on the point.

No less do I differ from yourself with regard to the principle of priority (alleged by you at the bottom of p. 38) being applicable to signs and symbols (or the like tools). However it would lead me too far to enlarge thereon at the present moment. (Meanwhile please compare the foot of p. 140 of my *vol. 1*). I may deal therewith in a more public form some day.

I ought sooner to have advised you by postcard of the receipt of your letters and of my actual inability to answer them at length, which from day to day I hoped to do, but was again and again disappointed by fresh and exigent calls on my time.

Yours very truly

Dr. Ernst Schröder,  
Prof.

Karlsruhe in Baden,

2<sup>nd</sup> March 1897.

(Gottesauerstr. 9)

P.S. [Dispatched]<sub>1</sub> only March 22: I had a vague recollection of an altered address having been given in one of your former letters, but, search made, only discovered "5 W. 50<sup>th</sup> St. New York" on Nov. 7, 1895, that I now believe to have been temporary.

Am not I entitled (by subscription) to receive your 52 columns on the general aspect of the logic of relatives in the Pike County Press?

E.S.





84 Broad St, New York 1897 April 7

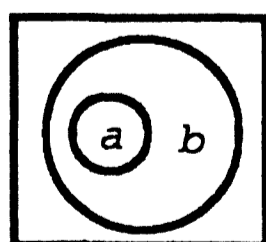
To my friend  
Prof. E. Schröder  
Carlsruhe in Baden.  
Gottesauer Str. 9.

My dear Sir:

Yours of March 2 (not finished till March 22) reached me today. I cannot tell you with what concern I hear of your danger of suffering from overwork. But I will return to personal talk after I have first replied in such fashion as I can without having either your book or my article at hand to your logical remarks.

You say "Your notion of 'hecceities' does not a[t]<sub>2</sub> first sight commend itself to my mind; and I have not yet overcome its deterring effect." Very just! I have always maintained, and I think always shall maintain, the wholesomeness of what we call Occam's razor (the maxim *Entia non sunt multiplicanda praeter necessitatem*) meaning that in explaining a given phenomenon we should refuse to admit any kind of element not already admitted by us until it becomes manifest that without such element the phenomenon cannot be explained. This is not distinctively nominalistic doctrine but is an indispensable condition to the rational coherence of philosophy. What distinguishes the nominalist is that he does *not* admit certain elements. The realist, if he is a sound thinker, must once have occupied the same position. By no means would I approve the ways of thinking of a man who did not hesitate long to admit my hecceities.

As I got no reply from you about my difficulties in regard to your position about the class calculus and the statement calculus, I studied your book until it become perfectly clear to me (as I think) and I endeavored to state the case in my article. Namely, it is a mere algebraic distinction. You can, as I do, write  $a \prec b = \bar{a} \vee b = (a = 0) \vee (b = \infty)$  and, then, when the quantifiers are added  $\Pi_i a_i \prec b_i$ , when it is not true that  $\Pi_i \bar{a}_i \vee b_i = \Pi_i \bar{a}_i \vee \Pi_i b_i$ . Or you may if you please use an algebra in which  $\Pi_i \bar{a} \vee \Pi_i b_i$  is written  $\bar{a} \vee b$ . You show me the figure



and of this state of things you say  $a \text{—} < b$  but neither is  $a = 0$  nor  $b = 1$ . I express this by writing  $\prod_i a_i \text{—} < b_i = \prod_i \bar{a}_i \vee b_i = \prod_i \{(a_i = 0) \vee (b_i = \infty)\}$ . To express "either  $a = 0$  or  $b = 1$ " in the sense you mean, I should write  $(\prod_i a_i = 0) \vee (\prod_i b_i = \infty)$ . Thus, everything which can be expressed by the class-calculus is readily expressed by the statement-calculus, *if the quantifiers are used*. But if the quantifiers are *not* used, an ordinary hypothetical proposition cannot be expressed. It is *not true* that  $a \text{—} < b = (a = 0) \vee (b = 1)$  in regard to statements *any more than* it is in regard to classes. The cases are precisely parallel. If you are admitting only a single state of things as possible, then it is true that to say that "If it is a pleasant day, then the sun shines" means no more than that "Either it is not a pleasant day or the sun shines." But so likewise about classes, if discourse is limited to a single individual (analogous to a single state of things), it is equally true that  $a \text{—} < b$  means precisely  $a = 0$  or  $b = \infty$ . That is, if our universe of discourse is a single individual, to say that Every wise God is a good God is to say either there is no wise God or there is nothing but a good God. If, however, (as is usually the case) I am *not* considering a single state of things as alone possible, then to say that "If I forget to mail this letter, then it will go by the next steamer" is *not* the same as to say "Either I shall not forget or it will go"; for it may be the latter will be true, but the former is false. For what the hypothetical means is: "Taking any possible case, it is either not a case of my forgetting the letter, or it is a case of its going by the next steamer," which is of course very different from saying "Either any possible case is a case of my not forgetting or any possible case is a case of the letter going." So, it is no more true of statements than of classes that  $a \text{—} < b = \bar{a} \vee b$ , unless an individual state of things or an individual thing constitute the universe of discourse.

You say  $(a \text{—} < b) \text{—} < (c \text{—} < a)$  is *my* formula. But you forget that in that paper I first stated the necessity of the quantifiers unless our universe of discourse is individual. The formula is just as true in the calculus of classes. Namely, if the universe of discourse is an individual, say God, then if not all that is wise is wicked, all that is happy is wise. For since we are speaking of but a single individual, if not all that is wise is wicked, that individual is wise, and therefore whether that individual be happy or not, in the one case (if he is happy) that happy [individual]<sub>2</sub> is the sole individual and is wise, and if he is not happy, 0 is included under everything. But the formula *in the sense you seem* to attach to it, is *not* true of the statement-calculus. That is, it is absurd to say that from the falsity of the consequence "If anybody is wise he is wicked" follows the truth of the consequence "If anybody is happy he is wise." It is evident that if it were necessarily true in the statement calculus it could not escape being true in the class calculus,

because every statement about the inclusion of one class under another is but a statement that one statement follows from another empirically.

So with the formula you write  $(\Pi a = 0) = \Sigma(a = 0)$  by which I suppose you mean  $(a \cdot b = 0) = (a = 0) \vee (b = 0)$ . Let  $a$  be the statement that on the first of next January Gen<sup>l</sup>. Weyler will be assassinated. That is quite possible, and it is therefore not 0. Let  $b$  be the statement that on the second day of next January Gen<sup>l</sup>. Weyler will be assassinated. That is almost equally probable. But that he should be assassinated both days is impossible. In short, any formula which does not hold in the class calculus when the universe of discourse embraces more than a single object, equally fails to hold in the statement-calculus when the universe of discourse embraces more than a single individual case.

What you call the statement-calculus is nothing but what the calculus of logic becomes when but a single individual forms the universe of discourse. You choose to consider the class-calculus as more fundamental. I think that an error; but if you think so, then it appears to me you ought to regard the statement calculus as idle nonsense. Not worth the space you give to it.

I, on the contrary, consider the calculus for an individual universe as fundamental. I hold that above all the other universes which may be employed in a given proposition this is always present, and  $x(1 - x) = 0$  means just what an ordinary algebraist would understand it to mean. I also hold that the way to understand any *other* universe most clearly is by means of  $\Pi$  and  $\Sigma$  with indices, the Boolean part being conceived to refer to the one-individual universe.

Between those two views, I see nothing more important than a slightly different algebraical conception. According to me, there are but two *values* of logical quantities,  $a = 0$  or  $a = \infty$ . You write  $a\bar{a} = 0$  but you do not understand this to mean  $a = 0$  or  $a = \infty$ . You say there are intermediate values. I say, there are not *in my conception of the matter*. There is something intermediate between  $\Pi_i (a_i = 0)$  and  $\Pi_i (a_i = \infty)$ . But there is nothing intermediate between  $\Pi_i a_i = 0$  and  $\Pi_i a_i = \infty$ . For  $\Pi_i a_i = 0$  is the same as  $\Sigma_i (a_i = 0)$  which is the same as  $[\Pi_i (a_i = \infty)] = 0$ .

When your second part appeared, I said to myself "When he comes to the logic of relatives and has to deal with indices, he will see that there is no difference between the calculus of statements and that of classes." How it is you fail *still* to see the identity of the two, long puzzled me. But I have concluded it is because you have accepted what I said about statements,—and my formulâ€—as concessions you might safely make, without remarking that I said they were false and that they manifestly *are* false—unless we confine our discourse to a universe of a single individual state of things. I think, too, that you are evidently confused by the difference between what *you* mean by  $a \prec b$  and what *I* mean by it. What you mean by it, is what I mean by  $\Pi_i (a_i \prec b_i)$ .

This letter is so long I will break off here and write again, shortly. I will just say that the editor of the Pike County paper broke his agreement shamefully; but I intend to print the articles before long in a new journal of exact thought to which I hope you will do me the honor to contribute. I don't yet speak of it publicly. I *think* I have made some money, though it is not yet certain. I undertook to place some bonds for a commission. A company in London agreed, as I understand, to take the bonds. But it turns out they did not absolutely agree; but still they probably will take them. If so and everybody is honest, I hope to give exact thought a helping hand. But naturally, with doubt hanging over it, I do not speak of it.

The address 84 Broad St New York City, will go for this summer I think. It is my business office. Or you can address me 108 West 89<sup>th</sup> Street where I have an apartment. When I write again, I shall tell you of some personal matters. My wife is in a dangerous condition of health.

very faithfully C. S. Peirce



*Karlsruhe in Baden*, 31. Oct. 1898.

Dear Sir,

Longing to send you four recently published papers of mine, several of which are costly and rather voluminous, I abstain from doing so because of my uncertainty as to your actual address: my registered letter of March 2, 1897 (dispatched March 22<sup>d</sup>) having never been acknowledged. May I ask, whether it has reached you?

If you are not opposed to receiving any personal communications from this quarter, you ought to confirm or renew to me your address, continuing to keep me well informed as to your whereabouts, some postcard sufficing.

Hoping soon to receive some token of life ("Lebenszeichen") from your side, I remain

very truly yours

Prof. Dr. E. Schröder,  
(*Gottesauerstr. 9*)





(Charles S. Peirce, Milford)

My dear Sir,

I duly received your letter of Nov. 19 and am sorry that mine of March 22 of last year, although registered, has not apparently reached you. The four promised papers, in two packets, were posted you a few days ago.

I hope they may find you in better circumstances and still full of high resolves. You must permit me to decline to accept a copy of your forthcoming book on Logic gratis. I shall insist upon remitting the [price]<sub>1</sub> immediately after it comes into my hands. Most regrettably the modest salary of a German professor of pure Science such as Mathematics, and the heavy sacrifices entailed upon me by the publication of my work—these being wholly disproportionate to my means—do not allow me to render assistance to any person in pecuniary straits.—

In my opinion there is no need of ampl[i]<sub>2</sub>fication of your theory for solving the Problems mentioned in your letter, the denotation-system of the former entirely sufficing for the purpose.—

If time serves me I may send you the gist of the lost letter.

Believe me,

very faithfully yours

Ernst Schröder,  
Dr., Prof.

Karlsruhe in Baden,  
7. Dec. 1898.

(Gottesauerstrasse 9)

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