A Study of Maurice Fréchet: II. Mainly about his Work on General Topology, 1909—1928

ANGUS E. TAYLOR

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Contents

1. Introduction

This is the second of my essays devoted to a study of FRÉCHET and his work on abstract spaces and general analysis. I plan to write a third essay; it will deal mainly with FRÉCHET'S work on polynomial operations, differentials, power series expansions, and general analysis in linear spaces. The first essay was mainly about his early work on abstract point set theory (*i.e.* general topology), culminating in his doctoral dissertation of 1906, and his work on linear functionals, the principal achievement of which was his representation theorem (of 1907) for continuous linear functionals on the class L^2. (But FRÉCHET did not use the symbol L^2 for that class. The notations L^2 and L^p, with $p \geqq 1$, were introduced by F. RIESZ.) For convenience I shall regularly refer to my first essay on FRÉCHET as Essay I. See the bibliography.

In Essay I I listed all of FRÉCHET'S publications through 1908 (and a few after that) even though I did not analyze or make reference to a number of them.

In this essay I list all the publications from 1909 through 1928, which was the year in which FRÉCHET'S book on abstract spaces was published. It was also the year in which he was appointed to the faculty of the University of Paris and began a new period in his life, a period in which he partially abandoned what had for long been his main line of work – general topology and general analysis – and turned his primary attention to the theory of probability. In my essays I make no attempt to analyze and evaluate the work of FRÉCHET on probability and statistics. He published voluminously in these fields, and from time to time after 1928 he also wrote papers that were related to his work before 1929. But I believe that FRÉCHET'S most important accomplishments were made in the subjects which I shall cover in my three essays. Certain of FRÉCHET'S publications after 1928 are listed because of their relevance to this essay.

The term 'general topology' as I use it in this essay usually means point set theory in an abstract space, as developed from certain axioms and definitions, and always involves the notion 'limit point of a set,' either as a primitive notion or as a notion or concept defined with the aid of some other primitive notion. An alternative term to 'general topology' is 'point set topology'. One may also speak of general topology in Cartesian or Euclidean space or in a non-abstract space whose elements, or 'points', are objects such as functions, curves, or surfaces. For a long time FRÉCHET avoided the words 'space' and 'topology' in his general theory of *ensembles abstraits*. He also preferred for a long time to speak of 'elements' rather than of 'points', unless the elements were defined by coordinates.

By 1909, at the beginning of the period dealt with in this essay, FRÉCHET had considered three methods of developing an axiomatic point set theory: (1) the method of L-classes, (2) the method of V-classes, and (3) the method of E-classes. These were set forth in the first part of his doctoral dissertation; I discussed them in Sections 4 and 5 of Essay I. In all three methods an element p is a limit element of a set S if there exists a sequence $\{p_n\}$ of distinct elements $p_1, p_2, p_3, \ldots$ such that the sequence converges to (or has the limit) p. The collection of limit elements (if any) of the set S is called the derived set of S and is denoted by S'. It may be empty. For an L-class the notion of a convergent sequence with its limit is a primitive notion satisfying certain axioms. For a V-class or an E-class the notion of a convergent sequence is defined with the aid of a real-valued binary function (a function of two elements). In the case of a V-class a value of this binary function is called by FRÉCHET a *voisinage* (which translates as 'neighborhood', but which is *not* a set of elements, as in standard modern terminology today, but a non-negative real number). In the case of an E-class, FRÉCHET speaks of an *écart* instead of a *voisinage*. An E-class is in fact a metric space and the écart of two elements is their distance apart. The concept of an E-class is due to FRÉCHET. The name 'metric space' for an E-class was introduced by FELIX HAUSDORFF (using the German name metrischer Raum) on page 211 of the book he published in 1914 [HAUSDORFF][1].

Independently of FRÉCHET, in the year after the publication of FRÉCHET'S thesis, F. RIESZ had proposed a general point set theory, using as primitive the

[1] An author's name or name and number, in square brackets, refers to the Bibliography at the end of the paper.

notion of the derived set of a given set (all sets being subsets of a given abstract class). This work [RIESZ 2, 3] is discussed in Section 8 of Essay I. Because RIESZ's axioms will play a role later on the this essay they are recapitulated here. For convenience I use modern set notation in doing this. There are four axioms.

1. If S is a finite set, $S' = \emptyset$ (the empty set).
2. If $S \subset T$, then $S' \subset T'$. ($A \subset B$ means A is a subset of B.)
3. $(S_1 \cup S_2)' \subset S_1' \cup S_2'$. ($A \cup B$, the union of A and B, is the set composed of all elements of A and all elements of B.)
4. If $p \in S'$ and $q \neq p$, there exists a subset T of S such that $p \in T'$ and $q \notin T'$. ($p \in U$ means p is an element of U; $\notin$ is the negation of $\in$.)

RIESZ defined the notion 'neighborhood' (in German, Umgebung) of an element and related it to the notion of a derived set. He called S a neighborhood of p if $p \in S$ but $p \notin (S^{\sim})'$, where $S^{\sim}$ is the complement of S (the set of elements in the basic class but not in S). RIESZ proved that if $p \in S'$ every neighborhood of p contains infinitely many elements of S and asserted (correctly) without proof that if p and S are such that every neighborhood of p contains infinitely many elements of S, then p must be an element of S'. The fourth axiom is not needed in the foregoing.

RIESZ busied himself with other things and never developed the consequences of his axioms extensively and systematically. His ideas were used by others, however, as we shall see.

Still another method of constructing a general topology came on the scene soon after 1910. It was a method in which the notion of 'neighborhood of a point' appears in the fundamental role. Neighborhoods are sets, subject to certain axioms. They are used to define derived sets.

Of course, the notion of a neighborhood as a set of some kind already existed in various forms prior to RIESZ, but not, I think, in the context of axiomatic abstract point set theory in the generality we are considering. The notion of 'nearby points' in CANTOR's point set theory and of 'nearby functions' in the calculus of variations are forerunners of the notion of neighborhood as we shall see it appearing later in this essay. FRÉCHET's use of the word *voisinage* in connection with his V-classes seems aberrant today, because it denoted a number rather than a set. But at the time it was not unnatural, for the methods of expressing 'nearbyness' with which FRÉCHET was familiar all involved the use of inequalities and positive numbers.

A general perspective on FRÉCHET's role in the early decades of the development of general topology is given in the concluding section of this essay.

All references to 'the Archives' in the Essay are to the Archives de l'Académie des Sciences de Paris. Unless otherwise noted, all documents and letters cited are in the Archives.

2. An overview of Fréchet's career, 1907–1928

The academic positions held by FRÉCHET after he obtained his doctorate in 1906, and through 1928 were as follows.[2]

1907: Professeur de Mathématiques spéciales préparatoires au Lycée de Besançon.
1908: Professeur de Mathématiques spéciales au Lycée de Nantes.
1909: Maître de Conférences à la Faculté des Sciences de Rennes.
1910–1918: Chargé de cours and then Professeur de Mécanique à la Faculté des Sciences de Poitiers.
1919–1928: Professeur d'Analyse supérieure à la Faculté des Sciences de Strasbourg.
1921–1929: Professeur de Statistique et d'Assurances à l'Institut d'Enseignement Commercial supérieur de Strasbourg.

Positions in Paris at various times from November 1, 1928 onward:

Initially Maître de Conférences à la Faculté des Sciences de Paris (Institut Henri Poincaré et Ecole Normale Supérieure). Also Directeur d'Etudes à la Première Section (mathématiques) de l'Ecole des Hautes-Etudes. Then (1928–1933) Professeur sans chaire à la Faculté des Sciences de Paris, and, after November 1, 1933, Professeur de Mathématiques générales à la Faculté des Sciences de Paris. From November 1, 1929 FRÉCHET was also Professeur d'Analyse et de Mécanique à l'Ecole Normale Supérieure de Saint-Cloud.

FRÉCHET married in 1908. He and his wife, born SUZANNE CARRIVE, had four children, HÉLÈNE, HENRI, DENISE and ALAIN. During the years of the Great War, 1914–1918, FRÉCHET maintained his appointemnt at the University in Poitiers, but was actually in military service. He was mobilized into the French Army on August 4, 1914. On May 8, 1915, with the rank of lieutenant, he was assigned to duty as an interpreter attached to the British Army. In this capacity he was at or near the front for about two and a half years. On November 4, 1917 he was sent to London as a member of a French mission on aeronautics.[3]

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[2] Sources of information: Primarily FRÉCHET's Notice sur les Travaux Scientifiques, Hermann, Paris, 1933. Also POGGENDORFF and documents in the Archives de l'Académie des Sciences, Paris. The exact dates of month for FRÉCHET's moves from one place to another are not indicated. Some of the correspondence suggests that FRÉCHET was already in Nantes late in 1907 and at Rennes already at some time in 1908.

[3] For both general and specific information about some aspects of FRÉCHET's life and career I am especially indebted to his daughter, Mme. HÉLÈNE LEDERER, with whom I had two long talks at her home in a suburb of Paris in 1979. Some information derives from various documents in the Archives. Additional information may be found in two Notices nécrologiques about FRÉCHET, one by SZOLEM MANDELBROJT in C. R. Acad. Sci., Paris, t. **277** (19 Nov. 1973), Vie Académique, 73–75, and one by DANIEL DUGUÉ, International Statistical Review **42** (1974), 113–114. There is a memorial article about FRÉCHET by FRANK SMITHIES in the Year Book of the Royal Society of Edinburgh, 1975. FRÉCHET was elected an Honorary Fellow of this society in 1947.

In 1919, while still in uniform, FRÉCHET was selected to go to Strasbourg to help with the reorganization of the University there. His appointment as Director of the Institut de Mathématiques at Strasbourg gave him a heavy responsibility. An examination of his publication list during the years 1920–28 shows that he was very active indeed in research and writing, along with his administrative duties.

If the war had not intervened, FRÉCHET would have spent the academic year 1914–15 in the United States as a visiting professor at the University of Illinois in Urbana. In a letter of February 23, 1914, LEBESGUE wrote to FRÉCHET: "Votre nomination à Urbana rendra à coup sûr services à l'influence mathématique française en Amérique. Je vous félicite de votre détermination." FRÉCHET's daughter (see Note 3) told me that the family got all ready to depart for America, with trunks packed and about to be sent off to the port of embarkation, when the war broke out. In the Archives there is a letter (dated September 15, 1914) from the office of the President of the University of Illinois regretfully accepting FRÉCHET's resignation of the appointment he could not keep. There is also a letter from group-theorist Professor G. A. MILLER in Urbana, dated September 19, expressing his disappointment that FRÉCHET cannot come, and wishing success to the French Army.

In spite of the fact that he was in military service during the Great War, FRÉCHET was somehow able to keep some of his mathematical work going. More than a dozen of his papers were published in the years 1915–19 inclusive. Quite a bit of this work was on subjects other than general topology, but in [FRÉCHET 63] and [FRÉCHET 66] he launched a new approach to general topology, breaking away from the approaches used in his doctoral dissertation. In this new work he used two different axiomatic methods. One method was borrowed from the method of F. RIESZ, mentioned in Section 1: use of axioms about the primitive notion of the derived set of a given set. FRÉCHET used some of the axioms of RIESZ and added an axiom not used by RIESZ. The other method, using axioms about families of sets called neighborhoods, was presented by FRÉCHET for the first time in a note [FRÉCHET 63] in the Comptes Rendus of the Paris Academy of date September 10, 1917. This method was presented in detail, but in a rather confusing way, in [FRÉCHET 66], published in 1918. Not until 1921, with the publication of [FRÉCHET 75], were the ideas broached by FRÉCHET in 1917 and 1918 presented in a more nicely finished way.

A fact of major significance in FRÉCHET's life occurred in 1914, namely, the publication in Germany of a book by Professor FELIX HAUSDORFF, then of the University of Greifswald. This book, entitled Grundzüge der Mengenlehre, contained a masterly development of a theory of general topology in an abstract space. I shall discuss this work of HAUSDORFF in some detail further on in the present essay, but a few words are appropriate here in order to indicate why the publication of HAUSDORFF's book was to be of great significance for FRÉCHET within the next ten or fifteen years. HAUSDORFF's exposition was systematic and clear. The book was studied by the oncoming generation of young scholars and university students of advanced mathematics in Germany, some other European countries, and the United States. Until the late 1920's it was the most convenient single source from which to learn abstract general topology, provided the learner could read German. Although FRÉCHET was really the founder of an effective theory of abstract general topology, whose work (mainly in his thesis) had made a major impact in the United States and Europe, the influence of HAUSDORFF'S book was dominant over the influence of FRÉCHET'S pioneering work by the middle of the 1920's, if not before.

According to a statement by FRÉCHET on page 367 of his paper [FRÉCHET 75], he did not read HAUSDORFF'S book until after the Great War. His exact words are: "Ce n'est qu'après la guerre que j'ai pu lire l'intéressant Livre de Hausdorff." Precisely when he learned of the existence of the book is not known, I believe. It may be that he got the information from T. H. HILDEBRANDT, who addressed a letter to FRÉCHET in London on February 2, 1919. He said he had been reading FRÉCHET'S paper of 1918 in the Bulletin des Sciences Mathématiques ([FRÉCHET 66]) as well as some prior papers of FRÉCHET. He expressed pleasure at the fact that FRÉCHET was distinguishing between the notions 'limit of a sequence' and 'limit element of a set'. Then he wrote: "I suppose you are aware of the fact that the idea of defining limit in terms of vicinity is not a new one. One finds something of the same kind with postulates similar to your own in the work of R. E. Root, Limit in Terms of Order, Transactions of the American Math. Soc. 15 (1914), 51–71, and in the work of Hausdorff, Grundzüge der Mengenlehre, page 209 and following. The treatment of this subject in the latter work is one of the best things I know along this line."

General knowledge of HAUSDORFF'S book by mathematicians in France may have been impeded by the Great War. The date at the end of the Foreword of the book is March 15, 1914, but the book may not have been off the press and in circulation until after the outbreak of war in August. There is no mention of the book in FRÉCHET'S writings prior to the one I have cited (which was in 1921).

Later on in this essay I shall discuss evidences of FRÉCHET'S tenderness and self-defensiveness because he knew that HAUSDORFF'S theory was superseding his own as the commonly used basis of general topology.

Early in the 1920's FRÉCHET began to gather his work on general topology together in a fairly systematic way. He did this at first in a sixty-page paper contributed, by invitation, to a volume published in India in 1922, celebrating the silver jubilee of a certain ASUTOSH MOOKERJEE. This is [FRÉCHET 76]. But this publication was not broadly available. Moreover, FRÉCHET merely stated his definitions and theorems; the publication was a narration of his theory through its various stages, but without the details of proofs. Hence it was not very useful to a student wishing to learn general topology in a systematic way.

At about this time FRÉCHET began to write a book about his general theory of abstract spaces, conceived of as an introduction to general analysis—that is, to a theory of functions in the context of abstract spaces. The book was finally published in 1928 ([FRÉCHET 132] in the bibliography). According to notes made by FRÉCHET, among documents in the possession of his daughter in 1979, the definitive manuscript of the book was handed over to the publisher at the end of December, 1926. But this book had no chance of making the kind of impact that had been made by HAUSDORFF'S book. It arrived on the scene too late, for one thing. Also, it was not arranged and written as a book from which advanced university students could learn general topology systematically in a form that would be currently useful in 1928 and immediately thereafter. In the main it was a presentation of FRÉCHET's own work on abstract spaces, generally without proofs.

FRÉCHET'S work at Strasbourg resulted in a large number of publications, especially in the years 1924, 1925, and 1928. In addition, he had serious administrative duties. One especially notable feature of the years 1923 and 1924 was his correspondence with the young and enthusiastic Russian mathematicians PAUL URYSOHN and PAUL ALEXANDROFF (I use the spelling of that time). The many letters to FRÉCHET from URYSOHN and ALEXANDROFF in 1923 and 1924, and from ALEXANDROFF alone for some years after the death of URYSOHN in 1924, are interesting not merely for what they show about the investigations being made by the two Russians, but for what they reveal, indirectly, about FRÉCHET.

While he was at Strasbourg FRÉCHET began to write on probability and related subjects. In the Bibliography see publications No. 73 (1921); No. 78 (1923); No. 95, No. 96, No. 100 (1924); No. 108, No. 115, No. 117 (1925); No. 125 (1927); No. 128, No. 133 (1928). As was noted earlier, FRÉCHET left Strasbourg and took up an appointment in Paris late in 1928. In conversations I had with Professor MICHEL LOÈVE in Berkeley not long before his death, he told me that he thought FRÉCHET'S move to Paris was at the behest of BOREL, who was anxious to have FRÉCHET write a book on probability as part of a series under BOREL's general direction. In his monograph on the life and work of BOREL ([FRÉCHET, BOREL monograph, 1965]), FRÉCHET wrote (on page 1) as follows about the call from BOREL: "Plus encore, en m'appelant, beaucoup plus tard, à venir a Paris le seconder dans son enseignement de Calcul des Probabilités, Emile Borel me prouva son estime, comme, d'ailleurs, en bien d'autres circonstances." In fact FRÉCHET did write a book in two volumes, the first volume of which came out in 1937, the second in 1938. See the Bibliography.

I sought to find out, if possible, from FRÉCHET'S daughter, more about the circumstances that accompanied FRÉCHET'S move from Strasbourg to Paris. In an exchange of correspondence in 1980 I asked her if she had memories about the decision FRÉCHET made to leave Strasbourg. What could she tell me about her father's thoughts concerning his role as the creator of general topology in abstract spaces and about his future ambitions in mathematics, just at the time when his book on Abstract Spaces was ready for publication? Did he, perhaps, feel that it was time for him to change the direction of his efforts, in view of the fact that his influence on general topology was diminishing? (She was aware of her father's sense that HAUSDORFF'S book had to some extent eclipsed his own pioneering work; we had talked about this in 1979.) I also remarked that doubtless FRÉCHET was happy for the opportunity to become a Parisian once more. Her reply was interesting. She said that her father was not in the habit of discussing, with the family, the decisions concerning his career. She thought his decision to leave Strasbourg was his alone. As for the change in the direction of his work, she avoided the question about the status of his influence on general topology. She mentioned the fact that for some time he had been interested in the *calcul des probabilités*, and in popularizing it. She cited the book written in joint authorship with MAURICE HALBWACHS [FRÉCHET 83]. Then she wrote "Doit-on rester toujours dans la même ligne?" On the subject of BOREL's influence she said that at the time there were two mathematicians who could be considered for the teaching in Paris of the *Calcul des probabilités*: PAUL LEVY and her father. She said that her father's work corresponded more closely with the tendencies of BOREL, and that he proposed (suscita) and then supported (soutint) the candidacy of FRÉCHET. As for FRÉCHET's interest in going to Paris, she indicated that a decisive factor might have been some disagreement he had the with Council of the Faculty of Sciences at Strasbourg. Here were her exact words: "Parisien pendant la plus grande partie de sa jeunesse, il quitta sans joie le capitale après l'agrégation. Mais une fois qu'il eut gouté le calme de la vie en province il eut preferé jamais revenir à Paris. Du reste il avait donné beaucoup de lui-même à l'Institut de mathématiques de l'université de Strasbourg et il aimait la proximité des Vosges et de la campagne. Mais je crois me rappeler qu'il se trouva en opposition avec le Conseil de la Faculté des Sciences et que, deçu, il se décida à repondre à l'appel d'Emile Borel."

Among the papers left by FRÉCHET and now in the Archives is an envelope dated 1907 in which are many small pages filled with closely written notes for use in teaching and setting examinations. The subjects include elementary calculus and differential equations with applications to curves, surfaces, and envelopes.

An undated letter from HADAMARD states that he has recently seen F. RIESZ and thus learned that RIESZ and FRÉCHET are in touch by mail. He expresses his pleasure that FRÉCHET already has "des continuateurs" and congratulates him on that, saying that that is the best outcome one can have from his work. This must refer to the period in 1907 when RIESZ and FRÉCHET were in correspondence about linear functionals (see pages 274–277 in Essay I).

In a letter of October 7, 1907, FRÉCHET's American friend E. B. WILSON writes to him from the Massachusetts Institute of Technology, to which he has recently moved from Yale. He reports his salary up from \$1800 to \$2500 and says he has just received FRÉCHET's new address in Nantes from VAN VLECK. There are two letters from WILSON in 1908. In the first (dated November 30 and sent to Rennes), he acknowledges FRÉCHET's card announcing his marriage. He says he likes his situation at M.I.T., where he has more time to work up his ideas; the teaching is not as advanced as at Yale. In the second letter he expresses pleasure that FRÉCHET is so well situated and congratulates him on having only three hours of lectures per week. WILSON himself has eleven hours. Speaking of his own work, WILSON laments that he has so much facility for learning too many things and writing little nothings on a great many subjects.

On December 11, 1908 HANS HAHN wrote a letter to FRÉCHET thanking him for sending a copy of his paper Essai de Géométrie analytique à une infinité de coordonnées (this is No. 28 in the list of FRÉCHET'S papers in Essay I). In the paper that HAHN wrote concerning FRÉCHET's thesis (see page 254 in Essay I) HAHN had, among other things, constructed an L-class for which the set of elements of condensation of a given set need not be closed, thus showing that the statement made by FRÉCHET on lines 7–8 of page 19 of his thesis as published is wrong. FRÉCHET had written to HAHN that he was aware of the mistake, which was a printer's error, he said. In the phrase "*et mème pour une classe* (L) *quelconque*" L should be changed to V. HAHN tells FRÉCHET he will prepare a note about this to go in the Monatshefte.

In 1910 was published HADAMARD'S book on the calculus of variations [HADAMARD], in the writing of which FRÉCHET played a major role as helper. There are two letters from HADAMARD to FRÉCHET (undated, as usual) that must pertain to this enterprise. One of them contains some interesting comments: "Je suis un adversaire très décidé de la méthode de Hilbert pour les conditions suffisantes de l'extrémum – une partie très médiocre de l'oeuvre de Hilbert, à mon avis. Je n'ai jamais compris – peut-être m'expliquez vous le succés fait – cet artifice qui n'apprend *rien*, absolument rien de plus que la méthode lumineuse, lapidaire, définitive, de Weierstrass, fondée sur la formule aux limites."

In 1912 was published FRÉCHET'S book [FRÉCHET 43] on the FREDHOLM integral equation, in collaboration with an Englishman, B. H. HEYWOOD. This did not represent any original mathematics on the part of FRÉCHET, but the book was well-received as a useful exposition of its subject. The publication of the book drew FRÉCHET into correspondence with ZAREMBA, who wrote FRÉCHET on March 11, 1912 to point out that FRÉCHET had cited work of STEKLOFF without mentioning ZAREMBA, whereas (ZAREMBA claimed) he had priority over STEKLOFF in the work cited. This did not prevent FRÉCHET from having cordial occasional correspondence with ZAREMBA in later years.

FRÉCHET was in touch with SIGISMUND JANISZEWSKI in 1912 and perhaps earlier. In a letter of February 29, JANISZEWSKI expressed to FRÉCHET an interest in the notion of having a mathematics journal devoted to set theory and topology, and broached the idea of having various journals, each with its own specialty of subject matter. FRÉCHET evidently mentioned this to BOREL, who, in a letter of 10, 1912, expressed disapproval of JANISZEWSKI's idea, saying he thought it would present a serious inconvenience to mathematicians. (Incidentally, in this letter BOREL told FRÉCHET that he had little interest in the researches of BERTRAND RUSSELL, which seemed to him to be more philosophy than mathematics.) JANISZEWSKI'S idea was eventually realized with the launching of Fundamenta Mathematicae.

In 1912, also, there was correspondence between L. E. J. BROUWER and FRÉCHET. In a letter of May 17 BROUWER, writing to FRÉCHET about the proposition that a domain in space of n dimensions cannot be homeomorphic to a domain in space in $n+p$ dimensions, explains the trouble with an attempted proof by LEBESGUE and states that he has a proof by a modified method. Later, in 1914, BROUWER wrote to ask FRÉCHET for a copy of his thesis, saying it was inconvenient not to have one.

FRÉCHET was in correspondence with F. RIESZ again in 1913–14. On December 29, 1913 RIESZ wrote to FRÉCHET, apparently in response to a query from FRÉCHET about the convergence

$$\int f\,du_n \to \int f\,du$$

of a sequence of STIELTJES integrals when the sequence $\{u_n\}$ of functions of bounded variation converges pointwise and the u_n's are of uniformly bounded variation. RIESZ says he may have given details of the proof in his paper [RIESZ 4]. But, he says, proofs for more special cases have been given by HAAR in his thesis [HAAR] and by LEBESGUE in [LEBESGUE], and these proofs can be adapted to the general case. Going on, RIESZ says that, as for the memoir of RADON ("who, being Austrian, is not my compatriot"), he also has received the memoir, but hasn't looked at it in detail. He then acknowledges that it is hard to follow RADON at a place remarked on by FRÉCHET, and offers to be of help to FRÉCHET with the paper since he reads German better than FRÉCHET. We can see from this letter that FRÉCHET had already, late in 1913, begun the reading of RADON that led him eventually to his work [FRÉCHET 55, 56] on integration of a function defined on an abstract space. (I expect to discuss this and certain other works of FRÉCHET in my third essay.) RIESZ wrote to FRÉCHET again from Koloszvar (later known as Szeged) on May 17, 1914. FRÉCHET had sent him one of his publications and invited RIESZ to see him in France. RIESZ says that perhaps he will be able to visit him before his (FRÉCHET'S) departure for the United States (see my earlier reference to FRÉCHET'S projected appointment at the University of Illinois). He tells FRÉCHET about a gathering planned for September in Hannover, where the subjects of discussion will be DIRICHLET series and the zeta function. Among those expected to attend: H. BOHR, G. H. HARDY, J. E. LITTLEWOOD, and MARCEL RIESZ. As for Esperanto (says RIESZ), he has great respect for it but thinks it more difficult than Italian, which he understands without having studied it. (FRÉCHET was an Esperanto enthusiast; he published some mathematical papers in that language, and later became President of an international Esperanto society.)

D. R. CURTISS, who had known FRÉCHET in Paris during the latter's student days at l'Ecole Normale Supérieure, and who was by 1914 an established faculty member at Northwestern Unversity in Evanston, Illinois, wrote to FRÉCHET several times in the years 1915–17. These letters were to some extent about FRÉCHET'S papers to be published in the Bulletin and the Transactions of the American Mathematical Society ([FRÉCHET 60a, 60b, 65]). The letters also contain remarks about the war. On October 30, 1915 CURTISS writes that it is commonly thought the United States cannot keep out of the war if it lasts for two or three years. Sympathy with France and England is growing steadily, he says. On May 20, 1916 he wrote that the U.S. "approaches crises from day to day, always to withdraw and yet always keeping near the edge of the war. Meanwhile we are totally unprepared." On August 16, 1916 he wrote: "The ring seems to be closing on Germany, but in the west it is slow." He opines the war may last another year or so. On February 2, 1917 CURTISS writes that mail is slow, that the second part of FRÉCHET'S paper on "limit and distance" has finally arrived, but that there may not be space for it in the Transactions until January, 1918. (That is when it did appear [FRÉCHET 65].) He says he is always relieved to hear that FRÉCHET is safe so far. Everyone is asking (he says) how we can avoid a break with Germany. He thinks some of WILSON'S manners of speaking have been unfortunate, but "I expect him to do the right thing in this crisis. Germany is evidently desperate." The letter of May 20, 1916 contains a reference to receipt by CURTISS of a list of FRÉCHET'S publications on le calcul fonctionnel, after which he writes: "I shall keep this with your other letter, for the use which I hope I shall not need to make of it. I have spoken to a number of mathematical colleagues (including Prof. E. H. Moore) and they seem to think the project of publication here is feasible, though agreeing that you could do it better yourself after the war. Of course that goes without saying." This presents a puzzle as to the exact nature of the publication project. It may perhaps be inferred that FRÉCHET was suggesting the possibility of having his collected works published in America in case he did not survive the war. This, in turn, may lead one to speculate that FRÉCHET felt that his work was more appreciated in America than in France. (That may well have been true.)

E. B. WILSON wrote to FRÉCHET several times during the war. His letter of June 16, 1915 mentions that DE LA VALLÉE POUSSIN had come to dinner and that OSGOOD tended to be pro-German (he had a German wife). He mentioned that WILLIAM JENNINGS BRYAN "has just resigned from the Cabinet, thus relieving the Wilson Administration of an unfortunate incubus." He hoped that the U.S. would not have to go to war with Germany, for he thinks it would be more helpful to continue sending supplies, which would have to be stopped at least temporarily if the U.S. went to war, because it was so unprepared.

PAUL MONTEL wrote to FRÉCHET on April 2, 1916 on letterhead of the Societé Mathématique de France, to tell him that he hadn't forgotten about him and that his article would appear soon. (This would be the paper [FRÉCHET 62].)

There is a letter to FRÉCHET from R. GARNIER, dated June 6, 1917 in Poitiers. It is a newsy letter, about teaching and about people. GARNIER mentions that his "journées parisiennes" are spent in "la Section technique de l'Artillerie où je fais différents calculs." He evidently sees LEBESGUE and MONTEL from time to time, their places of work in Paris being near his.

Among FRÉCHET'S effects in the possession of his daughter when she let me study them in 1979 were two small notebooks which FRÉCHET kept with him during the war. Most of the contents of the notebooks are miscellaneous mathematical jottings. One of them, on the first page, contains a reference to a commune with an illegible name in the Départment du Pas de Calais, with the date 9 juillet, 1915, followed by some notes concerning rules for military persons with relation to buildings in the town. On later pages there were queries and attempts at proofs of things about V-classes (in the sense defined in the thesis). It is hard to get a coherent sense of any accomplishment from these jottings. Perhaps the most interesting stuff in this notebook is what is revealed about FRÉCHET'S early plans for writing a book. Various thoughts about notation and typography are written down. There is no outline plan of the contents of the book, but there are a few specific indications of intent: "Pour mon livre faire des démonstrations avec l'écart en donnant l'énoncé avec le voisinage." Here he was using terminology from his thesis. He did not yet know what CHITTENDEN was to do to show the equivalence of *écart* and *voisinage*. (See page 254 in Essay I.)

The other notebook has written on its front: "Notes mathématiques. MF. Notes écrites sur le front entre 1914 et 1917 approximativement." On the inside is written: "FRECHET, Interpreter ASC HQ 1st Indian Cavalry Division." On the first page FRÉCHET is considering the problem of whether there are V-classes that are not E-classes. This probably relates to his work that was published as [FRÉCHET 66], in which V-classes and E-classes are defined differently from the usage in his thesis. This notebook contains pretty much the whole of the substance of the paper [FRÉCHET 103] published in 1925. Along with this there is a reference to the paper [FRÉCHET 38] of 1910, with a precise page reference in a manner that suggests that FRÉCHET had a copy of the paper with him. These things raise a question as to whether everything in the notebook was written there during the war. I think probably so. In this notebook too, one finds material about V-classes in the new sense presented in the papers [FRÉCHET 63, 66] of 1917 and 1918 respectively.

In his early years in Strasbourg FRÉCHET was very active in promoting contacts between his Institute of Mathematics and other mathematical centers in Europe. Among other things he sought to obtain copies of various mathematical journals on an exchange basis and asked various mathematicians to publicize within their universities the new mathematics program at Strasbourg. FRÉCHET was able to get for Strasbourg a meeting of the International Congress of Mathematicians in 1920. It was attended by about 200 mathematicians, including 80 from France and some from the United States. There were none from Germany or Austria. Among the Americans was NORBERT WIENER, who at that time was interesting himself in abstract spaces. FRÉCHET corresponded with SIERPINSKI and with ZAREMBA, who was usually in Cracow, but sometimes in Lwów. In 1919 ZAREMBA was telling FRÉCHET about the mathematical centers in the universities in Warsaw, Cracow, Lwów (formerly Lemberg, also Léopol), and Poznan (Posen). A university was being formed in Wilno (Vilna) and one might be formed in Lublin. He names the Polish journals in which mathematics might be published (the first issue of Fundamenta Mathematicae was to come out in 1920), and said he'd be glad to accept a memoir from FRÉCHET for the Bulletin of the Academy of Cracow. In a letter in 1920 he mentioned having met FRÉCHET in Cambridge, England. In July of 1920 he wrote about STEFAN BANACH and said he hoped that BANACH would be able to go to Strasbourg for the year 1921–22. Official arrangements were being instituted, he said.

There was a lengthy correspondence between FRÉCHET and SIERPINSKI, apparently beginning in 1919. FRÉCHET had sent SIERPINSKI some of his reprints and asked about making the mathematics program at Strasbourg known in Poland. SIERPINASKI was willing to help. He specifically asked FRÉCHET for a copy of his thesis, saying that they didn't have the Rendiconti del Circolo Matematico di Palermo in Warsaw. In reply to FRÉCHET'S comment that some things published by SIERPINSKI in December, 1911 and February, 1912, in the Bulletin of the Cracow Academy had been previously discovered by FRÉCHET himself, SIERPINSKI said that there was indeed a close connection between his work and that of FRÉCHET. In some letter FRÉCHET had evidently asked about LUSIN, with whom he wanted to get in touch, and also about ALEXANDROFF and EGOROFF. SIERPINSKI said he had last seen them in Moscow in January, 1918. He made some uncomplimentary remarks about "the barbarous Russians". He thought no one from Poland would be able to attend the mathematical Congress in Strasbourg. Later, in 1921, SIERPINSKI wrote that he had heard from LUSIN, who was living under difficult conditions in Moscow. On November 25, 1921, SIERPINSKI, replying to an inquiry from FRÉCHET, said about BANACH: "I know him. He is a very capable young man now an Assistant at the Ecole Polytechnique in Lwów." He said it would be a pity if BANACH couldn't deepen his studies in France that academic year.

In several more letters from ZAREMBA in the period 1919–1921 he mentioned the impending start-up in Warsaw of a new journal devoted particularly to the theory of sets. (This would be, of course, Fundamenta Mathematicae.) He hoped that some of the Polish mathematicians could come to the International

Congress in Strasbourg, but cited monetary problems, spoke of difficult times in Poland, and in July, 1920 said the international situation justified only black pessimism. In a letter of July 10, 1921, he mentioned some of BANACH's interests and publications and told FRÉCHET that BANACH had passed his doctorate earlier that year. BANACH was born in Cracow, came to know ZAREMBA there, then went to Lwów where he obtained his doctorate. FRÉCHET had learned about BANACH and his postulates for a complete normed linear space from NORBERT WIENER, who was his guest at the time of the Congress in Strasbourg (see page 15 in the article on BANACH by HUGO STEINHAUS in [BANACH, Oeuvres I]).

The paper [CHITTENDEN 3] came to the attention of HADAMARD, who wrote to FRÉCHET (the letter is undated, but is probably of late 1921 or early 1922) hoping that FRÉCHET could come to Paris to help out in HADAMARD'S seminar. Speaking about the paper of CHITTENDEN, he wrote "Je voudrais bien qu'on nous dise ce qu'il y a là-dedans et naturellement personne n'ose l'aborder." He said that FRÉCHET could "rendre un service sérieux qui personne d'autre ne peut rendre."

On April 3, 1924 LEBESGUE wrote to FRÉCHET in connection with the following matter. A certain American, B. Z. LINFIELD, had come to LEBESGUE to see about getting a French doctorate. He already had a doctorate from Harvard University. (It was taken under GEORGE D. BIRKHOFF.) He showed his Harvard thesis to LEBESGUE and asked if its contents 'convenablement complété' might be submitted for a French thesis. LEBESGUE was seeking some guidance from FRÉCHET because, he said, he was not accustomed to axiomatic considerations and didn't feel able to judge the originality and depth of LINFIELD'S work. He had consulted BOREL; both of them thought (of FRÉCHET) "que vous seul en France pourriez lui addresser un avis éclaire." In the next letter (April 28) LEBESGUE wrote: "C'est vous seul qui êtes juge; je ne m'occupe *nullement* de la thèse de M. Linfield. Mon rôle, purement consultatif, a été de décider l'homme le plus capable d'amener M. Linfield à faire une bonne thèse. Mon rôle est donc terminé. A vous de juger si le travail de M. Linfield constitute une thèse ou une base de thèse. Plus tard vous déciderez s'il faut qu'il la fasse à Paris ou à Strasbourg. ..." There followed some discussion about the options of a Doctorat de l'Université or a Doctorat d'Etat. LEBESGUE said he felt that the Doctorat de l'Université had been somewhat depreciated, but then said "... je suis donc loin de déprécier la titre." He believed that LINFIELD wanted nothing but a Doctorat de l'Université. Then he said "Quant à son travail, il a déjà servi à Harvard, il ne peut servir indéfiniment; aussi j'estime que dans tous les cas il doit être poursuivi pour faire une thèse quelconque. Ceci est d'autant plus nécessaire, que nous ne trouvons comment ce travail a été jugé à Harvard." The upshot was that LEBESGUE encouraged FRÉCHET to help LINFIELD by giving him some ideas for research at Strasbourg.

A year later, on May 25, 1925, LEBESGUE wrote again about LINFIELD. He said that if FRÉCHET was of a mind to "lui faire sortir quelque chose d'acceptable, c'est fort bien; je ne suis pas étonné d'apprendre que ça a été dur et que le résultat n'est pas extraordinaire. Mais c'est déjà très très beau; félicitations." "He advised FRÉCHET to let LINFIELD pass his thesis 'tranquillement." But he added some words about conducting the matter in a manner that would not encourage globe-trotting degree-seekers and that will not give Americans reason to think that French degrees are inferior to theirs.

LINFIELD did complete a thesis at Strasbourg; it was presented to the University July 30, 1925. The thesis and its title (it was about discrete spaces) are cited on page 285 in the bibliography of [FRÉCHET 132].

The years 1923 and 1924 were exceedingly busy ones for FRÉCHET. An unusually large number of his papers were published in 1924, and in the summer of that year he attended the International Congress of mathematicians in Toronto, Canada. Four of his papers [FRÉCHET 97, 98, 99, 100] were published in the proceedings of that Congress. In 1924, also, was published the expository paper [FRÉCHET 106]. (It is reprinted on pages 52–88 in FRÉCHET's book Les Mathématiques et le Concret, Presses Universitaires de France, Paris, 1955.) The writing of this article was solicited on behalf of XAVIER LÉON, Director of the Revue de Métaphysique et de Morale, by MAXIMILIEN WINTER. In his letter of June 4, 1923, WINTER flatteringly opened by recalling that POINCARÉ had contributed an article to the Revue every year for twenty years and that M. LÉON had asked him to solicit from FRÉCHET "un exposé d'ensemble sur les travaux récents de calcul fonctionnel (concernant notamment vos propres travaux, la 'general analysis' de Moore, les conceptions de Wiener et–s'il y a lieu–la conception de l'Ecole polonaise)." He said that FRÉCHET could in this way render a notable service to the scientific and philosophical public.

This circumstance illustrates the fact that FRÉCHET was already a quite visible figure in the French intellectual world. The article was finished and submitted in November of 1924. The correspondence indicates that FRÉCHET was paid ten francs a page for the article.

As I conclude this overview of FRÉCHET's career prior to his move from Strasbourg to Paris, it is important to be clear about the fact that FRÉCHET was not moving *totally* away from his previous mathematical interests. He continued to teach a wide assortment of courses, not just probability and statistics. And he continued his interest in abstraction and generality, bringing that interest to bear in his work on probability. But he never again did anything in topology or general analysis to make as fundamental an impact as what he had done earlier.

3. Fréchet and abstract point set theory, 1909–1913

FRÉCHET'S publications in the years 1909–1913 deal much more with functionals and differentials than with general topology, but there are several on the latter subject: paper No. 30 in 1909, No. 38 and No. 39 in 1910, and No. 48 in 1913. The first two of these four papers are concerned with FRÉCHET's initiation of what he calls *type de dimension* of a set in an abstract class with a topology. This subject is also treated in part of paper No. 39. Because FRÉCHET'S work relating to dimensionality has been examined and discussed at length in a paper [ARBOLEDA 3] published in 1981, I shall spend little time on this part of FRÉCHET'S work.

FRÉCHET considers a set G_1 in an L-class and a set G_2 in another or the same L-class. He follows terminology of HADAMARD in defining such a pair of sets as being homeomorphic if there is a one-to-one correspondence between G_1 and G_2 that is continuous in both directions (with continuity defined by means of sequential limits). Then he defines the dimensionl type of G_1 as being less than or equal to that of G_2, and indicates this by writing $dG_1 \leqq dG_2$, if G_1 is homeomorphic to some subset S of G_2 (S may possibly be *all* of G_2). If $dG_1 \leqq dG_2$ and $dG_2 \leqq dG_1$, FRÉCHET says that G_1 and G_2 are of the same dimensional type, and indicates this by writing $dG_1 = dG_2$. If $dG_1 \leqq dG_2$ and if $dG_2 \leqq dG_1$ is false, FRÉCHET writes $dG_1 < dG_2$. I emphasize that FRÉCHET does not actually assign a numerical value to the symbol dG_1 itself, even though the title of paper No. 30 is '*Une définition de nombre de dimensions d'un ensemble abstrait.*' Nevertheless, FRÉCHET does in certain situations treat the symbol dG_1 as if it were a nonnegative real number (not necessarily an integer) or the symbol $+\infty$. He assumes that $dR_1 = 1$, where R_1 is the set of all real numbers with the ordinary topology.

The initial impetus for this work of FRÉCHET on dimensional type seems to have come from his correspondence with RENÉ BAIRE in 1909, and perhaps also from FRÉCHET's reading of a paper by BAIRE. See pp. 348–350 in [ARBOLEDA 3]. In the Archives there are three communications from BAIRE to FRÉCHET in 1909 and two in 1911. They deal in part with the state of BAIRE's health and in part with the fact that BAIRE had been attempting to show generally that it is impossible to establish a homeomorphism between a domain in R_n and a domain in R_{n+p} if $p \geqq 1$ (where R_k is the Cartesian space of points $(x_1, \ldots, x_k)$). This question had not been settled at that time. BAIRE thought he had a proof in 1907, but his effort was flawed. BROUWER settled the issue in 1911. See [HUREWICZ & WALLMAN], page 5. See also the paper [DUGAC] pp. 335–336. For comments on the relationship between dimensional type and dimension in the sense of MENGER and URYSOHN, which is always an integer, see [HUREWICZ & WALLMAN], page 66.

[FRÉCHET 39] is mainly a sort of addendum to his dissertation. Nearly all the discourse is about E-classes (*i.e.* metric spaces) which, in FRÉCHET's terminology, "admit a generalization of the theorem of Cauchy on convergence." In modern parlance these are complete metric spaces, and for brevity I shall use this terminology in stating the results of FRÉCHET. His first theorem is that in a complete metric space a set G is compact if and only if, for each $\varepsilon > 0$, a subset S of G for which the *écart* (distance) between each two elements of S is greater than ε is necessarily a finite set. FRÉCHET also proves that if G is a compact set in a complete metric space, G contains a denumerable subset D such that $G \subset D \cup D'$. He also proves that in any metric space the derived set of a compact set is compact.

In another section of this paper FRÉCHET deals again with his generalization of the CANTOR-BENDIXSON theorem. (See the last complete paragraph on page 257 of Essay I.) This time he gives a proof that makes use of transfinite numbers. But, he says: "Nevertheless, the recent expositions of the theory of transfinite numbers have disengaged the theory from the metaphysical considerations that obscure it, and therefore it can only be advantageous to introduce it (that is, the theory) where it genuinely gives new precision." In this connection he cites the exposition of the theory in BAIRE's book of 1905, [BAIRE].

In the next part of this paper FRÉCHET's purpose is to show that various of the "concrete" E-classes of functions that he introduced in the second part of his thesis are of infinite dimensional type. What he does (on page 11) is to show that, if F is one of those E-classes composed of functions (for example, the class of real functions f continuous on [0, 1], with the distance between f and g equal to the maximum value of $|f(t) - g(t)|$ for t on the interval), then $dR_n \leqq dF$ for every n. After establishing this comparison of dimensional types FRÉCHET makes the statement: "De sorte que l'on peut bien dire maintenant que F est d'une type de dimension infini, que F est une classe à une infinité de dimensions." FRÉCHET speaks of R_n as a space of n dimensions, but he does not actually write $dR_n = n$.

One of the more interesting things in this paper No. 39 is FRÉCHET'S introduction of what he designates as "l'espace D". It is denoted by l^∞ in modern literature. It has also been denoted by (m). The points of D are bounded infinite sequences $\{x_n\}$ ($n = 1, 2, \ldots$) of real numbers; the distance between $\{x_n\}$ and $\{y_n\}$ is the supremum of the values $|x_n - y_n|$ as n varies. The space D is complete but not separable. (It should be noted that FRÉCHET'S definition of a separable class at that time, as in his thesis, meant that the entire class is the derived set of a denumerable set. He shifted to the modern definition in 1921, requiring that the class be the union of a denumerable set and its derived set; see [FRÉCHET 75], page 341.) FRÉCHET showed that D has the following special property. Any normal E-class, that is, any complete and separable metric space, can be imbedded isometrically in D. The method of imbedding is very simple. Suppose the normal E-class is the derived set of the sequence $A_0, A_1, A_2, \ldots$ of elements. If A is any element of the E-class, let the corresponding element $\{x_n\}$ of D be defined by

$$x_n = (A_n, A) - (A_n, A_0); n = 1, 2, \ldots,$$

where (A_n, A) is the distance between A_n and A. Then, if $\{y_n\}$ corresponds in this same way to the element B, so that

$$y_n = (A_n, B) - (A_n, A_0),$$

it is easy to show that the distance (A, B) is equal to the distance between $\{x_n\}$ and $\{y_n\}$, thus showing that the imbedding is isometric. This shows that $dF \leqq dD$ if F is any normal E-class. But, since F is separable and D is not, we cannot have $dD \leqq dF$. Therefore, $dF < dD$.

This work of FRÉCHET inspired URYSOHN, years later, to search for what he called a universal separable metric space. I will come back to this matter later, in Section 9.

Toward the end of the paper (No. 39) FRÉCHET indulges himself in some reflections about the status of his L-classes and V-classes, as compared with the status of his E-classes. In a paper [HAHN] published in 1908, it was shown that a theorem in FRÉCHET'S thesis, proved only for E-classes, was in fact true for V-classes, as FRÉCHET has conjectured (see page 254 in Essay I). Now, in this paper of 1910, FRÉCHET asserts that HAHN'S achievement confirmed his belief that there is no real difference between V-classes and E-classes. A little later on he observed that HAHN had demonstrated two things: (1) There exists an L-class in which the only continuous functionals are those which are constant in value, and (2) on a V-class there does always exist a non-constant continuous functional. FRÉCHET then comments (on page 23) that perhaps this second result could be used to prove the identity of a V-class with an E-class, by constructing an *écart* for the V-class which yields the same limit elements and derived sets as the already existent *voisinage*. It was, in fact, by this sort of use of HAHN'S work that it was proved in [CHITTENDEN 2] that a V-class can be regarded as a E-class.

Finally, FRÉCHET remarks as follows: "The theorem of Hahn, previously mentioned, seems indeed to confirm that for applications to the functional calculus it is better to abandon the too general consideration of L-classes and limit consideration to V-classes or even to E-classes. However, I do not believe it useless to study L-classes or even more general classes such as those considered recently by Riesz." (Here he makes reference to the paper [RIESZ 3] that was delivered at the Fourth International Congress in Rome. See pages 267–270 in Essay I.)

FRÉCHET'S desire to deal with extremely general situations seems to have been a characteristic of him throughout his life. It shows up in much of his published work, including his work on probability and his ventures back into general analysis during his later life. Professor LOÉVE once said to me that in certain ways he always found FRÉCHET surprising and cited to me cases in which he had taken some of the fruits of his research to FRÉCHET (during the Paris years), whereupon FRÉCHET, after looking it over, would say: "Well now, let's see, how can that be generalized?"

FRÉCHET'S paper No. 48 is a consequence of a paper published in 1911 by EARLE R. HEDRICK, and I need to comment on that paper before discussing FRÉCHET'S reaction to it. HEDRICK, an American almost two years older than FRÉCHET, received his Ph. D. in Göttingen early in 1901 and then spent a number of months in Paris at the Ecole Normale Supérieure (which FRÉCHET had entered in 1900). Whether HEDRICK and FRÉCHET met at that time I do not know. By 1911 HEDRICK was a full professor at the University of Missouri. His paper [HEDRICK], is about L-classes that satisfy certain additional conditions. It is clear that HEDRICK'S work was suggested by his having read FRÉCHET'S thesis. Some of the results in HEDRICK'S paper were presented to the American Mathematical Society in 1909, but it is indicated in the paper that he was acquainted with FRÉCHET'S paper No. 39.

It is worthy of note that HEDRICK and T. H. HILDEBRANDT were the first American mathematicians whose published researches were motivated by FRÉCHET'S thesis. Also, in his paper HEDRICK became the first mathematician to prove a 'BOREL covering theorem' in an L-class. (For a comment about this see page 406 in the paper [HILDEBRANDT 2].)

HEDRICK deals throughout with an L-class in which an additional axiom holds: *Each derived set is closed.* Before stating HEDRICK'S version of a BOREL covering theorem I give the following definitions and terminology for convenience of exposition: An element p is interior to a set G if it is in G and is not a limit element of the set complementary to G in the L-class. A family $\mathscr{M}$ of sets is a covering of a given set G if every element of G is interior to some member M of the family $\mathscr{M}$. HEDRICK'S 'BOREL theorem' is: If G is a closed and compact set and $\mathscr{M}$ is a denumerable family of sets that is a covering of G, then there is some finite collection of members of $\mathscr{M}$ which is also a covering of G. This is the only result from HEDRICK'S paper that I shall describe fully. There is a good deal more to the paper. For some of his later results he assumes as well that the L-class is compact, and he imposes a rather intricate condition which he calls 'the enclosable property'.

In the Archives is a letter, dated July 31, 1911, written by HEDRICK, who was then in Göttingen, to FRÉCHET. He said he was sending FRÉCHET a copy of his recent paper in the Transactions of the American Mathematical Society. He said "it follows closely your thesis". Another letter in the Archives, from HEDRICK to FRÉCHET, written on December 26 of the same year, from Missouri, is an answer to a letter from FRÉCHET which HEDRICK describes as of date December 10. Evidently FRÉCHET had told HEDRICK that he should have made explicit an assumption that his L-class was perfect, for he thought that assumption was needed at a certain place in an argument. In his reply HEDRICK refuted this assertion by giving an explanation. He then went on to say that he found it remarkable that, as FRÉCHET had asserted, his (HEDRICK'S) assumptions had the consequence that this L-class was in fact a V-class. He asked FRÉCHET to communicate the proof to him. The upshot of this correspondence was that FRÉCHET sent HEDRICK a detailed letter, an extract from which became FRÉCHET'S paper No. 48, as is noted in a footnote on the first page of the paper. It would appear that FRÉCHET must have prepared the requested proof as part of a detailed commentary on HEDRICK'S paper before he could have received HEDRICK'S letter of December 31, for the date January 3, 1912 appears at the end of FRÉCHET'S paper as published.

The essence of FRÉCHET'S paper is that, with axioms on an L-class very similar to, but weaker than those of HEDRICK, he is able to prove that the L-class is, in fact, a normal V-class. Consequently, HEDRICK'S special sort of L-class, buttressed by the extra axioms he imposes, is a normal V-class. Because of this, FRÉCHET asserts, some of HEDRICK'S results are not really new, having been already proved by FRÉCHET in his thesis. But he recognizes the fact that the theorems obtained by HEDRICK (including the BOREL theorem) without use of the enclosable condition are "essentially new and constitute an important generalization." FRÉCHET was sufficiently impressed by HEDRICK'S work, and especially by HEDRICK'S use of the axiom that all derived sets are closed, to cause him to give the name "une classe (H)" to a certain kind of topological space in his paper No. 75. Another consequence of the exchange between HEDRICK and FRÉCHET was the new attention that FRÉCHET would be devoting to BOREL and BOREL-LEBESGUE covering theorems in the years ahead.

It is appropriate to make brief mention here of the paper [HILDEBRANDT 1] based on HILDEBRANDT'S doctoral dissertation at the University of Chicago. In its original form this paper was submitted to the American Journal of Mathematics in April, 1910. Some material was added subsequently and some changes were made as a consequence of the publication by FRÉCHET of the addendum to his thesis, paper No. 36. The general goal of HILDEBRANDT, apparently, was to investigate the assumptions and results in FRÉCHET'S work in a meticulous and methodical way, breaking the assumptions down into various parts and showing that, to some extent, a number of FRÉCHET'S results can be obtained without use of *all* his assumptions. For instance, HILDEBRANDT showed that in a number of cases it was not necessary to assume uniqueness of the limit of a sequence in an L-class. As another example, HILDEBRANDT pointed out that FRÉCHET'S version (in his thesis) of the BOREL-LEBESGUE theorem (which HILDEBRANDT, following American practice, called the HEINE-BOREL theorem) and its converse, which FRÉCHET stated for a normal V-class, could be proved without normality (*i.e.* without separability or the use of the CAUCHY convergence principle). See FRÉCHET'S footnote about this on page 320 of his paper No. 48.

4. Neighborhoods in abstract general topology before 1917

Perhaps the first occurrence of the notion of neighborhoods in the context of an entirely axiomatic set theory is in the work of HILBERT. On two occasions in 1902 he used the neighborhood notion in discussing the foundations of geometry. See [HILBERT 1], [HILBERT 2]. The paper [HILBERT 2] and a footnote in [HILBERT 1] are included as Appendix IV in the second edition [HILBERT 3] of HILBERT's book on the Foundations of Geometry. In this appendix a plane is, for HILBERT, a collection of objects called points; with each point is associated a family of subsets of this plane, called neighborhoods of the given point. There are six axioms, two of which relate the "abstract plane" to the "number plane" of coordinate point-pairs (x, y):

(1) A point belongs to each of its neighborhoods.
(2) If B is a point in a neighborhood U of the point A, then U is also a neighborhood of B.
(3) If U and V are neighborhoods of A, there is another neighborhood of A that is contained in both U and V.
(4) If A and B are any two points, there is a neighborhood of A that contains B.
(5) For each neighborhood there is at least one mapping of its points, one-to-one onto the points (x, y) of some JORDAN region (the interior of a simple closed curve) in the number plane.
(6) Given a point A, a neighborhood U of A, and a JORDAN region G that is the image of U, then any JORDAN region H that lies in G and contains the image of A is also the image of some neighborhood of A. If a neighborhood of A has two different JORDAN regions as images, the resulting induced one-to-one correspondence between these images is bicontinuous.

As can be seen, the first four of these axioms are abstract. HILBERT's axiom system was not designed for the purpose of pursuing general point set topology in the abstract. Rather, HILBERT was intent upon founding plane geometry (either EUCLIDean or that of BOLYAI and LOBATCHEFSKY) solely on the foregoing axioms together with a group of three axioms about a group of continuous one-to-one transformations of points in the number plane. HILBERT was treating a problem that had been considered by SOPHUS LIE; but, unlike LIE, he was avoiding any assumption about differentiability of the transformations. However, this work of HILBERT was perceived by HERMANN WEYL as "one of the earliest documents of set-theoretic topology." See page 638 in [WEYL 2], or alternatively, [REID], page 267. (In the book by REID, WEYL's paper on HILBERT is reproduced in a shortened version.) Also, OTTO BLUMENTHAL, writing about HILBERT in an article in the Collected Works of HILBERT, refers to the paper [HILBERT 2] as being significant because, among other reasons, "it contains the first decisive application of the methods of Mengenlehre."[4]

To what extent HILBERT's use of the concept of neighborhoods influenced

[4] BLUMENTHAL's exact words, on page 40 of [HILBERT 4], are the following: Diese Untersuchung ist auch dadurch bedeutsam, dass in ihr zum ersten Male die Methoden der Punktmengenlehre entscheidend verwandt wurden.

the subsequent development of abstract general topology by means of axioms about neighborhoods is, I think, likely to remain speculative unless more firm evidence is found.

In [RIESZ 1] there is a reference to the idea of neighborhoods and the desirability of getting away from the notion of distance. (See page 267, including the footnote, in Essay I.) However, RIESZ did not formulate axioms about neighborhoods. As was noted in Section 1, RIESZ defined the notion of neighborhood of an element in his paper [RIESZ 2]. However, in the often quoted paper [RIESZ 3], read at the International Congress in Rome in 1908, RIESZ gives very little discussion of the consequences of his axioms about derived sets; in this paper he does not even define the notion of neighborhood. It may be observed, however, that RIESZ does refer explicitly to HILBERT'S writings on the foundations of geometry.

Before beginning a discussion of the definitively important formulation of axioms on neighborhoods by HAUSDORFF, it is necessary to consider the prior work of RALPH E. ROOT, who was one of E. H. MOORE'S doctoral candidates at the University of Chicago. He wrote two papers in which there were axioms about neighborhoods. Both were published in 1914, but [ROOT 2] was submitted for publication in 1912, the work having been completed in 1911 and announced in [ROOT 1], while another, [ROOT 3], was submitted in 1913. There is no evidence as to whether ROOT'S ideas were influenced by HILBERT'S writings.

I quote as follows a general statement by ROOT in [ROOT 2]: "The paper has its origin in the thought that in most of the definitions of limit that are employed in current mathematics a notion analogous to that of 'neighborhood' or 'vicinity' of an element is fundamental. In the domain of general analysis various ways of determining a neighborhood have been employed, notably the notion of *voisinage* used by M. Fréchet and the relations K_1 and K_2 used by E. H. Moore ..."

It is not the main purpose of any of the papers of ROOT to develop a systematic theory of abstract general topology. He is concerned with the construction of a general theory for the discussion of limits and iterated limits of functions defined on an abstract range, where the range may be composite, that is, composed of pairs of elements, each from a general range. However, in the course of his work in each of the two principal papers [ROOT 2], [ROOT 3], he does, in fact, construct structures which can be regarded as abstract topological spaces of a fairly general type. The method in each case is to lay down a set of axioms about special families of sets which are to be thought of as neighborhoods of certain elements. Instead of describing the main results toward which ROOT is working. I shall simply describe some of his axiom systems and the way in which the resulting general topology relates to the work of RIESZ and FRÉCHET. The paper [ROOT 2] is written with extensive use of logical notation; this makes the reading of it somewhat heavy work. ROOT considers what he calls 'actual elements' and also 'ideal elements,' but it is possible to interpret his work for the special case in which the class of ideal elements is empty. This is what I shall do in what I present here.

In one part of the paper, then, we have a general class with elements p, q, ..., and for each p a family of sets from the class, called neighborhoods of p. There are four axioms:

1. Each neighborhood of p contains p.

2. To each p corresponds a denumerable family of neighborhoods of p, say $P_1, P_2, P_3, \ldots$, such that if R is any neighborhood of p, P_n is a subset of R for all sufficiently large values of n.
3. If P is a neighborhood of p, there exists another neighborhood S of p, such that each element q in S has a neighborhood Q that is a subset of P.
4. If p and q are distinct elements, there exist neighborhoods P, Q, of p and q respectively, such that P and Q have no elements in common.

ROOT defines an element p to be a limit element of a set E provided that each neighborhood of p contains an element of E that is distinct from p. For a sequence $\{p_n\}$ of elements and an element q, he defines $\lim p_n = q$ to mean that each neighborhood of q contains p_n if n exceeds some N that depends on the particular neighborhood. He then shows that, with this definition of sequences that have limits, his basic class becomes an L-class of FRÉCHET; that an element q is a limit element of a set E if and only if there exists a sequence $\{p_n\}$ of distinct elements of E such that $\lim p_n = q$; and also that, in this general context, each derived set is closed. None of these conclusions requires the use of axiom 3.

ROOT also shows that the derived sets resulting from his axioms and definition of limit elements satisfy the four requirements placed on limit elements by RIESZ in his address to the International Congress in Rome in 1908 (on pages 19, 20 in [RIESZ 3]. These requirements are the same as the ones listed in Section 1 of the present essay, except for the fourth one, which is reformulated in [RIESZ 3] in the following way:

4′: Each limit element of a set E is uniquely determined by the totality of the subsets of E of which it is a limit element.

In the other long paper [ROOT 3] ROOT introduces neighborhoods in an abstract class of a special sort – one in which there is an undefined notion of one element being between two others. The set of all elements between two given elements is called a segment if it is not an empty set. A segment is then regarded as a neighborhood of each of its elements. ROOT then imposes three conditions on the neighborhoods:

I. Each element p of the basic class belongs to some segment (which is a neighborhood of p).
II. Given two neighborhoods P, Q of an element p, there is a neighborhood R of p such that R is a subset of both P and Q.
III. If p and q are distinct elements, there exist neighborhoods P and Q of p and q, respectively, such that P and Q have no common element.

ROOT then defines limit elements of a set just as in the other paper, and shows that the four axioms of RIESZ are satisfied. Moreover, each derived set is closed. ROOT defines the meaning of $\lim p_n = q$ just as before, and observes that this notion of the limit of a sequence satisfies FRÉCHET's requirements for an L-class. Moreover, if the notion of limit element of a set in this L-class is defined as was done by FRÉCHET, then it is true that each of the resulting derived sets is closed. However, as ROOT observes, it is not necessarily the case that a limit element of a set E (as defined by ROOT, using neighborhoods) is the limit of a sequence of distinct elements of E. Examples to show this are given on pages 68–69 of [ROOT 3].

I turn now to the work of HAUSDORFF. FELIX HAUSDORFF was born in Breslau on November 8, 1868. Thus he was about ten years older than FRÉCHET. He attained his doctorate at Leipzig in 1891. He taught at Leipzig and Bonn before being appointed as Professor at Greifswald in 1913 and then at Bonn in 1921. His earliest scientific work was in the physics of light, but he turned to pure mathematics soon after 1900. He was not primarily a topologist, but his book [HAUSDORFF] established him as a major figure in the development of abstract general topology during a formative period. More precisely, it was Chapters 7 and 8 in the book, and Chapter 7 especially, that exerted strong influence on general topology. There were ten chapters in all. The chapters prior to the seventh are not concerned with topology, but with the algebra of sets, with "power" or cardinal number, and with ordering, well-ordering, ordinal number, and transfinite induction.

Chapter 7 is entitled 'Point sets in general spaces.' It is in this chapter that the theory is developed from axioms about neighborhoods. The general theory continues in Chapter 8, which is entitled 'Point sets in special spaces.' Further axioms are imposed (the so-called first and second countability axioms), and then attention is largely restricted to metric spaces and finally to Euclidean spaces. On pages 456–457, in the notes on Chapter 7, HAUSDORFF writes that the principal features of his theory based on neighborhoods were presented in his lectures at the University in Bonn in the summer semester of 1912.[5] While I was searching for further information about HAUSDORFF and hoping to find clues that would lead me to a better insight into the origins of HAUSDORFF's ideas about neighborhoods, I read some *in memoriam* articles about HAUSDORFF in the Jahresbericht der Deutschen Mathematiker-Vereinigung, volume 69, 1967; see [DIERKESMANN], [LORENTZ], and [BERGMANN]. HAUSDORFF and his wife committed suicide together in January, 1942. Some of his papers were kept in the home of a friend, but they were buried in rubble when the house was bombed in 1945. This friend found them still in place in 1946, though badly disarranged and with some things probably lost. The Wissenschaftlicher Nachlass, as the surviving documents are designated, are at the University in Bonn. In the University of California Library at Berkeley I found two published volumes on HAUSDORFF's Nachgelassene Schriften. From these clues I gained hope that I could learn in some detail the contents of HAUSDORFF's lectures at Bonn in 1912. Through the kind assistance of Professor GÜNTER BERGMANN of the University of Münster, I received a photocopy of his extract from what I was seeking. The extract, in Professor BERGMANN's handwriting, is under the heading Einführung in die Mengenlehre, gelesen zweistündig in Bonn a. Rh. S. S. 1912. The heading is followed by this sentence: Die Vorlesungen konnte in den Jahren 1965–68 vom Bearbeiter dieses Auszuges, G. Bergmann, restituiert werden und gehört zum sogenannten "Wissenschaftlichen Nachlass" Felix Hausdorffs.

The portion of the lectures that is relevant to my present discussion is the following, which, according to the agreement I was required to make in order to receive the material, I report precisely word-for-word, and with the exact same symbolism:

[5] His exact words: Die Grundzüge der hier entwickelten Umgebungstheorie habe ich im Sommersemester 1912 in einer Vorlesung über Mengenlehre an der Universität Bonn vorgetragen.

Punktmengen

§ 6. Umgebungen.

Punktmengen auf einer Geraden (lineare), in der Ebene (ebene), im Raume (räumliche), allgemein in einem n-dimensionalen Raume $E = E_n$. Ein Punkt x ist durch ein System von n reellen Zahlen $(x_1, x_2, \ldots, x_n)$ und umgekehrt definiert, die wir als rechtwinklige Coordinaten deuten. Als *Entfernung* zweier Punkte definieren wir

$$xy = \sqrt{(x_1 - y_1)^2 + (x_2 - y_2)^2 + \ldots + (x_n - y_n)^2} \geqq 0.$$

Unter eine *Umgebung* U_x des Punktes x verstehen wir den Inbegriff aller Punkte y, für die $xy < \varrho$ (ϱ eine positive Zahl; Inneres einer "Kugel" mit Radius ϱ).

Wir werden zur Veranschaulichung in der Regel die Ebene $E = E_2$ nehmen; sollten die einzelnen Dimensionszahlen Abweichungen hervorrufen, so werden sie besonders hervorgehoben werden.

Die Umgebungen haben folgende Eigenschaften:
(α) Jedes U_x enthält x und ist in E enthalten.
(β) Für zwei Umgebungen desselben Punktes ist $U_x \subseteqq U'_x$ oder $U_x \supseteqq U'_x$.
(γ) Liegt y in U_x, so giebt es auch eine Umgebung U_y, die in U_x enthalten ist ($U_y \subseteqq U_x$).
(δ) Ist $x \neq y$, so giebt es zwei Umgebungen U_x, U_y ohne gemeinsamen Punkt ($\vartheta(U_x, U_y) = 0$).
Die folgenden Betrachtungen stützen sich zunächst nur auf diese Eigenschaften. Sie gelten daher allgemein, wenn E eine Punktmenge $\{x\}$ ist deren Punkten xU_x zugeordnet sind mit diesen 4 Eigenschaften.

Here ends my quotation from Professor BERGMANN'S transcription of material from the lectures at Bonn in 1912. This foregoing material is to be compared with the material to be found on pages 212–213 in HAUSDORFF'S book, published in 1914. Before making the comparison, however, let us observe that HAUSDORFF'S neighborhoods of the point x in 1912 were, quite explicitly, interiors of spheres centered at x. The space under consideration was the n-dimensional Euclidean space of points with n Cartesian coordinates. There was no talk about abstract classes in the defining of neighborhoods and the listing of the four properties. However, HAUSDORFF'S concluding words may be translated as follows: "The following considerations depend only on these properties. They are valid, therefore, when E is a point set to whose points x correspond sets U_x having the four properties listed." This is an indication that the four properties are going to play the role of axioms, and no explicit use is to be made of the nature of the points and the neighborhoods beyond what can be derived by use of the properties.

I turn now to Chapter 7 of HAUSDORFF'S book. HAUSDORFF begins with general remarks about the success of Mengenlehre in clarifying and sharpening the fundamental principles of geometry by its applications to point set theory. He makes some general comments about alternative ways of laying the foundations of point set theory. He speaks of using distance to define the notion of convergent sequences and their limits, or of using distances to define neighborhoods and then building up the whole theory from neighborhoods. Then he cites the usefulness of avoiding a redundancy (he uses the word Pleonasmus) of expositions of theory by setting up a general theory (based on just a few simple axioms) that will encompass, not merely point sets on the line or in the plane, but on RIEMANN surfaces or in space of a finite or infinite number of dimensions, including classes of curves and surfaces. He stresses that the generality gained is not at the expense of greater complications, but is actually accompanied, at least in the principal features (Grundzüge) of the theory, by simplification and protection against errors of reasoning caused by faulty intuition. Next he says that the choice between using distance, sequential limits, or neighborhoods as basic notions is to some extent a matter of taste. He opts for neighborhoods as being more general than the use of distance, and as being preferable to sequential limits, which bring in denumerability, whereas neighborhoods do not. However, he says, in order to provide the reader with a concrete picture, he will begin with the special neighborhoods defined by means of distance.[6]

HAUSDORFF then proceeds to define a metric space as a class of elements (points) with distance between x and y denoted by $\overline{xy}$ and subjected to three axioms: (1) $\overline{yx} = \overline{xy}$, (2) $\overline{xy} = 0$ if and only if $x = y$, (3) $\overline{xy} + \overline{yz} \geqq \overline{xz}$. The neighborhoods of x in a metric space are defined to be spheres with the center x and without the "surface;" that is, sets of points y such that $\overline{xy} < \varrho$, where ϱ can be any positive number. HAUSDORFF next states that spherical neighborhoods have properties of which only a few will be used. He indicates that, in accordance with his decision to make neighborhoods fundamental, he will abstain from using distance and will make use solely of certain properties of neighborhoods, thus treating the properties as axioms.[7]

Finally, on page 213, HAUSDORFF comes to his definition of what he calls a topological space – a class of elements (points) to each of which correspond certain sets from the class, called neighborhoods. There are four axioms:

(A) To each point corresponds at least one neighborhood U_x, and U_x contains x.
(B) If U_x and V_x are neighborhoods of x, there exists another neighborhood of x, W_x, which is a subset of U_x and of V_x.
(C) If y is in U_x, there is a neighborhood U_y of y such that U_y is a subset of U_x.
(D) For two distinct points x, y there exist two neighborhoods U_x and U_y with no point in common.

It is immediately evident that axioms (A), (C), (D) of the book are the same as axioms (α), (γ,) (δ), respectively, of 1912. But (B) is different from (β). In commenting to me about the comparison between the axioms of 1912 and those of

[6] Here are HAUSDORFF's exact words: "...; um aber dem Leser sogleich ein konkretes Bild zu erwecken, beginnen wir mit den speziellen Umgebungen, die durch Entfernung definiert sind."

[7] HAUSDORFF's words: Dabei ändern wir, wie vorhin angekündigt, unseren Standpunkt dahin, dass wir von den Entfernungen, mit deren Hilfe wir Umgebungen definiert haben, absehen und die genannten Eigenschaften demgemäss als Axiome an die Spitze stellen.

1914, Professor BERGMANN, when he wrote to me in 1979, said (I translate): "When seen as a whole, the foregoing axiom system of 1912 denotes a preliminary step toward the axiom system of 1914. Only in time yet to come (in 1912, 1913) did Hausdorff arrive at the formulation (B), although this might perhaps have occured during the holding of the lecture series. There are no memoranda about it." After looking through HAUSDORFF'S Nachlass, Professor BERGMANN was unable to give me any information bearing on possible relations between FRÉCHET and HAUSDORFF. There are no signs of any correspondence between them. Professor BERGMANN also said that he thought it was plausible that very few mathematicians considered opening a correspondence with HAUSDORFF, because he was unusually cautious in scientific matters and, although affable, was very critical in his reactions.

Among the documents I was shown by FRÉCHET'S daughter, Mme. HÉLÈNE LEDERER, was a very battered notebook in which FRÉCHET had made lists of his publications, notations about them, and had also entered other information. He numbered his papers according to a system of his own. There is a list of names and addresses, and FRÉCHET kept at least a partial record showing to whom he had sent reprints of which papers, with the papers identified by number. HAUSDORFF'S name is nowhere to be found in the notebook. It may well be that the notebook does not contain a complete and accurate record of all the matters with which it appears to deal. Nevertheless, it contains such names as BLUMENTHAL, HAHN, RADON, S. BERNSTEIN (in Kharkov), and ZERMELO, who are indicated as having been sent a copy of FRÉCHET'S published thesis, so the absence of HAUSDORFF'S name may be significant. FRÉCHET'S daughter told me she thought that her father never met or corresponded with HAUSDORFF. She was quite aware of a sensitivity of her father concerning the influence of HAUSDORFF'S book. She knew of this, if in no other way, because of her father's reaction to the credit given to HAUSDORFF in the BOURBAKI history of mathematics (see pp. 235–236 in Essay I). She said that her father had talked about the fortuitous consequences of one publication getting much more attention than another, with the implication that the journal in which his thesis was published made the thesis a far more obscure thing than HAUSDORFF'S book.

There is nothing I know of to indicate any specific inspiration or motivation for HAUSDORFF'S choice of the particular properties of spherical neighborhoods that he felt were appropriate ones to use as axioms. It seems plausible to me to suppose that, as he was preparing his lectures to be given in the summer semester of 1912, he scrutinized his arguments and realized that he was able to go quite far with nothing more than his four properties (α), (β), (γ), (δ).

On the broader question of the influence that might have led HAUSDORFF to choose to develop his point set topology on the basis of the neighborhood concept, I can only speculate. I think he probably was influenced by HILBERT and F. RIESZ. Careful and industrious scholar that he was, HAUSDORFF would surely have seen HILBERT'S work on the Foundations of Geometry and would, likewise, have seen the paper (RIESZ 3) that was read at the International Congress of Mathematicians in Rome in 1908. In that paper there are footnotes referring to work of HILBERT and RIESZ although not to [RIESZ 1]. This last paper was on a subject that lay close to HAUSDORFF'S particular interests (as evidenced by some of his publications on ordered sets and order types). It is highly likely that HAUSDORFF saw this paper. In it RIESZ stressed his view that one should get away from distance and use the notion of neighborhood (see my reference to this on page 267 of Essay I).

The notes that HAUSDORFF included in his book were not comprehensive enough to indicate the general source of his ideas; therefore I do not attach much significance to the lack of references to the foregoing works of HILBERT and RIESZ. He *does* refer to FRÉCHET occasionally, but not as often as if he were providing thorough scholarly documentation. For example, he does not give FRÉCHET credit for the notion of a metric space. There is a note on page 457 that cites the book [WEYL 1] (published in 1913); this is evidently tied to the reference to RIEMANN surfaces on page 211. WEYL's use of the neighborhood concept in connection with his discussion of RIEMANN surfaces probably owes something to HILBERT. What WEYL did evidently strengthened HAUSDORFF's claim of the cogency and utility of a treatment of topology with the use of axioms about neighborhoods, but WEYL's book was not the source of HAUSDORFF's motivation (which began in 1912 or even earlier). Whether HAUSDORFF was influenced by some knowledge of the content of WEYL's lectures at Göttingen in the winter semester of 1911–1912 (on which WEYL's book was based) is unknown to me.

5. Covering theorems and compact sets

In this section I discuss the work of FRÉCHET and others relating to the connection between compactness (in FRÉCHET's sense, of course) and covering theorems of BOREL and BOREL-LEBESGUE type. For economy of language it is convenient to lay down some definitions that will obviate the frequent repetition of certain phrases. A basic notion is that of limit element of a set. A set is closed if it contains all its limit elements. A point, or element, is interior to a set G if it is in G and not a limit point of the complement of G. A family $\mathscr{M}$ of sets M is called a covering of a given set G if each point of G is an interior element of some member M of $\mathscr{M}$. (I should mention here that in most modern treatments of coverings in the context of BOREL or BOREL-LEBESGUE theorems, *open* coverings are used, and by an open covering of G is meant a family $\mathscr{M}$ of sets M, all of which are open, such that each point of G is in some member M of $\mathscr{M}$. In this modern usage it is not necessary to specify that the point of G is an interior point of the set M, because the situations are such that all points of an open set M are interior points of M.) In FRÉCHET's work of the period here under consideration he did not use the concept of open sets. However, the following observations may be noted. If a set is defined to be open when its complement is closed, it is readily seen that any point of an open set is an interior point of the set. Moreover, if we are in a situation where the union of a set and its derived set is always closed, the set of all the interior points of a set form an open set.

Next, two more definitions. A set G is defined to have the BOREL[8] property if, whenever $\mathscr{M}$ is a denumerably infinite family forming a covering of G, there

[8] FRÉCHET himself introduced the notion of a set having the BOREL property, See Section XVIII, page 152 of [FRÉCHET 66].

is some finite subfamily of $\mathscr{M}$ that also forms a covering of G. A set G is said to have the BOREL-LEBESGUE property if, whenever $\mathscr{M}$ is a family of sets (which may possibly be nondenumerably infinite) forming a covering of G, there is some finite subfamily of $\mathscr{M}$ that is also a covering of G.

Evidently a set having the BOREL-LEBESGUE property also has the BOREL property, but in some situations a set may have the BOREL property but not the BOREL-LEBESGUE property.

We shall speak of a theorem as a BOREL theorem if it asserts that, for a topological space (or class) of a certain sort (*i.e.* subject to certain conditions), a set that is closed and compact has the BOREL property. For a space in which it is always true that the union of a set and its derived set is closed, we can state an alternative equivalent condition that a set have the BOREL property: A set G has the BOREL property if whenever $\mathscr{M}$ is a denumerable open covering of G, a certain finite subfamily of $\mathscr{M}$ is also an open covering of G. This follows from remarks made earlier about open sets. Similar remarks apply to open coverings and the BOREL-LEBESGUE property. HAUSDORFF (for example) stated his BOREL theorem in terms of open coverings. The topological spaces considered by HAUSDORFF have the property that $A \cup A'$ is always closed, for any set A. So do FRÉCHET'S H-classes.

The original BOREL theorem, proved by BOREL, was that a closed and bounded set on the real number line has the BOREL property, as here defined. It was then proved, by LEBESGUE and others, that such a set also has what is here called the BOREL-LEBESGUE property. Actually, the basic idea underlying the reduction, from an arbitrary infinite covering of a bounded and closed set (specifically, a finite closed interval) on the line, to a finite covering, had been used by HEINE in proving a theorem about continuous functions. It is for this reason that the name 'HEINE-BOREL theorem' is used by some writers; this is the common usage by writers in English. I adhere here to the French usage.

In his thesis (Section 42, page 26) FRÉCHET enunciated a theorem[9] which we can formulate as follows: In a normal V-class a set has the BOREL-LEBESGUE property if and only if it is closed and compact. As I remarked at the end of Section 3, HILDEBRANDT discovered that the assumption of normality is superfluous. HEDRICK'S theorem (1911) was that, in an L-class in which all derived sets are closed, each closed and compact set has the BOREL property. In the paper [ROOT 3] (see the discussion in Section 4) is the theorem that a closed and compact set has the BOREL property. The topology in this case is that based on ROOT'S axioms I, II, III. It need not be the topology of an L-class.

E. W. CHITTENDEN obtained an M.A. degree at the University of Missouri in 1910; he worked under the supervision of E. R. HEDRICK. He then obtained a Ph.D. in 1912, working under E. H. MOORE at the University of Chicago. CHITTENDEN wrote a number of papers that were closely related to the work of FRÉCHET on general topology. One of these papers was mentioned in Section 3. I mention another one of them [CHITTENDEN 1] here because it is so closely related to FRÉCHET'S result (to be discussed presently) on the converse of BOREL'S theorem. Apparently CHITTENDEN and FRÉCHET worked entirely independently of each other

[9] This theorem is cited on page 257 of Essay I, but there is a typographical error; the reference there should be to Section 42, not Section 26.

on this matter. Still other papers by CHITTENDEN will be mentioned later in connection with other work of FRÉCHET. For a general perspective on the role of CHITTENDEN in the development of abstract topology see [AULL]. For the text of an invited address (1926) by CHITTENDEN see [CHITTENDEN 6], in which is presented an historical overview of many of the things mentioned in the present essay, including more about the work of HEDRICK, CHITTENDEN, and URYSOHN (some of whose work is dealt with in Section 9 of this essay).

CHITTENDEN deals with what he calls a RIESZ domain, by which he means an abstract class whose topology is determined by the first three of the four axioms of RIESZ, as I have given them in Section 1 of the present essay. I will quote only one of CHITTENDEN'S results from the paper, and I will simplify matters by not giving the result in the full generality of CHITTENDEN'S presentation. (He deals with a notion of relativization that involves complications I wish to avoid. Therefore, I state a result about the entire RIESZ domain rather than about a particular set within it.) Here is the theorem: If a RIESZ domain has the BOREL property, it is compact. It may be noted that, although CHITTENDEN'S paper carries the phrase "converse of the HEINE-BOREL theorem" in the title, he makes use merely of denumerable coverings.

On page 231 in HAUSDORFF'S book we find theorems which can be stated as follows in the terminology I am using. BOREL theorem: A closed and compact set in a HAUSDORFF topological space has the BOREL property. Converse of BOREL-LEBESGUE theorem: If a set in a HAUSDORFF topological space has the BOREL-LEBESGUE property, it is closed and compact. Observe that these two theorems are not mutually converse.

For a metric space we *do* have mutual converseness in the theorem: A set has the BOREL-LEBESGUE property if and only if it is closed and compact. This is the FRÉCHET-HILDEBRANDT theorem, for metric spaces (*i.e.* E-classes).

In a note published in the Comptes Rendus of the Paris Academy of Sciences in 1916 [FRÉCHET 59], FRÉCHET asserts that the most general L-classes to which the theorem of BOREL is applicable are those L-classes having the property that every derived set is closed. What this means is that the proposition "Every closed and compact set has the Borel property" is a valid theorem in a particular L-class if and only if that L-class has the property that each of its derived sets is closed. The details of the argument for this are given, along with other results, in a paper published in 1917 [FRÉCHET 62]. In this paper FRÉCHET calls an L-class an S-class (une classe (S)) if it has the property that all its derived sets are closed. HEDRICK had proved the BOREL theorem for S-classes with the aid of the following result, called HEDRICK'S lemma by FRÉCHET: Suppose A is an interior element of a set G in an S-class, and let A be the limit of a sequence $\{A_n\}$ of elements of the class. Then all but at most a finite number of the A_n's are interior elements of G.

FRÉCHET also proves the following converse of the BOREL theorem, valid in *any* L-class. If G is a set in an L-class, and if G has the BOREL property, then G is closed and compact. This is different from CHITTENDEN'S converse of the BOREL theorem, because FRÉCHET is dealing with an L-class, whereas CHITTENDEN was dealing with a RIESZ domain. CHITTENDEN'S result was published before that of FRÉCHET. FRÉCHET does not mention the paper of CHITTENDEN in his own paper, but that is not surprising, in view of the war-time conditions affecting FRÉCHET.

At the end of this publication in 1917 FRÉCHET remarks that it would be interesting to know "what is the most general class for which one can state the BOREL-LEBESGUE theorem." The question thus raised by FRÉCHET was the starting point for investigations by a number of people, notably R. L. MOORE, CHITTENDEN, C. KURATOWSKI and W. SIERPINSKI (jointly), and P. ALEXANDROFF and P. URYSOHN (jointly). In the process there was an evolution of thinking about the concept of compactness, and the eventual introduction of the notion of bicompactness. Some of these developments will be discussed in Section 7.

It is appropriate to mention here one more result from HAUSDORFF'S book, dealing with compactness in FRÉCHET'S sense. On page 272 of the book, where the author is dealing with spaces that satisfy his four neighborhood axioms and also the second countability axiom (which requires that the topology be determined by a system of neighborhoods, the total number of which is countable, or denumerable), HAUSDORFF asserts the theorem which in our present terminology becomes: Each closed and compact set has the BOREL-LEBESGUE property. (HAUSDORFF calls it Satz von Borel, but in our present terminology it is a version of the BOREL-LEBESGUE theorem.)

6. Fréchet's new V-classes and his H-classes

In a paper [FRÉCHET 65] that was published in the issue for January 1918 of the Transactions of the American Mathematical Society, FRÉCHET took his first steps toward basing a topology on sets called neighborhoods. In the paper FRÉCHET announced as his objective to find what supplementary conditions must be imposed on an L-class to make it possible to define in the L-class a distance between pairs of elements in such a way that the convergence as given at the outset in the L-class will be the same as the convergence determined by the use of the distance that has been introduced. In other words, to use a terminology not then in vogue, but which became standard at a later time, under what conditions on an L-class it is metrizable? FRÉCHET made no significant progress in attempting to answer this question. At the time it was perhaps of some value to pose the problem as clearly as he did. Of greater significance was FRÉCHET'S fresh start on the approach to the formulation of a topology. In this particular paper he said he would call an L-class a V-class (in a sense wholly different from the notion of a V-class as defined in his thesis) if to each element A there corresponds a sequence $\{U_n(A)\}$ of sets such that a sequence $\{A_q\}$ of elements has the limit A if and only if for each q there is some N (depending on q) such that A_n is in $U_q(A)$ when $N < n$. It follows from this requirement and the axioms for L-classes that A is the unique element that is a member of all the $U_q(a)$'s. FRÉCHET calls these sets neighborhoods (voisinages) of A.

In this paper, also, he introduced other changes in his previous nomenclature. What he had called an E-class (une classe (E)) in his thesis, he said he would henceforth call a D-class (une classe (D)). Also, what he had previously called an *écart*, he would henceforth call a *distance*.

From letters in the Archives some dates can be established in relation to this paper. D. R. CURTISS wrote FRÉCHET on March 24, 1917 from Evanston, Illinois, informing him that his paper had been accepted for publication in the Transactions and that the (evidently handwritten) manuscript was being typed. The letter reveals that the paper had been read by E. W. CHITTENDEN; some of the latter's comments are passed on to FRÉCHET. Another letter from CURTISS, of date September 6, 1917, informs FRÉCHET that proof sheets of the paper have been sent to him. These mailings to FRÉCHET from America evidently were sent to the University in Poitiers.

FRÉCHET did not adhere for long to the foregoing definition of his new V-classes. In a short note in the Comptes Rendus [FRÉCHET 63] of date September 10, 1917, he decided to define the new V-classes in a more general way, and in such a way as to relate them directly to the notion of limit element of a set rather than to L-classes and the limit of a sequence. An arbitrary class is called a V-class if to each element A corresponds a family of sets called neighborhoods of A. Then, an element A is called a limit element of a given set G if each neighborhood of A contains an element of G other than A; A itself may or may not belong to G. In this definition, at the outset, no assumptions are made about special properties of the families of neighborhoods. It is not even assumed initially that each neighborhood of A contains A. There is no extensive development of a theory in this short note.

A rather full development of FRÉCHET'S ideas about these new V-classes is given in [FRÉCHET 66], to which I now turn. On page 367 of a later paper [FRÉCHET 75], published in 1921, FRÉCHET speaks of having presented his general definition of V-classes in 1918 "au moyen de Notes redigées avant la guerre." I found no definite evidence of such *pre-war* notes in the Archives, but some of the notes in one of the war-time notebooks can be interpreted as a rough beginning that *may* have been made quite early. The definition of V-classes in the paper [FRÉCHET 66] here under discussion is exactly as in the note in the Comptes Rendus of 1917. The general idea of the paper is to relate the new V-classes to what FRÉCHET calls R-classes, the R standing for RIESZ. These are classes in which there is a primitive notion of derived set, governed by four axioms as I have given them in Section 1 of the present essay. FRÉCHET gives the axioms in a slightly different way, and in a different order. Instead of the RIESZ axiom that the derived set of a finite set is empty, FRÉCHET uses the axiom that a set with just one element has an empty derived set. In conjunction with the other two of the first three axioms the effect is the same.

FRÉCHET begins by observing that the derived sets in an arbitrary V-class are such that the following two conditions are satisfied:

(I) If $F \subset G$, then $F' \subset G'$.

(II) An element A is in the derived set G' of G if and only if it is in the derived set F', where F is the set of all elements of G with the exception of A itself in case A happens to be an element of G.

Now, (I) is the same as one of the RIESZ axioms, and (II) is a logical consequence of the first three axioms of RIESZ.

On the other hand, as FRÉCHET observes, if one has an arbitrary class and in it a primitive notion of derived sets satisfying the foregoing conditions (I) and (II), it is possible to regard the class as a V-class having certain families of neighborhoods that yield limit elements, and thus derived sets, agreeing precisely with the primitive notion of derived sets. The procedure is to define a set S to be a neighborhood of A when A is not in $(S^{\sim})'$, where $S^{\sim}$ is the set complementary to S. It can then be shown that A is in G' if and only if each neighborhood of A contains an element of G other than A.

RIESZ himself, in the context of his axioms for derived sets (see [RIESZ 2]), provided the model for FRÉCHET's foregoing definition of a neighborhood. RIESZ called a set S a neighborhood of A if A is in S and is isolated from the complement of S (which is the same as saying that A is not in $(S^{\sim})'$). FRÉCHET does not insist that A be in S, and observes that in the use of neighborhoods to define when A is in G', it makes no difference whether A belongs to its neighborhoods, or not. When it comes to finding conditions on neighborhoods that express the conditions on derived sets imposed by the RIESZ axioms, FRÉCHET seems to think that the reasoning is made simpler by making the general assumption that A is *never* a member of one of its neighborhoods.

FRÉCHET begins (using the foregoing special assumption) by observing that the requirement that every set consisting of a single element have an empty derived set is equivalent to the requirement that the intersection of all the neighborhoods of any particular element be empty. This is, of course, the same as requiring that the intersection of all the neighborhoods of any particular element be just that element, if one makes the alternative special assumption that an element is *always* a member of every one of its neighborhoods. It is also true that the requirement that every set consisting of a single element have an empty derived set is equivalent to the following condition on elements and neighborhoods: If A and B are distinct elements, then each of these elements has a neighborhood that does not contain the other. FRÉCHET did not mention this form of the condition in the paper I am now discussing, but he does use this form of the condition in a subsequent paper [FRÉCHET 75], which I shall discuss a little later on in this essay.

Next, FRÉCHET shows that the requirement that $(F \cup G)' \subset F' \cup G'$ for all sets F, G is equivalent to the requirement that, given any element A and any two neighborhoods of A, the intersection of these neighborhoods contains a third neighborhood of A.

Condition (I), which is the same as one of the axioms of RIESZ, as I listed them in Section 1, is automatically satisfied in a V-class.

FRÉCHET's discussion of the fourth axiom of RIESZ is brief and unclear. Actually, what he says about a condition on neighborhoods (on page 143–144 of the paper), that is supposed to be equivalent to the fourth condition, is incorrect. He remedied matters somewhat when he wrote about this in his book. See pages 181–182 and 200–210 in [FRÉCHET 132]. I shall not say any more about this fourth axiom of RIESZ except to observe that it is satisfied by the topology that results from the four neighborhood axioms of HAUSDORFF. This fourth axiom of RIESZ has not played a significant role in later work on topology.

When we abandon FRÉCHET's temporary assumption that an element does not belong to any of its neighborhoods, and put together FRÉCHET's findings about neighborhoods in relation to the first three of the axioms of RIESZ, we see that a class in which the derived sets are governed by these three axioms can equally well be considered as a V-class (in FRÉCHET'S new sense) in which the neighborhoods are required to satisfy the following three conditions:

N_1: To each element corresponds a (nonempty) family of neighborhoods of the element. The element belongs to each of its neighborhoods.
N_2: Given two neighborhoods of an element A, there is a third neighborhood of A that is contained in each of the given neighborhoods.
N_3: Given two distinct elements, there is a neighborhood of each that does not contain the other. (Or, equivalently, the intersection of all the neighborhoods of an element is the element itself.)

By reference to the listing of HAUSDORFF'S axioms in Section 4 of this essay it will be seen that condition N_1 is the same as HAUSDORFF'S axiom (A), that condition N_2 is the same as HAUSDORFF'S axiom (B), and that condition N_3 is similar to, but less stringent than, HAUSDORFF'S axiom (D).

A further interesting comparison between FRÉCHET'S work and that of HAUSDORFF (with which FRÉCHET was, as he stated later, unacquainted at the time) can be made as soon as we discuss the next part of FRÉCHET'S work, in which he brings into consideration a further axiom – the axiom that every derived set is closed. As he shows, this axiom, along with the first three axioms of RIESZ, implies the following condition on neighborhoods:

N_4: If A is any element and V_A is any neighborhood of A, there exists a neighborhood W_A of A such that, if B is an element of W_A, there is a neighborhood V_B of B with V_B contained in V_A.

Furthermore, if we have a V-class in which the neighborhoods satisfy the axioms N_1, N_2, N_3, N_4, the resulting derived sets satisfy the first three axioms of RIESZ, and every derived set is closed. FRÉCHET labels as condition 5° the requirement that every derived set is closed.

Condition N_4 bears some resemblance to HAUSDORFF'S axiom (C), but they are not the same, and for a good reason. To understand the difference we need to consider the notions "interior point of a set," "interior of a set," and "open set." We do this in the context of a V-class in which axioms N_1–N_4 are satisfied. FRÉCHET, following RIESZ, defines A to be an interior element of a set S if S is a neighborhood of A, which means (in RIESZ'S terms) that A is in S and is not a limit element of the complement of S. An equivalent way of putting it is that there is some neighborhood of A wholly contained in S. But there is nothing that requires all elements of a neighborhood V_A of A to be interior elements of that neighborhood. What axiom N_4 requires is that, given A and V_A, there is another neighborhood W_A of A such that all elements of W_A are interior elements of V_A.

RIESZ defined a set to be open if all its elements are interior elements of the set. The interior of a set is defined as that set composed of all the interior elements of the given set. It is a consequence of axioms N_1, N_2, N_4 that the interior of a set is an open set (although it may be empty). With these same axioms it is true that the interior of a neighborhood of an element is itself a neighborhood of an element.

In his paper [FRÉCHET 75] FRÉCHET resumes consideration of V-classes in which the neighborhoods satisfy the four axioms N_1–N_4. He calls a V-class of this kind an H-class (une classe (H)); the first use of this designation, I believe, is on page 342 of the paper in question. The H is in honor of the American, E. R. HEDRICK, as FRÉCHET explains on page 212 of his book on abstract spaces. As FRÉCHET points out on page 365 of the paper, in an H-class it may be assumed that all of the neighborhoods used in defining it as a V-class are open. The reason for this is that, even if the initially given neighborhoods are not necessarily open, if we use only the interiors of these neighborhoods to define limit elements, we obtain exactly the same limit elements as before, as a result of the fact that the interior of each neighborhood is an open neighborhood that is contained in the original neighborhood. Consequently, an H-class can be defined as a special kind of V-class, in which the axioms on neighborhoods are N_1, N_2, N_3 as before, but with N_4 replaced by the modified axiom:

N_4': If A is any element and V_A is any neighborhood of A, and if B is any element of V_A, there exists a neighborhood V_B of B with V_B contained in V_A.

This axiom insures that all the neighborhoods are open; moreover, it plays the same role as N_4 in helping to show that all derived sets are closed. It will be observed that axiom N_4' is the same as HAUSDORFF'S axiom (C).

The difference between an (H) class and the kind of topological space defined by HAUSDORFF'S four axioms lies in the difference between HAUSDORFF'S axiom (D) and FRÉCHET'S axiom N_3. HAUSDORFF'S axiom states that, given two distinct elements, A, B, there exist neighborhoods V_A and V_B of A and B respectively, such that V_A and V_B have no points in common. FRÉCHET'S axiom requires merely that each of the two elements have a neighborhood that does not contain the other element. Because HAUSDORFF'S (D) implies FRÉCHET'S N_3 (but not vice versa), it follows that a HAUSDORFF topological space is a special sort of H-class. HAUSDORFF had used the unadorned name *topological space* for a class with topology derived from his four axioms. Because various writers have subsequently used the designation *topological space* in a more general sense, it will be convenient from now on to use the designation *Hausdorff space* for what HAUSDORFF called a topological space. FRÉCHET himself eventually used the generic name "topological space" for a class in which to every set corresponds a certain set, called its derived set, the elements of which are called limit elements of the original set, with only one requirement: that expressed by condition (II) earlier in this section (see pages 166–169 in his book).

FRÉCHET had the following to say by way of comparison between H-classes and HAUSDORFF spaces (I give a paraphrased translation): "It can be seen in the present memoir that one can extend to H-classes almost all of the properties that HAUSDORFF demonstrated for his topological spaces. Moreover, the definition of an H-class by the first three axioms of RIESZ and the requirement that all derived sets be closed seems much more natural than Hausdorff's four axioms." In spite of his having observed that H-classes can be defined in such a way that all the neighborhoods are open sets, FRÉCHET really felt that this last was an undesirable restriction on the notion of neighborhood. On page 367 he remarked that HAUSDORFF, and also CARATHÉODORY, in the edition of 1918 of his book on real functions, seem to regard the property of openess as an inherent property of neighborhoods; but then FRÉCHET said that while such a limitation might be useful for certain purposes, it wasn't really essential and might even run the risk of hiding the true nature of neighborhoods.

As far as I know, FRÉCHET himself never highlighted an important property of HAUSDORFF spaces not shared by all H-classes. There is such a property. A HAUSDORFF space has the property that every set in it with the BOREL-LEBESGUE property is closed. This fact is included in a theorem on page 231 of HAUSDORFF'S book (the theorem that asserts that a set with the BOREL-LEBESGUE property is both closed and compact in FRÉCHET'S sense). But it is possible to have, in an H-class, a set that is not closed, yet has the BOREL-LEBESGUE property. I am not sure when this possibility was first realized, but it was known to ALEXANDROFF and URYSOHN and mentioned by them in correspondence to FRÉCHET, as I point out later on, in Section 9, in the description of material accompanying the letter of January 28, 1924. An example of this situation is the following, taken from Problem 4 on page 105 of my book [TAYLOR 1]. Consider an arbitrary infinite class X. As the neighborhoods of any given point x in X take sets that contain x and have finite sets as complements. It is not difficult to verify that this definition makes X an H-class and that, if E is any infinite set, its derived set is X. It follows that every infinite set except X itself fails to be closed. Finally, every set has the BOREL-LEBESGUE property.

For convenience when, later on in this essay, I refer several times to H-classes (or, as FRÉCHET called them in his book, espaces (H), I include here in concise form two ways of defining H-classes.

Definition using derived sets. A class in which there is a primitive notion of derived sets is called an H-class when the following conditions on derived sets are fulfilled.

(1) $(E \cup F)' = E' \cup F'$ (equivalent to the combination of RIESZ's conditions 2, 3 in Section 1);
(2) E' is empty if E is a finite set;
(3) $(E')' \subset E'$ for every E (that is, every derived set is closed).

Definition using open neighborhoods. A V-class in which the neighborhoods satisfy the following conditions is an H-class.

(a) Every element has at least one neighborhood, and the element is in every one of its neighborhoods;
(b) If U and V are neighborhoods of x, there is a neighborhood W of x such that $W \subset U \cap V$;
(c) Given two distinct elements, there is a neighborhood of each one that does not contain the other;
(d) Given any element x and any neighborhood U of it, then for each y in U there is a neighborhood V of y such that $V \subset U$.

Condition (d) insures that the neighborhoods are open.

If one starts with the definition using derived sets, one can get to the characterization of H-classes by the use of open neighborhoods in the following way:

Given x, consider sets G such that $x \in G$ and x is not in $(G^{\sim})'$. Then consider the set U of those y in G such that y is not in $(G^{\sim})'$. Call U (which is the interior of G) a neighborhood of x.

7. Further consideration of covering theorems and compactness

The pursuit of the relationship between compactness and the BOREL-LEBESGUE property led to some interesting investigations and to proposals to introduce modifications in the notion of compactness. The eventual consequence, after some decades, was to assign a new meaning to compactness.

From the work of FRÉCHET in his thesis and a remark on that by HILDEBRANDT it became known that, in a metric space, sets which are closed and compact are identical with those that have the BOREL-LEBESGUE property. In more general sorts of spaces things are not so simple with the BOREL-LEBESGUE property.

The situation with the less restrictive BOREL property is not as complicated. From separate results by HEDRICK and FRÉCHET already mentioned in Sections 3 and 5, it follows that, in an L-class for which each derived set is closed, a set has the BOREL property if and only if it is compact and closed. In the paper [FRÉCHET 66], FRÉCHET considers the BOREL property in the context of his new V-classes (which need not be L-classes). There he proves the theorem (see page 154 of the paper): For a V-class of the type that he calls an H-class in a subsequent paper ([FRÉCHET 75]), a set G has the BOREL property if and only if each infinite subset of G has a limit point in G (*i.e.*, if and only if G is compact in itself).

The first person to attack successfully FRÉCHET's question: "What is the most general sort of space in which it is true that every closed and compact set has the Borel-Lebesgue property?" was the American, R. L. MOORE. In his paper [MOORE] of 1919 he considered S-classes, that is, L-classes in which every derived set is closed. To express his ideas he called a family of sets monotonic if, given any two members of the family, one contains the other. Then he gave a definition: a set G has property K if, whenever $\mathscr{M}$ is a monotonic family of closed subsets of G, there is a point that belongs to every member of $\mathscr{M}$. After this came the theorem: If and only if the S-class has the property that each compact set has property K, then it is true that each closed and compact set has the BOREL-LEBESGUE property. The proof made use of transfinite numbers. MOORE went on to propose a new definition of compactness to replace that of FRÉCHET: Call a set G compact if, whenever $\mathscr{M}$ is a monotone family of subsets of G with no point common to all the members of the family, there *is* a point common to all the derived sets of the members of $\mathscr{M}$. With this new meaning of compactness, MOORE gave the theorem: In an S-class a set has the BOREL-LEBESGUE property if and only if it is closed and compact.

In the paper [FRÉCHET 75] where FRÉCHET discussed his H-classes he took up MOORE's idea and introduced the name "perfect compactness" for MOORE's new notion of compactness. FRÉCHET borrowed the terminology from S. JANISZEWSKI's thesis, published in 1912 ([JANISZEWSKI]). JANISZEWSKI's definition of the concept was not expressed in the same way, and he was not considering the BOREL-LEBESGUE property. MOORE had conjectured that perhaps his proposed new sort of compactness was, for an S-class, equivalent to JANISZEWSKI's perfect compactness. However that may be, FRÉCHET defined a set G to be perfectly compact in itself if, when $\mathscr{M}$ is a monotonic family of subsets of G, either all the members of the family have in common an element of G, or their derived sets all have in common an element of G. FRÉCHET'S theorem then is, for a V-class (of the type defined in [FRÉCHET 66]) in which each derived set is closed, a necessary and sufficient condition that a set have the BOREL-LEBESGUE property is that it be perfectly compact in itself. This result is stated on pp. 348–349 of the paper [FRÉCHET 75]. The V-classes of this theorem include H-classes, but can be more general.

In 1921 C. KURATOWSKI & W. SIERPINSKI, in a joint paper (see the Bibliography), responded as follows to the query raised by FRÉCHET in his paper of 1917. They dealt with an L-class restricted in a certain way, to be explained presently. They called a set G an *entourage* of a point p if p is an interior point of G. Then, using the work "power" (*puissance*) to denote the cardinality of a set, they defined the concept "power of a point p relative to a set E" as follows: p is of power m relative to E if every *entourage* of p contains in its interior a subset of E of power m, and if the like statement cannot be made for any cardinality greater than m. They then state and prove: In an L-class, every closed and compact set has the BOREL-LEBESGUE property if and only if the L-class has the property that, given an infinite compact set E whose derived set is also compact, there is at least one point whose power relative to E is equal to the power of E itself.

The next published step in this process of considering the BOREL-LEBESGUE property came in a paper by PAUL ALEXANDROFF & PAUL URYSOHN [ALEXANDROFF & URYSOHN 2], submitted for publication in June, 1923 and published in 1924, shortly after the untimely death of URYSOHN; the authors assert that the principal results of the paper were announced in Moscow in 1922. They deal with HAUSDORFF spaces, which they (following HAUSDORFF) call merely "topological spaces". They call a point p a complete accumulation point (Häufungspunkt) of a set G if, for every neighborhood of p, the intersection of the neighborhood with G has the same power as G itself. Then they assert: A HAUSDORFF space has the BOREL-LEBESGUE property if and only if every infinite subset of the space has a complete accumulation point. They call such a space *bicompact*. This notion of bicompactness was communicated to FRÉCHET in a letter of 28 January, 1924 by ALEXANDROFF & URYSOHN. For more about this matter and other correspondence with FRÉCHET see Section 9 of the present essay.

CHITTENDEN, who followed FRÉCHET'S work closely, also contributed to the discussion of sets with the BOREL-LEBESGUE property. In his paper [CHITTENDEN 5], in which he deals with FRÉCHET'S new V-classes, CHITTENDEN characterizes sets with the BOREL-LEBESGUE property, using a concept of what he calls *hypernuclear points*. He also uses FRÉCHET'S concept of perfect compactness. Some years later, in a long paper [CHITTENDEN 7], he deals further with the notion of bicompactness in a very general type of topological space, using merely the idea that with each set E is associated another set E', called the derived set of E, but with minimal assumptions. In a theorem on page 306 of this paper, CHITTENDEN brings together the ideas of MOORE, FRÉCHET, SIERPINSKI-KURATOWSKI, and himself about the BOREL-LEBESGUE property.

From the foregoing we see that FRÉCHET, by his query of 1917, stimulated a great deal of activity. The most enduring consequences flowed from the work of ALEXANDROFF and URYSOHN, for, by focussing on the BOREL-LEBESGUE property and giving it a name, bicompactness, they shifted the emphasis to a property that possesses greater topological significance than FRÉCHET'S notion of compactness (even though FRÉCHET'S singling out of that notion had a tremendous impact in the developmental period of abstract general topology).

In the United States, to a great extent by the 1950's and even the later 1940's, the concept of compactness was defined by the BOREL-LEBESGUE property (under the name of the HEINE-BOREL property). This was probably because S. LEFSCHETZ chose this definition in his book, Algebraic Topology, published in 1942; he said he was following the lead of BOURBAKI. However, even as late as 1952, in his book Topologie II, published in Poland, C. KURATOWSKI was still distinguishing between FRÉCHET'S compactness and the bicompactness of ALEXANDROFF & URYSOHN. In the United States today, FRÉCHET'S compactness is often called *countable compactness.*

T. H. HILDEBRANDT, then visiting from the United States in Göttingen, wrote a letter to FRÉCHET on July 7, 1926 with the opening greeting, in familiar style, 'Dear Fréchet'. He had evidently talked personally with FRÉCHET quite recently. He said he was sending FRÉCHET what he called the 'last part' of his manuscript paper on the BOREL theorem [HILDEBRANDT 2], which is headed: II The Borel Theorem in General Spaces. This paper, published later in 1926, was an important exposition (in the form of an invited address to the American Mathematical Society) of the state of affairs concerning theorems of the BOREL and BOREL-LEBESGUE type (although HILDEBRANDT did not use the label 'BOREL-LEBESGUE'). In another letter a few weeks later (on July 31) HILDEBRANDT replied to a letter of July 25 from FRÉCHET, in which the latter had evidently queried HILDEBRANDT as to why he had not discussed in greater detail FRÉCHET'S H-classes, or accessible spaces. (I shall comment on the term 'accessible' presently.) From HILDEBRANDT'S paper as published we can see that HILDEBRANDT had, in part II of the paper, considered first metric spaces, then L-classes (referring in each case to FRÉCHET'S thesis), and then what he called 'vicinity spaces,' by which he meant using the notion of neighborhoods. In this connection he mentioned HEDRICK, ROOT, HAUSDORFF, and FRÉCHET. Of HAUSDORFF he wrote: "The Hausdorff postulates have come to be accepted as a satisfactory basis, and a space based on them is usually called a topological space." In a footnote on page 464 he referred to the paper [FRÉCHET 66], of which he wrote: "Fréchet considers a type of space that he has called 'espace accessible', which is equivalent to a vicinity space subject to postulates similar to those of Hausdorff, IV and especially III being replaced by weaker ones." (The labels IV and III were those of HILDEBRANDT in his paper, and they referred to HAUSDORFF'S axioms (C) and (D) respectively, as I have given them in Section 4 of this essay.) Evidently trying to write tactfully and placatingly to FRÉCHET in his letter, HILDEBRANDT wrote that he thought FRÉCHET was right; that he (HILDEBRANDT) had not sensed entirely the importance and nature of accessible spaces, especially as outlined in FRÉCHET'S later paper ([FRÉCHET 75]) in the Annales de l'Ec. Norm. Sup. HILDEBRANDT stated that he had used HAUSDORFF'S axioms because they seemed to be the most elegant for his use in the paper; also, he had thought there was not much difference between accessible spaces and HAUSDORFF'S topological spaces. He promised to consider the matter further when FRÉCHET returned the manuscript. It is possible, I suppose, that the footnote about [FRÉCHET 66] was added after this exchange. But [FRÉCHET 75] is not mentioned in the paper.

FRÉCHET'S explanation for calling an H-class an 'accessible space' depends on what he called 'the generalized Hedrick property.' This property was enunciated on page 154 of [FRÉCHET 66] as follows: Suppose x is an interior point of a set E and a limit point of a set F. Then there exists a subset G of F such that x in G' and all points of G are interior points of E. On page 185 of his book [FRÉCHET 132] FRÉCHET states that, for reasons to be given later "nous avons appelé espace (H), puis espace accessible" every space in which the points of accumulation are defined in such a way as to satisfy the specified conditions on derived sets (conditions (1), (2), (3) as I have given them near the end of Section 6 of the present essay). On page 212 of the book FRÉCHET explains that the name 'espace (H)' was given in recognition of the fact that the space possessed the generalized HEDRICK property. Then he writes: "C'est pour la même raison, mais pour adopter un nom se justifiant naturellement que nous avons appelé cet espace un *espace accessible* (on peut accéder a l'intérieur d'un ensemble E en se déplacant sur un ensemble F ayant pour point d'accumulation un point intérieur à E)."

8. Fréchet's Esquisse d'une Théorie des Ensembles Abstraits

The publication to be discussed in this section is a long paper forming part of a collection of papers in two volumes assembled to honor a certain man in India, Sir ASUTOSH MOOKERJEE, on his Silver Jubilee. Just who he was and what scientific contact, if any, existed between him and FRÉCHET are unknown to me. FRÉCHET states in the preface to his book [FRÉCHET 132] that the Esquisse was prepared upon invitation by the University of Calcutta. In the introduction of the Esquisse [FRÉCHET 76] FRÉCHET describes it as an exposition without proofs but in a systematic and natural order of the results he has obtained in the theory of abstract sets. It is apparent that the material forming the Esquisse had already been composed and was soon to be printed when FRÉCHET'S paper No. 75 was published (in 1921). According to FRÉCHET's own statement on page x of the introduction to his book of 1928, the Esquisse served as a foundation for the book. I had some difficulty in locating a copy of the Esquisse. It is clear to me that it is essentially a compilation of results from FRÉCHET'S publications up through his paper No. 75, with attention confined to the work on general point set topology and closely related matters. There are few new insights going beyond his previously published work. Nevertheless, the Esquisse played an important role for a few years, at least, in stimulating communication between FRÉCHET and other mathematicians interested in abstract topology.

There is little basis for knowing how many people saw the Esquisse and examined it with some care. Evidence in correspondence shows that ALEXANDROFF and URYSOHN, CHITTENDEN, KERÉKJÁRTÓ, and SIERPINSKI had access to the Esquisse. The only copy in Moscow (for a period of several years) was borrowed from SIERPINSKI, according to a letter to FRÉCHET of date March 17, 1925 from ALEXANDROFF. From a conversation with L. C. ARBOLEDA in Paris in 1979 I can report the following: ALEXANDROFF told M. A. YOUSKEVITCH in 1978 (who then passed the word along to ARBOLEDA) that he, ALEXANDROFF, had very much appreciated the Esquisse at the time when he and URYSOHN were reading it in 1923 and 1924. The conversation between ALEXANDROFF and YOUSKEVITCH had taken this turn because YOUSKEVITCH had told ALEXANDROFF of the discovery of the ALEXANDROFF-URYSOHN letters to FRÉCHET by ARBOLEDA. (See [ARBOLEDA 1].)

The Esquisse is divided into a short introduction and two main parts: Part I (24 pages) on the evolution of the notion of limit point of a set, and Part II (32 pages) on classification and general properties of abstract sets and functionals. There is quite a bit of overlap between parts, because Part I is designed to present motivation and historical insight, while Part II is supposed to be a systematic and orderly presentation of concepts, axiomatics, and results.

Among FRÉCHET'S comments about historical developments and certain motivating factors I cite the following from Part I. FRÉCHET portrays the notion of compactness as something he evolved from consideration of bounded sets on the real line. (See pages 355–357.) He says that in studying point sets on a line not much importance had been attached to the condition that a set be contained in a finite interval. In fact, there was often neglect to specify whether or not the sets under consideration were bounded. The risk of confusion was perhaps small, but it existed. The matter became more serious in the case of plane sets, and especially in the definition of a continuum given by CANTOR and JORDAN, which were equivalent for the case of bounded continua, but not for unbounded ones. FRÉCHET speaks about problems in the matter of extending the notion of a bounded set to the case of sets in a more general sort of class, especially when the class is wholly abstract. For the general case, he said, it is not just a matter of a *natural* extension of the definition, but of a *useful* one. FRÉCHET says that in his thesis he had in mind to preserve the property embodied in the BOLZANO-WEIERSTRASS theorem. This, FRÉCHET says, was the property he selected in his thesis as the basis for defining a compact set in an L-class.

On the general subject of functions in relation to the theory of abstract sets FRÉCHET says (pages 358–359): "The general concept of a function depending on something other than one or a finite number of numerical variables developed little by little according to the needs of analysis. Ascoli and Arzelà are among the first to have studied properties of functions of lines (fonctions de ligne), of which a masterly and systematic study has been made by Volterra." He mentions other precursors of the general (abstract) theory: LE ROUX, HILBERT and HILL, POINCARÉ, and VON KOCH (the latter three on infinite determinants), and, finally, in his listing, HADAMARD, and E. H. MOORE.

In Part II FRÉCHET confines his attention mainly to the subject of abstract point set topology, taking the notions of element of accumulation (the term he is using for limit element) and derived set as fundamental. He proceeds for a while with no restriction on the relationship of E' to E, introducing nearly all the notions of general topology in this very general setting. Then he considers, in succession, his new *V*-classes of 1918, *H*-classes, *L*-classes, *S*-classes, *E*-classes (in a sense different from the usage in his thesis, and *D*-classes (which are metric spaces, *D* standing for distance). The new *E*-classes were first defined by FRÉCHET in [FRÉCHET 65]. An *L*-class is an *E*-class in the new sense if there is a distance-like binary real function of two elements, called an *écart*, not necessarily possessed of the triangularity property of a metric, but otherwise like a metric and used to define convergent sequences just as in a metric space.

It is noteworthy that FRÉCHET'S definition of completeness for a metric space (une classe (D) complète) is not what we would expect from modern usage. He first formulated this definition in [FRÉCHET 75], on page 341: "j'appelle classe (D) complète une classe (D) où, parmi toutes les définitions de la distance compatibles avec la définition supposée préexistante des éléments d'accumulation, l'une au moins admet une généralisation du théorème de Cauchy sur la convergence d'une suite." FRÉCHET then immediately raises a question by saying that it would be interesting to know if there exists such a thing as a non-complete *D*-class. This is not an entirely trivial question. It is possible to have a class on which there are defined two different metrics which yield the same derived set E' for every set E, and such that the theorem of CAUCHY is satisfied with one metric but not with the other. In FRÉCHET'S concept of a metric space, as presented here, the metric itself is not an essential constituent of the space itself; it is only the relation between the sets E and their derived sets E' that is essential. The space is what we today call *metrisable*. FRÉCHET'S complete *D*-class is a metrisable space such that, with a least one of its equivalent metrics, the CAUCHY convergence criterion is a necessary and sufficient condition that a sequence have a limit.

The question (in the preceeding paragraph) raised by FRÉCHET was settled in the paper [CHITTENDEN 4], which is an extract from a letter sent to FRÉCHET by CHITTENDEN in April, 1922. SIERPINSKI, a little later, also disposed of the problem. FRÉCHET gives an account of the matter in [FRÉCHET 79] and [FRÉCHET 91]. CHITTENDEN proved that if a *D*-class is complete in FRÉCHET'S sense and contains a set that is dense in itself, then every neighborhood of an element in this set contains a subset that is homeomorphic to an interval of the real line. Such a set, therefore, cannot be merely denumerably infinite. The class of rational points on the real line, with the ordinary metric, is denumerable and dense in itself. Hence it cannot be a complete *D*-class. SIERPINSKI (according to FRÉCHET) proved directly that the class of rationals with derived sets determined by the ordinary metric is not a complete *D*-class in FRÉCHET'S sense. Apparently SIERPINSKI communicated to FRÉCHET what he had done; I have not found a publication by SIERPINSKI on this. However, in his paper [SIERPINSKI 1] (which concerns a different matter) SIERPINSKI pointed out in a footnote on page 203 that for a metric space to be complete in FRÉCHET'S sense it is necessary and sufficient that it be homeomorphic to a metric space that is a vollständiger Raum in HAUSDORFF'S sense (that is, a space in which the CAUCHY convergence condition is a sufficient criterion for a sequence to have a limit). As FRÉCHET observed in [FRÉCHET 91], referring to the top of page 355 in FRÉCHET [75], he could, himself, have drawn the same conclusions as CHITTENDEN and SIERPINSKI by using the theorem given (in 1910) at the beginning of Section 14 on page 8 of [FRÉCHET 39]. This is an interesting example, it seems to me, of a certain quality or tendency in FRÉCHET'S thinking.

He seemed to lack facility, or insight, or power of imagination, to enable him to make as much of his ideas as he might have done with a little more reflection and penetration.

In the following section I shall indicate some of the more interesting things said about the Esquisse in the letters of ALEXANDROFF and URYSOHN to FRÉCHET.

9. Alexandroff, Urysohn, and Fréchet, 1923–1924

When the young scholar, LUIS CARLOS ARBOLEDA, from the Universidad del Valle in Cali, Colombia, undertook to examine the letters and papers of MAURICE FRÉCHET that were deposited in the Archives of the Académie des Sciences in Paris, he found an extensive collection of letters from PAUL ALEXANDROFF and PAUL URYSOHN to FRÉCHET. He published an article about them [ARBOLEDA 1], quoting some passages from the letters and focussing attention on what the correspondence reveals about the impetus given to topology by the work of ALEXANDROFF and URYSOHN. In my discussion here of aspects of this correspondence my purpose is to show how FRÉCHET influenced these two Russians and how the correspondence enables us to form a better understanding of FRÉCHET's place in the development of abstract topology. On occasion it turns out that I have quoted a passage that ARBOLEDA also quoted in his paper about these letters. But generally I have quoted more than ARBOLEDA did, in order to bring out something relevant to my purpose. It is also true that there are places where what I have written overlaps with the exposition in ARBOLEDA's Paris thesis of 1980 [ARBOLEDA 2]. An important difference between this essay and ARBOLEDA's thesis (unpublished as of now) is that I am making a study and appraisal of a part of FRÉCHET's work, whereas ARBOLEDA's intent was to study the early investigations of general topology by FRÉCHET and others using the letters and documents in the FRÉCHET collection as the resource. Our work runs rather close together at times but the point of view is different.

The correspondence was initiated by a letter to FRÉCHET written jointly by ALEXANDROFF and URYSOHN from Moscow on October 23, 1923. They identified themselves as adjunct professors (professeurs adjoints) at the University of Moscow. They were young (ALEXANDROFF[10] was born in 1896, URYSOHN in 1898). At that time FRÉCHET was forty-five years old. We cannot be sure that all the letters written to FRÉCHET in this correspondence are in the collection in the Archives, but from internal evidence in the letters we can surmise some things about FRÉCHET's replies; from the responses made to FRÉCHET in the letters from ALEXANDROFF and URYSOHN it seems reasonable to infer that the collection in the Archives may be complete so fas as concerns what was sent to FRÉCHET in 1923 and 1924 (except for a postcard from ALEXANDROFF to FRÉCHET sent in August of

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[10] I use the spelling ALEXANDROFF, rather than ALEXANDROV, because that is what ALEXANDROFF himself used in writing and publishing in French and German in the period I am considering.

1924 and mentioned in ALEXANDROFF's letter of September 22, 1924). It is known that FRÉCHET's letters to the Russians have not survived.[11]

URYSOHN's active role in this correspondence is limited. There are twelve letters in the correspondence written in 1923 and 1924; eight are joint letters signed by both the young Russians, two are from URYSOHN alone and two are from ALEXANDROFF alone. The dates of the letters are, in 1923: October 23, November 22 and 24, and December 19 (this last one from URYSOHN alone). The dates of those in 1924 are January 28, February 28, March 22, April 15, May 18, August 3 (from URYSOHN alone), September 22 and November 10 (the last two from ALEXANDROFF alone). All were written from Moscow except the one of August 3, written from the French coastal village of Le Batz.

URYSOHN died by accidental drowning in the sea at Le Batz on August 17, 1924. ALEXANDROFF's letter of September 22, reproduced hereafter, gives details of the accident. There are many letters from ALEXANDROFF to FRÉCHET in 1925 and subsequent years. I discuss some of them in Section 10.

Some details about URYSOHN's life and short career are contained in a note written by ALEXANDROFF and published on pages 138–140 in volume 7 (1925) of Fundamenta Mathematicae.

In quoting from the letters I have, in general, refrained from calling attention in detail to faults of punctuation or grammar, lack of accents in appropriate places, and so on. For instances, peut être is often written where it should be peut-être, with hyphen, and I have reproduced what is written. The situation with accents is at times vague, for the reason that the photographic reproductions of the letters from which I have worked are not always good enough to be certain where accents are and where they are not.

Here is the opening letter of October 23, with faults of language as written, complete except for the formal closing sentence:

"La célébre Théorie des ensembles abstraits que Vous avec créée nous a déjà depuis longtemps inspiré dans nos recherches. L'exposé du premier group de résultats que nons avons obtenus dans cette ordre d'idées forme plusieurs mémoires qui sont maintenant au cours d'impression dan les 'Fundamenta Mathematicae' et dans les 'Mathematische Annalen.'[12]

"Aujourd'hui nous sommes en possession de quelques nouveaux résultats que Vous trouverez, peut-être, non dépourvus d'interêt: il contiennent, en particulier la resolution de Votre beau problème sur les relations entre les notions de limite et de distance (Trans. Amer. Math. Soc. 1918, 53–65, ainsi qu'une condition (topologique) nécessaire et suffisante pour qu'une classe (D) séparable soit une classe (D) complète, etc.

"Nous nous permettons donc de Vous envoyer les copies de trois notes que nous avons écrites sur ce sujet et que nous envoyons avec la même poste à Monsieur

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[11] On this point see the first footnote on page 74 of [ARBOLEDA 1].

[12] There were six of these papers altogether, all published in 1924: One by ALEXANDROFF in Fundamenta Mathematicae, the rest all in Mathematische Annalen—two by ALEXANDROFF, one by ALEXANDROFF & URYSOHN as joint authors, and two by URYSOHN alone. See the Bibliography.

Lebesgue: nous espérons notamment qu'il consentira à les présenter à l'Académie des Sciences pour les faire imprimer dans les 'Comptes Rendus'.

"Si vous désirez, cher Maître, d'avoir quelques renseignements de plus sur nos travaux, nous serons heureux de Vous les communiquer."

There happens to be in the Archives a letter from LEBESGUE to FRÉCHET dated November 11, 1923, that illuminates to some extent what happened next. LEBESGUE told FRÉCHET that he had received the notes for the Comptes Rendus from ALEXANDROFF and URYSOHN, and had also received what FRÉCHET had sent him. I quote the following words of LEBESGUE: "La note que vous critiquez est le 2^{e} des trois – Si donc j'allais demain à l'Institut, ce dont je doute, ce n'est pas celle là que je présenterai. Mais en realité je n'en présenterai actuellement aucune. Votre aussi m'oblige à m'abstenir. Théoriquement je suis responsable de l'exactitude des notes que je présente. Je ne me frappe pas et ne prend pas cette responsabilité au tragique, mais pourtant je ne puis présenter une note ayant déjà en main une réponse disant: cette note est fausse dans telle partie. Mon devoir est de signaler la fausetté à l'auteur – Mais, puis que les auteurs vous ont envoyé des doutes[13] de leurs notes et que c'est vous qui ayez reconnu l'erreur,[14] voulez vous me rendre (et leur rendre) le service d'examiner de la même manière les trois notes et le leur envoyer vos observations en leur disant que je les prie de m'envoyer une rédaction nouvelle tenant compte de vos observations (dans la mesure qu'ils jugeront convenables). Ajoutez qu'il seraient désirable qu'ils réussissent à condenser leur trois notes en deux – Naturellement cette façon de procéder ferait sans doute tomber votre note car les auteurs tiendraient sans doute assez compte de vos observations de priorité pour vous donner satisfaction. En tout cas, si une nouvelle note de vous restait nécessaire à vos yeux, je ne pourrais la présenter que dans la séance postérieure à celle où j'aurais présenté la note motivant cette réponse. Je vous renvoi votre note ci inclus. Voir mes observations sur son premier paragraphe. Merci a l'avance."

More information about FRÉCHET'S reaction to the three notes sent by the two Russians can be inferred from the contents of two manuscripts in FRÉCHET'S handwriting that I found in the Archives. One of them must be the note by FRÉCHET referred to by LEBESGUE. The titles of the two manuscripts are, respectively, Remarques sur la communication de M. Urysohn: *Les ensembles (D) séparable et l'espace Hilbertien*, and Remarques sur la communication de M. M. Paul Alexandroff et Paul Urysohn: *Une condition necessaire et suffisante pour qu'une classe (L) ou un espace topologique*[15] *soit une classe (D)*. These were, quite evidently, the titles of two of the three notes as originally submitted to LEBESGUE and FRÉCHET. As matters finally turned out, the notes were rewritten to some extent and resubmitted, and all three were published in the Comptes Rendus. They are listed

[13] This reference by LEBESGUE to doubts by ALEXANDROFF and URYSOHN is a mystery. There is no indication of doubt in the letter of October 23 from them to FRÉCHET.,

[14] Perhaps the reference to an error recognized by FRÉCHET pertains to his belief that what the Russians called condition 3° was unnecessary. I discuss this issue later on.

[15] A topological space in HAUSDORFF'S sense is meant here.

in the Bibliography. In the correspondence to be discussed ALEXANDROFF and URYSOHN refer to their notes as numbers 1, 2 and 3. From the context it is possible to deduce that Note 1, Note 2 and Note 3, after revision, were published as [ALEXANDROFF & URYSOHN 1], [URYSOHN 1], and [ALEXANDROFF 2], respectively. The titles of Notes 1 and 2, as given in FRÉCHET's manuscripts, were slightly modified in the published forms. I know of no evidence that either of the two manuscripts by FRÉCHET was ever published. From the letters to FRÉCHET from the two Russians it seems clear that he must have written them some of the things that are contained in these manuscripts. In the first manuscript, for example, he comments on the fact that in his paper [FRÉCHET 75] he changed the definition of separability that he had used in his thesis. In the thesis a class was called separable if it contains a denumerable set whose derived set is the entire class. In the new definition a set E is called separable if it contains a denumerable set N such that $E \subset N + N'$. There is a reference to this matter in the letter of November 22. Another clue about what he wrote to the Russians is contained in the following: In the first manuscript he mentioned that he had himself obtained a result of the type found by URYSOHN. He cited his paper [FRÉCHET 39], in which he had proved that every complete and separable metric space is homeomorphic to (indeed, isometric with) a subset in a certain sequence space (today known as l^∞), which, however, is not separable. FRÉCHET points out that URYSOHN's work has an advantage over his own, because URYSOHN shows that every separable metric space is homeomorphic to a subset in a certain *separable* sequence space (the HILBERT space today known as l^2). URYSOHN's letter of November 22 explains why they haven't seen [FRÉCHET 39].

Next I go into some detail about the second of the manuscripts of FRÉCHET. He wrote:

"M. M. ALEXANDROFF et URYSOHN ayant bien voulu me communiquer le texte de leur note, j'en prends occasion pour énoncer leur intéressante proposition sous une autre forme que me paraît plus maniable."

"J'ai été amené par des généralisations successives de mes premières recherches à la conception de classes d'elements qui j'ai appelées classes (H) parce qu'elles m'ont été suggérées par une extension intéressante d'une propriété signalée par Professeur Hedrick."

"Il se trouve que l'espace topologique du Professeur Hausdorff est une classe (H) mais que toutes les propriétés de l''espace topologique' parvenues à ma connaissance (j'entends celles qui généralisent des propriétés importantes de l'espace euclidien), sont partagés par la classe (H)." In a footnote referring to the term 'espace topologique' FRÉCHET remarked that he thought there were advantages in reserving the term for those more general spaces in which the topology is specified merely by having, corresponding to each set E, a set E' (perhaps empty), consisting of the accumulatlon points of E, without any conditions on this correspondence. In fact, FRÉCHET does use the term 'espace topologique' in this way in his book [FRÉCHET 132] (see page 167 there), but he does impose at least this condition: a point x belongs to E' if and only if it also belongs to F', where F is composed of all points of E except x (in case x belongs to E).

Next, FRÉCHET describes H-classes and points out the two different ways of axiomatizing them, either by axioms on neighborhoods or by axioms on derived sets, as is done in his paper [FRÉCHET 75). Then he continues:

"Si maintenant on reprend la suite des raisonnements de M. M. ALEXANDROFF et Urysohn en faisant intervenir les classes (H) au lieu des espaces topologiques de F. Hausdorff, on obtient les résultats suivants:

I. La condition nécessaire et suffisante pour qu'une classe (L) soit une classe (H) est que tout ensemble derivé y soit fermé.

II. La condition nécessaire et suffisante pour qu'une classe (H) soit une classe (D) est qu'il y existe une chaîne complète regulière (au sens de M. M. Alexandroff et Urysohn).

"La séconde proposition s'obtient par le même raisonnement que les deux auteurs ont appliqués a l'espace topologique de F. HAUSDORFF."

"La première résulte immediatement de la définition même des classes (H) et du fait que les classes (L) possèdent les proprietes 1), 2), et 3) mais pas toujours 5)." Here FRÉCHET is referring to the axioms on derived sets that characterize an H-class. He continues:

"Il est manifeste que l'emploi des classes (H) donne à l'ensemble des conditions pour qu'une classe (L) soit une classe (D) une simplicité plus grande que celui auquel l'emploi de l'espace topologique a conduit M. M. ALEXANDROFF et Urysohn, à qui reste pourtant le mérite d'avoir les premiers resolu le problème posé."

"Cette resolution pourrait être utilement completée si on parvenait à établir à quelques conditions doivent satisfaire les voisinages dans une classe (H) pour que celle-ci soit une classe (D). Il serait d'ailleurs préferable de ne pas imposer à ces voisinages la condition d'être ouverts, condition qui est étrangère a la notion de voisinage."

The letter of November 22 opens with an expression of thanks to FRÉCHET "pour Votre lettre si aimable et si suggestive." This must have been FRÉCHET's letter conveying LEBESGUE's message and some of his own comments about the notes (especially Notes 1 and 2), including, no doubt, some of the things that he had put into his two manuscripts. The letter of November 22 continues, after stating that the two Russians have read FRÉCHET's letter with the greatest interest: "En particulier, la grande simplification qu'apporte l'emploi des classes (H) nous était tout à fait inattendue. Il nous semble seulement que la condition 3° de notre Note:

3° Si toute suite partielle σ_1 d'une suite σ contient une soussuite σ_2 qui converge vers l'élément a, alors la suite totale converge vers le même élément a."

– que cette condition ne peut être supprimé.

"C'est à Vous, en effet, qu'on doit l'exemple instructif d'une classe (S) qui n'est pas (E) (Trans. Amer. Math. Soc., 1918, p. 56). En reprenent l'idée de Votre construction, on obtient aisément un exemple d'une classe (S) admettant une chaine complète regulière et qui n'est pas (D) par les mêmes raisons que celles que vous avez indiquées dans la discussion de Votre exemple cité tout à l'heure:

"il suffit, par exemple, de définir comme il suit la convergence dans la classe de tous les nombres réels: cette convergence coïncide avec la convergence arithmétique à une exception près, à savoir qu'une suite convergente (au sens arithmétique) vers 0 et contenant l'élément 1 sera dite divergente. (On pouvait d'ailleurs montrer par un exemple un peu plus compliqué que la condition 3° ne peut être remplacée par des conditions plus simples, p. ex. par celles que Vous avez indiquées dans un autre but, dans Votre Esquisse d'un théorie des ensembles abstraits de Calcutta, p. 344 3° et 4°.)

"Cette chose étrange est due à ce que la convergence est définie dans les classes (L), (S), (D), mais ne l'est pas dans les classes (H). Il en résulte que pour qu'un classe (L) soit (H) resp. pour qu'une (H) soit (D), il suffit que les éléments d'accumulation y coïncident; tandis que pour qu'une (L) soit (D) il faut encore que la *convergence* soit la même dans les deux cas. C'est justement la coïncidence de la convergence qu' a en vue la condition 3°. Si l'on aurait défini la convergence dans les classes (H), la condition 3° serait nécessaire même pour qu'une (L) soit (H)."

"En ce qui concerne le terme *séparable*, c'est Votre nouvelle définition que nous avions en vue; nous avons seulement oublié d'indiquer ce que nous entendons par 'partout dense': B est partout dense sur A, si $B \subset A \subset B + B'$. ..."

"Quant à l'objection que Vous avez faite dans Votre second mémoire des Rend. Palermo (1910), il nous a malheureusement été impossible de l'apprendre: il paraît qu'il n'existe actuellement à Moscou aucun exemplaire de ce tome des Rendiconti ..."

"Nous nous permettons enfin de vous communiquer un exemple (de P. Urysohn) d'une classe qui est à la fois (S) et (H) sans être un espace topologique. Les éléments de cette classe sont tous les nombres rationnels situés entre 0 et 1 (limites comprises) et le nombre $\sqrt{2}$. Une suite sera convergente dans les deux cas suivantes: (1) si elle converge (au sens arithmétique) vers ce même élément; (2) une suite ne possédant (au sens arithmétique) aucune élément d'accumulation rationnel convergera vers l'élément $\sqrt{2}$. C'est une classe (S) vérifiant la condition 3° ci-dessus, donc une classe (H). On pourrait vérifier directement que ce n'est pas un espace topologique. Cela résulte d'ailleurs d'un théorème de P. ALEXANDROFF d'apres lequel l'ensemble des points d'un espace topologique compact et parfait est nécessairement indenombrable."

The two Russians wrote FRÉCHET again on November 24, beginning this letter on the same page that contained the last few paragraphs of the letter of November 22. The letter of the 22^{nd} is in URYSOHN'S handwriting and that of the 24^{th} is in ALEXANDROFF'S handwriting. I quote, starting from the first of the letter of November 24 and going almost to the end:

"Votre seconde lettre est arrivée au moment même où nous avions terminé notre lettre ci-dessus. Nous Vous remercions maintes fois pour la flatteuse attention que Vous prétez à nos résultats. Nous vous envoyons en même temps les rédactions nouvelles de nos trois Notes: malgré tous nos efforts nous ne sommes pas arrivés à réduire le nombre total des notes de 3 a 2; or si l'impression de trois notes présentait des difficultés, la réduction de leur nombre pourrait être faite par une simple omission de la Note No. 2, 'Les classes (*D*) séparables et l'espace Hilbertien' (il faudrait alors seuelement omettre aussi dans la Note No. 3 les passages marqués au crayon *vert*)."

"Pour faire des nouvelles réductions nous avons relu avec la plus grande attention les remarques dont Vous avez honoré nos résultats. Dans la Note No. 2 la définition de la séparabilite a été précisée selon Vos indications et une remarque relative a Vos résultats de 1910 a été ajoutée (ces résultats nous étaient jusque à présent inaccessibles). Quant à la limitation que Vous faites à ce résultat, il nous semble que la portée de cette limitation peut être diminuée si l'on tient compte des faits suivants: le rôle fondamentale que jouent les ensembles fermés (bornés) dans l'Analyse n'est pas dû à ce qu'ils sont fermés, mais à ce qu'ils sont compacts en soi (extrémals). En effet cette dernière notion que Vous est due est d'une importance extrême dans toutes les parties des Mathématiques; en particulier, elle est topologiquement invariante, tandis que la propriété d'être fermé ne l'est pas (comme Vous venez de le remarquer). Il nous semble donc que si on regarde un ensemble comme un *être topologique*, la propriété d'être fermé ne sera pas une propriété de l'ensemble même: elle caractérisera plutôt sa situation dans l'espace."

"En ce qui concerne la Note No. 1 il nous a semble préférable d'exposer Votre résultat comme un *addendum*: nous voudrons notamment souligner que cette simplification et, en même temps, généralisation considérable de notre résultat, est due exclusivement a Vous; nous croyons d'autre part qu'il n'est pas peut être inutile d'indiquer l'énoncé relatif aux espaces topologiques et cela par les raisons suivantes. Il n'est pas à douter que dans les questions d'Analyse les espaces topologiques ne se rencontrent pas, tandis que les notions de limite, de distance et d'ensemble dérivé s'introduisent d'elles-mêmes. Or c'est en partant de questions topologiques que nous sommes arrivés aux espaces abstraits et il nous semble que dans cet ordre d'idées les espaces topologiques ont, ceux aussi, leur raison d'être; nous avons, en particulier, trouvé que certaines propositions de la théorie des ensembles (p. ex. celles qui concernent la puissance des ensembles) s'appliquent encore aux espaces topologiques, tandis qu'elles sont en défaut dans les classes (*H*) et même (*S*)."

The letter of November 24 is of particular interest for two reasons. It stresses that the property of being 'compact en soi' is a topological invariant. Neither the property of being compact or that of being closed is such an invariant. I do not think that, up to this point, FRÉCHET himself had ever singled out topologically invariant properties in themselves as being of particular interest. This is a case, I believe, that illustrates the superior insight of ALEXANDROFF and URYSOHN. The other point of great interest in the letter is its stress on reasons for regarding HAUSDORFF'S concept of a topological space as more appropriate (in certain situations) than FRÉCHET'S concept of an *H*-class. I have mentioned before that I find it odd that FRÉCHET never seems to have investigated, by himself, significant properties of a HAUSDORFF space not necessarily shared by *H*-classes. Indeed, his general attitude seems to have been that *H*-classes were 'just as good' as HAUSDORFF spaces for dealing with general questions in topology. Even though the two Russians told FRÉCHET they found that the use of *H*-classes simplified some of their arguments, they still wished to point out to him reasons for thinking

HAUSDORFF spaces preferable, in certain respects, to H-classes. I suspect that they did so in response to something in one of FRÉCHET's letters that emphasized to them his strongly held preference for H-classes, which he claimed were more 'natural' in concept than HAUSDORFF spaces. The fact that H-classes could be defined entirely by axioms concerning derived sets made them convenient.

A thing worth noting is that ALEXANDROFF and URYSOHN were finding it worth their while to devote effort to the tackling of problems posed, but either not solved or left in a partial state of solution by FRÉCHET. Two of the three notes for the Comptes Rendus were of this character. The letter of December 19 from URYSOHN (to be discussed presently) was also concerned with a problem that FRÉCHET had considered.

For a better understanding of the letters of November 22 and 24 and of Fréchet's second unpublished manuscript, we need to compare the manuscript with the published version [ALEXANDROFF & URYSOHN 1] of what had been Note 1 of the two Russians. It is entitled 'Une condition nécessaire et suffisante pour qu'une classe (L) soit une classe (D).' It opens as follows:

"C'est M. Fréchet qui a le premier formulé explicitement le problème d'indiquer les conditions pour qu'une classe (L) soit une classe (D), c'est à dire pour qu'on puisse déterminer dans une classe (L) une distance telle que les relations limites aux quelles elle donne naissance soient identiques à celles qui étaient définies d'avance. Ce problème auquel plusieurs auteurs (M. M. Hedrick, Fréchet, Chittenden, Moore [RL], Vietoris, Urysohn, Alexandroff) ont déjà apporté des contributions importantes en le resolvant dans des cas particuliers, est équivalent au problème suivant: quelles sont les conditions pour qu'un espace topologique soit un espace métrique? En effet, tout espace métrique peut être regardé comme un espace topologique et comme une classe (L) (même comme une classe (S)) et l'on peut indiquer facilement les conditions pour qu'un espace topologique soit une classe (L) et *vice versa.*"

At this point the authors insert the following proposition in a footnote, to which I shall refer hereafter as Footnote 4. I quote it:

"Par exemple, pour qu'une classe (L) soit un espace topologique il faut et il suffit que les trois conditions suivantes soient remplies:
1° C'est une classe (S) [*i.e.* an L-class in which all derived sets are closed].
2° Il existe pour tout couple d'éléments deux domaines (= ensembles complémentaires à des ensembles fermés) sans elements communs qui contiennent respectivement les deux éléments donnés.
3° Si toute suite partielle σ_1 d'une suite σ contient une sous-suite σ_2 qui converge vers l'element a, alors la suite totale σ converge vers le même élément a."

After this the paper continues with some technical definitions and a theorem stating a necessary and sufficient condition under which a topological space (in HAUSDORFF's sense) may be considered to be a metric space. It is not germane to my purpose to go into detail about this theorem. Suffice it to say that what is involved in defining a metric in the HAUSDORFF space is, first of all, to define what FRÉCHET, in his thesis, called a voisinage, and then use CHITTENDEN's theorem about the equivalence of voisinage and écart to conclude that the space can be given a metric that is compatible with the original topology.

What FRÉCHET did in his manuscript was to consider H-classes instead of HAUSDORFF spaces. His alternative to Footnote 4 was his proposition I, which I have already quoted. He reduces the three conditions of Footnote 4 to a single condition, the same as condition 1°, namely, he claimed, an L-class is an H-class if and only if every derived set in the L-class is closed. He abandoned condition 2°, which is the stronger separation axiom that distinguishes HAUSDORFF's spaces from H-classes. And he ignores condition 3°. As we have seen, ALEXANDROFF and URYSOHN wrote him that he couldn't suppress condition 3°. The explanation of the divergence in views on this matter is simple. What FRÉCHET was showing (correctly), was that the topology of an L-class is the same as the topology of an H-class, *i.e.* that the derived sets in the L-class satisfy the axioms for an H-class, merely by insisting on what FRÉCHET called condition 5° in connection with RIESZ's axioms 1°, 2°, 3° (entirely distinct from the conditions 1°, 2°, 3° of ALEXANDROFF and URYSOHN). It is always true that the derived sets determined by convergent sequences in an L-class satisfy RIESZ's axioms 1°, 2°, 3°. FRÉCHET had made note of this on page 140 in [FRÉCHET 66]. But, evidently, FRÉCHET did not intend to get into the problem of defining a type of convergence by using neighborhoods in the H-class and showing that this convergence was the same as the convergence originally postulated in the L-class. Perhaps he refrained from investigating this issue because of his awareness that it is possible, in an L-class, to enlarge the class of convergent sequences in certain ways without altering the derived sets. See Section XIII, pp. 147–148 in [FRÉCHET 66].

On the other hand, the intent of ALEXANDROFF and URYSOHN, in Footnote 4, was to put conditions on the L-class so that its derived sets (and hence closed sets and their complements) would have all the properties enjoyed by such sets in a HAUSDORFF space, and furthermore, such that a sequence $\{x_n\}$ in the L-class converges to x (in the originally given postulated convergence) if and only if, for each neighborhood V of x, all but a finite number of the x_n's are in V. Their condition 3° plays an essential role in the establishment of this requirement on convergent sequences.

How much the published version of Note 1 differs from its original, as first sent to FRÉCHET and LEBESGUE, it is impossible to know precisely. The title was shortened by omission of the words 'ou un espace topologique' (as may be seen by comparing [ALEXANDROFF & URYSOHN 1] with the title mentioned in FRÉCHET's draft manuscript about it. Also, a change is manifest at the end, in what was referred to in the letter of November 24 as an addendum. I quote: "Note supplémentaire – M. Fréchet a eu l'obligéance de nous communiquer que la condition qu'une classe (L) soit (D) peut être énoncé d'une manière bien plus simple que celle qu'on obtient en se servant des espaces topologiques. En effet, notre théorème relatif à ces espaces de même que la demonstration ci-dessus) s'applique aussi *directment* aux classes (S) vérifiant la condition 3° (voir la Note No. 4) et même, plus généralement, aux classes (H)."

The issue of FRÉCHET's disinclination to regard condition 3° as essential, and the Russians' insistence upon it, did not drop out of sight. In [FRÉCHET 66]

FRÉCHET had investigated the question of when an L-class could be regarded as a V-class (in the new sense of that paper), and had introduced the notion of convergent sequences in a V-class defined with the aid of neighborhoods. On page 417 he defined a sequence $\{x_n\}$ to be convergent to x if, given any neighborhood U of x, x_n is in U for all sufficiently large values of n (*i.e.* for all except perhaps a finite number $1, 2, \ldots, N$ of indices, where N may depend on U). I shall refer to this as 'the neighborhood definition of convergence.' In the context of FRÉCHET'S discussion, assuming the V-class to be such that, whenever $x \in E'$, there is a sequence $\{x_n\}$ of distinct elements of E which is convergent to x under the neighborhood definition, he asserted that the convergence would satisfy the axioms for convergent sequences in an L-class. But he overlooked the possibility that a sequence convergent in this matter might be convergent to more than one limit, and so his discussion was flawed. I think he did not realize this at the time he wrote the paper, nor even at the time of the correspondence with ALEXANDROFF and URYSOHN in November, 1923.

In a letter of December 7, 1923, sent by FRÉCHET to the two Russians (of which no known copy survives), FRÉCHET raised a question the general nature of which can be inferred from URYSOHN'S response, written on December 19. Here is the opening paragraph of that response.

"Je viens de recevoir Votre lettre du 7 XII et vos tirages à part; je les enverrai aujourd'hui a M. Alexandroff qui est actuellement à Smolensk (il reviendra dans quinze jours à peu près). Permettez de vous remercier bien vivement d'avoir bien voulu nous envoyer vos tirages à part et de nous avoir communiqué l'intéressante question relative à la modification de la convergence dans les classes (L) et à la convergence déduite de la dérivation. Il me semble que j'ai bien compis [sic] cette question et que les considérations suivantes en donnent une résolution satisfaisante."

Although URYSOHN'S letter does not indicate exactly how FRÉCHET'S 'interesting question' was worded, we can infer the essence of the question from the content of the letter. It would seem also, from the content of the letter, that when it was written URYSOHN had not yet read the paper [FRÉCHET 66], although it was probably included among the copies of his papers that FRÉCHET had just sent.

Here, in condensed form is the main substance of what I take to be URYSOHN'S solution of the problem posed in FRÉCHET'S question. URYSOHN considers at first what he calls a T-class (une classe (T)), which is like an R-class (discussed in Section 6 of this essay), except that the only axioms on the derived sets in a T-class are

$$(1) \qquad (A \cup B)' = A' \cup B',$$

$$(2) \qquad A' \text{ is empty if } A \text{ has only one element.}$$

An L-class is a special case of a T-class (the derived sets E' in an L-class being generated by convergent sequences of distinct elements from E). Given a sequence $\{x_n\}$, URYSOHN defines as follows what he means for it to be *topologically convergent* to x. Let E be the set of distinct elements among the x_n's and let F be the set of those elements x_n that are repeated infinitely often in the sequence. The sequence is said to be topologically convergent to x if $E' \cup F$ consists of the single element x and if x also stands in this same relationship to every subsequence of $\{x_n\}$.

Turning to the special case of an L-class, URYSOHN refers to the initially given notion of convergent sequences in the L-class, and calls such convergent sequences *primitively convergent*. He observes that a primitively convergent sequence is topologically convergent, but not necessarily conversely. However, taking note of the fact (which, unknown to URYSOHN, had been observed by FRÉCHET on pages 147–148 of [FRÉCHET 66]) that there may be more than one notion of convergent sequences that leads to the same derived sets in an L-class, URYSOHN asserts that the notion of topological convergence, when substituted for primitive convergence, leads to the same derived sets. He asserts that the primitive convergence in an L-class coincides with the topological convergence induced by the derived sets in the class if and only if the primitive convergence satisfies condition 3°.

The foregoing does not touch the question of the relation of topological convergence to the neighborhood definition of convergence, which URYSOHN does not mention in his paper. One might conjecture that URYSOHN avoided the latter definition of convergence because of the possibility of lack of uniqueness of the limit of a convergent sequence. I don't think one can, from the letter, come to any firm conclusion on this matter. As will be pointed out in Section 10, ALEXANDROFF, in a letter of April 29, 1926 to FRÉCHET, gave an example of an H-class in which a sequence converges (in the neighborhood sense) to two distinct limits. Such a thing cannot occur in a HAUSDORFF space.

At the top of the letter of December 19 appears the following notation in FRÉCHET's handwriting: "répondu le 30 Dec. on peut remplacer les classes T par les les classes (V)." I infer from this that FRÉCHET answered the letter of December 19 by calling URYSOHN's attention to his paper [FRÉCHET 66], citing in particular his discussion of L-classes as special V-classes on pages 146–148. In the next letter to FRÉCHET from the Russians (that of January 28, 1924) URYSOHN added the following as a P.S.: "En ce qui concerne les observations sur la convergence dans les classes (V) que vous avez bien voulu me communiquer, il me semble qu'elles sont non seulement justifiées par leur généralité, mais qu'elles présentent encore un intérêt intrinsèque considérable; elles montrent en effet, que la notion de voisinage suffit à elle seule pour pouvoir définir la convergence." This is not to be interpreted as meaning that URYSOHN accepted FRÉCHET's ideas as the last word on the matter. It is sure, however, that FRÉCHET's ideas altered URYSOHN's thinking, for in the posthumously published paper [URYSOHN 9] that was prepared by ALEXANDROFF for publication, we find that URYSOHN is making use of convergence by neighborhoods.

In this paper URYSOHN introduces the notion of what he calls an L_t-class, or topological L-class. It is an L-class in which a sequence that satisfies the heretofore stated condition 3° is convergent to the indicated limit. That is, if $\{x_n\}$ and x are such that in every subsequence of $\{x_n\}$ there is a further subsequence that converges to x (in the original L-class sense), then $\{x_n\}$ is convergent to x. The notion of convergence in any L-class can be modified to convert the L-class into an L_t-class without altering the derived sets. One merely augments the sequences that are primitively convergent by those that satisfy condition 3° but were not primitively convergent. The paper then goes on to deal with the question of when an L-class can be regarded as an H-class and when an H-class can be regarded as an L-class, with due regard in both cases for both derived sets and convergent sequences. The main theorems are:

I. In order that an L-class be an H-class in which convergence by the neighborhood definition coincides with the primitive convergence in the L-class it is necessary and sufficient that the derived sets be closed and that the L-class be an L_t-class.

II. In order that an H-class be an L-class (that is, that its derived sets be those generated by a definition of convergence that satisfies the axioms for an L-class), it is necessary and sufficient (1) that a sequence that is convergent by the neighborhood definition have just one limit, and (2) that if x is a point of a derived set E', there exist a sequence of points of E that is convergent to x.

I should remark that in describing this paper of URYSOHN I have used the term 'primitive convergence' and 'convergence by the neighborhood definition' in place of the terms 'convergence donnée à priori' and 'convergence à posteriori,' respectively, the latter terms being used by URYSOHN in the paper.

On page 82 in the paper it is noted explicitly that in the most general case, a sequence in an H-class that is convergent à posteriori may have more than one limit.

Finally, a remark about ALEXANDROFF'S footnote on page 78 of the paper. It states: "La solution d'Urysohn est équivalente a celle donnée par M. Fréchet en 1918, mais elle ne fait pas usage de la notion de voisinage. Comme elle présente une certain intérêt propre (surtout au point de vue méthodologique) M. Fréchet, consulté, m'a vivement engagé à la publier." In saying that URYSOHN'S solution does not make use of voisinages, ALEXANDROFF was surely referring only to the Theorem I, for neighborhoods *are* used in Theorem II. Also, it is not strictly accurate to say that URYSOHN'S solution is equivalent to that of FRÉCHET, for FRÉCHET did not invoke condition 3° in his version of Theorem I and he did not bring in the necessity of uniqueness of the limit in his attempt at Theorem II. I daresay that the wording with regard to FRÉCHET was designed to be generous to him, for ALEXANDROFF had reason to know that FRÉCHET was touchy about being given credit where his own work was involved. See the discussion of this issue in Section 10, where I discuss ALEXANDROFF'S letter of February 18, 1926. What is demonstrated in the letter of December 19 and in this paper is that URYSOHN, starting from questions that had been posed and worked on with only partial success by FRÉCHET, was able to arrive at more complete answers.

There is evidence that URYSOHN was familiar with some of FRÉCHET'S work as early as 1921 or 1922. In the first part of his very long paper [URYSOHN 8], in a footnote on page 39, URYSOHN wrote, in referring to the definition of a metric space: "Cette definition est due a M. Fréchet, de même que celle de la compacticité et beaucoup d'autres; c'est en effet M. Fréchet que s'aperçut le premier de ce fait, si important, que la théorie des ensembles n'utilise que peu de propriétés de l'espace Euclidien. Il en conclut, par une abstraction hardie, que cette théorie s'applique à des formations beaucoup plus générales, dont il indique plusieurs. L'une de ses définitions les plus heureuses est justement celle des espaces métriques. Il est d'ailleurs à remarquer que la terme que j'emploi est due a M. Hausdorff; je le prefère à celui de M. Fréchet (classe (D)) que me semble peu suggestif."

In this long paper by URYSOHN (which contains, among other things, the exposition of his theory of dimension) there is ample evidence that he had familiarized himself with HAUSDORFF'S book. As we see from a consideration of their correspondence with FRÉCHET, both URYSOHN and ALEXANDROFF were stimulated by FRÉCHET'S work, by some of the questions he posed, and by their correspondence with him.

Evidently FRÉCHET, knowing that the two Russians were in possession of the Esquisse,[16] invited them to send him their comments on it, for in their letter[17] to him of January 28, 1924, they wrote: "... nous voulions, notamment, exécuter aussi bien qu'il nous était possible votre aimable offre d'indiquer les additions et rectifications qu'il y aurait peut être lieu à faire à Votre 'Esquisse' de Calcutta; or, l'étude approfondie de Votre beau Mémoire a exigé beaucoup de temps, ... Nous vous envoyons aujourd'hui une série de petites remarques dont les unes (intitulées 'additions et rectifications diverses')[18] se rattachenet le plus étroitment a Votre 'Esquisse', tandis que les autres contiennent un exposé succinct d'une partie de nos résultats (la plupart de ces résultats paraîtra dans les Mathematische Annalen et dans les Fundamenta Mathematicae): nous y avons rassemblé ceux qui, à ce qu'il nous semble, sont assez étroitement liées aux questions traiteés par Vous. – Nous vous envoyons encore un petit manuscript 'Sur un problème de M. Fréchet relatif aux classes des fonctions holomorphes.' Ne serait il pas possible de le publier dans un des périodiques mathématiques français?"

With a later letter, that of February 28, they sent an additional page to be added to the manuscript, with remarks engendered by FRÉCHET'S comments on the original manuscript. The paper was published in 1924 (see [URYSOHN 4]). It settles in the negative a question that FRÉCHET had raised in his thesis, about the space composed of functions f that are holomorphic in a given (bounded) open set G, with a certain metric that renders a sequence $\{f_n\}$ convergent to f in the space if and only if $f_n(z)$ converges to $f(z)$ uniformly in each closed subset of G. FRÉCHET'S question was whether there exists an equivalent metric $\varrho(f, g)$ with the property that $\varrho(f, g) = \varrho(f - g, 0)$ and $\varrho(\lambda f, 0) = |\lambda| \varrho(f, 0)$. URYSOHN proved that, in fact, there is no equivalent metric satisfying the second of these two conditions.

The comments on the Esquisse form a long list of twenty seven items. The comments range from calling attention to misprints or inadverent slips to the noting of some erroneous claims or to statements requiring qualification. There are also suggestions for amplification. It would not be worth while here to go into the

16 In the letter of December 19 URYSOHN mentioned that ALEXANDROFF had the only copy of the Esquisse in Moscow.

17 For discussion of a part of this letter not touched on in what follows see page 82 of [ARBOLEDA 1].

18 On page 83 of [ARBOLEDA 1] the "additions et rectifications" are identified as belonging to the letter of November 22, 1923; this is an error by oversight, for they belong to the letter of January 28, 1924.

details. The Russians answered some questions posed by FRÉCHET in the Esquisse. The pages of their list bear markings and notes made by FRÉCHET, some of which were probably made in preparation for a letter of response by him. In a later letter from ALEXANDROFF & URYSOHN (that of February 28) there is a short list of their replies to the responses made by FRÉCHET on five of the original twenty seven items. Obviously, the two Russians had worked through the Esquisse carefully, supplying proofs as needed, constructing counter-examples to show where FRÉCHET had erred, or refining and completing his results in some cases. In one or two cases FRÉCHET was able to rebut their criticism successfully.

Their 'succinct exposition of their own results' also accompanying the letter of January 28, occupy seven large pages (thirty five lines to a page) of the prints made from my film copies of the letter and its attachments. I shall quote selections from this exposition that are of particular interest and relevance in connection with my study of FRÉCHET. Some of the material is the same as or similar to material in some of the papers published in 1924 in Mathematische Annalen or Fundamenta Mathematicae (listed in the Bibliography). The first topic introduced is perfect compactness, and in this connection they introduce bicompactness. Their first published introduction of this concept occurs on page 260 in [ALEXANDROFF & URYSOHN 2]. At the end of this paper, as published, appears the following: Eingegangen am 1. 8. 1923. The authors state in a postscript that the principal results in the paper were presented in March and June of 1922 in Moscow. Publication did not occur until after the death of URYSOHN. I now quote from the letter of January 28:

"Nous avons, il y a quelques années, introduit, (sans connaître la litérature mathématique postérieure a 1916) une notion que pourrait remplacer la parfaite compacticité et dont la définition a l'avantage d'être plus conforme à celle de la compacticité ordinaire.

Déf. 1. Un point ξ (dans une (V)) s'appelle *point d'accumulation* complète de l'ens. A si la puissance de l'ensemble $A.V_\xi$ est égale à celle de A pour tout voisinage V_ξ de ξ (on pourrait étendre cette définition à des classes plus générales en copiant celle que vous avez donnée pour les elem. de condensation.

Déf. 2. L'ens. A est dit *bicompact* [en soi] si chacun de ses sousensembles infinis donne lieu à au moins un elem. d'accumul. complète [appartenant à A]. (The square brackets afford an alternate reading.) Nous avons démontré que dans les (H) les ensembles bicompacts en soi coincident avec les ensembles parf. comp. en soi. Dans un espace de Hausdorff tout ensemble bicompact en soi est fermé (cette propriété n'est pas vraie dans les (H). Exemple: classe composé d'un circonférence et de son centre ξ. Le dérivé de tout ensemble infini est son dérivé ordinaire augmenté du point ξ. La circonférence est bicompact en soi mais n'est pas fermée). Dans un espace de Hausdorff tout ensemble bicompact et parfait a une puissance $\geqq 2^{\aleph_0}$. (Ceci est aussi en défaut dans les (H)). Il suffit d'examiner le premier exemple d'une (H) qui n'est pas un esp. de Hausdorff que nous vous avons communiqué. ..."

In this discussion of bicompactness there is no mention of covering theorems or the BOREL-LEBESGUE property (as defined in Section 5 of the present essay). Nevertheless, Theorem I (on page 259) of the paper [ALEXANDROFF & URYSOHN 2] asserts that the property here designated as bicompactness is, for a topological space of HAUSDORFF, equivalent to the condition that the space have the BOREL-LEBESGUE property (although the theorem does not employ this latter terminology).

The next topic in the exposition with the letter is 'dissociation', a French word which is used in a technical sense and is evidently to be translated into English as the technical term 'separation'. I quote: "Dans beaucoup de questions les classes (H) sont trop générales et il en est de même des espaces de Hausdorff. Cela nous a amené à introduire les espaces topologiques réguliers ou classes (H_r)."

They call an H-class regular and designate it an H_r-class, using the definition of regularity that is still standard in topology. They also call an H-class normal and designate it an H_n-class, using the definition of normality familiar today. They observe that every H_r-class is a HAUSDORFF space, and give an example to show that a HAUSDORFF space need not be regular. They assert that a bicompact HAUSDORFF space is an H_n-class, but that a bicompact H-class can fail to be a HAUSDORFF space. Other assertions: Every D-class (metric space) is regular. On a regular H-class there can be defined a continuous, non-constant function (there is evidently a tacit assumption that the class has more than one element).

There is no mention here of VIETORIS and TIETZE. In [VIETORIS] (which is the author's doctoral thesis[19] of 1919 in Vienna), VIETORIS treats his subject with the use of five axioms, one of which is equivalent to the axiom of regularity. TIETZE, in [TIETZE 1], lists four possible separation axioms. His word for a separation axiom is Trennbarkeitsaxiom. I describe these axioms briefly in order, *not* in his terminology: (1) HAUSDORFF's axiom (D) about separation of two distinct points, (2) the regularity axiom, about separation of a closed set and a point not in it, (3) the normality axiom, about separation of two disjoint closed sets, (4) the axiom of complete normality, which asserts that if A and B are two disjoint sets (not necessarily closed) and if each set is disjoint from the derived set of the other, then there exist disjoint open sets U, V containing A and B respectively.

On a page of notes made by FRÉCHET that I found in the Archives along with the letters from the two Russians, FRÉCHET wrote: "Il me semble qu'il doit y avoir un lien étroit entre vos recherches sur les H_r et H_n et les considérations developpées par Tietze," following which he cites the two papers [TIETZE 1] and [TIETZE 2]. The first of these papers by TIETZE bears the record of having been received by the editors on June 1, 1922; the second paper is based on lectures given in Hamburg on June 14, 15, and 15 of 1922. The two Russians acknowledge in a footnote on page 263 of their paper [ALEXANDROFF & URYSOHN 2] that TIETZE's first paper contains definitions 'analogous' to theirs. A fuller account of the relation in time between their definitions and those of TIETZE is given in what they wrote to FRÉCHET in their letter of March 22, 1924:

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[19] A note at the beginning of the paper by VIETORIS states: "Die Arbeit ist in den Jahren 1913–1919 zum grössten Teil im Felde entstanden und in Dezember 1919 in Wien als Doktordissertation eingerichtet worden."

"Nous avons pris connaissance l'été dernier du 1er Mémoire de M. Tietze, mais ce n'est qu'é votre Lettre que nous devons la connaissance de ce qu'il a écrit un second Mémoire. Comme il résulte d'un échange de lettres avec M. Tietze, nous avons trouvé ces conditions à peu près en même temps que lui; il paraît d'ailleurs que M. Tietze avait des buts différent des notres: du moins dans son premier mémoire il ne s'occupe pas de questions qui font l'objet des théorèmes que nous vous avions communiqués. L'exposition de nos résultats (communiqués à la Societé Mathématique de Moscou printemps 1922) a été transmise aux *Fundamenta Mathematicae* mai 1923 (avant d'avoir pris connaissance du Mémoire de M. Tietze), et aux *Mathematische Annalen* juillet 1923 (après cette connaissance). La priorité de ces *définitions* appartient donc a M. Tietze; nous avions surtout en vue les théorèmes qui s'y rattachent quand nous vous les avions communiquées."

I note that neither FRÉCHET nor the Russians mention VIETORIS (but FRÉCHET mentions both VIETORIS and TIETZE in the bibliography of his book [FRÉCHET 132]).

In the letter of January 28 ALEXANDROFF and URYSOHN raised with FRÉCHET the question of whether he could help them get visas to enable them to come to France for a personal conference with him. I quote:

"Nous voudrions encore, cher Maître, demander Votre conseil à propos de la question suivante. Il paraît qu'il nous sera possible de nous rendre à l'Etranger l'été prochain; nous serions heureux si nous pouvions profiter de cette possibilité pour visiter la France et surtout, pour recevoir l'honneur de faire Votre connaissance personnelle. Vos Lettres étant si suggestives pour nous, il se comprend de soi-même combien d'inspirations scientifiques pourrait nous donner un entretien personnel avec Vous. Malheureusement, le visa français est presque inaccessible pour les sujects russes. Seul le concours d'un illustre savant Français tel que Vous êtes, pourrait, peut-être, nous aider; mais nous ne savons pas si nous pouvons oser de Vous le demander."

"En terminant, permettez, tres honoré Monsieur, de Vous exprimer notre vive reconnaissance pour l'aimable et précieux concours que Vous avez bien voulu nous préter dans tout ce que concernent nos Notes aux 'Comptes Rendus.'"

The same subject came up again in their letter of February 28:

"Nous avons bien reçu vos deux Lettres du 9 et 13 février; nous sommes vraiment touchés par l'aimable bienveillance que Vous avez bien voulu préter a nos plans de voyage en France; nous espérons que votre départ en Amérique ne nous empêchera pas de faire votre connaissance. Nous comptons, en effet, arriver en France vers le premier juillet et revenir à Moscou vers le commencement du sémestre russe (1 octobre); or un retard de quelques jours nous sera en tout cas possible. Nous vous envoyons, conformément à votre aimable conseil, une lettre adressée à l'Association Française pour l'avancement des Sciences; nous espérons aussi que nous serons délegués par l'Institut Mathématique de l'Université de Moscou et que nous pourrons Vous envoyer dans quelques jours le document qui s'y rattache. Ne devons nous pas en outre écrire directment au Ministère aux affaires étrangères, ou bien cela serait inutil?"

The reference to FRÉCHET's departure for America is explained by the fact that he attended the International Congress of Mathematicians in Toronto. (Material in the Archives indicates that while FRÉCHET was abroad he gave lectures during the summer term at the University of Chicago, by invitation of E. H. MOORE. He was paid \$1400. He also journeyed to Urbana to give a lecture at the University of Illinois, receiving \$25 plus train fare.)

In the letter of March 22 the Russians report that they will write to the French minister of foreign affairs as soon as they get a response from the French Association for the Advancement of the Sciences. On April 15 they write in discouragement:

"Il paraît que nous devons ajourner notre voyage jusqu'un temps où les visas ne seront plus tellement inaccessibles. Nous sommes désolés qu'il nous sera impossible de faire votre connaissance, du moins pendant un temps encore indéterminé. Nous nous consolons seulement par l'espoir que vous consentirez de continuer l'échange des lettres qui, sans pouvoir remplacer un entretien personnel, nous a cependant donné tant d'inspirations, et dont nous savons apprécier la valeur."

Their disappointment was short-lived. On May 18 they wrote again:

"Nous vous sommes extrêmement reconnaissants pour votre aimable Lettre et les bonnes nouvelles qu'elle nous apporte; nous comprenons très bien que c'est à vous qu'est du le succès obtenu par l'Association Française pour l'Advancement des Sciences en ce qui nous concerne."

"Nous profitons de l'occasion pour vous communiquer un exemple assez curieux de deux classes (L) *cogrédients* (c. à d. telles que la dérivation y est la même, tandis que la convergence ne l'est pas). – Eléments: Fonctions mesurables sur [0, 1], deux fonctions presque partout égales étant régardées comme identiques. *Convergence*: dans le premier cas, convergence presque partout; dans le second cas, convergence en mesure. La seconde classe est une (L_t) (= classe dans laquelle toute suite convergente dans une définition équivalent au point de vue de dérivation est *à priori* convergente). La première classe n'est pas évidemment une (L_t). La cogrédience de ces deux classes a été demontrée récemment dans un séminaire de M. Egoroff par M. Kreyness (un mathématician encore tout jeune): il a notamment démontré le théorème suivant: Soit f une fonction mesurable et

$$f_1, f_2, \ldots, f_n, \ldots \tag{1}$$

une suite de fonctions mesurables; pour que (1) converge presque partout vers f, il faut et il suffit qu'on puisse de toute suite partielle

$$f_{n_1}, f_{n_2}, \ldots, f_{n_k}, \ldots$$

extraire une sous-suite convergeant en mesure vers f."

"En renouvelant nos remerciments les plus impressés, nous vous souhaitons, cher Maître, un heureux et intéressant voyage ...".

The next letter in the series was written by URYSOHN (and signed by him alone). It is dated Le Batz, 3 VIII, 1924. Batz is a small town an the southern coast of Brittany, not far west of St. Nazaire. The letter is interesting for its mathematical content; it demonstrates that the stimulus of FRÉCHET on URYSOHN was significant. I quote it all here except for the opening greeting and formal closing.

"M. Alexandroff et moi, nous venons de recevoir votre aimable lettre du 23 juin (adressée à Moscou), et nous vous sommes très reconnaissant pour les intéressants problèmes que vous avez bien voulu nous communiquer."

"Inspiré par le premier de vos deux problemes (relatif à l'expression la plus générale de la "distance" sur une droite[20]) j'ai trouvé quelques résultats qui me semblent assez intéressants."

"Les voici: j'ai construit un espace métrique *séparable* que j'appelle "espace métrique universel" on "espace U" et qui jouit des propriétés suivantes:

1. Quel que soit l'espace métrique séparable E, il existe dans U un sous-ensemble U_E *congruent* à E, c. à d. tel qu'il existe entre E et U_E une correspondence biunivoque et *conservant la distance*. U est donc, même au point de vue purement métrique, le plus grand des espaces métriques séparables, tandis que E_ω, l'espace de Hilbert et les autres espaces que vous indiquez dans votre Note, ne le sont qu'au point de vue topologique (D_ω possède, comme vous l'avez montré, la propriété 1., mais n'est pas séparable).

2. U est *homogène* en ce sens qu'étant donnés deux ensembles *finis* $(a_1, a_2, \ldots, a_n)$ et $(b_1, b_2, \ldots, b_n)$ situés dans U et congruente (c. à d. qu'on a $(a_i, a_k) = (b_i, b_k)$ pour tout couple i, k), il existe une transformation biunivoque et conservant la distance de U en soi-même, qui transforme a_i en b_i (pour tous les i en même temps).

3. U est complet (avec la distance donnée à priori).

4. U est le seul espace métrique séparable jouissant de toutes les propriétés 1, 2, 3 (c. à d. que tout autre espace de la sorte lui est congruent). Il existe par contre, des espaces ayant les propriétés 1 et 3 et non congruents à U.

"L'espace U (dont la construction est d'ailleurs assez compliqúee) resoût evidemment votre problème de remplacer D_ω (pour la "distance" sur une droite) par un espace séparable. J'ai d'ailleurs montré que ni E_ω, ni l'espace de Hilbert ne sauraient y être substitués (on peut toujours arranger la distance sur une droite de manière qu'il y ait 4 points 0, a , b, c tels que $(0, a) = (0, b) = (0, c) = 1$, $(a, b) = (b, c) = (c, a) = 2$; ce qui est impossible dans l'espace de Hilbert. Quant à E_ω, c'est un espace borné.)"

"M. Alexandroff et moi, nous voudrions vous remercier encore une fois pour votre si aimable concours, qui nous a donné la possibilité de venir en France."

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[20] FRÉCHET presented a paper on this subject at the International Congress of Mathematicians of 1924 in Toronto. See [FRÉCHET 97]. A fuller presentation on this subject appears in [FRÉCHET 103].

At the top of this letter written from Le Batz, FRÉCHET wrote the following: reçu le 18 Sept. (ou avant, pendant mon absence), répondu le ______: proposant demander insertion espace *U* journal français." So, by the time FRÉCHET saw the letter, URYSOHN had been dead about a month.

The next letter in the collection, written by ALEXANDROFF from Moscow on September 22, recounts the details of URYSOHN's demise. It is a moving letter. I quote it exactly in its entirety, including several missing accents on écrite, étais and était.

"Cher Maître, permettez moi de dire aussi Cher Ami! Je viens de recevoir votre Lettre de 18 août, votre Lettre ecrite le lendemain de la mort tragique de mon pauvre Paul Urysohn. Je ne sais pas si vous aviez reçu ma carte que je vous aie écrite de Paris, le 20 ou le 21 août; je vous ai envoyé aussi le numéro du "Populaire de Nantes" où se trouve exposé cet accident fatal."

"Nous nous sommes baignés comme chaque jour à Batz. La mer était très mauvaise mais nous étions des najeurs [sic] trop bons (malheureusement) pour que cela puisse nous effrayer. Une grande vague nous sépara l'un de l'autre de sort que mon ami arriva dans une petite baye, et moi, j'etais emporté en dehors, en pleine mer. Les minutes suivantes, le vent et les vagues m'emportèrent assez loin de l'endroit où nous nous sommes deshabillés, tandis que mon ami reussit de traverser la petite baye et saisissa dejà une grande pierre pour prendre terre; à ce même moment (comme on me racontait) une lame de fond [a ground swell] le saisissa et lui projeta, la tête contre le rocher où il voulait s'accrocher. J'étais à cet instant eloigné de quelques dizaines de mètres de lui, mais je puis tout de même prendre terre. Quand je suis accouru là, où nous nous sommes deshabillés (c'etait quelques secondes après la catastrophe) je l'apercevai ballotant dans l'eau; ça durait environ 20 minutes avant que je pouvais le trouver entre les vagues, le saisir et l'amener au bord—mais c'etait déjà trop tard—le docteur, qui etait déjà là ne pouvait que constater le décès."

"Il est enterré au cimitière de Bourg de Batz. M. Hausdorff nous appelait toujours "les inséparables;" nous l'étions en effet, et nous voilà maintenant separés pour toujours. Hier, dimanche, c'etait dejà 5 semaines que je suis privé de mon seul Ami, avec lequel j'avais tout commun—le travail, le repos, les voyages, toute la vie. Vous comprenez, cher Maître, qu'il y a des chagrins inconsolables, quand vraiment le coeur va se briser; c'est précisément mon cas maintenant."

"Paul Urysohn était agé de 26 ans; il a un père de 70 ans, dont il est le seul fils, et qui viendra l'été prochain, et peut-être même plus tôt visiter sa tombe; je voudrais maintenant du moins qu'il la trouve en ordre. Peut être puis je vous prier, cher Maître, de me rendre une grande service, à savoir d'écrire une lettre au Maire de la Commune de Batz (Loire-Inférieure) qu'il s'intéresse un peu de cette tombe, qu'on y met la pierre et la plaque du marbre qui est déjà expédiée de Paris. *Tout est payé d'avance*, il faut seulement qu'on fait tout ce qu'on a promis de faire. Pardonnez moi que je vous adresse cette prière de rendre quelque service à son séjour, maintenant éternel, en France ..."

"Eternel séjour en France—nous n'avons pas pensé que c'est ainsi que se terminera notre voyage en France qui était entreprit avec tant de joie, de bonheur, de vie. Maintenant tout est fini ..."

"Je vous ecrirai bientôt encore une lettre. Pendant une année au moins je m'occuperai exclusivement des travaux posthumes—extrêmement importantes de mon pauvre ami. Je vous en ecrirai encore des details. Agréez, cher Maître, mes tristes salutations. Tout à vous."

Paul Alexandroff

URYSOHN had been very industrious while at Le Batz; his long paper [URYSOHN 6] on the cardinality of connected sets had been completed by him on August 14 (the date and place appear at the end of the published paper, which was received by the editors of the Mathematische Annalen on August 23). Another paper [URYSOHN 7], partly written and fully sketched out in Le Batz, was prepared for publication by ALEXANDROFF and sent to the Mathematische Annalen in the following month. In each of these papers is to be found the famous 'URYSOHN'S Lemma', which enabled URYSOHN to give a simple proof of his result that a normal HAUSDORFF space satisfying HAUSDORFF'S second denumerability axiom is homeomorphic to a metric space (and therefore metrisable). In an earlier paper [URYSOHN 2] had shown, by a very complicated proof, that a HAUSDORFF space that is compact (in FRÉCHET'S sense) is metrisable if and only is it satisfies HAUSDORFF'S second countability axiom. Thus we see that FRÉCHET'S rather naive query about metrisability led to a number of interesting and high-powered answers by ALEXANDROFF and URYSOHN.

The result of URYSOHN about a universal metric space was written up by ALEXANDROFF for a note [URYSOHN 5], on which FRÉCHET commented in his paper [FRÉCHET 112], which I shall discuss briefly in connection with the later correspondence between ALEXANDROFF and FRÉCHET. Evidently the fuller account of URYSOHN'S work on the universal metric space was originally planned for publication in the Annales de l'Ecole Normale Supérieure, but for some reason this plan fell through, and the work was published elsewhere (see [URYSOHN 10]), but not until 1927.

ALEXANDROFF wrote to FRÉCHET once more in 1924, on November 10. I quote more than half of this letter, continuously from the beginning:

Mon cher Maître!

Excusez moi, je vous en prie, de n'avoir pas répondu jusqu'à présent a votre lettre, pour laquelle je vous remercie de tout mon coeur; vous, qui n'aviez pas connu personnellement mon ami et moi, vous avez trouvé néamoins les paroles pénétrant au fond de mon malheur. Je vous remercie encore de plus pour votre promesse d' écrire au Maire de Batz. Nous avions tant revé, l'année passée, de ce voyage en France, de la possibilité de faire votre connaissance—si nous pourrions penser de la cause qui nous empêchera de faire cette connaissance, si nous pourrions penser de la fin de ce voyage.

"Maintenant je chercherai toujours tous les moyens pour pouvoir passer un mois par année en France. En particulier, je me propose y aller l'été prochain: pour visiter Strasbourg, et pour visiter Batz—".

"Je vous prie de vouloir bien m'écrire, cher Maître: pendant l'été prochain, où comptez vous séjourner, pour que je puisse, maintenant moi seul, vous voir et vous parler, Peut être dans cette année les formalités des visas seront plus simples, le gouvernement des soviets étant reconnu par la France. Mais, dans le cas le plus pire, pourrai-je de nouveau compter sur votre precieux concours?"

"Je m'occupe maintenant exclusivement de ce qui a été laissé par mon pauvre ami. Son grand Mémoire sur la dimension des ensembles ("Mémoire sur les multiplicités Cantoriennes," I^er Partie: Théorie de la dimension des ensembles, Chapitres I–VI, plus de 200 pages) sera publié en VII et VIII tomes des "Fundamenta Mathematica." La seconde partie est actuellement en préparation, à laquelle je porte tous mes soins. Elle s'occupera de la théorie des Courbes Cantoriennes. Ce sera un mémoire à peu près aussi volumineux que la première Partie."

"Quant à son dernier travail sur l'espace métrique universel, il le voulait bien faire imprimer dans un périodique français, ce travail étant fait en France et sous l'influence d'un problème posé par vous. Peut être aurez vous l'obligeance de m'informer quelles peuvent être les perspectives à cet égard. Je voudrais aussi publier à coté de ce dernier travail de Paul Urysohn mon article sur les espaces complets, contenant la démonstration du critère *topologique* (que j'ai resumé dans ma Note des Comptes Rendus janvier passé pour y revenir dans un autre recueil) pour qu'un espace métrique séparable soit complet*.

* [Footnote in letter] En appelant système déterminant tout système de voisinages équivalent au système de tous les sphéroïdes de l'espace métrique, je dis qu'un système déterminant est *clos* si, pour toute suite descendente des voisinages $V_1 \supset V_2 \supset \ldots \supset V_n \supset \ldots$ tirés de ce système il existe au moins un point limite commun pour toutes ces V_n. Alors, pour qu'un espace métrique séparable soit complet, il faut et il suffit qu'on puisse de tout système déterminant extraire un système déterminant clos. [End of footnote.] Je voudrais dédier ce travail, auquel Paul Urysohn s'intéressait beaucoup, à sa mémoire. Si cette dernière publication présente quelque difficulté, je pourrai la faire dans les "Mathematische Annalen" ou dans la "Mathematische Zeitschrift" mais je dois l'avoue, je voudrais bien publier ce travail dans le même Recueil que le travail de mon ami. En tout cas, cela ne vous doit du tout gêner – enfin ce n'est qu'une raison absolument subjective, et je n'insiste sur elle d'aucune façon.

"Je ne sais pas si vous connaissez le théorème suivant de mon ami:

Pour qu'une classe (H) séparable soit métrisable, il faut et il suffit que tous deux ensembles fermés F_1, F_2 sans points communs puissent être séparés par deux domaines (= ensembles ouverts) G_1 et G_2, $G_1 \supset F_1$, $G_2 \supset F_2$, $G_1 \cdot G_2 = 0$.

"La démonstration (très simple et élegante) est actuellement sous presse dans les Mathematische Annalen.[21] Si vous désirez, je peux rédiger une courte Note contenant cette démonstration pour les Comptes Rendus ou pour un autre périodique français. Une conséquence immédiate de ce théorème est que la *séparabilité* est une condition nécessaire et suffisante pour qu'un espace topologique compact soit métrisable – théorème dont la première démonstration (Mat. Ann., 92) est très compliquée.

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[21] ALEXANDROFF must have been referring to [URYSOHN 7], although this paper deals with a normal HAUSDORFF space that satisfies the second countability axiom (and does not mention H-classes). However, a normal H-class is of necessity a normal HAUSDORFF space.

"Bien d'autres travaux, dans d'autres directions, sont laissés par Paul Urysohn. Je ne pense pas que j'employerai moins qu'une année pour les préparer à être imprimés."

In the remainder of this letter ALEXANDROFF told FRÉCHET about some of his own recent investigations.

10. Alexandroff and Fréchet after 1924

After the death of URYSOHN the correspondence between ALEXANDROFF and FRÉCHET went on quite actively. In the Archives there are thirteen communications to FRÉCHET in 1925, eleven in 1926, and six in 1927. Then the rate slacked off: two letters to FRÉCHET in 1928, two in 1930, one in 1932, and one in 1933. Only one other letter from ALEXANDROFF to FRÉCHET is known to me: that of October 21, 1967, cited on page 287 of my Essay I.

In reviewing this considerable collection of letters I shall comment on or quote from only those letters that contribute to my study of FRÉCHET. Anyone studying the roles of URYSOHN and ALEXANDROFF in the history of topology would need to give much more extensive attention to these letters.

In a letter of February 22, 1925, ALEXANDROFF describes in outline a methodology for developing a general theory of topology by groups of axioms. He envisages the use of neighborhood axioms to define elements of accumulation. Alternatively, one can use the RIESZ axioms about derived sets. He speaks of "Axiome quantitatif (= séparabilité)," by which I presume he means (as he has explained elsewhere) HAUSDORFF'S second axiom of countability. He then cites a number of theorems that can be obtained from the axioms mentioned. Next, he lists a series of four separation axioms of increasing strictness: (a) the one used by FRÉCHET for H-classes, (b) HAUSDORFF'S separation axiom, (c) the axiom of regularity, (d) the axiom of normality. He calls it remarkable that the axioms for a separable and normal H-class yield (as demonstrated by URYSOHN) "les espaces métriques séparables." Then he adds that, "un de nos étudiants, M. Tychonoff," has recently proved that URYSOHN'S result can be generalized by putting regularity in place of normality. Here is how he phrased the matter: "c. à d. que la Regularité (qu'on peut formuler aussi en disant que tout $U(x) \supset$ un $\overline{V(x)}$) exprime la condition *définitive* necéssaire et suffisant pour qu'une (H) séparable soit un espace métrique."

As we shall see later, the things I have just quoted from ALEXANDROFF'S letter appear in a paper written by two of the students of ALEXANDROFF and URYSOHN (of whom one was TYCHONOFF), and I think we can infer that the paper, as well as this letter of February 22, indicate that, in the seminar that ALEXANDROFF and URYSOHN had been conducting in Moscow, they were pulling together ideas from both FRÉCHET and HAUSDORFF and adding their own insights and discoveries.

The final part of this letter of February 22 is especially interesting because of his expression of the view that the true domains of existence of topological objects are compact and separable metric spaces. Here is how he put it: "Enfin, si on ajoute encore l'axiome de compacticité on obtient les espaces métriques compacts et séparables dans lesquels *toute* la théorie des ensembles et *toute la Topologie* intrinsèque est valable. On peut ainsi considérer cette dernière classe d'espaces comme le vrai domaine d'existence de tous les êtres topologiques. (Pour la théorie des ensembles proprement dit il suffit que l'espace métrique séparable soit complet); et on peut la définir par les axiomes formant une échelle très naturelle."

In a letter of March 17, 1925, evidently in response to a question from FRÉCHET (whose letter of March 3 ALEXANDROFF acknowledges), ALEXANDROFF wrote: "La question que vous voulez bien me soumettre se resout comme je le crois, par négative. Il suffit évidemment de construire pour s'en apercevoir un espace accessible (= une classe (H)) vérifiant la $4^{\underline{me}}$ condition de M. Riesz (sur la séparation des points limites d'un ensemble), et qui n'est pas un espace topologique." ALEXANDROFF describes the counterexample and elaborates some of the details of the argument.

There is also in this letter an indication that FRÉCHET had suggested to ALEXANDROFF that he and his student TYCHONOFF should write up for publication in France something about TYCHONOFF'S work done in ALEXANDROFF'S seminar on topology. What happened as a result, apparently, was that TYCHONOFF and another student in the seminar, named VEDENISOFF, wrote a joint paper [TYCHONOFF & VEDENISOFF] that was published in France in 1926. More about this paper later.

In the letter of March 17, in response to FRÉCHET'S indication that he would like to know which of his own publications were lacking in Moscow, ALEXANDROFF sent a list of those that he knew of which *were* in Moscow. Concerning the Esquisse, he wrote amusingly as follows: "Votre Esquisse de Calcutta (exemplaire, en quelque sorte exproprié de chez M. Sierpinski – d'après des méthodes de mon pays! – : M. Sierpinski a bien voulu de nous envoyer temporairement ce mémoire, mais étant le seul exemplaire à Moscou, il reste ici déjà quelques années et je ne crois pas qu'un traité international quelconque pourra faire rendre dans un intervalle borné de temps, cette dette à Varsovie)."

When ALEXANDROFF wrote next (on May 5, 1925), he was at Blaricum, in the Netherlands. Through the efforts of L. E. J. BROUWER he had received from the Rockefeller Education Board a grant in support of his study and research. He asked FRÉCHET to help him again to obtain a visa to go to France. He was continuing his efforts with the posthumous works of URYSOHN. In this connection he wrote: "Un des premiers travaux que je vais maintenant préparer pour l'impression sera le mémoire sur l'espace universel. Je partage entièrement votre point de vue à savoir qu'il serait très intéressant de donner une définition directe de l'espace U sans se servir de la construction donnée par Urysohn. Il me semble que cette question est assez difficile.[22] In his paper [FRÉCHET 112] FRÉCHET commented on the desirability of having a more concrete presentation of URYSOHN'S universal separable metric space and of avoiding the explicit use of URYSOHN'S "abstract space." Evidently FRÉCHET had communicated this thought to ALEXANDROFF. Further ideas of FRÉCHET on this subject appear on pages 99–100 of his book [FRÉCHET 132].

In this letter, also, there is a paragraph that indicates that FRÉCHET had at

[22] For more discussion of the opinions about unsatisfactory aspects of URYSOHN'S definition of his universal metric space see pages 84–85 in [ARBOLEDA 1].

some earlier time written to ALEXANDROFF about a M. TAMARKINE. Here is the paragraph: "Je ne pouvait rien faire en ce qui concerne M. Tamarkine et je ne pouvais même vous rien écrire sur ce suject en étant en Russie: M. Tamarkine a, en effet, quitté la Russie d'une façon non légale (sans passeport) et j'aurais pu avoir des grandes difficultés s'il résulterait de ma correspondance que j'aie des relations quelconques avec M. Tamarkine. Je ne connais pas l'adresse de M. Tamarkine; je pense qu'il est en Amérique." I presume this refers to J. D. TAMARKIN, who *did* settle in America, and whose departure from Russia in the company of A. S. BESICOVITCH made quite a story.

In the next letter (of date June 5) it is evident that FRÉCHET has seen and commented back to ALEXANDROFF on the manuscript of the paper by TYCHONOFF & VEDENISOFF, which ALEXANDROFF is now sending back to FRÉCHET after making some revisions. He writes: "Je refais le manuscript de M. M. Tychonoff conformement aux indications que vous avez bien voulu me faire. C'est seulement un point où je me permets de ne partager entièrement votre point de vue: vous préférez toujours les ensembles compacts (situés dans des divers espaces), tandis que, Urysohn et moi, nous avons toujours étudié les espaces compacts (resp. bicompacts) eux-mêmes. Et cela par des raisons suivantes. Tout d'abord, la propriété d'un ensemble être compact dans un espace n'est pas un propriété intrinsèque de l'ensemble, mais une propriété caractérisant seulement la façon de la situation de l'ensemble dans l'espace donné, c'est pourqui, la droite infinie p. ex. qui n'est pas compacte (dans le plan, ou, si l'on préfère, en soi-même) est nêamoins homéomorphe à l'intervalle ouvert quelconque, situé sur cette droite et qui est bien compact. En suite, on ne connait que peu des propriétés intéressantes concernant les ensembles bornés les plus générales (situés, p. ex. dans le plan euclidien) bien qu'ils soient compacts. Quand on veut avoir des propriétés topologiques plus précises, on doit se borner à l'étude des ensembles qui sont compacts *en soi*, c. à d. des ensembles bornés et fermés. Qu'est ce qu'on appelle la Topologie contemporaine des continus?—telles qu'elle se présente dans les recherches be Brouwer (sur la dimension), de Janiszewski, de Sierpinski, Mazurkiewicz et d'autres Polonais, et surtout dans les recherches d'Urysohn que vous n'avez pas encore eu la possibilité de voir, et qui constituent toute une ère nouvelle dans notre science?—il me semble que ce n'est aucune que l'étude systématique des classes (*D*) connexes et compactes en soi. Et c'est précisement vous, cher Maître, qui avez rendu possible cet éclat des découvertes nouvelles ayant eu données vos définitions de l'espace métrique compact, qui comblait précisément la lacune logique qui, si elle resterait, tournerait à l'impossible toute théorie vraiment profonde et générale.

"C'est aussi la propriété de la compacticité en soi qui a rendu necéssaire de remplacer dans beaucoup des questions (p. ex. dans toute la théorie des fonctions analytiques d'une variable complexe) le plan ordinaire par "le plan des variables," c. à d. par une sphère. On pourrait poursuivre très loin ces avantages des espaces compacts, mais je n'ose pas d'ennuyer votre attention par ces choses. Enfin, nous devons tous à vous l'un et l'autre sorte de compacticité, et c'est votre droit, cher Maître, de préférer celle parmi vos créations, qui vous fait plus de plaisir!

"Si vous trouverez, dans la nouvelle rédaction du travail encore quelques modifications à faire, surtout dans les questions de terminologie, vous avez sans doute une carte blanche de ma part. Seulment, je voudrais conserver *quelques fois* l'expression 'l'espace topologique de M. Hausdorff,' ou 'e.t. au sens de M. Hausdorff,' parce que nous nous sommes toujours servis de cet adjectif dans nos publications antérieurs. Mais, certainement, je ne vois là aucune question importtante."

In closing the letter ALEXANDROFF mentions that PAUL URYSOHN's father has received a French visa, thanks to FRÉCHET'S intervention.

From the foregoing letter of ALEXANDROFF it can be seen that he is solicitous in paying homage to FRÉCHET's pioneering role in abstract topology. At the same time, from this and an earlier letter it is evident that ALEXANDROFF thinks the most interesting part of topology, currently, has to do with compact metric spaces. In this respect ALEXANDROFF differs greatly from FRÉCHET, whose interests remain on the very general aspects of topology and seldom focus on highly specific or 'concrete' issues. (As we shall see presently, FRÉCHET's interest in dimension theory was an exception.)

The next letter, of date August 31, 1925, was written from Le Batz, where ALEXANDROFF was mixing mathematical work with time spent at the beach. He said he found the people there very congenial. "Je connais tout ce petit bourg, et tout le monde connait moi, je me sens ici comme à la maison. Surtout je suis ému par la touchante attention qu'on porte toujours ici á la mémoire de mon pauvre, dont la tombe est souvent visitée par diverses personnes qui y apportent des fleurs."

In this long letter, written with a pencil, ALEXANDROFF addresses himself to five issues that were brought up in a letter of August 22 that FRÉCHET had written to him. The subjects running throughout this part of the letter are dimension theory and FRÉCHET's "type de dimension." My friend ARBOLEDA has commented on parts of this letter on pages 362 and 367–368 of his paper [ARBOLEDA 3]. Because I am not dealing with FRÉCHET's work on dimension theory I pass on to other things.

Near the end of the letter ALEXANDROFF says he expects to remain in France at least until October and that doubtless he will come to see FRÉCHET again in Strasbourg (thus indicating that he had visited there earlier in the summer). He did go to Strasbourg again, as is shown by his letter of November 29, in which he apologizes to FRÉCHET for not having written to him after leaving Strasbourg.

The next letter (dated September 8 in Le Batz) is much taken up with more of ALEXANDROFF'S comments on what FRÉCHET has written about the dimension theories of URYSOHN and MENGER and relationships with FRÉCHET's 'type de dimension.' There is also reference to the expected arrival of "votre manuscrit, qui m'intéresse au plus haut degré." In a later letter (of September 29), written from Collioure, in the Pyrenées Orientales, where ALEXANDROFF had gone to walk and climb, he wrote to FRÉCHET: "j'ai viens de recevoir votre manuscrit sur les nombres ordinaux et sur les types locaux de dimension. Je trouve votre exposé réussi d'une façon si excellente que je ne vois aucune amélioration possible." He then made a couple of comments on details and continues "Voilà c'est tout que j'ai à vous dire au propos de cette partie de votre Livre." It is easily inferred that at least part of the manuscript in question eventually appeared on pages 110–113 of FRÉCHET's book on Abstract Spaces. See also [FRÉCHET 126], which is identical to part of the book.

In a prior letter (of September 21) from the Pyrenees, ALEXANDROFF wrote at some length, evidently in response to something FRÉCHET had said about the use of the term 'séparable' in the manuscript by TYCHONOFF and VEDENISSOFF. Their usage was that of ALEXANDROFF and URYSOHN, a usage different from that of FRÉCHET, but equivalent to it when applied to metric spaces. I quote from the letter: "Dans tous mes travaux sans aucune exception j'ai employé le mot séparable toujours dans le sens d'existence d'une famille au plus dénombrable de voisinages définissant l'espace total. Ce sens est identique avec l'existence d'une sous ens. $\leq$ denombrable partout dense seulement pour les espaces métriques. J'ai mentionné aux plusieures reprises (par ex. dans mon article "Ueber die Metrisation der im kleinen kompakten top. R." Math. Ann. 92 où tout un paragraphe: Das II Abzählbarkeitsaxiom und die Metrisierbarkeit der Räume[23] est consacré à cette question) que l'existence d'une sous ens. dénombrable partout dense n'entraîne en général nullement la séparabilité (au sens ci-dessus indiqué) non seulement dans l'espaces V les plus généraux mais même dans les espaces *bicompacts* et topologiques (au sens de Hausdorff), (donc *normaux*) et même vérifiant le I Abzählbarkeitsaxiom de M. Hausdorff. Dès la première lettre que Urysohn et moi nous vous avions écrit, j'ai appelé votre attention sur ce fait, et comme jamais vous n'avez exprimé aucune opinion différente, j'estimais toujours que vous même, cher Maître, aviez toujours en vue cette définition de la séparabilité *quand* il s'agit des *espaces* V. En effet, en introduisant cette belle notion de séparabilité, qui vous est entièrement due, vous avez, sans doute, cherché a généraliser, pour les espaces V quelconques, la propriété des espaces élémentaires (des espaces *D* pour fixer les idées) de posséder un sous ensemble dénombrable dense. Or, il est aisé de voir, que c'est *précisément* l'existence d'un système dénombrable de *voisinages* définnisant l'espaces qui est une vraie généralisation en question. Cette dernière existence est *dans les classes D* équivalente à l'existence d'un sous-ensemble dénombrable [the intended word 'dense' is omitted here], tandis que dans les espaces plus généraux, il c'est (*sic*) facile de prouver par des exemples que l'existence d'une sous ensemble dénombrable dense se montre comme *une propriété tout à fait accidentelle.*

"Si vous êtes de mon avis, comme la définition de séparabilité dans le sens employé dans la note de MM. Tych. et Ved. se trouve bien précisée dans leur article, il me semble que rien n'est à changer dans cet article, si cela n'est pas peut-être une petite note qu'on pourrait adjoindre en bas de la page correspondante, où on peut indiquer que l'existence d'un sous-ens. dén. dense n'entraîne en général, la séparabilité que dans les cas des espaces *D*."

This long explanation of the meaning attached to the notion of separability by ALEXANDROFF and URYSOHN is somewhat impatient and testy in tone. The possible justification for ALEXANDROFF'S impatience cannot be judged in the absence of precise knowledge of what FRÉCHET had written to him. Nor can one be sure how the manuscript of TYCHONOFF & VEDENISSOFF was worded in the form of it seen by FRÉCHET before final revision and publication. (I will discuss the pub-

[23] ALEXANDROFF'S memory of the title of the paragraph was slightly inaccurate. It begins on page 297 of the paper [ALEXANDROFF 4].

lished version presently.) It is possible that FRÉCHET objected to applying the term 'séparable' in a situation where it was not equivalent to the meaning of the term given by him in his paper [FRÉCHET 75]. ALEXANDROFF'S memory was faulty when he claimed that, from the very first of the letters he and URYSOHN wrote to FRÉCHET, he had called FRÉCHET'S attention to the distinction between HAUSDORFF'S second denumerability axiom and FRÉCHET'S notion of separability. The first letter (that of October 23, 1923) certainly does not contain anything of the kind. It does mention "une classe (*D*) séparable," but contains no definition or comment on the word 'séparable'. In the paper to which ALEXANDROFF refers in volume 92 of the Mathematische Annalen the word 'separability' never occurs, although the distinction is made between a space possessing a denumerable dense set and one satisfying HAUSDORFF'S second denumerability axiom. In the letter of November 22, 1923, URYSOHN wrote (as I have quoted earlier): "En ce qui concerne le terme *séparable*, c'est votre nouvelle définition que nous avions en vue." This was written after ALEXANDROFF and URYSOHN had received *just one* letter from FRÉCHET, and it is evident that the latter had asked some question about their use of the word 'séparable.'

If we examine the paper by TYCHONOFF & VEDENISOFF to see how the matter is treated there, we find the following: They define "une espace (V)" in the very general way used by FRÉCHET in his Esquisse. After explaining the notion of equivalent systems of neighborhoods they single out those spaces (V) in which, among all the equivalent systems of neighborhoods there is a system with a denumerable family of neighborhoods, and of these spaces they say: "En se servant d'une dénomination due à Fréchet, nous appellerons ces espaces, espaces (V) séparables." Later they emphasize that their definition is one "qui diffère d'ailleurs de la définition primitive de M. Fréchet." This decision, by ALEXANDROFF and his group, to appropriate the word 'séparable' from FRÉCHET and give it a different meaning, was not conclusive so far as subsequent usage has been concerned. Many, perhaps most, writers on topology continue to follow FRÉCHET in the definition of separability.

I have already noted, in connection with the letter of November 22, 1923, that ALEXANDROFF and URYSOHN were greatly interested in the unexpected simplification that was afforded by the use of *H*-classes. Further indication of the appreciation in Moscow of *H*-classes is afforded by the following quotation from the paper of TYCHONOFF & VEDENISSOFF (on page 19): "Les espaces accessibles forment donc une construction logique non seulement très naturelle, mais vraiment logiquement indispensable. Dans les espaces accessibles ont lieu toutes les propriétés élémentaires formant la théorie des ensembles fermés; mais pour aller loin dans l'ordre d'idée topologique, il faut introduire une suite nouvelle d'axiomes; chacun de ces axiomes sera plus restrictif que le précedent." They are here referring to the several separation axioms: that of HAUSDORFF and the axioms of regularity and normality. The recognition given to FRÉCHET's work in this paper no doubt pleased him, but he was progressing little, if at all, as a topologist, while his younger contemporaries were going forward in significant ways.

In their discussion of bicompactness (on page 23) TYCHONOFF & VEDENISSOFF made an error that was not noticed by either ALEXANDROFF or FRÉCHET, who read the manuscript; FRÉCHET also read the proof sheets. After the paper was published this error and a correction of it became a matter of correspondence between ALEXANDROFF and FRÉCHET, as I shall indicate further on.

In a letter of January 26, 1926, ALEXANDROFF asked FRÉCHET to send him copies of three of his papers on topological affine spaces ([FRÉCHET 109], [FRÉCHET 118], [FRÉCHET 120]). By way of explanation for the request he wrote: "Je m'intéresse surtout pour ces travaux, parce qu'il me semble qu'on y pourrait tirer peut-être une méthode conduisant à la resolution du problème suivant que j'ai posé (dans ma conférence faite a la Société Mathématique de Göttingen) l'été dernier: Quelles sont les conditions nécessaires et suffisantes pour qu'un espace métrique (classe (D)) avec une *définition fixée* de distance, (starre Entfernungsdefinition) soit *congruent* (= isométriquement représentable) au plan euclidien ordinaire (avec la distance ordinaire)? Ce problème a surtout appelé une certain attention de M. Hilbert qui y voit une possibilité d'une fondation toute nouvelle des principes de géométrie."

The next paragraph is of particular interest for what it shows about ALEXANDROFF'S thoughts about abstraction and more concrete sorts of mathematics. One may speculate as to whether he was speaking solely about his own views, or whether he intended the suggestion to be taken seriously by FRÉCHET as well. Here are his words:

"Je crois en général que le temps est venu pour descendre des hauts cimes de la pure abstraction dans l'espace ordinaire et de montrer comment toutes les géométries connues (celle de Euclide, de Lobatschweski etc.) sont des cas particuliers de vos théories générales, c. à d. d'indiquer comment *peut on obtenir* ces géométries classiques par une spécialisation systématique des axiomes de l'espace métrique. Il me semble que ce problème est maintenant tout à fait à l'ordre du jour."

In this letter, also, it is revealed to us that FRÉCHET has sent to ALEXANDROFF some of the manuscript of his book on abstract spaces in hectographed form. ALEXANDROFF says he hasn't yet had time to make a careful study of the material, but that he intends to do so and wants FRÉCHET to keep on sending the subsequent chapters.

ALEXANDROFF'S letter of February 18, 1926 opens with a discussion of some aspects of the manuscript of the posthumous paper [URYSOHN 9]. From the discussion one can see how the footnote in this paper that I discussed in Section 9 came into being. I reproduce this piece of correspondence because it is a good example of evidence that FRÉCHET was rather touchy about appropriate recognition of his own role in connection with a piece of mathematics written by someone else. I give other examples elsewhere. I think it has to be assumed that the opening part of this letter from ALEXANDROFF was triggered by something in a letter from FRÉCHET.

"Je vous envoie la remarque concernant l'article d'Urysohn sur les espaces (L). Je me permis d'ajouter qu'Urysohn n'a pas connu votre Mémoire, parce qu'autrement on lui pourrait reprocher, peut-être, de publier un travail trop voisin, en ce qui concerne le résultat principal, à un travail déjà publié—je ne crois pas que maintenant cette reproche pourrait avoir lieu, puisqu'il s'agit d'un travail posthume, publié plutôt par des raisons en moitié métodologiques (sic), en moitié par la raison de donner une image complète des intérêts et de l'action scientifique de son auteur, *sans aucune prétention* de priorité (j'ai signalé explicitement cette dernière circonstance, bien qu'il me paraissait presqu'inutil de la signaler—car tout prétention de cette sorte serait tout à fait absurde dans ce cas).

"Ursyohn n'ayant pas connu votre Mémoire, il y aurait une difficulté, à mon avis, d'insérer cette remarque au corps même de l'article: il me parut donc préférable d'en faire un note en bas de la page, de façon qu'on aurait le passage suivant:

* Quand Paul Urysohn—etc.

* Ces inconvenients provenant de la même source, on peut les supprimer d'un façon radicale par introduction d'un nouvel axiome—etc.

"Bien entendu, si vous attribuez une valeur quelconque à conserver sans aucune modification *votre* rédaction de cette remarque (qui serait alors inseré dans l'article lui-même, non en note), je me déclare de ne posséder aucune objection—si je considère, peut-être, mon projet comme préférable, cela ne veut du tout dire que je ne pourrais pas m'adjoindre parfaitement à votre projet, les deux projets étant d'ailleurs presque identiques.

"Je voudrais vous dire, mon cher Maître, encore un mot au propos de cette question. Je suis sûr, que si Urysohn avait pu rédiger lui-même cet article, il l'aurait mis complètement à votre disposition (de même que je l'aurais fait moi-même si un pareil article était écrit par moi), en ce sens, qu'il n'aurait le publié que dans le cas où vous le considériez comme assez intéressant pour ce dernier but. Aussi suis-je sûr, qu'il y apporterait toute modification que vous jugiez propre à le perfectionner (dans un sens quelconque).

"C'est seulement par cette raison que j'ai vous proposé d'apporter vous-même, des modifications nécessaires; j'étais donc très éloigné de la pensée de me rétirer du travail ou de la responsabilité nécessaire.

"C'est aussi par cette raison que je vous prie de demander l'impression de cet article seulement si vous estimez que, *même après* votre mémoire, l'article d'Urysohn a conservé une certaine partie de son intérêt (ne soit ce qu'au point de vue méthodologique), suffisante à elle seule pour la publication."

The rest of this letter is interesting for a different reason, namely, the indication it gives of FRÉCHET'S persistent interest in questions about H-classes. ALEXANDROFF'S letter continues: "Je vous remercie bien pour les deux problèmes intéressants que vous me signalez. Ne pourrait on d'alleurs voir la solution d'un de ces problems dans la définition suivante des espaces réguliers (définition dont nous sommes entretenus l'été dernier); un espace accessible est dit régulier, si on obtient un système de voisinages definissant cet espace en considérant les ensembles fermés $\overline{V(x)}$ au lieu des ensembles ouverts $V(x)$ (où $V(x)$ est un voisinagae ouvert quelconque du point x, c. à d. p. ex. un *ensemble ouvert* quelconque contentant x). Toute fois, pour les espaces normaux votre problème reste entier."

In a letter of February 28, 1926 ALEXANDROFF, evidently responding to some reaction from FRÉCHET about his use of the greeting 'cher Maître,' wrote as follows: "Je voudrais vous écrire encore un mot sur la question du cher Maître. Si je me permis de vous nominer toujours ainsi, sans avoir la droit formel de le faire, puisque je n'avais pas eu l'honneur d'être votre élève, c'est que je crois, toutes les personnes qui s'occupent de la théorie des espaces abtraits sont, au sens large, vos disciples, puisque vous étiez le premier qui aviez introduit dans la science cette discipline toute nouvelle. (Au propos, Flaubert écrivait jusqu'à la fin à George Sand en l'appelant chère Maître de *même qu'à Edmund de Goncourt*, à l'e final près.* Or, il savait, a cette époque, voler de ces propres ailes bien mieux que je ne le puisse!!!). *Voir p. ex. Gustave Flaubert, Correspondence, quatrième série (1869–1880), Paris, Eugène Fasquelle, éditeur, 1917.

"Aussi voulais-je toujours sousligner (sic) un peu le caractère très respectueux de mon amitié envers vous, qui s'impose, il me semble, tout naturellement. Ce n'est pas donc pour vous rendre plus vieux que j'ai choisi cette forme plus respectueux.

"Comme vous voyez, je me suis très bien défendu! Mais vous voyez aussi, que j'ai accepté de ne pas vous écrire cher Maître, à condition, que ce soit placé, pour ainsi dire, *devant* les parenthèses à y enfermer toutes mes lettres."

On April 3, 1926, ALEXANDROFF wrote from Berlin to FRÉCHET and commented on some things in the manuscript of FRÉCHET'S book. "C'est vraiment une grande joie de lire cet exposé tout à fait artistique; je ne crois pas qu'on pourrait exposer d'une façon plus esthétique et en même temps d'une façon si expressement philosophique ces idées, qui deviendront enfin le bien commun à tous les mathématiciens.

"Si vous me permettriez cependant d'exposer, sur quelques points de détail, mon gout personnel, j'aurais peut être préféré de sacrificier tout à fait la condition 4° de M. Riesz; il me semble que cette condition, si peu intuitive, présentera des difficultés au plusieurs lecteurs non familiarisés avec la théorie des espaces abtraits, et, ce qui est pire encore, que ces difficultés seront tout à fait inutiles, la condition 4° de M. Riesz n'intervenant point dans l'exposition postérieure. Aussi il me semble que la discussion detaillée de la condition de M. Hedrick est peut être, à *l'heure actuelle*, superflue; les espaces accessible une fois introduites, il me semble que trop de détails sur les intermédiaires (entre espace accessible et l'espace topologique le plus général) pourraient seulement disperser l'attention du lecteur, en l'attirant du chemin directe."

The letter also contains other mildly critical remarks on some details. It can be inferred from the context indicated in the letter that ALEXANDROFF was examining the part of the book comprising about pages 157–187 as printed in 1928.

ALEXANDROFF'S letter of April 14, 1926 is long (seven large pages) and full of technical mathematical discussion, centering on a matter concerning which the paper of TYCHONOFF & VEDENISSOFF was in error (as I mentioned earlier in discussing the paper). I quote selectively from this letter with two objectives in mind. One point revealed by the letter is that FRÉCHET seemed to require ALEXANDROFF'S help in reasoning out things; the other point (begun in this letter and carried on in some later ones) is the historical interest in seeing how ALEXANDROFF dealt with the ideas that originally came together in the formation, by ALEXANDROFF and URYSOHN together, of the notion of bicompactness. I begin by quoting a part of the paper of TYCHONOFF and VEDENISSOFF. They are referring to a proposition on page 259 in [ALEXANDROFF & URYSOHN 2].

"MM. Alexandroff et Urysohn ont demontré un théorème analogue:

Appelons point d'accumulation complète d'un ensemble M tout point ξ tel que pour tout $V(\xi)$ la puissance de l'ensemble $M.V(\xi)$ soit égale a celle de l'ensemble M tout entier. Les trois propriétés suivantes sont équivalents dans les espaces (V):

A. Tout ensemble infini possède au moins un point d'accumulation complète.

B. La partie commune aux ensembles d'une suite (dénombrable ou non) d'ensembles fermés décroissants est non vide.

C. De tout système d'ensembles ouverts recouvrant l'espace, on peut extraire un nombre fini d'ensembles jouissant de la même propriété (Théorème de Borel-Lebesgue).

Les espaces (V) vérifiant une de ces conditions et, par conséquent, les deux autres, sont nommés (d'apres MM. Alexandroff et Urysohn) espaces bicompacts."

In the letter of April 14, ALEXANDROFF writes: "Quant à l'équivalence des propriétés A, B, C, du § 4 de l'article de MM. Tychonoff et Vedenissoff, c'était une erreur de la supposer vraie dans les espaces (V) les plus généraux: *elle ne l'est que dans les espaces H*; cette erreur qui s'est glissée dans leur travail (et dont moi-aussi, je porte la reponsabilité de ne pas l'avoir remarqué au juste temps), j'ai la signalée (il y a 1 ou 2 mois) dans une lettre écrite à ce propos à M. Gauja, où je l'ai prié d'apporter (à la fin de l'article de MM. T. et V.) une *correction* spéciale. Je n'ai pas vu encore ni la tome correspondant du Bull. Sc. Math., ni de tirés à part de cet article, mais j'espère que M. Gauja a pu accomplir ma demande, puisqu'il n'a donné aucume reponse à ma lettre." [M. GAUJA was the secrétaire de la rédaction of the Bull. Sci. Math. No correction appeared with the article.]

ALEXANDROFF then wrote that the proof of the equivalence in question for H-classes was entirely analogous to the proof for HAUSDORFF spaces, as sketched in [ALEXANDROFF & URYSOHN 2], and that it would be given in detail in the memoir by him and URYSOHN that he was preparing for publication in Amsterdam (this appeared, after much delay, as [ALEXANDROFF & URYSOHN 3]). He went on to say that, for general V-classes, properties B and C were equivalent and that property A implied both property B and property C, although an example shows that a V-class can have property C but not A. Consequently, said ALEXANDROFF, if one understands bicompactness to mean property A and perfect compactness to mean property B, then the concepts of bicompactness and perfect compactness are not the same for the most general V-classes. I forego discussing the most technical part of the letter, which goes into considerable detail to explain certain things to FRÉCHET.

This line of discussion was continued in the letter of April 22, which plunges immediately into an answer to a letter from FRÉCHET:

"Quant à la question d'équivalence des 2 propriétés dans un espace V quelconque, de celle que vous désignez comme parfaitement compact et celle de Borel-Lebesgue,[24] je n'ai pas réussi de me faire une opinion précise sur ce sujet. La question est autant plus difficile pour moi, puisque, dans le cas de espaces V quelconque, la propriété que vous designez comme parf. comp. est loin d'être équivalente à celle que j'entends sous bicompact: en effet, vous éxigez l'existence d'un point commun aux ensembles donnés d'une suite monotone où a leurs dérivés, tandis que, moi, je n'éxige que l'existence d'un point commun a tous les ensembles d'une suite monotone, d'ensembles fermés. Or, un ensemble dérivé n'étant pas, en général, fermé, dans un espace V, la notion d'un ensemble parfait. compact (dans votre sens) est *plus restrictive* que la notion d'un ensemble bicompact.

"La même différence se manifeste au cas du Th. de B.-L.: Vous preférez de considérer de familles *F d'ensembles quelconques* tels que tout point de l'ensemble donné est intérieur a un ensemble au moins, appartenant à la famille F, tandis que moi, je ne considère que les familles d'ensembles *ouverts*. Toutes ces differences deviennent illusoires dans les espaces accessibles (*puisque là l'ensemble de points intérieurs à un ensemble quelconque est toujours ouvert*), mais dans le cas présent des espaces V les plus généraux, il s'agit au fond de 4 propriétés deux à deux différentes."

Later in the letter ALEXANDROFF once again asks FRÉCHET for help in getting a visa to enable him to go to France. He has been trying, unsuccessfully, to get the visa while in Berlin.

FRÉCHET continued to seek ALEXANDROFF'S help in his understanding of the equivalence of the three properties A, B, C (which were described in the letter of April 14). In a letter of April 29 ALEXANDROFF repeats FRÉCHET'S question: Does the proof of the equivalence of properties A, B, C in accessible spaces make use of the property called condition 5° by FRÉCHET (it is the condition that every derived set is closed)? ALEXANDROFF writes that he thinks the best response is to reproduce the complete proof of the equivalence of A, B, C in H-classes, thus permitting FRÉCHET to see clearly where each property of H-classes enters into the argument. He then gives the demonstration, in which he uses well-ordering and transfinite numbers.

Further on in this letter there is something more of interest about bicompactness. I quote:

"Au propos: vous m'écrivez du malentendu avec l'emploi du mot 'bicompact.' La vraie source de ce malentendu (ou lapsus) est la suivante. En étudiant les espaces compacts, Urysohn et moi, nous nous sommes bornés à priori par la considération des espaces (H) (même, d'abord des espaces de M. Hausdorff, puisque

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[24] For more about the letters of April 22 and 29, 1926 in relation to bicompactness and to the BOREL-LEBESGUE property, see page 78 in [ARBELODA 1].

nous n'avons pas connu a cette époque là l'existence des espaces (H)). Pour ces espaces, les trois propriétés (A), (B), (C) sont équivalentes. Apres avoir demontré cette equivalence, nous avons appelé bicompacts les espaces où une quelconque, et par conséquent les deux autres des propriétés (A), (B), (C) se trouvent vérifees.

"Dans les espaces (V) l'équivalence des proprietes (A), (B), (C) cesse d'être vraie, c'est pourquoi *je ne sais point*, pour vrai dire, qu'est ce *qu'un espace* (V) *bicompact*! C'est pourquoi j'emploi cette expression moi-même une fois dans un, l'autre fois dans l'autre sens. Je crois, qu'il serait juste d'appeler bicompacts ceux-ci parmi les espaces (V), où les trois propriétés mentionnées se trouvent vérifiées *en même temps*.

"Quant à vos autres questions: La proposition d'Urysohn que tout espace de Hausdorff compact et séparable *au sens strict* est métrisable, cette proposition ne reste pas vraie pour des espaces (H) les plus généraux. Exemple: L'espace E est formé d'une infinité dénombrable de points isolés $c_1, c_2, c_3 \ldots$ et des deux points a, b. Le voisinage quelconque $V(a)$ de a, de même qu'un voisinage quelconque $V(b)$ de b est formé de ce point et de tous les points c_n sauf un nombre fini quelconque d'entre eux. Cet espace est un espace (H); il est séparable au sens strict; il est compact. Il n'est pas un espace (D), puisqu'il n'est pas même un espace (L), la suite $c_1, c_2, c_3, \ldots$ etant, dans cet espace, convergente vers les deux points a et b."

The phrase 'séparable au sens strict' has been used before by ALEXANDROFF to mean separable in the sense he preferred – *i.e.* that the space satisfies the second axiom of countability. By 'séparable au sens large' ALEXANDROFF means FRÉCHET'S separability.

Other interesting remarks from ALEXANDROFF in the letter of April 29 were the following:

"Tout espace (H) peut être transformé en un espace (H) bicompact par l'adjonction d'un seul point. En effet, soit E un espace (H) absolument quelconque. Formant l'espace $E + \xi$ en laissant invariable les voisinages des points x de E, et en donnant au point ξ comme voisinages les ensembles $V(\xi) = \xi + \Gamma$, ou Γ est l'ensemble de tous les points de E, sauf un nombre fini quelconque d'entre eux. On voit de suite que $E + \xi$ est bicompact. On peut évidemment dire aussi que tout espace (H) peut être obtenu en supprimant un seul point dans un certain espace (H) bicompact, de sorte que la propriété, qui, dans les espaces de M. Hausdorff, caractérise les espaces localement bicompacts, appartient, dans les espaces accessibles, à tous les espaces sans exception."

On July 21 of 1926 ALEXANDROFF wrote FRÉCHET from Göttingen: "J'ai donné pour la durée de mon séjour ici votre manuscrit à M. Hildebrandt [T. H. HILDEBRANDT, of the University of Michigan]; pendant les vacances j'aurai enfin la possibilité de la lire en toute attention." Then, on October 15, he wrote again to say: "M. Tychonoff est maintenant occupé (avec d'autres jeunes mathématiciens de Moscou) de lire (après moi) la dactylographie de votre Livre. Quand ils sera prêt, je vous enverrai infin les remarques que nous avons faites."

The promised remarks attributed to NIEMYTSKY, TYCHONOFF, and WEDE-

NESSOFF, now spelled with a W rather, than a V, were sent with a letter dated December 25 in Smolensk (where ALEXANDROFF'S mother lived). Before turning to those remarks I wish to quote from the letter something about the then forthcoming new edition of HAUSDORFF'S book that indicates FRÉCHET'S evident vexation about the fact that HAUSDORFF'S book of 1914 had paid scant attention to FRÉCHET'S pioneering role in abstract topology. Here is what ALEXANDROFF wrote: "C'est vrai que j'ai lu une partie des épreuves de la nouvelle édition du livre de M. Hausdorff, mais je n'étais nullement chargé d'y rédiger une partie quelconque, je ne pourrais pas, par conséquent, me sentir responsable pour les fautes de citation qui s'y pourraient glisser. A ce que je sais, la dernière épreuve du livre est déjà passé, ce qui rend difficile des changement quelconques.

"A ma connaissance, M. HAUSDORFF, comme tout le monde, n'est jamais exprimé de doute au propos de ce que la conception des classes (D) (espaces métriques) est exclusivement due a vous. J'espère donc que vous trouverez des citations correspondantes dans la nouvelle édition de son livre."

Then: "Ci jointe une liste de remarques diverses qui m'ont été suggerés par la lecture de votre Livre. Il se comprend que, si dans la plupart des cas je me suis permis de vous signaler quelques résultats obtenus par moi, ou par nous autres, topologues de Moscou, c'était exclusivement pour vous tenir au courant de nos recherches et non pour vous suggérer l'idée d'introduire dans votre Livre les résultats."

ALEXANDROFF indicated a long list of pages of the manuscript that he for some reason never received or saw, amounting perhaps to almost two fifths of the entire manuscript. Even without knowing the correspondence between the pagination of the manuscript and that of the published book, it is possible in some cases to verify that FRÉCHET accepted and used some of the suggestions relayed to him by ALEXANDROFF and his students. FRÉCHET abandoned his previous definition of the notion of perfect compactness and utilized the definition favored by ALEXANDROFF and URYSOHN. See pages 192 and 195 of the book [FRÉCHET 132] for the definition, and lines 3–5 on page 230. FRÉCHET'S final definition of perfect compactness, then, is the same as property A discussed earlier (in connection with the letter of April 14, 1926). But FRÉCHET did not use the term bicompact in his book.

The second part of the paper [FRÉCHET 123] deals with the subject 'Prolongement d'un espace non-compact en un espace compact.' In some respects the paper is unclear. FRÉCHET cites some work on this general subject, especially for HAUSDORFF spaces, by ALEXANDROFF in [ALEXANDROFF 4] and then states that he will prove certain things, his wording being such that the implication is that he is supplementing ALEXANDROFF'S work. Later on in the paper FRÉCHET indicates how he can extend ALEXANDROFF'S results for the case of H-spaces, but with certain differences. His treatment of matters for H-spaces is more clearly set forth in his book, on pages 221–224. The paper [FRÉCHET 123] was seen by ALEXANDROFF, with the result that he wrote to FRÉCHET to indicate that some of the results claimed by FRÉCHET were already contained in the paper cited by FRÉCHET. He wrote:

"Aujourd'hui je vous écris pour vous signaler quelques passages dans votre récent mémoire. 'Quelques propriétés des ensembles abstraits' (Fund. Math. t. 10) qui pourraient donner lieu, il me semble, à des malentendus (d'ailleurs peu importants)." He then cites three statements by FRÉCHET in the paper in question, on pages 344–345, and 355, respectively, and asserts:

"Or les faits mentionnés dans ces passages se trouvent déjà énoncés dans mon article 'Ueber die Metrisation der im kleinen kompakten topologischen Räume.' Nous y trouvons, en effet, les passages suivants (pagination du tome 92 des Mathematische Annalen):" ALEXANDROFF then cites and quotes, in German, the passages from his paper that he places in comparison with assertions in FRÉCHET'S paper. He concludes his letter in a cordial and respectful tone:

"Soyez sûr, cher Monsieur Fréchet, que je me permets de vous signaler ces passages seulement parce que vous m'avez vous-même demandé de vous signaler toute chose concernant des renseignements bibliographiques (surtout ceux qui se rapportent à travaux d'Urysohn et de moi-même). Il se comprend que je ne vois de ma part, aucune importance de revenir sur ces questions: je ne m'intéresse du tout pour des 'reconstructions des droits de l'auteur'!"

This particular episode was mentioned a final time in the correspondence. Here is the opening of a letter of date November 1, 1927:

"Je viens de recevoir votre carte postale et la lettre que vous m'avez addressée à Moscou. Je suis en même temps embarassé et touché par la façon si cordiale et délicate dont vous avez bien voulu de réagir sur la remarque que je me permis de faire sur les rapports entre votre travail et ma note des Math. An. *Bien. entendu, je suis très satisfait de la Note que vous voulez envoyer aux Fundamenta* sur ce sujet, et je n'ai aucune observation à y faire; seulement je vous prie croire que (comme je vous ai écrit dans ma lettre précédente) je n'attribue aucune importance a mes 'droits de priorité'; en conséquence je n'éprouverais aucune inconvénient, si vous n'aviez pas envoyée une rectification quelconque a M. Sierpinski.

"J'espère, en tout cas, que vous considérez cette question comme complètement epuisée."

ALEXANDROFF spent the academic year 1927–28 in America, at Princeton, which he was able to do with the aid of a Rockefeller grant. In a letter of September 26, written on board a Cunard liner, he wrote as follows to FRÉCHET:

"Quant à moi, je m'occupe toujours des propriétés topologiques des ensembles situés dans des espaces euclidiens. Je m'intéresse surtout des propriétés et des méthodes permettant établir des liens étroits entre l'Analysis Situs telle qu'elle etait créée par Poincaré et les methodes nouvelles de la théorie des ensembles de points et des espaces abstraits." He also said that he intended to write a book to be called 'Vorlesungen über die topologischen Grundbegriffe der Geometrie' for the series "dite de 'livres jaunes'."

On November 1 he wrote again, saying: "Maintenant je suis à Princeton où j'espère de pénétrer plus profondément dans l'Analysis Situs classique qui semble être si bien cultivé ici. Princeton est une toute petite ville très gentille, formant plutôt un seul immense parc dans lequel les maisons (souvent en bois) sont parsemées. J'espère, il me sera commode de travailler ici à mon livre (un travail qui est trop long pour pouvoir devenir un travail de vacances et auquel à Moscou jamais je pourrais parvenir."

In a letter of February 11, 1928, ALEXANDROFF sketched for FRÉCHET the proof of a new theorem by TYCHONOFF and NIEMYTZKI, which he had mentioned to FRÉCHET in a previous letter. The theorem: If a metric space satisfies the condition of CAUCHY (*i.e.* is complete) with respect to every metric that is compatible with the definition of the limit of a sequence in the space, then the space is compact.

After the year in Princeton ALEXANDROFF'S letters to FRÉCHET became infrequent. Writing from Göttingen on July 2, 1930, ALEXANDROFF wrote as follows in part of his letter:

"Mon ami et collègue, M. Kolmogoroff, dont les recherches sur la théorie des probabilités, les séries trigonométriques et plusieurs autres questions, vous sont probablement connues, viendra en France en meme temps que moi; il s'intéresse tout particulièrement pour vos recherches sur la théorie des probabilités et aussi pour vos recherches sur la théorie générale d'intégration sur les ensembles abstraits." ALEXANDROFF requested FRÉCHET'S permission to let his name be used to help both himself and KOLMOGOROFF to get French visas. (By this time, of course, FRÉCHET was established in Paris after his move from Strasbourg, and he was working on the theory of probability.)

There is a good deal more about KOLMOGOROFF in a letter of July 22, 1930, evidently written in reply to a letter from FRÉCHET answering the letter of July 2. Evidently KOLMOGOROFF, then in Göttingen, was hoping, with the recommendation of COURANT, and a Rockefeller grant, to spend a year partly in Paris and partly in Göttingen. Speaking of KOLMOGOROFF'S work, ALEXANDROFF wrote: "A Moscou, ces travaux sont considérés comme présentant la plus haute valeur scientifique et, en général, nous estimons M. Kolmogoroff comme un de nos meilleurs jeunes mathématiciens, peut être même le meilleur parmi les mathématiciens de sa génération." Then, after further discussion, ALEXANDROFF wrote: "Etant donnés les intérêts mathématiques de M. Kolmogoroff, c'est vous et M. Hadamard à qui je pense en première ligne parmi les mathématiciens français chez qui M. Kolmogoroff pourrait travailler à Paris."

Perhaps things did not work out as quickly as hoped for KOLMOGOROFF. At any rate, almost three years later, writing from Moscow on June 7, 1933, ALEXANDROFF mentioned KOLMOGOROFF again, as follows:

"M. Kolmogoroff, qui vous envoie ses meilleurs et respectueuses salutations, se propose d'aller (avec une bourse Rockefeller) à Paris l'hiver prochain. Il espère de pouvoir profiter de vos conseils scientifiques."

From the foregoing it sounds as though KOLMOGOROFF had become acquainted with FRÉCHET, but it is of course possible that he was being courteous on account of what FRÉCHET may have done to help him by correspondence. There is little if anything to go on in speculating about FRÉCHET'S possible influence on KOLMO-

GOROFF. What does come through in the letters from ALEXANDROFF is a sense that FRÉCHET'S position and work gave him stature.

In between the letter of July 27, 1930 and that of June 7, 1933, there is an undated letter from ALEXANDROFF, the content of which identifies it as having been written late in 1931 or early in 1932, for it conveys good wishes for 1932. The final paragraph of the letter is as follows:

"Je serai très heureux d'apprendre de vos nouvelles; je viens d'ailleurs d'écrire à M. Veblen des questions dont nous avons échangé de lettres le printemps dernier (il s'agissait de votre participation éventuelle à l'Académie des Sciences de U.S.A.)"

The meaning of 'participation éventuelle' is not clear. Was ALEXANDROFF hinting at associate membership for FRÉCHET?

There are other things of interest in the letter of June 7, 1933.[25] The first part of it is taken up with some remarks about ALEXANDROFF'S book [ALEXANDROFF 5], which was published in the year prior to the year of the letter here in question. It seems evident from the letter that FRÉCHET had seen the book, but had not received a gift copy from his Russian friend, the author. Moreover, he was disturbed at not finding any reference to himself in the book. Here is what ALEXANDROFF wrote: "Je vous remercie vivement de votre lettre du 11 Mai. J'en apprends que vous n'avez pas reçu mon petit livre d'introduction à la Topologie par cause d'un malentendu quelconque, car ce sont déjà plusieurs mois qu'il devait être entre vos mains. En tout cas, je ferai immédiatement le nécessaire pour que vous soyez enfin en possession de ce livre.

"Je regrette infiniment d'avoir donner lieu, de ma part, à ces méditations tristes, mais bien justifiées que vous appelez, d'un façon trop modeste, 'votre plaidoyer.' N'ayant pas oublié, à ce qui me semble, de rendre hommage, dans mes mémoires, à vos découvertes si profondes et si brillantes, j'ai commis cette faute dans mon petit livre, et je vous prie d'accepter tous mes regrets, toutes mes excuse [*sic*] les plus sincères. Je vous prie seulement de croire que ce n'est pas une faute de mauvaise volonté." ALFXANDROFF goes on to explain that the book was originally intended to be devoted exclusively to combinatory analysis situs and that parts of it had been done hastily under pressure from the editor. This pressure had also caused him, at the last minute, to omit an historical introduction. I continue quoting from the letter: "J'avais omis au dernier moment une introduction historique, où votre nom trouvait la place d'honneur qui lui convient; me trouvant l'été hors de Moscou, je n'ai pas reçu la dernière épreuve *où je devait changer ce qui était à changer après la suppression de l'introduction.* C'est de cette façon qu'il y a dans ce livre des omissions bien pénibles (il y manque par exemple le nom d'Urysohn ce qui est bien contre mes intentions).

"Il me reste d'espérer seulement que le Traité de Topologie que j'écris en collaboration avec M. Hopf et dont le premier volume sera donné à l'impression d'ici

[25] For another discussion of the contents of this letter see pages 85–86 in [ARBOLEDA 1]. He includes what I do not, ALEXANDROFF'S extensive plans for a three-volume work on topology, to be co-authored with HEINZ HOPF.

en quelques mois, me donnera l'opportunité de corriger tous ces malentendus regrettables."

In his book co-authored with HEINZ HOPF (see the Bibliography) ALEXANDROFF *did* recognize the important role of FRÉCHET. On page 6 in the Introduction, after stating that "Bei Cantor ist ein geometrisches Gebilde eine beliebige Punktmenge des Euklidischen Raumes," the authors observe that FRÉCHET, in his thesis, had the insight to realize that CANTOR'S point of view was needlessly special – that there are sets of things other than point sets in Euclidean space to which the ideas of set-theoretic topology can be usefully applied. Moreover, they indicate, FRÉCHET'S ideas inaugurated a new epoch in point set topology. "Mit diesen von Fréchet geschaffenen Ideen der sogenannten 'abstrakten' Topologie beginnt eine neue Epoch der mengentheoretischen Topologie."

In their book ALEXANDROFF & HOPF keep the notions of compactness and bicompactness separate, retaining FRÉCHET'S definition of compactness for a space, and defining a space to be bicompact if it has the HEINE-BOREL property. Their general topological spaces are defined by certain closure axioms. In their hierarchy of separation axioms, FRÉCHET'S axiom N_3 (as given in Section 6 of this essay), but with the assumption that neighborhoods are open sets, is labelled as FRÉCHET'S axiom and called the first separation axiom. A space in which this axiom is satisfied is called a T_1-space. The name T_2-space is given to a space that satisfies HAUSDORFF'S stronger separation exiom. The third separation axiom is named after VIETORIS; a T_3-space (also called a *regular* space) is a T_1-space that satisfies the VIETORIS axiom. The fourth separation axiom is named after TIETZE; a T_4-space (also called a *normal* space) is a T_1-space that satisfies the TIETZE axiom. There is also a T_0-space, with the weakest separation axiom, named after KOLMOGOROFF: Given two distinct points, at least one of them has a neighborhood that does not contain the other. The use of T in these designations comes from the German word Trennungsaxiom, meaning separation axiom. FRÉCHET'S names, H-space and accessible space, for a T_1-space, have not survived.

One last quotation from ALEXANDROFF'S letter of June 7, 1933:

"Quant à moi, je ne sais pas quand je visiterai pour la prochaine fois l'Europe occidentale; les moyens pour mes séjours prolongés a l'étranger provenaient, en ce qui concerne notre continent, des cours que je donnais presque chaque été a Göttingen. Maintenant je n'ai nul désir d'aller en Allemagne hitlerienne, et je n'ai rien en vue ailleurs ...

"Je serais heureux d'apprendre de vous nouvelles; je m'intéresse surtout pour vos plans pour les prochaines vacances. Je me souvient [*sic*] toujours des belles semaines que nous avons passé ensemble, à Sanary. Moi aussi je suis bien heureux de voir nos deux pays se rapprocher, et c'est aussi le sentiment de mes collègues. Je me sentais toujours heureux en France, et j'espère fermement que j'aurai une fois l'occasion de la revoir."

What overall impression is conveyed by the many letters that ALEXANDROFF wrote to FRÉCHET? For me, two things stand out: the enthusiastic concentration on mathematical topics, and ALEXANDROFF'S courtesy, patience, and respectfulness. I've no doubt that, when ALEXANDROFF and URYSOHN initiated the correspondence in 1923, they had several things in mind. They must have wanted to open up another channel of communication with mathematicians in western Europe. Given their current interest in abstract topology and their awareness of FRÉCHET'S pioneering role, he was their obvious target in France. Moreover, they wanted his help on two matters – publication in France and the getting of French visas. He proved to be useful to them on both counts. The letters, by their frequency, tone, and contents, demonstrate unequivocally, I think, that ALEXANDROFF found in FRÉCHET an older friend with whom he was glad to talk about mathematics and keep in touch, mainly by mail, but with occasional personal contacts. It seems evident that both URYSOHN and ALEXANDROFF were more powerful and insightful mathematicians than FRÉCHET. They had more in the way of results to tell him than he had to tell them. One gets the very clear impression that FRÉCHET asked questions more than he communicated results. Both ALEXANDROFF and URYSOHN were interested in FRÉCHET'S H-classes, of which they had either not been aware or had not properly appreciated until the beginning of their correspondence with FRÉCHET, but they made clear, at various times, significant ways in which H-classes differ from HAUSDORFF spaces, thus demonstrating that FRÉCHET'S obsessiveness about H-classes as compared with HAUSDORFF spaces was not well justified.

If ever ALEXANDROFF was bothered by FRÉCHET'S sensitivity about receiving the credit which he felt was his due for his work on abstract spaces, the letters do not show it; on the contrary, ALEXANDROFF was always reassuring and respectful about FRÉCHET'S importance. As shown in the letter of June 7, 1933, he was apologetic, perhaps more than he was obliged to be, when he realized that FRÉCHET felt neglected by something ALEXANDROFF had failed to include in a small book he had published.

In the comments of URYSOHN and ALEXANDROFF about the Esquisse and in ALEXANDROFF'S reactions to the manuscript of FRÉCHET'S book on abstract spaces one can see, I believe, that they were deliberately cautious about offering penetrating general evaluation and criticism, while at the same time pointing out a few specific places where corrections and improvements were needed. I suspect that they were not enthusiastic about the attention FRÉCHET gave to extremely general topological spaces (the V-spaces of his work of 1918 and later).

As it happens, something is known about ALEXANDROFF'S estimate of the work of FRÉCHET, at least as he reported that estimate himself in 1978, when he was about eighty-two years old. The availability to me of this estimate came about in April of 1979 when I was working at the Archives of the Académie in Paris. L. C. ARBOLEDA (mentioned at the beginning of Section 9 of this essay), told me the following about his correspondence with A. P. YOUSCHKEVITCH, the Russian historian of mathematics, in connection with ARBOLEDA'S study of the FRÉCHET documents in the Archives, and in particular the letters from ALEXANDROFF to FRÉCHET. Among other things, in a letter that ARBOLEDA wrote to YOUSCHKEVITCH in July of 1978, he asked about ALEXANDROFF'S opinion of FRÉCHET'S work. YOUSCHKEVITCH was able to visit ALEXANDROFF and tell him the contents of ARBOLEDA'S letter, after which YOUSCHKEVITCH conveyed some of ALEXANDROFF'S remarks to ARBOLEDA in August of 1978. As I learned from ARBOLEDA, ALEXANDROFF was of the opinion that FRÉCHET'S most important work was in his thesis, especially the part about metric spaces. He thought the level of FRÉCHET'S subsequent work was never the same. He thought that the credit for the neighborhood method of dealing with abstract topology belonged to HAUSDORFF and that L-classes were of secondary importance. Nevertheless, ALEXANDROFF appreciated FRÉCHET'S Esquisse very much. In it, he said, was defined in substance the notion of a general topological space (by which I presume he meant either FRÉCHET'S 'new' V-classes or the concept of a space with the barest possible structure based on the notion of derived sets subjected to few or no assumptions). He indicated, however, that much the same thing was done and developed with more success by KURATOWSKI, with his axioms on the closure of sets (in KURATOWSKI'S thesis [KURATOWSKI 1]). By the attribution of 'more success' to KURATOWSKI I've no doubt that ALEXANDROFF'S point was that KURATOWSKI went on to build a coherent theory that was carried beyond the very general beginnings and into the richer body of topology that could be erected for metrisable spaces. He did this in his book [KURATOWSKI 2].

11. Fréchet's book: Les espaces abstraits

The full title of the book here under discussion is *Les espaces abstraits et leur théorie considérée comme introduction à l'analyse générale.* From notes made by FRÉCHET in an old notebook used for many records over a period of many years (which I was able to borrow from FRÉCHET'S daughter in 1979) it appears that the definitive manuscript was sent to the publisher on December 30, 1926. The Preface of the published book is dated 'Strasbourg, décembre, 1926.' The notebook also revealed that FRÉCHET was dealing with the galley proofs from late November, 1927 to early March, 1928 and with page proofs through the month of March. The book must have come out as early as June, for one of FRÉCHET'S correspondents, in a letter dated July 2, 1928, thanked FRÉCHET for the copy he had recently received (this was B. DE KERÉKJÁRTÓ, who had earlier been reading proof sheets of the book). Others who read all or a substantial part of the book in pre-publication form were FRÉCHET'S close friend G. BOULIGAND, T. H. HILDEBRANDT, VALIRON (on the faculty at Strasbourg), W. SIERPINSKI, and P. ALEXANDROFF, as well as students of the latter in Moscow. HILDEBRANDT, who had access to the copy of the manuscript that ALEXANDROFF had with him in Göttingen, wrote to FRÉCHET in a letter of July 31, 1926, that he was mostly interested in the Introduction and in what FRÉCHET had remarked about E. H. MOORE'S general analysis.

BOULIGAND, in a letter to FRÉCHET dated April 15, 1927, wrote: "Je suis à plus en plus enthousiaste à l'idée de voir paraître votre livre sur les espaces abstraits. Je crois sincèrement que vous avez devancé [outrun, gone ahead of] les mathématiciens contemporains en matière de théorie générale des ensembles, d'une manière telle qu'on n'a pas su toujours juger de l'importance de l'oeuvre que vous avez édifié: Son influence est nettement visible dans une quantité d'autres travaux, et notamment, vous avez trouvé pour la construction de la topologie, la voie qui semble la meilleure (ce qui n'est pas peu dire, car cela me semble éventuallement nouveau et fondamentale)."

In the Preface to the book it is made clear that FRÉCHET'S plan was not to write a text book on the topology of abstract spaces. Rather, he envisaged the book as a presentation of a certain part of his own work on general topology, taking up the ideas and results in their natural order, indicating the general lines of development and showing, insofar as possible, the origins and connections between the fundamental ideas, but without going into the details of proofs of things asserted. Here is the leading paragraph of a section of the Préface with the heading *Mode d'exposition adopté*. "Comme dans les Mémoires que nous venons de citer, nous nous proposons seulement dans ce volume, en rappelant les principaux résultats acquis, de replacer ceux-ci dans leur ordre naturel et d'indiquer dans la mesure du possible, l'origine et l'enchaînement des idées fondamentales. Notre désir est d'attirer l'attention sur l'Analyse générale, d'en marquer les lignes directrices, plutôt que d'en faire un exposé detaillé. Nous nous abstiendrons donc de démontrer les propriétés énoncées, mais nous indiquerons à chaque fois les réferences qui permettraient au besoin de retrouver les Mémoires où ces propriétés ont été établies."

He referred to the need of a book of this sort in the French language, saying: "Le besoin d'une publication, en français, sur ces matières, se faisait, en effet, d'autant plus sentir que l'Analyse générale n'est connue en France que de quelques-uns, alors qu'à l'étranger le nombre va croissant des Mémoires que lui sont consacrés." In his expressions of gratitude to various persons the following is notable: "J'ai aussi à coeur de mentionner le concours que m'a prèté le regretté Urysohn. Son ami, M. Alexandroff et lui, ont grandement facilité la rédaction de ce livre en procédant sur ma demande à une révision minutieuse de l'Esquisse ...' qui a servi de base au présent Ouvrage." Indeed, the influence of ALEXANDROFF and URYSOHN greatly exceeded their review of and commentary on the Esquisse. All through the book, especially in its second half, one can see the influence of the letters written to FRÉCHET by ALEXANDROFF and URYSOHN jointly and by ALEXANDROFF alone.

The book is divided into an Introduction and two parts. The first part (pages 23–155) deals mainly with ideas about dimensionality and metric spaces. The second part (pages 157–274) deals mainly with non-metric topology. The emphasis throughout is centered on FRÉCHET's own work, but consideration is given to the work of others where such work bears a close relation to that of FRÉCHET. The Introduction (pages 1–21) provides a kind of overview of the notions of functional analysis and abstract general analysis. All of this is broadly conceptual, with no technical elaboration. It is interesting to observe what FRÉCHET said at the end of the Préface in the way of guidance to readers of the book, as well as what he said about his own larger intentions.:

"... ceux qui s'intéressent surtout aux applications de l'Analyse fonctionnelle pourrant se contenter de lire la première Partie. A ceux qui sont attirés par la Théorie des ensembles abstraits en raison surtout de sa portée philosophique, la lecture de la seconde Partie pourra suffire. Ils s'apercevront qu'elle permet de pénétrer plus intimement la nature des notions de distance, de limite et de voisinage.

"D'ailleurs, pour les raisons développées pages 11–14, le présent Ouvrage ne doit être considéré que comme un préambule. C'est l'extension de l'Analyse classique à l'étude des fonctions abstraites de variables abstraites; en deux mots, c'est l'Analyse générale, qui a toujours été le but ultime d'un très grand nombre de nos travaux.

"*Nous espèrons pouvoir étudier plus tard, en un volume distinct, l'Analyse générale proprement dite.*"

Pursuant to this plan, this book contains nothing about FRÉCHET's definition of the differential in general analysis, nothing about generalized power series expansions, and nothing about the application of general topology to the theory of surface area. The discussion of functionals and interspace abstract transformations is limited to discussions of continuity, equicontinuity, and semicontinuity.

In the Introduction, after several pages on 'Les Méthodes de l'Analyse générale,' FRÉCHET undertakes to meet objections to his plunge into extreme generality by playing devil's advocate for a bit, expressing objections that he knows are expressed against overly general theories, and then offering his refutations.

At the end of the Introduction he makes a point of distinguishing his interest in topology from the interests of those who view topology as a contribution to the foundations of geometry. I quote:

"*Mais l'étude des fondements de la géométrie n'est pas l'objet principal* des travaux de l'auteur. ...

"Notre but est surtout de faire une étude générale des relations entre variables abstraits, étude enterprise, non seulement, pour obtenir des résultats nouveaux, mais aussi pour réaliser l'unification des énoncés classiques de la Théorie des fonctions et de l'Analyse fonctionnelle. C'est dire que nous irons chercher – toutes les fois que cela nous sera possible – nos exemples parmi les conceptions mathématiques dont l'utilité a été déjà éprouvée, plutôt qu'au moyen de constructions spécialement imaginées en vue d'un théorème d'existence. Nous voyons à cette façon de procéder les deux avantages suivants. Nos exemples étant puisés dans [drawn from] l'Analyse (fondée sur la notion de nombre), l'utilité de la théorie qu'ils illustrent apparaîtra mieux comme indépendante des fluctuations de la pensée moderne concernant le modèle mathématique de notre monde physique. Il ne peut en être de même de la topologie, qui a en vue l'élaboration de ce modèle. Le seconde avantage consiste simplement en ce qu'il est toujours profitable en mathématique d'aborder le même problème de divers côtés à la fois. Sans compter que les résultats que nous obtiendrons ont un intérêt propre pour le développement de la théorie des divers champs fonctionnels les plus importants, indépendamment de toute application géométrique."

The latter half of this paragraph seems rather obscure to me. Perhaps it is an expression of an aspect of FRÉCHET's thought that (as we shall see in Section 12) caused some of FRÉCHET's comtemporaries to label him, somewhat contemptuously, as more a philosopher than a mathematician. I put this last issue aside as an irrelevant distraction from the task of appraising FRÉCHET as a mathematician. FRÉCHET's most important work as a mathematician, *not excepting, I think, his later work in probability and statistics*, was in abstract general topology and in general analysis. He stated his interests and his goal in the concluding portions of the Préface and the Introduction to this book. In this essay I have been considering him as a topologist. In the third and concluding essay about FRÉCHET I shall consider him as an analyst.

FRÉCHET's book was most certainly not appropriately designed as an instrument for aiding a student who wished to learn systematically the most important things about the state of topology in the second half of the 1920's. For such a student an effective instrument would have been one that selected a certain starting point, hewed to a certain line of development to reach the fundamental ideas and results without much distraction with side issues, and displayed enough of the arguments and proofs needed along the way to enable the student to understand the subject and become proficient in demonstrating the theorems and making investigations independently. FRÉCHET's decision to omit proofs and merely to describe a great assortment of ideas and results, with not much selective emphasis, made the book merely a compendium of definitions, facts, and relationships, with a guide to the periodical literature as the only help, if furthei help were needed. This deprived the book of the appeal of a well planned textbook which would instruct, inspire, and encourage young scholars.

FRÉCHET's book was too late on the scene to have any hope of displacing the influence of HAUSDORFF's book of 1914. Moreover, it was not constructed in a manner to capture the minds of young French mathematicians who might readily have preferred a French book to a German book on Topology.

Ironically, it fell to a Polish author to write a book that gave FRÉCHET's H-classes a prominent position in a systematic exposition of abstract general topology. The author was WACLAW SIERPINSKI, one of the principal leaders of the brilliant surge of mathematicians in Poland in the period immediately following the great war of 1914–18. SIERPINSKI wrote a book on set theory in the Polish language. It was in two parts, the first on transfinite numbers, the second on general topology, Being in Polish, it had to be translated into more widely known languages before it could exert the influence of which it was capable. The part on general topology was translated into English and published in Canada in 1934 as [SIERPINSKI 2]. The preface to the original Polish text, bearing the date February, 1928, was translated into English and included in the translated book. This book was of great value as a textbook. I know of no other place where the theory of H-classes is developed as clearly, systematically, and thoroughly. It is ironic that SIERPINSKI's book does a better job of putting H-classes in a favorable light than is done in any of FRÉCHET's own writings.

In the first chapter SIERPINSKI studies an abstract space in which there are certain sets, called open sets, whose only properties are those that can be inferred from three axioms: (1) the empty set is open, (2) the entire space is an open set, (3) the union of any collection of open sets is open. In the second chapter two more axioms are added: (4) given two distinct elements in the space, there exists, corresponding to each element, an open set containing that element but not the other, (5) the intersection of two open sets is an open set. It turns out that, with neighborhoods of an element defined as open sets containing that element, the space becomes a FRÉCHET V-class of precisely the sort called an H-class by FRÉCHET, as defined by his four axioms for neighborhoods that are open (see Section 6). In the ensuing (third) chapter SIERPINSKI adds a sixth axiom, that the space contains a denumerably infinite family F of open sets such that every open set is a union of some subfamily of F. This is equivalent to HAUSDORFF's second axiom of countability. It is worth noting that SIERPINSKI has the BOREL theorem (a closed and compact set has the BOREL property) in his second chapter and the BOREL-LEBESGUE theorem (a closed and compact set has the BOREL-LEBESGUE property) in his third chapter, which deals with H-classes satisfying the second axiom of countability.

HAUSDORFF spaces and metric spaces are considered in later chapters of [SIERPINSKI 2]. In a way the first three chapters of SIERPINSKI'S book provide a strong justification for FRÉCHET's claim that much of general topology can be developed for H-classes, without the necessity for invoking the HAUSDORFF separation axiom.

The significance of H-spaces can be recognized in the work of another man from Poland. One of the younger Polish mathematicians of the 1920's, CASIMIR KURATOWSKI, obtained a doctorate at the University of Warsaw in 1920 with a thesis in which he began his development of general topology with four axioms about the notion of the closure $\bar{A}$ of a set A. The axioms were

1) $\overline{A \cup B} = \bar{A} \cup \bar{B}$;
2) A is contained in $\bar{A}$;
3) the closure of the empty set is empty;
4) the closure of $\bar{A}$ is $\bar{A}$.

These axioms and some of their consequences are contained in [KURATOWSKI 1]. KURATOWSKI, referring to FRÉCHET's thesis, pointed out that, with $\bar{A} = A \cup A'$, the foregoing axioms are satisfied in an L-class. He made no mention of [FRÉCHET 66]. As SIERPINSKI pointed out on page 33 of the book [SIERPINSKI 2], KURATOWSKI's four conditions on the closure of a set are consequences of SIERPINSKI's five axioms on open sets (as I presented them in an earlier paragraph). ALEXANDROFF & HOPF, in their book, used the four axioms of KURATOWSKI to define what they called a topological space. Such a space is more general than a T_1-space (which is an H-space).

When KURATOWSKI came to write his book [KURATOWSKI 2], he used a modified set of axioms (given on page 15 of the book):

I) $\overline{A \cup B} = \bar{A} \cup \bar{B}$;
II) $\bar{A} = A$ if A is empty or contains just one element;
III) the closure of $\bar{A}$ is $\bar{A}$.

He remarked as follows: "M. M. Fréchet appelle "accessibles" les espaces assujettis aux axiomes I–III." This is not quite accurate, for FRÉCHET did not use these axioms. However, with $\bar{A} = A \cup A'$, KURATOWSKI's three axioms are equivalent to the axioms on derived sets that FRÉCHET used to define H-spaces. Thus we see that KURATOWSKI as well as SIERPINSKI built up in a book a systematic presentation of ideas and results about spaces that are in fact H-spaces. KURATOWSKI's book, unlike that of FRÉCHET, contained demonstrations and served as an influential textbook. After an initial chapter it moved on rapidly to metric and metrisable spaces.

12. Fréchet and the Paris Académie des Sciences

In Section 6 of my essay I, I quoted excerpts from the report that HADAMARD made to the Académie in 1934 in support of FRÉCHET'S candidacy for election to the Section de Géométrie of the Académie. A vacancy in the Section had been created by the death of PAUL PAINLEVÉ on October 29, 1933. As was customary, candidates prepared a statement of their accomplishments which was printed and made available to members of the Académie who were to vote on the filling of a vacancy. FRÉCHET'S statement, *Notice sur les Travaux Scientifiques de M. Maurice Fréchet*, bears the date 1933. It is listed for the year 1933 in the Bibliography; I refer to it hereafter as [FRÉCHET, Travaux]. It contains a chronology of FRÉCHET'S teaching appointments and honors he received, followed by lists of his publications in seven categories. The greater part of the Travaux is devoted to discussion of his ideas and his writings. At the head of his introduction to the discussion of his work FRÉCHET quoted the following statement by LEIBNIZ:

"Ceux qui aiment à pousser le détail des sciences méprisent les recherches abstraites et générales et ceux qui approfondissent les principes entrent rarement dans les particularités. Pour moi, j'estime également l'un et l'autre, car j'ai trouvé que l'Analyse des principes sert à pousser les inventions particulières."

FRÉCHET put immediately following this quotation the following sentences:

"Je me sens confondus d'admiration et d'humilité devant la profondeur des conceptions de Leibniz et l'universalité de son génie. Mais l'épigraphe ci-dessus m'a paru si bien s'appliquer, toutes proportions gardées, à mon propre état d'esprit, que je n'ai pu résister à la tentation de le placer en tête de cette Notice. Ce sont certainement mes recherches 'abstraites et générales' qui ont le plus contribué à me faire connaître des mathématiciens ... Mais, de tout temps, je me suis aussi intéressé activement à diverses questions particulières qui se sont présentées à mon esprit en géométrie et en Analyse. Et dans la dernière quinzaine d'années, je me suis efforcé de contribuer à la vulgarisation des applications scientifiques et industrielles des mathématiques."

The person elected in 1934 was GASTON JULIA, who was more than fourteen years younger than FRÉCHET. The next election to the Section de Géométrie occurred in May of 1937, when PAUL MONTEL (about two and a half years older than FRÉCHET) was elected to replace EDOUARD GOURSAT, who died on November 25, 1936. On this occasion FRÉCHET was presented by HADAMARD as a candidate "in 3rd line" which meant, as I understand it, that the Section de Géométrie placed two other candidates ahead of him for the position. In 1934 he had been a candidate "in 4th line". Even though a person might not be a leading candidate, merely being a candidate could be useful for a subsequent occasion. HENRI LEBESGUE died un July 26, 1941, creating a vacancy again. At that time FRÉCHET became a candidate once more. The first line candidate was ARNAUD DENJOY, who was more than five years younger than FRÉCHET. He was elected in June of 1942. In presenting the case for FRÉCHET to the Académie in 1942 EMILE BOREL wrote in the document bearing his signature that if the candidate being presented were not "tout à fait exceptionnel" he would have insisted on presenting FRÉCHET "en 1e ligne," and he said that, short of unforeseen circumstances, the first place to become vacant in the Section de Géométrie should be reserved for FRÉCHET (who was then nearing age 64).

It is worth noting much of what BOREL had prepared to say about FRÉCHET at the time of the election of 1952. He observed that FRÉCHET's work had made him distinguished abroad perhaps even more than in France. He had been invited to give addresses, not merely to a section, but to a general session of the International Congress of Mathematicians in Rome in 1928 and in Oslo in 1932. Speaking of FRÉCHET'S membership in the Polish Academy, BOREL said that as of 1942, FRÉCHET was the only "membre titulaire français de cette Académie n'appartenant pas à l'Institut de France." BOREL said that FRÉCHET's work seemed to fall into two distinct periods and to be devoted to two different domains "d'esprits presque opposés." The first period, up to 1928, was primarily occupied with the theory of sets and general analysis. In the second period FRÉCHET was more and more occupied with probability and its applications to statistics. BOREL called this change astonishing; he said that FRÉCHET had been saluted as the creator of the theory of abstract spaces, and then had been recognized, both in France and abroad, as an expert in probability, his new field. One evidence of this had been that he had, in 1927, been asked to direct the Colloque International de Genève sur le Calcul des Probabilités. Speaking of FRÉCHET'S work on the theory of probability, BOREL mentioned several particular areas in which FRÉCHET had worked, and said that, especially in certain domains of the theory, FRÉCHET had transformed "un chantier de construction [a work-yard] en une maison habitable." Also "il a transformé un ensemble hétéroclite [irregular, eccentric, odd] de résultats partiels en une théorie rigoureuse et cohérente." Here BOREL was presumably referring to [FRÉCHET-F188][26] and [FRÉCHET-F188 bis]. He made clear that in these remarks he was praising FRÉCHET for a useful accomplishment in exposition and systematizing.

After some discussion of FRÉCHET'S pioneering work in abstract spaces and his introduction of the concepts of compactness, completeness, and separability, BOREL then raised the question of whether it is a work of mathematics to obtain useful definitions. Is that a genuine invention? BOREL stated that POINCARÉ had given an answer in his writing, described by BOREL as follows: "la mathématique n'est qu'une langue bien faite. Sous une forme volontairement exagéré, il a voulu faire ressortir que l'introduction d'une nouveau mot est souvent précédé d'un travail au cours duquel l'auteur a fait de nombreuses *démonstrations* qui l'amenaient chacune à la conclusion négative que telle ou telle notion ne pouvait convenir au but qu'il s'était assigné et devait être écartée ou modifiée. Après quoi le travail préparatoire doit disparaître: tout devient plus facile à celui qui trouve la définition toute préparée et risque d'oublier qu'un travail d'élimination préalable a été nécessaire et qu'il comportait des suites de syllogismes de même nature qu'il a suivi."

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[26] My own chronological enumeration of FRÉCHET'S publications has not been carried far enough to include this and the next-following publication of FRÉCHET (both of them books), to which he assigned the numbers 188 and 188 bis.

BOREL said that one of FRÉCHET'S characteristics was his refusal to be satisfied by theories that are admitted without discussion. He cited various instances in which FRÉCHET, by probing into things that had been taken for granted, came upon new findings or better proofs.

In a statement of summation by BOREL one can read between the lines that there were conflicting views about the merits of FRÉCHET'S work. He said that FRÉCHET was among those mathematicians for whom the attraction of a question consists not so much in the difficulties to be conquered as in the discovery of a new field or a new method. They do not mind leaving unsolved problems behind if they have succeeded in opening a new "champ d'action" and resolved some of the questions thus raised. BOREL said that while some mathematicians "de grande valeur" had been "insensibles ou dédaigneux" with respect to the theories that occupied FRÉCHET, other eminent mathematicians appreciated them. Particularly abroad and among the young in France, according to BOREL, were research workers with an enthusiastic interest in the fields opened by FRÉCHET. BOREL thought that the trace FRÉCHET would leave behind would in later years be even greater than it appeared to be at that time, I quote: "Des maintenant, en effet, où quelques années se sont ecoulées depuis le moment où il a cessé de s'occuper activement d'analyse générale, on observe que les idées qu'il y a introduites n'en ont pas moins continué à faire sentir leur influence. Sans parler du développement propre d'analyse générale, qui s'est poursuivi, ces idées ont aussi envahi de nouveaux domaines. C'est ainsi que la notion et les propriétés des espaces distanciés ont été utilement employées par M. M. Bohr et Besicovitch dans la théorie des fonctions presque-périodiques; par M. Kürschack puis par de nombreux mathématiciens, grace à la notion de 'Bewertung' liée à l'inégalite triangulaire, par M. Menger dans sa nouvelle conception des intégrales du Calcul des Variations comme dans sa géométrie métrique, par M. Paul Lévy en ce qui concerne la distance de deux lois de répartition comme d'ailleurs par M. Fréchet lui-même pour la distance de deux variables aléatoires etc. etc."

There are in the Archives some letters to FRÉCHET, from members of the Académie, touching on the election of June, 1942. Several are of interest. One dated May 21 is from MARCEL BRILLOUIN. He wrote: "Mon cher camarade, Votre candidature me paraît toute naturelle, et je serais bien embarrassé pour avoir une préference si j'étais à Paris. Je comprends à peu près les questions que vous traitez. Je n'en saurait dire autant pour Denjoy." (He indicated that he was living without too much difficulty in an old family home and didn't intend to return to Paris until the war was over.) A letter of May 18 with an illegible signature came from St. Emilion. The writer said that he thought DENJOY might have the greater chance of success, but that FRÉCHET'S record as an "ancien combattant" would count strongly in his favor. A letter of May 26 came from LANGEVIN in Troyes. He thought it unlikely he would get to Paris for the election, but if he did he would talk to FRÉCHET. He said he was very favorably disposed "des maintenant," and wished FRÉCHET "bonne chance." Here is a quotation from a letter by JULES DRACH, who said he couldn't get to Paris for the vote:"Vous vous êtes créé avec l'étude des ensembles abstraits un domaine personnel qui s'est montré extrêmement fertile. Il est naturel que vous soyiez candidat en même temps que Denjoy et peut-être emporterez vous sur lui—qui s'est attaqué à des questions plus classiques. Quoique il arrive prochainement, votre place est marquée à la section de Géométrie: je souhaite en tout cas que vous ne preniez pas trop à coeur un échec possible, cela n'a pas une si grande importance. Nous allons bien et souhaitons que cette carte vous trouve ainsi que les vôtres en bonne santé."

The next vacancy in the Section de Géométrie occured when ELIE CARTAN died on May 6, 1951. The person elected on this occasion (in March, 1952) was RENÉ GARNIER, more than eight years younger than FRÉCHET. In connection with this election there is an archival document handwritten on BOREL's stationery and dated 25 février 1952 and marked comité secret. It begins by explaining why FRÉCHET had been presented only in the 2nd line in 1942: "il s'agissait de notre confrère M. Denjoy," who was then the 1st line candidate. Now BOREL states, he presents FRÉCHET with the Section de Géométrie unanimous, less one voice, for FRÉCHET. (The members of the section then were BOREL, DENJOY, HADAMARD, JULIA, and MONTEL. The negative voice was JULIA, about which I will comment later.) BOREL's report asserts that FRÉCHET's most original work was that on abstract spaces. He then recapitulates a number of things from his report of 1942. BOREL observed that, at an earlier time, many mathematicians had reservations about FRÉCHET's ideas, regarding them more as pure speculations, more philosophical than mathematical. But FRÉCHET persevered, and the developments in topology led to an enlargement of the domain of mathematics. In this way BOREL strove to emphasize the originality of FRÉCHET's mind and the important effect of his work on the development of mathematics. He concluded by stating that the time had come for FRÉCHET to take a seat "entre nous." But it did not occur. Later that year, however, on June 30, FRÉCHET was elected 'member correspondent.' filling a vacancy caused by the death in April of GUIDO CASTELNUOVO of the University of Rome.

A tip-off about how things might go in the election of a successor to CARTAN came to FRÉCHET in a letter from his good friend DENJOY on July 13, 1951. Evidently, four of the five surviving members of the Section de Géométrie had quickly reached agreement to support FRÉCHET's candidacy. DENJOY wrote: "Mon cher Fréchet – En effet, pour barrer la route au caprice saugrenu [preposterous, absurd] du cinquième membre de la Section, les quatre autres été immédiatement d'accord pour nous unir sur ta candidature. Par ailleurs ton rôle historique dans l'orientation des mathématiques depuis un demi-siècle place notre clan au-dessus de la critique. Bien à toi – A. Denjoy."

More information about FRÉCHET's candidacy for the vacancy caused by the death of CARTAN is to be found in letters written to FRÉCHET by PAUL LÉVY in 1951 and early 1952. LÉVY, a gifted protegé of HADAMARD, was eight years younger than FRÉCHET and held a position at the Ecole Polytechnique (which he lost temporarily during the Second World War). The voluminous file of letters from LÉVY to FRÉCHET in the Archives of the Académie begins with a letter of December 29, 1918. This correspondence would be of prime importance to anyone making a study of the life and work of LÉVY. It is evident that LÉVY was also a candidate for election to the Section de Géométrie in 1952, and was thus in competition with both FRÉCHET and GARNIER. The letters to FRÉCHET are quite open about this. LÉVY and FRÉCHET were good friends. This seems evident from the letters, which are filled with both scientific and personal matters. Their close friendship was confirmed to me orally by FRÉCHET's daughter in 1979. In a letter of May 19, 1951, shortly after CARTAN's death, LÉVY indicated to FRÉCHET that it was being questioned whether he (LÉVY) should be a candidate, for by doing so he might "se jeter en travers de la chemin de Fréchet." According to FRÉCHET's daughter, BOREL had said to LÉVY, "Rétirez vous," so as not to hinder FRÉCHET's chances. Also, according to LÉVY himself, HADAMARD had asked him about his intentions. LÉVY wrote that, in his opinion, the third candidate (GARNIER) had a very strong position, but didn't have "de chances sérieuses contre Fréchet." LÉVY had decided to maintain himself as a candidate because he didn't want to let GARNIER have a big advantage over him the next time. He assured FRÉCHET that in his visits to members of the Académie (to present his credentials) he would make clear his esteem for FRÉCHET. He said that FRÉCHET should not have to "attendre plus longtemps." Then, in a letter of July 2, he wrote FRÉCHET that he was sending out a letter to members of the Académie indicating that if, after two rounds, there was no chance of his election, he hoped they would rally for FRÉCHET. He said that JULIA was making a campaign for GARNIER and against LÉVY and FRÉCHET, saying that FRÉCHET was a philosopher, not a mathematician. JULIA had said that directly to LÉVY. In the next letter (of July 4) LÉVY told FRÉCHET of having talked again with JULIA, who was very eloquent for GARNIER. Indeed, LÉVY wrote of GARNIER, "il a abordé et résolu des problèmes difficiles." Also, "Il est certain qu'il y a des gens qui disent que vous êtes plus philosophe que mathématicien; il vaut mieux que vous le sachiez."

On February 26, 1952, LÉVY wrote FRÉCHET that he had talked with JOLIBOIS (a professor of chemistry in the Ecole nationale supérieure des mines), after the meeting of the secret committee and he, LÉVY, was sure that the Académie would be impressed by the "exposés concordantes de M. M. HADAMARD, BOREL, et DENJOY, et que votre election est assurée." LÉVY was still determined not to withdraw, and he told FRÉCHET he thought BOREL was wrong in thinking that LÉVY's position would benefit GARNIER.

There is in the Archives a handwritten joint statement by LÉVY and FRÉCHET, signed by them both, bearing at the top a request to the chairman that it be read when the candidates were being discussed. The gist of the statement is to make the point that when, in 1928, FRÉCHET commented on GAUSS's law concerning accidental errors, saying that it was only true under certain restrictions, and when in 1933 he drew attention to PAUL LÉVY's work, indicating that LÉVY had not understood the necessity of considering these restrictions, this action was not really correct, for on page 72–74 of his book on the calculus of probability LÉVY *did* show proper care. Thus the statement was a sort of open admission by FRÉCHET that he had mistakenly but inadvertently and unintentionally given the impression that LÉVY had made a mistake.

After he learned that FRÉCHET had been elected as a corresponding member, LÉVY wrote to speak about that. Then: "Mais je ne peut pas m'empêcher de constater qu'il est sans exemple depuis que l'Académie existe qu'un mathématicien français, ayant les titres [qualifications] que vous avez et ayant eu l'influence que vous avez eu, n'arrive pas à être membre d'une des deux premières sections [Géométrie and Mécanique]. Malgré l'avantage qui peut un jour en résulter pour moi, ce n'est pas sans peu de regret que j'apprends la conséquence actuelle – maintenant normale – des erreurs antérieures. Mais je veux surtout vous féliciter et exprimer l'espoir que vous arriverez bientôt à l'échelon supérieur dans cette nouvelle voie."

FRÉCHET'S next opportunity came after BOREL'S death on February 3, 1956; he was elected to the Section de Géométrie on May 14. In support of his candidacy on this occasion FRÉCHET prepared a brief typed "notice abregé" of his scientific works from 1902 to 1956. In it he enumerated the extent of his publications in each of nine different classifications:

Mathématiques appliquées	12
Statistique mathématique	36
Calcul des probabilités	77
Géométrie	36
Analyse classique	65
Analyse fonctionelle	16
Espaces abstraits	28
Analyse générale	25
Philosophie et pédagogie des mathématiques	18

After this he quoted again the statement by LEIBNIZ that he had used in [FRÉCHET, Travaux] (given earlier in the present section of this essay), and then he added the following remarks:

"En jetant un regard en arrière, il nous est plus facile de discerner les tendances inconscientes qui ont orienté nos travaux: C'est peut-être, d'abord, un souci constant de dégager l'essentiel de l'accessoire et, d'autre part, un penchant à nous écarter des sentiers battus, à tenter de résoudre des questions qui se posent plutôt que des questions déjà posées.

"C'est une obsession de rigueur qui ne nous a que rarement fait défaut. C'est enfin un éclectisme déjà exprimé dans la citation ci-dessus qui nous a porté à nous intéresser de plus en plus aux applications – même, s'il le fallait, purement numériques – aussi bien qu'aux théories abstraites par lesquelles nous avions débuté."

In his monograph about the life and work of BOREL [FRÉCHET on BOREL] FRÉCHET made (on page 2) the following point about his own situation in relation to that of BOREL: "Je considère comme le plus grand honneur de ma vie d'avoir été élu deux fois comme successeur d'un illustre savant: d'abord à sa chair de la Faculté des Sciences, puis dans son fauteuil de l'Académie des Sciences."

PAUL LÉVY was the next person elected to the Section de Géométrie. He succeeded HADAMARD in 1964 at age 77; HADAMARD was almost 98 when he died.

That FRÉCHET should have had to wait until he was in his seventy-eighth year was the result of special circumstances, some of which are apparent in the accompanying tabular display.

Name	Year of election	Age at election	Age at death
PAINLEVÉ	1900	37	69
HUMBERT	1901	42	66
HADAMARD	1912	47	97
GOURSAT	1919	61	78
BOREL	1921	50	85
LEBESGUE	1922	46	66
CARTAN	1931	61	82
JULIA	1934	41	85
MONTEL	1937	60	98
DENJOY	1942	58	90
GARNIER	1952	65	92
FRÉCHET	1956	77	94
LÉVY	1964	77	85

The names are those, in order of election to the Section de Géométrie, who were elected in 1900 or later and prior to the election of MANDELBROJT, who succeeded LÉVY in 1972. Just prior to the death of LÉVY in 1971 the average age of the members of the Section de Géométrie (membership was limited to six) was approximately 88! Out of thirty-four persons elected to the section from 1803 (BIOT) to 1964 (LÉVY), only FRÉCHET and LÉVY were in their 70's and only four were in their 60's. The median age of election was 42 and the average was 47. FRÉCHET was unlucky in his competition with JULIA, MONTEL, DENJOY, and GARNIER, all of whom were in, or closer to, the tradition of classical analysis.

13. Conclusions

In Section 12 of my Essay I, I stated a major conclusion based on my study of FRÉCHET, namely that he, as the first mathematician to make a systematic and extensive study of general point set topology using an abstract and axiomatic approach, opened the way for this sort of study and that his work, culminating in his doctoral thesis, had an impact of major importance.

In this essay, after studying FRÉCHET'S subsequent contributions up through the publication of his book in 1928, I conclude that FRÉCHET's accomplishments in topology during this period were much less important. They were not negligible, but they were not as significant in substance and influence as the thesis. Probably his most significant contribution to topology after 1906 was his theory of H-classes. For this work he blended two very general abstract approaches to topology. The first of these was borrowed from F. RIESZ: the idea of an abstract space in which with each set E in the space is associated another set E', the derived set of E. The association of E' with E is initially subjected to minimal conditions, but later to added conditions. The second approach was via a notion of neighborhoods of a point, using this notion to define derived sets. The eventual product of the consideration of these two notions in tandem was the concept of an H-class, which in FRÉCHET'S book is called un espace (H), or, alternatively, un espace accessible,

FRÉCHET used the term espace topologique (see pages 166–169 in [FRÉCHET

132]) for an abstract space in which the notion of a derived set is subject to the single condition that E' and $(E - (x))'$ are the same whenever $x \in E'$; here $E - (x)$ denotes the set of all points that are in E and distinct from x. One way of defining an H-class, using the notion of derived sets, is set forth at the end of Section 6. The other route to H-classes is via FRÉCHET'S general notion of a V-class, in which derived sets are defined by saying that for x to be in E' means that, for each neighborhood U of x, $(E - (x) \cap U$ is not empty. The conditions for an H-class determined by neighborhoods that are open sets are given at the end of Section 6.

FRÉCHET'S working out of the two methods of defining H-class are contained in [FRÉCHET 66] and [FRÉCHET 75], but the definitions do not stand out very clearly from the rest of the contents of these two papers. H-classes are defined by the two methods quite explicitly and clearly on pages 354–355 in the Esquisse, [FRÉCHET 76]. The definitive presentation of H-classes by FRÉCHET is on pages 185–187 in his book.

As can be seen in Sections 9 and 10 of this essay, ALEXANDROFF and URYSOHN recognized that there were distinct merits to H-classes. In particular, it is convenient that they can be defined so easily by conditions on derived sets. Evidently SIERPINSKI found H-classes interesting, for he presents them ahead of his presentation of HAUSDORFF spaces (as I noted in Section 11). On the other hand, the separation axiom for H-classes (condition (c) at the end of Section 6) renders H-classes less satisfactory for the applications of topology in analysis than HAUSDORFF spaces with their stronger separation axiom. H-classes are presented as T_1-spaces in the very influential book [ALEXANDROFF & HOPF], while HAUSDORFF spaces are presented there as T_2-spaces.

When considering and evaluating the total body of FRÉCHET'S work on topology in the period 1907–1928, I think it must be said that it was diffuse, too general to fit well with the needs and tastes of the times, and not accompanied by the development of a methodology to attack with significant success problems whose conquest might have helped to give his work prestige. FRÉCHET did, in fact, pose problems, but usually he left them unsolved or only partially solved. It seems to me that he lacked the disposition, and perhaps the talent, for the sort of work that involves the development of technique or new ideas for attacking specific hard problems successfully.

I can give several citations that help to give insight into FRÉCHET'S characteristics as a mathematician.

From a letter to FRÉCHET from DAVID EUGENE SMITH, dated April 19, 1935, one can infer that FRÉCHET had raised with SMITH the question of considering the comparative values of the works of those mathematicians who are the first to solve difficult problems and of those who are successful in building up new theories. SMITH said he would take that question up with Professor GINSBURG. Then he pointed out to FRÉCHET that the solution of a difficult problem sometimes leads the way to an important general theory. I conjecture from this correspondence that FRÉCHET recognized that he was essentially not a problem solver, but prized his work as a creator of a general theory.

I remember a conversation I had with ARBOLEDA in Paris in 1979. From what he told me about what he had heard from certain persons who had known FRÉCHET and had working relations with him before 1950, I gained the impression that FRÉCHET'S ideas were so general, the breadth of his interests so varied, so many possibilities were opened for inquiry but so little was done to develop a precise and sustained methodology, that the net effect was generally antithetical to the prevailing spirit of the times. In spite of this, there were those (G. CHOQUET, for example) who generally defended FRÉCHET'S mode of work as having value.

In Essay I (on page 234) I cited A. D. MICHAL'S high praise of FRÉCHET'S thesis in a book by MICHAL that was published in France. In a letter of October 23, 1962, PAUL LÉVY thanked FRÉCHET for letting him see MICHAL'S book. Referring to MICHAL'S praise of FRÉCHET'S work, LÉVY wrote that he knew that many savants shared MICHAL'S opinion, but that he would be "un peu plus prudent que Michal, parce que je suis incapable de savoir si ce n'était pas une idée 'dans l'aire', et si Moore, par exemple, n'aurait pas écrit sa 'general analysis' si vous n'aviez pas écrit votre mémoire. Mais je n'ai jamais entendu contester qu'en fait vous avez été le premier."

The question of how much FRÉCHET owed to ARZELÀ for the notion of compactness remains a matter of conjecture in the minds of some, I believe. There is no doubt about the fact that ARZELÀ had enunciated the proposition that, given a family of continuous functions defined on a finite closed interval, necessary and sufficient conditions that, in any infinite sequence of functions from the family, there should be a uniformly convergent subsequence, are that the functions in the family be uniformly bounded and equicontinuous. But that is a far different thing from defining the concept of compactness in an L-class, as FRÉCHET did in the Comptes Rendus in 1904 and in his thesis in 1906. PAUL MONTEL may have expressed himself on this matter, but the evidence is not certain. In a letter of November 20, 1951 from PAUL LÉVY to FRÉCHET, LÉVY stated that MONTEL had told him, at least twenty years earlier, that the "notion d'ensemble compact était due à Arzelà." LÉVY then went on: "Votre notice me prouve qu'il s'était trompé. ... Peut-être y avait-il un malentendu, et avait-il voulu parler de l'application aux ensembles de fonctions. Mais je ne le crois pas. Inutil de vous dire que je considère qu'il s'agit d'une notion très importante. Personne ne peut le contester." I do not know what 'notice' of FRÉCHET'S proved to LÉVY that MONTEL was mistaken.

When FRÉCHET was established in Paris, late in 1928, after leaving Strasbourg, he was almost exactly fifty years old. A very full and long life still lay ahead of him. But his important work in topology was over. Activity in topology was flourishing in Europe and America and the direction of work in topology had passed him by.

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1909

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31. (F 28) Une définition fonctionnelle des polynômes, Nouv. Ann. Math. (4) **9** (1909), 145–152.

32. (F 29) Toute fonctionnelle continue est développable en une série de fonctionnelles d'ordres entiers, Comptes Rendus Acad. Sci. Paris **148** (1909), 155–156.

33. (F 30) Représentation approchée des fonctionnelles continues par une série d'intégrales multiples, Comptes Rendus Acad. Sci. Paris **148** (1909), 279–280.

34. (F 36) Les fonctions d'une infinité de variables, Comptes Rendus Congrès des. Soc. Savantes à Rennes (1909), 44–47.

1910

35. (F 32) Sur une généralisation de la formule des accroissements finis et sur quelques applications, Travaux. Sci. Univ. Rennes **9** (1910), 61–67.

36. (F 31) Sur les fonctionnelles continues, Comptes Rendus Acad. Sci. Paris **150** (1910), 1231–1233.

37. (F 31 bis) Sur les fonctionnelles continues, Annales Ecole Norm. Sup. **27** (1910), 193–216.

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46. (F 44) Développements en série, Encyclopédie des sciences mathématiques pures et appliquées, édition française, tome II, vol. 1, 210–241.

47. (No F number) Ouvrage revisé par M. Fréchet: Advanced Calculus, par E. B. Wilson, Enseignement Math. **15** (1913), 189–190.

1913

48. (F 45) Sur les classes (V) normales, Transactions Amer. Math. Soc. **14** (1913), 320–324.

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Department of Mathematics
University of California
Berkeley

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