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Michèle Audin

Fatou, Julia, Montel

The Great Prize of Mathematical Sciences of 1918, and beyond

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Here I am before you all a sensible man
Who knows life and what a living man can know of death
Having experienced love's sorrows and joys
Having sometimes known how to impose my ideas
Adept at several languages
Having travelled quite a bit
Having seen war in the Artillery and the Infantry
Wounded in the head trepanned under chloroform
Having lost my best friends in the frightful conflict
I know of old and new as much as one man can know of the two
And without worrying today about that war
Between us and for us my friends
I am here to judge the long debate between tradition and invention
Between Order and Adventure

You whose mouth is made in the image of God's
Mouth that is order itself
Be indulgent when you compare us
To those who were the perfection of order
We who look for adventure everywhere
[...]

The pretty redhead, in [Apollinaire 1980]

Contents

Introduction

For whom is this book written?

Licensed historians do not write much on contemporary history of mathematics. Yet, more and more mathematicians are interested in the history of their discipline, namely in the history of the mathematics they do every day. This book is written for them. This does not prevent it from being written with the intellectual rigour one can expect from a historian as from a mathematician.

The book

Its goal is the investigation of the works of several mathematicians on the iteration of rational functions during the years 1917–1918, in relation to the topic of the Great Prize of mathematical sciences that was announced by the Paris Academy of Sciences for 1918. We know that this Prize was awarded to Gaston Julia, and that Pierre Fatou, who had also worked on the subject, did not enter the competition. We also know that the subject remained dormant from say the 1920's to the 1980's, when it found a new dynamic, popularising the name of Julia and, to a lesser extend, that of Fatou.

Our problem could be roughly summarised in two questions:

– The reasons why the most influential part of the mathematical community gave preference to Julia will be investigated; Julia was severely injured during the 1914–1918 war, a “gueule cassée” (broken face), he would still be considered sacrosanct in 1944, he was, in 1917–18, a kind of icon; we will shed light on this position by investigating the context, the mathematical texts, and by publishing previously unpublished sources.

– To understand the reasons for a fifty year long eclipse in so beautiful a subject is also an interesting question. Briefly: Julia was surrounded by many students but he did not make any of them want to work on the subject; Fatou, as an astronomer at the Paris Observatory, had no students and he died in 1929 (leaving quite a few problems to posterity); on the other hand, the

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notions which could have made the open questions progress, those contained in Felix Hausdorff's works for instance, were not spread well in France (for reasons arising from the historical context).

Beyond the description from day to day of the mathematical discoveries of Fatou, Julia, Lattès, Ritt and others, which I hope to have made interesting; beyond the obvious effects of the war, the First World War, a never-ending slaughter, on the behaviour of the mathematicians—effects on which the readers will find in this book unpublished information; beyond the admiration modern readers cannot help but feel, they who are used to the strange beauty of Julia sets, when they see Fatou and Julia invent them *by hand* and understand their properties without the intuition given by a computer [1]; we shall emphasise on the quality of Pierre Fatou's modern and open work.

This is not a history of iteration (moreover, there is already such a history, that by Alexander [1994][2], for historical comments on Julia's works, see also Hervé's text in [Julia 1968] and article [1981]). Our perspective here is rather on the sequel, on what happened next. It is hard for a mathematician of today to speak of Julia sets without thinking of Siegel discs, of the Mandelbrot set, of Hermann annuli and of Douady's rabbit. It is hard to speak of the First World War without remembering that it was followed by the Second World War[3] (and not only in the chronological sense). The paths followed by the mathematicians considered here collided with these wars through which they had to live.

Each in its own way, all of these questions show the importance of context... This is why there are many digressions in this text. On the other hand, despite these digressions, there is a unity in the style: to give an example, we have chosen to favour quotations and examples coming from the Paris Academy of Sciences and its *Comptes rendus*[4]. Since the history considered here is that of

[1] According to John Hubbard, Lars Ahlfors told him in 1984 that Lindelöf asked him to read the papers of Fatou and Julia when he was a student... but that he only understood what was in Fatou's and Julia's minds when he saw the pictures that Hubbard and Mandelbrot produced (see the preface of [Tan 2000]).

[2] Although it is possible to disagree with some assessments of this book, it is nevertheless an important source of information on the history of iteration, especially before 1906.

[3] Or even by the Algerian war. Although Montel and Julia were still professionally active during this war, we will not mention it here.

[4] To read the *Comptes rendus*, which give an account (this is what the title means) of what happened during the sessions of the Academy of Sciences—presentation of scientific notes, election of new members, reading of obituaries on deceased members, choice of professors at the Collège de France or of astronomers at the Observatory, choice of delegates sent to such and such meeting, speeches on "politics" at the end or at the beginning of the year—is instructive and even exciting, even if, as one can imagine, not "everything" was written (the investigation of part of what was not written, at another time, is one of the subjects of our paper [2009a]). The decision made by the Academy of Sciences, a few decades ago,

a Prize of the Academy of Sciences, it is by no means absurd to wonder what was in the mind of the members of this assembly, in addition to science, when they assessed the works in competition.

Let us now look at the contents of this book. For simplicity, it follows a more or less chronological order.

The scene (subject of the Great Prize, the war, the first examples of iterated rational functions) is set, and the main characters (Fatou, Julia, Lattès, Montel and the others) are introduced in the first chapter.

Then (in Chapter II), the research of Fatou, Julia, Lattès and Ritt, and that of Montel, are described, more or less day by day, as they appear in the Academy of Sciences, from Fatou's note of May 21st 1917 to the proclamation of the Great Prize on December 2nd 1918, through Julia's fit of anger in December 1917.

The mathematical contents of the memoirs of Julia and Fatou are described (rather quickly) in Chapter III. Let me make a personal comment: before reading their papers, I tried to review all the prejudice I could have about the subject and especially against the two authors, in order to avoid letting this prejudice influence the issue of my reading of these mathematical texts; I can tell that it is indeed because it struck me, that I emphasise the quality of Fatou's work, and especially its modernity.

In the next chapter (Chapter IV), the question of the almost complete cessation, as early as in the twenties, of research on the subject, is raised. We also introduce some close relatives of the Julia set, the J-points (which probably lie at the source of the "Julia set" terminology) which appeared in a paper by Ostrowski in 1927 and which play a role in the rest of the story.

As we shall see, in this story, Pierre Fatou never stops disappearing. Chapter V is dedicated to his personality and to the few things we were able to learn about his biography.

In the last chapter (Chapter VI) we reproduce letters, most of which have not been previously published, sent to Paul Lévy in 1965. On the occasion of a rather late priority dispute between Montel and Julia on the question of "J-points", we will see the criticisms made of Julia and of his attitude during the time he was working on iteration, this reproach having never been, to my knowledge, explicitly stated.

We quote, here and there, some excerpts of a report by Hadamard on Fatou, and also some letters of Fatou to his friends Fréchet and especially Montel. These cuts can be frustrating, so we include these texts in their entirety and in an appendix.

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to let this mixture disappear will certainly be lamented by the historians who will investigate the end of the 20th century.

Warning

A complete biography of all the protagonists will not be found in this book. This is in particular the case of that of Gaston Julia; there is some sporadic information; for more detail, see, e.g., the obituary by Garnier [1978], and also what can be deduced from the papers in Volume 6 [Julia 1970] of Julia's complete works. An exhaustive list of his publications will not be found here either, after all, his Complete Works were published in his lifetime. In § VI.4, I mention a few reasons why it would be very hard to write a biography of Julia.

The same is not true of Fatou's Works—although the paper [Nathan 1971] that was devoted to him in the *Dictionary of scientific biography* announced that the French mathematical society (SMF) planned to publish them (Szolem Mandelbrojt even said [Lebesgue 1991, note 358] that he was in charge of this publication). This was never done, thus posthumously confirming the under-evaluation of his work that lead, in his lifetime, to Fatou's slow and modest career. Without pretending to be exhaustive, this text is more complete regarding Fatou's works. For the same reasons, it contains some information on his biography, some of which is new, unpublished information, obtained with the help of several members of his family (see below).

Regarding the other mathematicians we shall mention, we shall provide some information about Montel, of course, but also about Picard, Siegel, Hausdorff and Garnier, which will be scattered in the text—but it is equipped with an index, which will hopefully be effective. Thus this book might contribute to a (perhaps less monolithic than usual) history of the French mathematical community in the 20th century.

War dead

The First World War, which is the scene of the beginning of this story, is already far enough in the past for most the witnesses to be already dead[5]. But it is culturally very close to us, and it is not easy to consider it with serenity, it is not even easy to use at least some historical perspective[6]. Millions of young

[5] The last two French "poilus" (hairy, French soldiers) died while I was writing this text, the last fighting Tommy died the day I began to translate it into English.

[6] This is shown by the confusion made by the French President in his speech of March 17th 2008, between the "poilus" of 1914–18 and the combatants of the Glières plateau, a centre of the French Resistance against German Occupation during WWII, who

> were going, *they too* [emphasised by me] to sacrifice their lives while proclaiming "live free or die" [allaient eux aussi faire le sacrifice de leurs vies en proclamant "vivre libres ou mourir"]

(speech for the funeral of Lazare Ponticelli). One can wonder whether this absence of historical perspective is related to the present fashion in WWI "history"?

people died during this war, many of them having agreed to die for their homeland. Among those whose homeland was France, a whole generation of young scientists, who were not devoted to remaining anonymous and silent, but who did not have time to give their names to discoveries, theorems, or other mathematical objects. In respect for these millions of unknowns, I have tried to name all the ones (in general, mathematicians) whom I have met during this work.

Women

I am quite unhappy to say so, but this story is a story of men. However, as it is well known, one of the effects of WWI was the massive arrival of a new class of women on the job market[7].

The story you are going to read is almost exclusively a masculine one—nobody thought of replacing by women the mathematicians fighting at the front or killed in war! Some wives play their anonymous and discreet role, for instance, Charles Hermite's daughter whom the young Émile Picard married, this having probably sped up the establishment of his power over the community, and the devoted nurse who became Julia's wife[8], who is present in the speeches and writings of [Julia 1970] (her first name never being mentioned[9]) as a wife, mother of Gaston Julia's six sons, and she too, as a daughter, in her case, daughter of the composer Ernest Chausson. Wives and daughters, but also sisters and mothers... and even female workers! Let us quote here, for the first time, a speech of the President of the Academy of Sciences in 1915, Edmond Perrier [1915, p. 806]:

> Haunted by the vision of the trenches, mothers and sisters were too anxious to think of adorning themselves; the sober elegance of the almost religious dress of the nurses hardly allowed the ornaments Mimi Pinson uses for a living, and one could not expect that the lively and nimble fingers of the

[7] It would be wrong to say, as it is often done, that "the women" arrived on the job market because men were fighting at the front. There were already many women at work long before WWI, e.g. among peasants and in the working class.

[8] In the story of Julia's wound, there are, of course, some nurses, the one who saved him from death and was Norwegian (see the speech Julia gave at the Oslo International Congress in [Julia 1970, p. 34]) and the one whom he married, in 1918.

[9] A family (or a world) in which wives have no first names: in the biography [Gallois 1994] of the composer Ernest Chausson, two daughters in law of Marianne Chausson and Gaston Julia, Madame Marc Julia and Madame Sylvestre Julia, are thanked.

> dressmakers would courageously agree[10], as they did, to calibrate explosive shells, to fill hand grenades, or to manipulate explosives[11].

We shall nevertheless encounter a female scientist, Jeanne Lattès, the wife (and, later the widow) of Samuel Lattès, a discrete and modest scientist, inventor of an important discovery, autohistoradiography.

As for Pierre Fatou, well, he was a bachelor. In his own discrete way, he nevertheless introduces (as an astronomer) a female scientist in this story, the astronomer Rose Bonnet, with whom he used to observe double stars.

Let us mention also the refreshing appearance of Marguerite Borel (1883–1969), daughter of the mathematician Paul Appell (and of a niece of Joseph Bertrand, so that she was also a relative by marriage of Hermite and Picard) and the wife of Émile Borel, a remarkable woman[12], who was, under the pen name of Camille Marbo, a recognised writer (for instance, she was awarded the Femina Prize in 1913) and whose memory book [1968] is a very lively source of information on the period 1883–1967 (which includes the periods we consider in this book). If, for reasons that would be interesting to understand, she practically does not mention Julia in this book written in 1967, she is, in one of the lyrical outbursts of a speech of the latter [1970, p. 72], the Gretchen (Marguerite) of a Faust personified by Émile Borel.

Illustrations

This text is about more than delightful mathematics, which has won a great aesthetic popularity over the last twenty or thirty years, but which was invented during the course of a more than serious story, millions of people killed of an atrocious war prefiguring the tens of millions killed in an even more atrocious (and abject) war, and it is about the individual destinies of the protagonists in this not so old story... It was thus desirable to keep a certain sobriety. This is why, although we shall give references to some modern texts on what are nowadays called "complex dynamical systems", the Julia

[10] The young dressmakers ("midinettes") indeed accepted, courageously but also with lucidity, as the fourteen day strike they staged in the spring of 1917 shows... after which they obtained, among other things, some collective employment contracts and days off. Regarding women and the war, see the paper [Thebaud 1992].

[11] [Hantées par la vision des tranchées les mères et les sœurs avaient trop d'angoisse au cœur pour songer à se parer; la sobre élégance du costume presque religieux des infirmières n'admettait guère les fantaisies qui font vivre Mimi Pinson, et l'on ne pouvait s'attendre à ce que les doigts alertes et légers des "midinettes" consentissent courageusement, comme ils l'ont fait, à calibrer des obus, emplir des grenades ou manipuler des explosifs.]

[12] Marguerite Borel founded, with her husband, in 1906, the *Revue du mois* (Journal of the month), a monthly journal that was quite successful. She was the President of the "Société des gens de lettres" (Pen club) in 1937, 1938 and 1947. She also chaired the committee of the literary Femina Prize.

sets, "common beauties of fashion pictures of mathematics", and " 'fractals' as 'super-models' of mathematics"[13], do not proliferate in this text: this would have made it anachronistic and also too light.

Sources

Except in the case of unintentional omission, we quote all the sources at their right place (this is elementary rigour). Among the sources, there are mathematical papers, discourses and speeches, letters, birth and death certificates, testimonies and memories. On the history of WWI, we have quoted papers that were written for a general audience [Audoin-Rouzeau 1992 ; Becker 1992 ; Pierrard 1992 ; Thebaud 1992 ; Sirinelli 1992] and first published in the magazine *l'Histoire*. These papers contain detailed bibliographical references.

In addition to the published texts[14] that can be found in the bibliography, we have used

– Julia's sealed letters and the manuscripts of some *Comptes rendus* notes, from the archives of the Academy of Sciences,

– the file concerning the prizes of the Academy of Sciences of 1918,

– the correspondence of Julia and Fatou to Borel, from the Borel collection, that of Fatou and Lebesgue to Montel from the Montel collection (72J) and that of Fatou to Fréchet from the Fréchet collection in these archives,

– again from these archives, the biographical files of some of the mathematicians we discuss in this text (in these files, one can find, among other things, Fatou's notices, a hand-written report by Hadamard that we shall quote several times (and reproduce in full in an appendix), the letters of Picard which we shall quote[15] in Chapter V) and some information from the Villat collection (61J),

– the register of secret committees[16] in the archives of the Academy of Sciences,

– the administrative archives of the Henri Poincaré Institute,

– the Paul Lévy file at the Chevaleret library,

– the annual reports of the Observatory (kept at the library of the Paris Observatory),

– the archives of the Collège Stanislas,

– the personal archives of some members of Pierre Fatou's family,

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[13] ["beautés banales des gravures de mode de la mathématique", les "fractales" comme "top-models" de la mathématique" [Roubaud 1997, p. 135]]

[14] Among the published texts, there are several papers that appeared in the *Cahiers du séminaire d'histoire des mathématiques*, a very rich publication, the disappearance of which may be deplored.

[15] As a rule, in this book, when we quote an unpublished document, we give a precise reference in the text or in a footnote.

[16] The Academicians met in public sessions, then *in camera*, namely in non-public meetings. Reports of these meetings are kept in a register.

– the Bourbaki archives, digitised by the *Archives Henri Poincaré* and on-line at http://math-doc.ujf-grenoble.fr/archives-bourbaki

– the files and the archives of the library of IRMA,

– the database “memory of men” [mémoire des hommes] of the French Ministry of Defence, that contains more than 1.3 million records of people who “died for France” during WWI (http://www.memoiredeshommes.sga.defense.gouv.fr).

Dates of birth and death given in this text come from reliable sources (encyclopaedias, the website http://www-groups.dcs.st-and.ac.uk/~history/ of St-Andrews for most of the mathematicians, a dictionary [Véron 2004] for the astronomers, the yearbook of the association of former students of the ENS...). I am solely responsible for the errors that remain. In a few cases however, I was not able to find this information (I have replaced it by an “XX”), in some other cases, I have given approximate data (an approximate date comes with a “c.” (for “circa”)).

The information on Paul Flamant was given to me by Jean Cerf (I have used it already in [Audin 2009b]), it complements the article [Sartre 1948].

Most of the unpublished biographical and personal information on Pierre Fatou was given to me by members of his family: one day, I sent a letter to twenty-two people with the name Fatou and living in Brittany or in the Paris region whom I found in the telephone directory. By the following day, I had already spoken with four cousins or great-nephews of Pierre Fatou, and they had already told me everything they knew about “Pierre Fatou the astronomer”, as he is called in the family, and they put me in contact with other cousins. They all soon sent me copies of all the documents they had. Other information comes from the registry offices of the towns of Pornichet and Lorient.

Credit for photos

– The photo of the family of Pierre Fatou that is reproduced on page 137 belongs to Madame Gladys Sérieyx, that of Pierre Fatou at the Collège Stanislas (page 139) belongs to the archives of the Collège Stanislas, that of the young Pierre Fatou on page 149 belongs to Monsieur Henry Fatou. The photo of the astronomers taken at the time of the transit of Mercury (figures V.1 and V.2) belongs to the Paris Observatory.

– The portrait of Felix Hausdorff (page 117), was published in his Works, and is reproduced here with the kind permission of the Hausdorff edition.

– The photo of Gaston Julia and Charles de la Vallée Poussin in Ravenna (page 210) comes from the photo album of George Pólya [Alexanderson 1987].

– The photo of Samuel Lattès on page 82 belongs to the Academy of Sciences, inscriptions and literature of Toulouse.

– The portrait of Paul Lévy (page 215) was published in his Complete Works.

I myself took the other photos. Nevertheless, the rights of the documents on pages 98, 110, 258 and 276 belong to the Academy of Sciences.

For the English edition

The main additions for this edition are the two (unpublished) letters written by Montel and Lebesgue to Élie Cartan just before Julia's election to the Academy of Sciences. I found these two letters while helping the Cartan family to sort out the letters received by Élie and Henri Cartan during the 20$^{\text{th}}$ century. They confirm and enlighten the analysis made here and I am grateful to the Cartan family for the authorisation to publish them.

I translated the text myself, but the result was in need of careful editing. This was carried out by Ruth Preater and John Preater, to whom I am most grateful. I am of course responsible for any remaining errors.

Thanks

I thank:

firstly, the institutions:

– the town archives service of Lorient for the birth certificate of Pierre Fatou and the information on Florian La Porte,

– the registry office of the town hall of Pornichet for the death certificate of Pierre Fatou,

– the Collège Stanislas for the information on Pierre Fatou and Désiré André, the digitisation of and the permission to publish the photo on page 139,

– the library of the Paris Observatory for the permission to look at the annual reports and the inventory books,

– the libraries of IRMA (Institut de recherche mathématique avancée at Strasbourg), of Chevaleret (Bibliothèque interuniversitaire scientifique Jussieu, mathématiques recherche), of the École normale supérieure and of the Henri Poincaré Institute for all I learned there,

– the *Mathematisches Forschungsinstitut Oberwolfach* for having conveniently organised an *Arbeitsgemeinschaft* on "positive measure Julia sets" in March-April 2008,

– and above all the archives service of the Academy of Sciences for its reception of me, and especially its curator Florence Greffe for having, in March-April 2008, filed and made an inventory of Paul Montel's correspondence, for having created the Paul Montel collection, and for having given me the permission to publish the picture in Figure III.1, the letters from Fatou to Montel and Fréchet and Hadamard's report,

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then, Pierre Fatou's family, namely

– his great-nephew Henry Fatou for the photos, the text written by his father, and for the humorous encouragement he sent me,

– his cousins

– Admiral Alain Fatou for the documents he collected around 1992 and that he sent me,

– Anny Fatou for the family tree of the Fatou family,

– Gladys Sérieyx, for the photos and other documents she sent me,

– Hélène Fatou, for the connections she made between her family and me, and for the suggestions she made,

– Michel Fatou, for the oral memories he collected among other cousins and which he passed on to me,

⋆

and also, the four anonymous referees, who read a preliminary version of this text for Springer, for all their suggestions and their constructive criticism.

⋆

and lastly

– David Aubin, historian of mathematics, for his knowledge of the history of the observatories, for the references he sent me on Pierre Fatou and for the hints he gave me on the research on ballistics mentioned in Chapter I,

– June Barrow-Green, historian of mathematics, for the reference to her paper [2008] on British mathematicians,

– Xavier Buff, Arnaud Chéritat and all the participants in the *Arbeitsgemeinschaft*, for the help they gave me in understanding the mathematics of the subject, and again

– Xavier Buff for his careful reading of a preliminary version of this text, for the innumerable corrections and mathematical refinements he suggested to me—thanks to which there are perhaps not too many mathematical mistakes in this version (I am of course solely responsible for those which, unavoidably, remain)—and for the references that motivated Remark IV.5.1,

– Arnaud Chéritat for the pictures of Julia sets he produced (at my request), which he sent me, and which that I used to illustrate this book (Figures I.2, I.4, II.1, III.2, that in Note 45 of Chapter III, Figure IV.1 and all the components of Figure IV.2[17])[18] and, above all, Figure IV.4,

– Dominique Dartron, librarian, for his help with the archives at Henri Poincaré Institute (IHP),

– Suzanne Débarbat, astronomer and historian, for the information on the Observatory she gave to Alain Fatou in 1992–93, for the invitation to the

[17] Which I made for a talk at the *Arbeitsgemeinschaft*.

[18] Each of these figures took him only ten minutes (this is what he said)... until I asked him to also produce Figure IV.4, which took him a long day of work, one of the reasons being that he had to first compute the exact positions of the five white circles in the figure.

Observatory in December 2007, the introduction to the library there, and for all the information she provided,

– Maurice Galeski, librarian, for the invaluable information he gave me on the history of the library at the Centre international de rencontres mathématiques (CIRM) and especially, for what we are interested in here, on the Gaston Julia collection of this library,

– Étienne Ghys because he mentioned the paper [Garnier 1954],

– Catherine Goldstein, historian and mathematician, for numerous discussions, for her rigour, for the documents she showed me, for all the information she gave me and for all the help she provided, of which these few lines cannot give any idea,

– Nathalie Granottier, librarian, for her help with the early collections of the library at CIRM,

– Jeremy Gray, historian of mathematics, for his comments and suggestions after reading a first version of this text,

– Tara Holm for her help in finding the English translation of *Under Fire* used here,

– Jean-Pierre Kahane for a discussion at the very beginning of this work,

– Nicolas Lecervoisier, in charge of archives at the Collège Stanislas, for his search concerning Pierre Fatou and Désiré André, at my request, in his archives, for his reception of me and for the guided tour of the Collège,

– Juliette Leloup, historian of mathematics, for her help with the theses we mention in this book, and especially for the reports on Gaston Julia's thesis,

– Odile Luguern, librarian, for her help in searching the "Fatou donation" in the library at the ENS,

– Philippe Maisonobe, mathematician from Nice, for having sent me a copy of the book [MathNiçois 1966],

– Doctor Pierre Marinelli for the posthumous consultation of Pierre Fatou,

– Frédéric Paulin for the information on the Ahlfors conjecture,

– Jacques Roubaud for a useful conversation,

– Claude Sabbah, for his TEXnical help,

– Arnaud Saint-Martin, sociologist and historian, for having sent me his thesis [2008], for his kind answers to my questions, and for the comments he sent me on a preliminary version of the chapter on Pierre Fatou,

– Norbert Schappacher, historian and mathematician, for many conversations,

– Marie-Hélène Schwartz and Claudine Schwartz for the permission to publish the letters sent to (their father and grand-father) Paul Lévy,

– Norbert Steinmetz for Figure III.4, coming from his book [1993],

– Philippe Véron, astronomer and historian, for having sent me (on a CD) his unpublished dictionary [2004] of astronomers,

– Brigitte Yvon-Deyme, librarian, for having shown me the registers of the library at IHP,

– Liliane Zweig, librarian, for her reception of me at the library of Chevaleret.

⋆

While I was writing this text, I read (or reread) *The Shell Shard*, *All Quiet on the Western Front*, *Under Fire*, *Storm of Steel*, *The Bells of Basel*, *Wooden Crosses*, *Calligrammes* and *Poems to Lou*, *The Acacia*... It would be surprising if none of these readings [Leblanc 1916 ; Remarque 1928 ; Barbusse 1916 ; Jünger 1920 ; Aragon 1934 ; Dorgelès 1919 ; Apollinaire 1980 ; Simon 1989] had left a trace in this work. It is thus fair to thank the authors.

I

The Great Prize, the framework

> Bertinck has a chest wound. After a while a fragment smashes away his chin, and the same fragment has sufficient force to tear open Leer's hip. Leer groans as he supports himself on his arm, he bleeds quickly, no one can help him. Like an emptying tube, after a couple of minutes, he collapses.
> What use is it to him now that he was such a good mathematician at school?
>
> Erich Maria Remarque, *All Quiet on the Western Front* [1928]

In this chapter, we describe the setting for the beginning of our story: the topic of the Great Prize of mathematical sciences, the historical context in mathematics and the historical context in general, namely the First World War. After a quick introduction of the various characters and of their roles, we establish some notation and give the first pertinent examples of the mathematical question under consideration, that is, the iteration of rational functions, setting it as it was when the story began.

I.1 The iteration problem in 1915

In 1915, The Paris Academy of Sciences announced that it would award in 1918 a "Great Prize of mathematical sciences". This was a prize, financed by the French State, of 3 ,000 Francs. The topic was published on December 27$^{\text{th}}$, 1915 in [Académie 1915, p. 921], and was presented as follows:

> The *iteration* of a substitution with one or several variables, namely, the construction of a system of successive points $P_1, P_2, \ldots, P_n, \ldots$, each of them being deduced from the previous one by the same operation:
>
> $$P_n = \varphi(P_{n-1}) \qquad (n = 1, 2, \ldots, \infty)$$
>
> (where φ depends rationally, say, on the point P_{n-1}) and such that the first point P_0 is also given, appears in several classical theories and in some of the most famous papers of Poincaré.

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> Up to now, the well known works devoted to this investigation are mainly about the "local" point of view.
> The Academy considers that it would be interesting to proceed from here to the examination of the whole domain of the values taken by the variables. In this spirit, it opens a competition, for the year 1918, on the following question:
> *To improve in an important point the investigation of the successive powers of a same substitution, the exponent in the power increasing indefinitely.*
> *One will consider the effect of the choice of the initial element P_0, the substitution being given, and it will be possible to limit the investigation to the simplest cases, such as that of rational substitutions of one variable.*[1]

It would be interesting to know exactly how and why this topic was chosen. We know that some of the Academicians thought initially of another

[1] L'*itération* d'une substitution à une ou plusieurs variables, c'est-à-dire la construction d'un système de points successifs $P_1, P_2, \ldots, P_n, \ldots$, dont chacun se déduit du précédent par une même opération donnée:

$$P_n = \varphi(P_{n-1}) \qquad (n = 1, 2, \ldots, \infty)$$

(φ dépendant rationnellement, par exemple, du point P_{n-1}) et dont le premier P_0 est également donné, intervient dans plusieurs théories classiques et dans quelques-uns des plus célèbres Mémoires de Poincaré.

Jusqu'ici les travaux bien connus consacrés à cette étude concernent surtout le point de vue "local".

L'Académie estime qu'il y aurait intérêt à passer de là à l'examen du domaine entier des valeurs que peuvent prendre les variables. Dans cet esprit, elle met au concours, pour l'année 1918, la question suivante:

Perfectionner en un point important l'étude des puissances successives d'une même substitution, l'exposant de la puissance augmentant indéfiniment.

On considérera l'influence du choix de l'élément initial P_0, la substitution étant donnée, et l'on pourra se borner aux cas les plus simples, tels que les substitutions rationnelles à une variable.

subject, nothing less than Fermat's Theorem[2], before they chose iteration. It had happened before, and was even a common practice, that a prize topic was announced because it was known that such and such a mathematician had made great progress on this topic[3]. This was apparently not the case here, where it seems, on the contrary, that the Prize itself would stimulate new research in the field. It is however possible that one of the Academicians, for instance Hadamard, thought of Fatou's note [Fatou 1906d] and of the fact that the latter had done further work on the topic (without writing it up) , when declaring the subject (see the excerpt of the letter quoted on page 18). The allusion to Poincaré might come from the same source: "as everybody", Hadamard admired Poincaré a lot, but *he himself* organised a seminar on his work in 1913 and should have known this work quite well[4]. The fact (note 2) that Darboux, Jordan and Picard thought of proposing Fermat's theorem is a hint that none of the three was at the source of the actual subject.

Note that, in the statement of the subject, the question is on iteration *per se* (in modern and anachronistic terms, on dynamics): mathematicians had iterates for a very long time, for instance in order to find approximate solutions of algebraic equations, and more than one of them had carried out

[2] A little note, signed Darboux, Jordan and Picard, Prizes file, archives of the Academy of Sciences, reads:

> The Academy of Sciences opens a competition on the proof of the celebrated theorem of Fermat about the impossibility of the equation
>
> $$x^n + y^n = z^n.$$
>
> We are mostly looking forward to progress in number theory that could lead to this proof.

We cannot resist saying here that, a few years later, on September 19th 1923, in a letter to Pierre Gauja, the "secretary-archivist" of the Academy of Sciences, Picard asked Gauja to write to a correspondent

> that the Academy never declared a competition on this question [Fermat's Theorem], if this is indeed the case [que l'Académie n'a jamais mis la question [le théorème de Fermat] au concours, si toutefois il en est bien ainsi]

(Picard file, archives of the Academy of Sciences).

[3] Among the most famous examples, are that of the Bordin Prize of 1888, for the progress made by Sophie Kowalevski on the question of the rigid body (see [Audin 2008]), and that of the Great Prize of mathematical sciences, which was declared at the end of 1890 because Stieltjes thought he had a proof of the Riemann hypothesis (and which was eventually awarded to Hadamard). For a concise but efficient history of the mathematical prizes, see [Gray 2006].

[4] Note besides that Hadamard was the author of the paper [Hadamard 1921] on Poincaré's mathematical work that would be published by *Acta Mathematica* in its special issue of 1921.

such an activity[5]. One of the most interesting aspects of the subject was the global, "general", as they said at that time, nature of the expected research. Let us quote now, to whet the appetite, the start of the report [Académie 1918, p. 811] written by Émile Picard and Georges Humbert[6] for the award ceremony of the Prize in 1918. They mentioned the history of the subject and clarified, for instance, the allusion to Poincaré's works[7]:

> The Academy declared a competition on the investigation of the *iteration* of a substitution, recalling that only the *local* point of view had been considered until then and it invited the competitors to take a *general* point of view.
> Previous work, especially the fundamental works of M. Kœnigs, for a substitution S, $z_1 = \varphi(z)$ of one variable, led to the notion of *points of attraction:* if ζ is a point that is fixed under S or one of its powers (an *invariant point*), and if a corresponding quantity, called the *multiplier*[8], has absolute value *less* than unity, all the successive transforms (*consequents*[9]) of a point z, taken in a neighbourhood of ζ, tend to ζ or periodically tend to p points, one of which is ζ and the others its first $(p-1)$ consequents.
> These initial results raised many problems: are the attracting points of limited number; what exactly is the domain of attraction of one of them; what division of the plane is associated with a given function $\varphi(z)$?
> On these fundamental questions, we had only a Note of M. Fatou (October 1906), where the author showed that, in some examples, it could happen

[5] To give a flavour of this: the "Newton method" is, according to Cajori [1893, p. 363–366], due to Raphson, so that he calls it the "Newton-Raphson method"; as for Cayley, whom we shall have the opportunity to meet again, he calls this same method the "Newton-Fourier method". Newton, Raphson, Fourier, one could even add two pages by Galois [1962, p. 379], following an Appendix in Legendre's book on number theory [1955, Appendice, Section I] (see also [Galuzzi 2001])... but let's remain serious. See [Alexander 1994] for a prehistory of the subject, from Newton's method to what we are discussing here.

[6] Georges Humbert (1859–1921), a member of the Academy of Sciences from 1901, plays an important supporting role in our story and in Gaston Julia's life. We shall see him settle the argument in a priority quarrel (below, in December 1917). Let us point out also that he, together with Painlevé, would back Julia in his election as a member of the French Mathematical Society (SMF) on March 13th 1919. Julia would participate in the publication of Humbert's Works, with Pierre Humbert (1891–1953), the son of Georges Humbert and a mathematical contemporary of Julia. See also Note 23 in Chapter II.

[7] It seems clear that the Academy of Sciences makes no connection with Poincaré's work on Kleinian groups, which we shall have the opportunity to mention again (see Note 14 and § IV.5.b), and this despite the fact that Fatou had noticed, as early as 1906, the analogy with automorphic forms [Fatou 1906d].

[8] The definition of the word multiplier, together with other useful definitions, can be found in § I.4.

[9] The beautiful word consequent (conséquent in French), which our protagonists will use quite a lot, deserves to be defined: the consequents of a point z are its successive images $z_n = R^n(z)$ $(n \geq 1)$, its iterates.

> that the regions of the division are bounded by non-analytic curves, thus highlighting the difficulty and the complexity of the question.
> Finally, from another point of view, Poincaré had established that, in certain cases, it is possible to associate with S a function $\theta(u)$, meromorphic in the whole plane, such that, if one puts $z = \theta(u)$, one has $z_1 = \theta(su)$, s being a constant of absolute value *greater* than 1, thus reducing the study of the iteration to that of $\theta(u)$; but no application was made of this *parametric iteration* method[10].

The Note [Fatou 1906d]

It seems that the only global work that had been undertaken in this field before the publication of the Prize topic was indeed Pierre Fatou's note [1906d].

> Nobody had dared to tackle the question in the whole plane when, in 1906, in a short *Comptes rendus* note, M. Fatou, giving the example of the extraordinary results met, showed at once the interest and the high difficulty of doing so,

Hadamard would comment in 1921 in a report that we shall have the opportunity to quote several times[11]. In this note, Fatou investigated the rational

[10] L'Académie avait mis au concours l'étude de l'*itération* d'une substitution, en rappelant que le point de vue *local* avait seul été considéré jusqu'alors et en invitant les concurrents à se placer d'un point de vue *général*.

Les travaux antérieurs, notamment les travaux fondamentaux de M. Kœnigs, avaient, pour une substitution S, $z_1 = \varphi(z)$, à une variable, conduit à la notion des *points d'attraction:* si ζ est un point laissé fixe par S ou par une de ses puissances (*point invariant*), et si une quantité correspondante, dite *multiplicateur*, est de module *inférieur* à l'unité, les transformés successifs (*conséquents*) d'un point z, pris au voisinage de ζ, tendent tous vers ζ, ou tendent périodiquement vers p points, dont l'un est ζ, et dont les autres sont ses $(p-1)$ premiers conséquents.

Ces résultats initiaux soulevaient bien des problèmes: les points attractifs sont-ils en nombre limité; quel est le domaine exact d'attraction de l'un d'eux; quelle division du plan est ainsi associée à une fonction $\varphi(z)$ donnée?

Sur ces questions fondamentales, on ne possédait qu'une Note de M. Fatou (octobre 1906), où l'auteur montrait, sur des exemples, que les régions de la division pouvaient être limitées par des courbes non analytiques, mettant ainsi en évidence les difficultés et la complexité de la question.

Enfin, à un autre point de vue, Poincaré avait établi que, dans certains cas, on peut associer à S une fonction méromorphe dans tout le plan, $\theta(u)$ telle que, si l'on pose $z = \theta(u)$, on ait $z_1 = \theta(su)$, s étant une constante de module *supérieur* à 1, ce qui ramène l'étude de l'itération à celle de $\theta(u)$; mais aucune application n'avait été faite de cette méthode d'*itération paramétrique*.

[11] Hand-written report, July 4th 1921, Fatou file, archives of the Academy of Sciences. The unabridged text (together with the French original) can be found in the Appendix at the end of this book.

maps the unique attracting[12] orbit of which is a fixed point. Despite the attention directed to functional equations, the questions Fatou raised are stated in dynamical terms: an attracting fixed point attracts a whole neighbourhood; what do the boundaries of these various convergence domains look like? He proves (with some additional assumptions) that the iterates of a rational function with a unique fixed point converge to this point... except on a set, which he denotes by E and of which he proves that it is totally discontinuous (*i.e.* its connected components are points) and perfect (*i.e.* it is closed and without isolated points). See, more precisely, Example I.4.2 below. In addition to being the first global result, it seems that this is also one of the first times that what we now call general topology was used in the iteration problem. Fatou also considered, in the second part of this note, the case where the rational function has several limit points, and proved that the lines between their convergence domains are not, in general, analytic, proving that this is the case for $R(z) = (z^2 + z)/2$ (see Example I.4.3 below). Fatou did continue to work on the subject, as he wrote in a letter to Fréchet on February 10$^{\text{th}}$ 1907:

> [...] I have undertaken more extensive research on iteration; but I lack the energy to write it all up [...]

(see the complete letter and the French original on page 256)—given the very close relationship between Fréchet and Hadamard, it is very probable that the latter was aware of this.

Digression (on general topology). What we today call "general topology" arose from a part of "set theory"[13] or "point set theory"—*Mengenlehre*, since

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[12] The word "attracting" did not exist in 1906, neither did it exist when the Prize was declared. But it was used in the report on the Prize: we shall see an efficient terminology being set up as the work on the subject progresses (see page 78). We remind ourselves of the definition in § I.4.

[13] Regarding set theory in France at that time, see the very interesting paper [Gispert 1995].

this theory was invented in Germany and in German[14]: the paternity of Cantor is generally acknowledged.

Even if the term did not yet exist, general topology was used in analysis, even in France, at least since the work of Poincaré, then Borel[15].

> The impression produced on us by the articles of M. Cantor is appalling; to read them seems to all of us a genuine torture, and while we pay tribute to his merit, while we recognise that he has opened a new field of research, none of us is tempted to follow him[16],

Hermite had written [Dugac 1985, p. 209], but this was already very old (1883).

Let us mention for instance, the Borel-Lebesgue theorem, which was called "a lemma in set theory" (in particular in [Julia 1918f]). The work of Borel, Baire and Lebesgue uses set theory quite a lot. Since we have not much space here for that, we direct the reader, for a more detailed history, to Taylor's papers [1982 ; 1985] on Fréchet, to the introduction [Purkert 2002] to the edition of the book [Hausdorff 1914] contained in the complete Works [Hausdorff 2002], and to [James 1999]. It was Bourbaki who, much later, would make the separation between set theory (cardinals, and so on) and general topology[17].

Among the books devoted to this theory, let us quote here those of Borel [1898] and Baire [1905], then that of Grace Chisholm Young and her husband [1906], written with the blessing of Cantor himself (see the letter Cantor wrote to Grace Chisholm that can be found in the 1972 Chelsea edition of that book), and that of Schoenflies [1913]. There was also a book by Sierpinski, in Polish, a part of which was translated into French, but only in 1928.

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[14] It should be noticed that, the same year, Mittag-Leffler published in "his" journal French translations of some of Cantor's papers (among which [1884]) and the article of Poincaré on Kleinian groups, in which the latter writes [1883, p. 78]:

> The vertices of various polygons R form *eine undendliche Punktmenge* P and, to get the line L, we must add to this *Punktmenge* its *erste Ableitung* P'. One sees that the line L is *eine perfekte und zusammenhängende Punktmenge*. It is in this sense that it is a line. [Les sommets de divers polygones R forment *eine undendliche Punktmenge* P et pour obtenir la ligne L, il faut ajouter à cette *Punktmenge* son *erste Ableitung* P'. On voit que la ligne L est *eine perfekte und zusammenhängende Punktmenge*. C'est en ce sens que c'est une ligne.]

A passage which shows that the terminology (derived, perfect, connected) existed only in German... and the subject of which is not unrelated to that of this book, so that it deserves to be quoted here (see also § IV.5.b).

[15] See his book [Borel 1898].

[16] L'impression que nous produisent les mémoires de M. Cantor est désolante; leur lecture nous semble à tous un véritable supplice, et en rendant hommage à son mérite, en reconnaissant qu'il a ouvert comme un nouveau champ de recherches, personne de nous n'est tenté de le suivre.

[17] In 1965, Denjoy [1980] would complain that the students who learned under Bourbaki hardly knew the notions of power, order, and transfinite.

Neither the notion of metric space, nor, *a fortiori*, that of topological space, appear in these books that are, indeed, books on set theory—in which powers and cardinals are important. The notions of derived set (due to Cantor under the name of *Ableitung*), of perfect set and of boundary are set out.

It seems that in France, the analysis course of Jordan at École polytechnique (at least its second edition [1893]) played an important role in the popularisation of this subject among young mathematicians (this book was used by the generations of Lebesgue, Baire, Fatou... and at least until the thirties). It was essential to understanding measure theory and the Lebesgue integral. This is what one of our protagonists, Pierre Fatou, said, at the very beginning of his thesis [1906c, p. 335]:

> The problem of the measure of sets was first tackled by M. G. Cantor; his definitions were clarified by M. Jordan in his course on analysis; but it is M. E. Borel [...][18]

In this book by Jordan, one finds the word "écart" (gap), that Fréchet would still use and that would become, with Hausdorff, our "distance"[19].

The courses given by Borel at ENS from the spring of 1897 also played a not insignificant role. About Borel again: his series *Collection de monographies sur la théorie des fonctions* published by Gauthier-Villars had a notable effect on the spreading of what would become general topology.

Let us mention also the article of Zoretti [1912], under the influence of Borel (whose Peccot course of 1901–1902 on meromorphic functions he transcribed), in the *Encyclopédie des sciences mathématiques*[20]—and in which he introduces the measure theory of Borel and Lebesgue. It is worth noticing that the original German edition has no article on this subject:

– there is an article (by Schoenflies) on set theory in Volume I (arithmetic), the French version of which was published in 1909 and adapted by Baire, but this is devoted more to cardinals and ordinals than to point sets,

– there is an article by Dehn and Heegaard on the *Analysis situs* in Volume III (geometry), which is more on "geometry of situation" than on general

[18] Le problème de la mesure des ensembles a été abordé pour la première fois par M. G. Cantor; ses définitions ont été précisées par M. Jordan dans son cours d'analyse; mais c'est M. E. Borel [...]

[19] Regarding Jordan's analysis course, see also [Gispert 1983].

[20] This is the French edition, "written and published following the German edition under the direction of Jules Molk" of the *Encyclopädie der Mathematischen Wissenschaften mit Einschuss ihrer Anwendungen, Herausgegeben im Auftrage* (under the auspices) *des Akademien des Wissenschaften zu Göttingen, Leipzig, München und Wien, so wie unter Mitwirkung zahlreicher Fachgenossen* (with the collaboration of numerous scientists), started under the direction of Felix Klein, planned as an international collaboration... the French edition has been the only non-German edition to appear, before it stopped for good in 1916, because of the war. An example of international co-operation that was interrupted, brutally and for a long time.

topology—it seems to me that this paper had no analogue in the French version.

The additional chapter that contains Zoretti's paper is inserted between the adaptations of that of Pringsheim (on the fundamental principles of function theory) and that of Voss (on differential calculus). Slightly biased (!) information on the organisation of the writing of this encyclopaedia can be found in [Lebesgue 1991]. Baire also was asked by Molk to contribute. In a letter to Borel [1990], he complains he must read

> some spiels of Hausdorff to which the Germans attach such great importance[21].

If I am not mistaken, Lebesgue did not contribute to this work.

Besides, the book which is now considered as the first "true" topology book would be written by a German mathematician, namely Felix Hausdorff [1914] (and he would dedicate it to Georg Cantor, "creator of set theory"). The title is still *Grundzüge der Mengenlehre* (foundations of set theory). Of course, and even if he amply quotes Borel and even Baire, this book, which appeared in Germany in 1914, was not used in France at the time we are interested in: we shall see that the war would interrupt, for a long time, all communication between French and German mathematicians[22]. On the reception and the use of Hausdoff's work, see also § IV.2.

Hausdorff also mentioned the words "*Analysis situs*" (Latin root, as in the famous paper of Poincaré and in its no less famous supplements, and as in the chapter by Dehn and Heegaard cited above) and "Topologie" (Greek root[23]), neither of the two having already been adopted. The first one would become, more or less, algebraic topology, the second would have to take on the adjective "general" before it would replace "point sets"[24].

⋆

As this digression shows, the beginning of the 20$^{\text{th}}$ century is not a kind of appendix to a 19$^{\text{th}}$ century which would never finally terminate. The period

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[21] certains topos de Hausdorff dont les Allemands font le plus grand cas.

[22] Also note that the Borel series *Collection de monographies* has no German contributor.

[23] It seems that the word "Topologie" was first used (at least first published) as early as in 1847 by the German mathematician Johann Benedict Listing [1847]. See a reproduction of its front page in [James 1999].

[24] The terminology "ensembles de points" has completely disappeared from French today, but the English *point set topology* persists in being synonymous with *general topology*.

just before the war was indeed, in mathematics as well[25], the beginning of a modern time, which war and its consequences would retard.

I.2 The protagonists around 1917–1918

The main people working on iteration between 1915 and 1918 were, in alphabetical order, Pierre Fatou (1878–1929), Gaston Julia (1893–1978) and Samuel Lattès (1873–1918), and also, as we shall see, the American mathematician Joseph Fels Ritt (1893–1951). The work of Fatou and Julia would make great use of the notion of *normal family*, due to Paul Montel (1876–1975). Here are a few words on these protagonists at that time, again in alphabetical order.

Pierre Fatou

Born in 1878 in Lorient (in Brittany), he entered the ENS in 1898, graduated in 1901, became assistant-astronomer ("astronome-adjoint") at the Paris Observatory, and passed his thesis in 1907. Of this thesis, he "often talked" with Henri Lebesgue [Lebesgue 1991, p. 112] (so that it is not surprising that he left his name to a lemma in integration theory). He was promoted to astronomer (permanent) in 1928 and died the following year. See Chapter V for portraits and for more information on his life and his work.

Gaston Julia

Born in 1893 in Sidi Bel Abbès, in Algeria, he entered (as the top-student) the ENS in 1911 (he was also ranked first at the École polytechnique) after only one year of preparation (while it usually takes two) at the lycée Janson de Sailly, and graduated in 1914. He was severely injured in the face (his nose was obliterated, his jaw was smashed) at "Chemin des Dames"[26] in 1915, had to undergo numerous operations (he was a "gueule cassée" (broken face), and would wear for the rest of his life a leather mask[27]). He passed his thesis in 1917 (this was on a different subject, the theory of forms, see page 64[28]), he was rewarded with the Bordin Prize of the Academy of Sciences and began to

[25] This is here an allusion to the modernity, for instance of a Picasso, a Schönberg or an Apollinaire. Thinking of the modernist mathematicians of the beginning of the 20$^{\text{th}}$ century, Borel, Baire, Lebesgue and Fatou, for instance, we quoted, as an opening to this book, another who was wounded in the head in the 1914–18 war, "trepanned under chloroform", and who was, indeed, a modernist.

[26] Several battles took place at Chemin des Dames (not far from Soissons, 150 km northeast of Paris), the most bloody of which was that of April 1917.

[27] There is a (later) photograph of Julia on page 210.

[28] The first paper published by Julia [1913] is called "On the singular lines of some analytical functions" [Sur les lignes singulières de certaines fonctions analytiques]. It appeared in Volume 41 of the *Bulletin* of the SMF... in the same volume two

work on the topic of the Prize we are discussing here. See Chapter VI to find out what happened next.

Samuel Lattès

Born in Nice in 1873, he entered the ENS in 1892 after only one year of preparation in Marseilles, graduated in 1895, defended his thesis in 1906, was professor at Toulouse University from 1911, died from typhoid fever during the summer of 1918. Regarding his work and his life, see Note 60 in this chapter, and Chapter II (where there is a photograph of Samuel Lattès), especially Note 65 and the references given there.

Paul Montel

Born in 1876, also in Nice, he entered the ENS in 1894, graduated in 1897, he liked to travel and to teach[29], he took his time before working on a thesis[30], passed it in 1907, taught in secondary schools then, from 1911, at the University of Paris. He was awarded the Gustave Roux Prize by the Academy of Sciences in 1913. He would die as an almost hundred-year-old, but we shall speak of him again (in Chapter VI). More precise biographic information can be found in the papers [Cassin 1966 ; Beer 1966].

As the readers will certainly have noticed, all the protagonists of this story, the son of a mechanic from Algeria (Julia), the sons of a photographer and a shopkeeper in Nice (Montel and Lattès), and the son of a Breton sailor as well (Fatou), all received the same scientific education, through preparatory classes and the École normale supérieure. We could think that they acquired the same knowledge, a common corpus—notice however that neither Lattès nor Montel benefitted from the courses of Borel at the École normale supérieure.

Digression (Reports on the theses). The reports on the theses of our protagonists (except for that of Julia, which was defended too late and of which we shall speak again in the next chapter) can be found in [Gispert 1991]: written by Painlevé on Fatou on p. 397 and Montel on p. 399, and by Hadamard on Lattès on p. 396 (Lattès' thesis was called "On the functional equations that define a curve or a surface that is invariant under a transformation" [Sur les équations fonctionnelles qui définissent une courbe ou une surface invariante par une transformation]).

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papers of Fatou [1913a ; 1913d] appeared a well, the title of one of them being "On the singular lines of analytic functions" [Sur les lignes singulières des fonctions analytiques]. The similarity between the titles is, taking the rest of the story into account, rather surprising, but it conceals deep differences: Julia was twenty and produced a classical work on complex analysis, as for Fatou, he proceeded in his using the Lebesgue integral.

[29] One of his pupils in Poitiers in 1898 was Raoul Dautry, who became a politician and with whom he kept good relations for the rest of his life.

[30] At the instigation of the historian Albert Mathiez, his fellow student at the ENS.

I.3 The war

The First World War has already made an appearance in this text, when we discussed the references on general topology. And indeed, this story takes place during the final two years of a war which was an absolute slaughter: it killed eight million people and produced six million disabled people, among whom were 1,400,000 French victims, that is, approximately one tenth of the male working population, and almost as many disabled, among whom were numerous “broken faces”.

France sent its elite to the front line. The students of the French “grandes écoles” were most often in the infantry, in general with the rank of second lieutenant, a rank that put them at the head of their soldiers so that they were especially vulnerable. Both Gaston Julia and René Gateaux (of whom we shall speak more below) were infantry second lieutenants, the first in the 34th and the second in the 69th regiment. This was an effect of a 1905 law, called “the two year law”[31]. This “egalitarian” French policy had deeply unequal effects. If the proportion of mobilised soldiers who were killed was a dreadful 16,8%, this proportion was 30% for the infantry officers and 41% for the students of the ENS (figures given in [Audoin-Rouzeau 1992 ; Becker 1992]). It was also reckoned that 40% of the students who had registered at a French university in 1914 were killed or mutilated (figure from [Beaulieu 1990, p. 41]).

The other warring countries had different policies. This does not mean that the young German intellectuals were not called up, nor does this mean that they did not enlist. For instance, Richard Courant was wounded in the trenches [Reid 1976][32], Max Dehn enlisted and was in the war from 1915 to 1918, Heinz Hopf enlisted as well, was in the war as a lieutenant on the western front and was wounded twice, Emil Artin was enlisted in the Austrian army[33] (see also the examples of Siegel and Hasse on page 126), the narrator of *All Quiet on the Western Front* [Remarque 1928] was also a student. Let us mention also, briefly, the British case. The army engaged by the United Kingdom in the war was at first a professional one. It then called for volunteers. The first wave of conscription began after the *Military Service Act* of January 1916: single men from 18 to 41 were enlisted. This was of course

[31] Despite strong opposition from the socialists, the two year law was replaced, in July 1913, by a “three year law”, which announced the forthcoming war and which modified the duration of military service and the age of call-up (20 instead of 21), but which did not modify the status of the students of the grandes écoles.

[32] See the same book, pages 47–49, for information and comments on the mobilisation in Göttingen at the beginning of the war.

[33] Max Dehn was rewarded by the *Ehrenkreuz*, a military decoration, but this did not prevent him in 1935 from having to leave, first his position at Frankfort, then Germany. See [Burde et al. 2002 ; Siegel 1978]. As for Artin, who was a teenager when he was enlisted, it was Hamburg that he would have to leave in 1937. See [Brauer 1967]. Regarding Heinz Hopf, see [Frei & Stammbach 1999]. He was a professor in Zurich from 1931.

very different from the situation of the French army. On the other hand, there were, for instance in Cambridge, during the whole war, active pacifists (often people who remembered the Boer war), conscientious objectors (often for religious reasons), and even, after the Military Service Act, associations fighting against conscription (Hardy was the secretary of one of these associations, *The Union of Democratic Control*)—such phenomena had no analogue in France after the start of the war. If this war slaughtered 800,000 British soldiers (among them the eldest son of Grace Chisholm and William Young, who were mentioned above) and if two million people were wounded (among them the mathematician Ralph Fowler, wounded at Gallipoli), this was not a massacre of the intellectual elite comparable to that which affected French students. See [Barrow-Green 2008].

But let us come back to the French mathematicians. Paul Lévy and Émile Borel served in the artillery, Maurice Fréchet, born in 1878, was mobilised with the rank of sergeant and served as an interpreter with the British troops (but on the front), André Bloch[34] was injured, René Thiry was injured twice and was taken as a prisoner, Louis Antoine, second lieutenant[35] in the 151$^{\text{st}}$, was wounded three times and became blind, Louis Sartre, another 1911 student of the ENS, was taken prisoner, Paul Flamant, second lieutenant in the 77$^{\text{th}}$, was wounded in Charleroi and taken prisoner, André Marchaud, a 1909 graduate of the ENS, second lieutenant in the 344$^{\text{th}}$, was taken prisoner as early as August 20$^{\text{th}}$ 1914, Henri Mineur, who entered the ENS at eighteen in 1917, enlisted in the army.

Many young scientists died. According to [Guiraldenq 1999], of 265 students who entered the ENS between 1910 and 1913, 109 were killed (this is the 41% mentioned above)[36]. Of most of them, the names have been forgotten—they are nevertheless still visible, in golden letters engraved in the marble of the Memorial at the École normale supérieure, as well as on the yellowing pages of the yearbook of former students. One thinks for instance of the brilliant young mathematician René Gateaux, killed as early as October 3$^{\text{rd}}$ 1914, at the age of twenty-five, when he had not quite finished his thesis[37], of Joseph

[34] André Bloch and Paul Lévy are the only former students of the École polytechnique in our list of young mathematician soldiers, all the others being from the École normale. As was Julia, André Bloch was born in 1893. He entered the École polytechnique in 1912. For more information about this uncommon mathematician, who, by the way, was also a specialist in the Picard theorems that we mention here and there in this text, see [Cartan & Ferrand 1988].

[35] The ranks and the numbers of the regiments given here come from the yearbook of the association of former students of the École normale supérieure.

[36] I don't know the proportion of students of the École polytechnique who were killed. In his book [1970], Paul Lévy writes that the fact that he chose the École polytechnique (rather than the ENS) may have saved his life.

[37] Regarding René Gateaux' life, death and destiny, see the paper of Laurent Mazliak [2007]. The Academy of Sciences posthumously awarded the Francœur Prize to Gateaux in 1916.

The Memorial, unveiled on December 9th 1923,
at the École normale supérieure

> M. Gateaux, formerly a student of the École normale, then disciple of our illustrious correspondent M. Senator Volterra, who recognised his high value, also died for France, thus disappointing the legitimate expectations created by his first work [M. Gateaux, naguère élève de l'École normale, puis disciple de notre Illustre correspondant M. le Sénateur Volterra, qui en avait reconnu la haute valeur, est aussi mort pour la France, trompant ainsi les légitimes espérances que suscitaient ses premiers travaux]

Jordan [1916] would say during the public session in which the prizes were announced. Two of his posthumous papers [1919a ; 1919b] would be published by Hadamard and Paul Lévy, in the same volume as an article [Fatou 1919b] we shall have the opportunity to mention again.

Marty, a specialist in Fredholm equations, who died for France in 1914[38], of Roger Vidil, a 1911 student of the ENS as was Julia, who had written up the notes he took during the Peccot course of Châtelet [1913], killed near Arras on November 27th 1914 and reported missing until his body was identified in 1917, of Paul Lambert, an algebraist, ranked second at seventeen in the same year 1911, the author of a paper on Gauss integers, corporal in the 60th, killed at the front in 1915, of Jean Piglowski, who was always in a good mood and had written a small paper on the motion of projectiles in 1911, killed in in the Vosges on February 18th 1915, of Louis Néollier, second lieutenant in the 258th killed in 1914, whose disastrous agrégation oral exam was recounted by Lebesgue [1991, p. 308] (in 1913)[39], and who was still reported missing[40] after the armistice in 1918, or of Roger Félix, ranked first at the ENS, at seventeen, in 1916, who enlisted before call-up because he wanted to be with his classmates and who "died for France" shortly before the armistice[41].

This is how the destiny of these young people is related, at the Academy of Sciences, at the end of 1915:

> They were pacifists, so to speak, by purpose, because scientific works are, more than anything else, works of peace and quiet; they were also pacifists by reason, because an intelligence which has been charmed by the enchantments of Science, delighted by its wonders, refuses to understand that men should use all the resources of their mind to collect the most effective ways of killing one another. They enroled willingly among the disciples of the naïve school which pretended "to declare peace to the world".
> But now sounds the call to arms. The Homeland is attacked, and all these pacifists stand: no one will miss the call. Farewell! the quiet work in the laboratory; they are now only soldiers; they do not even think—and it might be a pity that nobody thought of it for them—to take advantage of their knowledge to obtain special positions; had they not been called to handle the shovel and the gun like their friends who just had left the file or the plough, they would think themselves demeaned[42]. They are all brothers; all

[38] See also Note 81 in Chapter VI.

[39] The algebraists Lambert and Vidil are mentioned in [Dubreil 1982]. Julia mentioned Lambert in a speech [1970, p. 169] he gave in 1950. Lambert, Piglowski and Néollier appear, very much alive, in [Lebesgue 1991]. The article [Piglowski 1911] of Piglowski is referenced in the *Jahrbuch über die Fortschritte der Mathematik*. I found mention of the paper [Lambert 1912] in the notice [Julia 1919a] which Gaston Julia devoted to his friend Paul Lambert.

[40] This war also caused numerous disappearances. The record of Louis Néollier in the database "memory of men" [mémoire des hommes] of the French Ministry of Defence says that he "died for France between September 20th and 26th 1914"—his body was never found and it was a judgement dated May 21st 1920 that declared him dead.

[41] Regarding Roger Félix, see the memoires of his sister in [Félix 2005].

[42] Let us listen to another point of view, that of Camille Marbo [1968, p. 165 and 172], regarding her adopted son Fernand Lebeau, a student at the ENS, a socialist who was an opponent of the war:

> will walk hand in hand, under the horizon blue uniform, the uniform of equality that mistakes them for the sky! Only, when the assault time comes, they will remember that the more educated have to set the example; they will be the first to jump on the embankments, the first to run to the barbed wires, the first to die [Perrier 1915, p. 803][43].

Note that the rhetoric that would accompany the mention of the war wound of Julia all the way through speeches by himself or by others that are collected in [Julia 1970] is very close to that which we have just had the opportunity to

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> As a physicist with a great future ahead of him, he was posted in a sound reconnaissance section [...] Without mentioning it to anybody, he managed to get a position of lieutenant in the infantry, like almost all his colleagues. During his first leave, he told me
>
> "We socialists, who want to work for harmony between peoples and peace, we decided to be sent to the front line in order to prove that we are as brave as anybody. Those who survive will have the right to speak loudly in front of the shirkers." [...]
>
> "Never forget it", Fernand told me.
> I never forgot it.

[En tant qu'agrégé physicien promis à un brillant avenir, il avait été affecté à une section de repérage par le son [...] Sans en parler à personne, il avait fait des démarches pour obtenir un poste de lieutenant d'infanterie, comme la presque totalité de ses camarades. À sa dernière permission, il m'avait dit: " Nous, socialistes, désireux de travailler pour l'entente des peuples et la paix, nous avons décidé de nous faire envoyer en première ligne afin de prouver que nous sommes aussi courageux que n'importe qui. Ceux qui survivront auront le droit de parler haut devant les embusqués." [...] "Ne l'oublie pas, m'avait dit Fernand". Je ne l'ai pas oublié.]

Numerous pacifists and socialists enlisted, like Henri Barbusse who described his experience in *Under Fire* (*le Feu*) [Barbusse 1916], Goncourt Prize 1916.

[43] Ils étaient, pour ainsi dire, pacifistes par destination, parce que les œuvres scientifiques sont avant tout des œuvres de calme et de sérénité; ils l'étaient aussi, par raison, parce qu'une intelligence séduite par les enchantements de la Science, ravie par ses merveilles, se refuse à comprendre que des hommes emploient toutes les ressources de leur esprit à rassembler les plus sûrs moyens de s'entre-tuer. Volontiers, ils se rangeaient parmi les disciples de cette école naïve qui prétendait "déclarer la paix au monde".

Mais voilà que résonne l'appel aux armes. La Patrie est attaquée et tous ces pacifistes sont debout: pas un ne manquera à l'appel. Adieu! le tranquille travail du laboratoire; ils ne sont plus que des soldats désormais; ils ne songent même pas — et il fut peut-être dommage qu'on n'y ait pas songé pour eux — à se réclamer de leur savoir pour obtenir des postes spéciaux; ils croiraient déchoir s'ils n'étaient pas appelés à manier la pelle ou le fusil, tout comme les camarades qui viennent de déposer la lime ou de quitter la charrue. Tous sont frères; tous vont marcher la main dans la main, sous l'uniforme bleu horizon, l'uniforme d'égalité qui les confond avec le ciel! Seulement, quand arrivera l'heure de l'assaut, ils se souviendront que les plus instruits doivent l'exemple; ils seront les premiers à sauter sur les glacis, les premiers à courir aux fils barbelés, les premiers à mourir.

admire. More of a politician, Painlevé would say, in his victory speech [1918c, p. 799]:

> Ah! Gentlemen, the horrifying holocaust demanded by the monstrous Moloch erected by pangermanist ambition before he was annihilated! However stoical we decide to be, our heart becomes heavy when we think of our deserted laboratories, of our university chairs from which eloquent and deep voices will not be heard anymore, of so many young and powerful brains whose fertile thought was interrupted forever by a stupid iron fragment. Our grandes Écoles, the breeding grounds of our engineers and scientists,—École polytechnique, École centrale, École normale supérieure, to quote only these,—how empty will be their audience[44] when they meet for the first time![45]

From this evocation, we understand that a whole generation of intellectuals, of scientists and, for what interests us here, a generation of mathematicians, was sacrificed and decimated. According to [Leloup 2009], the last thesis in mathematics defended in Paris by a French mathematician in 1914 was that of Georges Valiron, on June 20th, there were three of them in 1915, including that of Joseph Pérès, one in 1916, then the next one was that of Julia, of which we shall speak at length later (see page 64), in December 1917. There was then only one in 1918, that of Pierre Humbert (Georges Humbert's son) on June 18th 1918 (in Paris)[46]. One should note that he was wounded during the war and, like Julia, started his research again after his injury.

Fathers bury their sons. Julia and the mathematicians

This is one of the reasons why Gaston Julia, a brilliant young former student of the ENS, seriously and atrociously injured on January 25th 1915, was the darling child of the mathematicians of the previous generation.

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[44] Let us quote Camille Marbo again [1968, p. 171]:

> Back in his position of scientific director [at the ENS], Émile Borel found the school filled with ghosts. [Revenu prendre son poste de directeur scientifique [à l'ENS], Émile Borel trouva l'École peuplée d'ombres.]

[45] Ah! Messieurs, l'effroyable holocauste qu'a exigé, avant d'être anéanti, le Moloch monstrueux dressé par l'ambition pangermaniste! Si stoïques que nous voulions être, notre cœur se serre quand nous songeons à nos laboratoires déserts, à nos chaires où des voix éloquentes et graves ne se feront plus entendre, à tant de cerveaux jeunes et puissants dont un éclat de fer stupide a interrompu pour jamais la pensée féconde. Nos grandes Écoles, pépinières de nos ingénieurs et de nos savants, — École polytechnique, École centrale, École normale supérieure, pour ne citer que celles-là, — quels vides présenteront leurs auditoires quand ils se réuniront pour la première fois!

[46] I owe this information to Juliette Leloup.

He was seen as a substitute son for a lot of them, who had lost theirs. Hadamard, for instance, lost two of his sons[47], Jordan three sons and one grandson.

Closer to Julia, let us mention Émile Borel, who was the scientific director of the ENS, and who lost his nephew and adopted son, also a former student of the ENS, in the storm on September 29th 1915 (Fernand Lebeau has already been mentioned in Note 42, see the memories book [Marbo 1968]), Émile Picard, whose elder son was killed[48] in Crouy in January 1915 (just before Julia was wounded), Georges Humbert, whose son was wounded. All took it in turns at his bedside in the military hospital of Val-de-Grâce.

The atmosphere in which Julia's work took place is perfectly described in the report that Picard would deliver at the end of the viva for his thesis (see page 67): through Julia, the sacrificed generation[49] would be glorified[50].

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[47] While their mother, Louise Hadamard, was a nurse, Pierre Hadamard, a student of the École polytechnique, was killed in February 1916 in Verdun at the age of 22, and Étienne Hadamard, who passed the exam to enter the École centrale, was killed in June 1916, also in Verdun, at the age of 19. Let us add that the third son of Jacques and Louise Hadamard, Mathieu, a member of the FFL (the French resistance army), would be killed in 1944. See [Maz'ya & Shaposhnikova 1998].

[48] The elder son of Picard, Charles (1884–1915), was killed in Crouy, near Soissons, on January 8th 1915; his youngest daughter Madeleine (1892–1915) was killed during the war as well, she was a nurse. A few years later, Picard would lose his last son Henry (1886–1926) killed by tuberculosis. Two daughters would survive him, Jeanne and Suzanne (Picard's biographical file, archives of the Academy of Sciences). Jeanne and Charles Picard (born in 1882 and 1884) were childhood friends (and cousins) of Marguerite Borel (born in 1883), see her book [Marbo 1968].

[49] A more detailed discussion of the notion of "generation" (and of the generation in question here) is given in [Sirinelli 1992].

[50] Julia played very well the role of a representative of this generation. The way Paul Dubreil [1950, p. 149], a 1923 graduate of the ENS, remembered the inauguration of the Memorial shown on page 26 shows this:

> Shortly after the start of the academic year, the "École" was awarded the War Cross, during a ceremony in which the sacrifice of its Sons, dead and alive, was exalted. When Dupuy finished reading the long list of those who fell, we saw you [Dubreil is addressing Julia] move forward to receive the Cross of the École and to carry it to the Memorial. Among the memories I have that are related to the 1914–1918 war, this is one of the deepest; it still gives me a striking impression of seriousness and greatness. [Peu après la rentrée, l'École, cette année-là, reçut la Croix de Guerre, au cours d'une cérémonie où fut exalté le sacrifice de ses Fils, morts et vivants. Quand Dupuy eut fini de lire la longue liste de ceux qui étaient tombés, nous vous [Dubreil s'adresse à Julia] vîmes vous avancer pour recevoir la Croix de l'École et la porter au Monument aux Morts. Dans ceux de mes souvenirs qui se rapportent à la guerre 1914–1918, celui-ci est l'un des plus profonds; il me laisse encore une impression saisissante de gravité et de grandeur.]

Volume 6 of the Complete Works of Julia [1970], the one in which his speeches and non-mathematical texts are collected, is full of hints of the fact that Picard was, for Julia, a fatherly figure. The very catholic Gaston Julia loved to compare Picard with Saint Christopher (see for instance [Julia 1970, p. 50 and 262]). One can find in the archives of the Academy of Sciences a letter sent by Julia to Picard in February 1936 to give him an account of the state of the International Mathematical Union (IMU) (Julia was a member of the Commission in charge of the preparation of the refounding of the IMU at the Oslo conference[51]), a letter the main part of which is devoted to a description of the pain and health problems of Julia. The way Julia, who, at the time of this letter, was a man of 43, complains, shows the kind of relationship he had with his correspondent. In addition, Julia was the one who gave a speech "on behalf of the students of M. Émile Picard" during the ceremony of awarding the medal of the Mittag-Leffler Institute to Picard on July 6th 1937 (see [Julia 1970, p. 39]). It is probable that, conversely, Julia was a kind of son for Picard.

There was a similar relationship between Julia and Borel. All the students of the ENS, and in particular Julia, who was writing up Borel's lectures, were, even before the war, the "children" of the assistant director Borel[52]. The letters of Julia to Borel[53] show that Borel sent money to Julia, in 1914, to help him to equip himself before leaving for the front. Borel was also, for quite a long time, a kind of father for Julia[54].

War effort

Montel, who finished his military service in 1898 with the rank of corporal [Cassin 1966] was mobilised, despite some "eye problems":

> Montel writes to me that his eyes begin again to worry him[55]

Lebesgue [1991, p. 144] wrote on March 23th 1906, and again on October 7th 1910,

> The state of Montel's eyes has not improved, on the contrary[56] [Lebesgue 1991, p. 268].

[51] Regarding the IMU, see also page 209.
[52] Regarding the family atmosphere at the ENS, invitation of the students by the scientific director, and so on, see again [Marbo 1968].
[53] Borel Collection, archives of the Academy of Sciences.
[54] These mathematicians constituted a kind of "parental generation", even if it is not absolutely correct, in terms of their influence, to put Borel and Hadamard in the same generation as Picard and Jordan. Actually, Borel, as the scientific director of the ENS and with his *Collection de monographies sur la théorie des fonctions*, and Hadamard, setting up his legendary seminar at the Collège de France in 1913, would have a more direct scientific influence than Picard and Jordan.
[55] Montel m'écrit que ses yeux recommencent à l'inquiéter
[56] L'état des yeux de Montel ne s'est pas amélioré au contraire

After a few months, Painlevé called him to the Department of Inventions and he contributed to the war effort by Lebesgue's side (see below).

Other mathematicians were not called up, Baire for instance, due to failing psychological health[57], Denjoy, who was declared unfit for military life because of his very bad eyesight, was only called up for the auxiliary service [Cartan 1974] and worked on the mathematical problems of ballistics (see below)[58]. The two other protagonists of the story of iteration, Fatou and Lattès, did not fight in the war.

Pierre Fatou had a very weak constitution (see Chapter V and the references there), which explains why he was not called up.

Samuel Lattès also had some health problems. For him too, we have a testimony of Lebesgue. In 1910, he wrote about the question of academic positions:

> Lattès' health is such that he might not agree to go to Clermont, but it should be offered to him[59] [Lebesgue 1991, p. 251][60],

and, a few days later:

> Lattès is neurasthenic[61], true, but not in his work [Lebesgue 1991, p. 256].[62]

It seems that he stayed in Toulouse for the whole duration of the war. If he contributed to the war effort and how, we do not know.

⋆

This is not the place to investigate in great detail and systematically how the scientists contributed to the war effort. This question goes far beyond the scope of the present study and deserves to be left to "real" historians—who do indeed investigate it: see the very interesting study [Aubin & Bret 2003] and, more specifically on mathematics and mathematicians, the works of the "project on the history of mathematics" of the Institut mathématique de Jussieu (for instance [Mazliak 2007 ; Goldstein 2009]).

We nevertheless mention briefly (based solely on published sources which are easily accessible[63] and which concern, for the most part, mathematicians

[57] Declared unfit for duty after a few weeks of military service at the end of 1897, he was exempted permanently on December 1st 1914 (see [Dugac 1990]).

[58] According to [Choquet 1975], he was sent on a mission to Utrecht in 1917 and was torpedoed twice on the way there.

[59] La santé de Lattès est telle qu'il n'acceptera peut-être pas d'aller à Clermont, mais on devrait le lui offrir

[60] Lattès was a teacher in a secondary school in Algiers, then in Dijon, in preparatory classes in Nice, then in Aix after a sick leave. He was then, after his thesis in 1906, appointed in Montpellier in 1908, "chargé de cours" (assistant-professor) at the Faculty of sciences in Besançon in 1911 and eventually, a few months later, professor in Toulouse.

[61] In a letter to Borel, dated March 11th 1902, Baire described Lattès as a "fellow in neurasthenia" [confrère en neurasthénie] [Baire 1990, p. 51].

[62] Lattès est neurasthénique, soit, mais pas dans ses travaux.

[63] To browse the *Comptes rendus* is rather informative. In 1915, the following titles can be found:

who appear elsewhere in this text) that some scientists, and in particular some mathematicians, contributed to the war effort without actually fighting in the war. The mobilisation started in France on August 2nd 1914. As early as August 3rd, the President of the Academy of Sciences (who, in 1914, was Paul Appell) declared:

> My dear Fellows
>
> In the serious situation the country is facing, I am certain that I am the spokesman of all the Members of the Academy who have not been called up to a public service, when I declare, on their behalf, that they are at the disposal of the Government to help in the national defence, each within his own special field[64].

And our commentator Perrier, who was the president the following year, confirmed, in his inexhaustible speech [1915, p. 809]:

> Since August 3rd 1914, its members [of the Academy of Sciences] split up into four big Commissions corresponding to their specific competence, with respect to the various aspects of the war. A Mechanics Commission prepares for the study of the possible improvements of the Air Force, electric traction, destruction of barbed wires and even of artillery. Numerous and especially delicate are the problems the Physics Commission deals with. The Chemistry Commission prepares to know everything concerning explosives and gas, either tear gas, suffocating gas, or deadly gas, by the use of which the Germans have managed to deepen further the barbarity of their war; it thinks of organising, not without feeling nauseous, the way of paying back dishonourable enemies, but who should nevertheless be controlled by their own means, gas for gas[65], asphyxiation for asphyxiation, as our threatened soldiers and the neutrals themselves urge us to do [... the fourth Commission is that of hygiene, health and diet][66].

New treatments of the injuries of nerves by projectiles, On the ration of the soldier in wartime, On the wounds of the external genital organs, Feeding armies in campaign, Radioscopic methods to locate projectiles, On an induction device for searching for projectiles...

During 1916, just Ernest Esclangon published three notes under the titles:

On the air trajectories of projectiles, On cannon shots and silence zones, On the Doppler principle and the whistling of projectiles.

64 Mes chers Confrères,

Dans la situation grave où se trouve la Patrie, je suis assuré d'être l'interprète de tous les Membres de l'Académie non mobilisés dans un service public, en déclarant en leur nom qu'ils se tiennent à la disposition du Gouvernement, pour aider à la défense nationale, chacun selon sa spécialité.

65 There was, in the ENS, a chemistry laboratory that produced deadly gas [Lebesgue 1991, note 996].

66 Dès le 3 août 1914, ses membres [de l'Acadmie des sciences] se répartissent en quatre grandes Commissions correspondant à leurs compétences particulières, relativement aux divers aspects de la guerre. Une Commission de Mécanique s'apprête à étudier les perfectionnements qui peuvent être apportés à l'Aviation,

As we shall see (in § II.2), at the beginning of 1918, Painlevé would use a more sober style to describe this contribution of the Academy of Sciences. In what we have already called his "victory speech" [1918c, p. 808], he draws up a list and recalls the "mobilisation of science":

> All the problems raised by the war, on land, on sea or in the air, the war of mines, the submarine war, all the attack and defence methods in the war of trenches, and so on, have been studied, explored, by a multitude of researchers, scientists, engineers, workers. Applications and improvement of the T.S.F. [wireless transmission (radio)]; long distance land communications; sound reconnaissance of enemy batteries and saps; radiowave tracking or guiding of dirigibles and aeroplanes; reconnaissance of enemy positions by aerial photographs; new explosives; smoke projectiles; toxic gas (as attack or protection means); aircraft engines; trench mortar shells; infantry cannons; aeroplane cannons; and lastly tanks[67], all subjects (and how many subjects do I not forget!) that required the intervention of the most diverse intelligence and to which all the sciences contributed: chemistry, mechanics, thermodynamics, optics, acoustics, electricity, meteorology, up to the investigation of new problems the interest of which will appear in the future. The most abstract or the most subtle mathematics contributed to the solution of reconnaissance problems and to the computation of very new range tables which increased by 25 per cent the efficiency of the artillery.[68]

à la traction électrique ou à vapeur, à la destruction des fils barbelés ou même à l'Artillerie. Nombreux et particulièrement délicats sont les problèmes qui doivent occuper la Commission de Physique. Celle de Chimie se dispose à connaître de tout ce qui concerne les explosifs et ces gaz lacrymogènes, asphyxiants ou meurtriers par l'emploi desquels les Allemands ont trouvé moyen d'avilir encore la barbarie de leur guerre; elle songe à organiser, non sans un haut-le-cœur, les moyens de rendre à des ennemis déshonorés, mais qu'il fallait cependant contenir par leurs propres moyens, gaz pour gaz, asphyxie pour asphyxie, comme le réclament instamment nos soldats menacés et les neutres eux-mêmes

[67] The production of tanks was determined in June 1917 and played a decisive role in the last phase of the war. Jules Breton, who would be elected as a free Academician in 1920, contributed, at the Department of Inventions, to developing this vehicle. Regarding this topic, see the speech [Perrier 1940] of the President of the Academy of Sciences, another Perrier, during another war.

[68] Tous les problèmes que posent la guerre sur terre, sur mer ou dans les airs, la guerre de mines, la guerre sous-marine, tous les moyens d'attaque et de défense dans la guerre de tranchées, etc., ont été étudiés, fouillés, approfondis par une multitude de chercheurs, savants, ingénieurs, artisans, ouvriers. Applications et perfectionnements de la T.S.F.; communications à distance par le sol; repérage par le son des batteries ou des sapes ennemies; repérage ou guidage par les ondes hertziennes des dirigeables ou des avions; repérage des positions ennemies par photographies aériennes; explosifs nouveaux; projectiles fumigènes; gaz toxiques (moyens d'attaque ou de protection); moteurs d'avions; mortiers de tranchées; canons d'infanterie; canons d'avions; enfin tanks, autant de sujets (et combien j'en oublie!) qui ont sollicité les intelligences les plus diverses et mis à contribution toutes les sciences: chimie, mécanique, thermodynamique, optique, acoustique,

Let us thus come to the mathematicians[69]. Here is, for instance, what Montel [1941] said in his obituary of Lebesgue[70] in 1941:

> During the 1914–1918 war, he chaired the Mathematics Commission of the Service of Inventions, Investigations and Scientific Experiments the director of which is our fellow member M. Maurain[71] in this Department of Inventions which was created by Painlevé. With a tireless energy, he worked on the solution of the problems raised by the computation and correction of the trajectories of projectiles, sound reconnaissance, and so on. With the help of a large team of volunteers, he prepares a triple entry collection of trajectories[72], which would be used, by interpolation, for the fast computation of range tables[73].

This Commission was created by Painlevé, then the minister of Public Education and the minister of Inventions regarding national defence, in November 1915. He called Émile Borel (who was a second lieutenant in the artillery, in a fighting company although he was forty-four) to take care of it, together with Maurain and Lebesgue. Montel does not say so, but he was there too, with Lebesgue[74]:

> Besides the examination of the inventions, they had to establish the range tables of the enemy cannons for the sound reconnaissance of their position. With the help of the information given by the intelligence service, they had

électricité, météorologie, jusqu'à l'étude de phénomènes nouveaux dont l'intérêt apparaîtra dans l'avenir. Les mathématiques les plus abstraites ou les plus subtiles ont participé à la solution des problèmes de repérage et au calcul des tables de tir toutes nouvelles qui ont accru de 25 pour 100 l'efficacité de l'artillerie.

69 See [Barrow-Green 2008] for the considerable contribution of British mathematicians to the war effort.

70 Other mathematicians contributed in a different way, for instance Sergeant Élie Cartan who was director of the "auxiliary hospital 103" set up in the premises of the École normale supérieure (see, for instance [Julia 1970, p. 59]). Montel probably died too late for his own obituary [Mandelbrojt 1975] to be concerned about telling us such old stories about him.

71 Charles Maurain, mobilised in 1914, was sent in 1915 to a sound reconnaissance station (like many others), then to the Department of Inventions (see [Coulomb 1968]).

72 According to [Félix 1974], Lebesgue corrected obvious (and dangerous) errors, one of which was foreseeing that the projectile could fall behind the gunman.

73 Pendant la guerre de 1914–1918, il préside la Commission de Mathématiques du Service des Inventions, Études et Expériences scientifiques que dirige notre confrère M. Maurain dans cette Direction des Inventions que Painlevé avait créée. Avec une énergie inlassable, il travaille à la résolution des problèmes soulevés par la détermination et la correction des trajectoires des projectiles; le repérage par le son etc. Aidé par une nombreuse équipe de travailleurs bénévoles, il prépare un recueil de trajectoires, à triple entrée, qui doit servir par interpolation à l'établissement rapide des tables de tir.

74 According to a letter quoted in [Taylor 1985, p. 289], René Garnier, then in Poitiers, used to come to Paris to make computations "at the Artillery Section".

> to reconstruct the trajectory of the projectile, piece by piece. They also had to modify some of their tables and to study the monstrous sketch which led them to the tank[75] [Beer 1966, p. 67].

Hadamard worked there too [Maz'ya & Shaposhnikova 1998, p. 100]. In [Guiraldenq 1999] is reproduced a journal article reporting Borel's nomination[76]. Also, in the last letters of [Lebesgue 1991], information is given on the work done under his direction. Jules Drach and Ernest Vessiot applied their research to problems in ballistics, the work of Drach was communicated to Denjoy, so that he could study it from a practical point of view, when he was at the "Grâves polygon[77]" (see [Drach 1920, note 1]). Henri Villat, who was then a private, computed, at the range centre of Bourg d'Oisans, range tables against aircraft [Leray 1973]. The report written by Hadamard on the work of the Commission of ballistics can be found in [Hadamard 1920].

The obituary of Gabriel Kœnigs [de Launay 1931], one of the main characters in the prehistory of the iteration problem, also mentions his mechanics laboratory (especially devoted to engine thermodynamics), opened in 1914 and which was very useful for national defence.

As for Paul Appell, he was the one who founded "National Relief" (Secours National) (see [Buhl 1931b]).

At the Observatory

Thinking of Pierre Fatou who worked there, let us come now to the Observatory. The Paris Observatory did not contribute as such to the war effort[78], but it is known that some astronomers of the Observatory did contribute. The staff gave their knowledge to help the army... but not during their working hours, as the annual report of the Observatory for 1915 points out. Charles Nordmann, for instance, who joined the army and who, as a second lieutenant in the Engineers, developed the first sound reconnaissance machines[79]

[75] En dehors de l'examen des inventions, ils eurent à établir les tables de tir des canons ennemis pour le service de repérage par le son de leur emplacement. À l'aide des éléments fournis par le service d'espionnage, il fallait reconstituer la trajectoire du projectile morceau par morceau. Ils durent aussi modifier certaines de leurs tables de tir et s'intéresser à la première et monstrueuse ébauche qui devait les conduire au tank.

[76] When Painlevé left the government in 1917, Borel was forty-six and went back to the army. Regarding Borel's activities, see also the obituary by Paul Montel [1956].

[77] Regarding the work on and investigation of ballistics done at the fort of Grâves, see [Aubin 2008].

[78] Regarding the astronomers in the war effort, see [Saint-Martin 2008, § 3.3.2].

[79] There was also a sound reconnaissance laboratory at the ENS, directed by Jacques Duclaux, the husband of Germaine Appell, herself a sister of Marguerite Borel, in short a brother-in-law of Borel. Jean Chazy worked there. See, in [Lebesgue 1991] the letter dated January 2nd 1915 and note 973 and following. Chazy located very precisely the position of the so-called "big Bertha" (a German cannon)—and for this he was awarded the War Cross (see [Denjoy 1956]).

in October 1914—and he actually located an enemy battery on December 8th 1914 [Lebesgue 1991, Note 978]. There is no precise information that Fatou participated in these experiments, but it is likely that he did so, since we know he was aware of them, he spoke of them with Lebesgue before January 15th 1915 [Lebesgue 1991, p. 317].

The annual reports of the Observatory[80] are extremely discreet on these questions. They do not even mention, for instance, this activity of Nordmann. The report for 1916 notes that the service of physical astronomy of Maurice Hamy was suspended and that Hamy was "given the responsibility of various missions related to national defence", but without further detail. That of 1919 would nevertheless list those people, working at the Observatory, who where not in the army but were rewarded for "astronomical work and the way they served, in the interest of the war" (among them, Pierre Fatou who was awarded the title "Officier de l'Instruction publique" (Public education officer)).

"Patriotic" atmosphere

This was a time of "patriotism" and above all of wild propaganda[81]. The popular novels that appeared at that time, for instance *The Shell Shard* [Leblanc 1916] or *Rouletabille at Krupp's* [Leroux 1917], to cite here only the best-selling authors[82], show the strength of anti-German feeling. The most reasonable among the French agreed with it: we need only think of *Noël des enfants qui n'ont plus de maison* (Christmas for children who have lost their home) by Claude Debussy in 1915:

> Christmas, little Christmas, do not visit them, never visit them again, punish them!
> Avenge children from France![83]

If Henri Barbusse was awarded, in 1916, the Goncourt Prize for *Under Fire* (*le Feu*), a dreadful account of life and death in the trenches, in the last pages of which one can read:

> "After all, why do we make war?" We don't know at all why, but we can say *who* we make it for. We shall be forced to see that if every nation everyday brings the fresh bodies of fifteen hundred young men to the God of

[80] Library of the Observatory.

[81] Concerning the effect, both of this patriotic and anti-German atmosphere and of the war experience on the "fire generation", driven to pacifism, communism, or collaborationism, see again the article [Sirinelli 1992].

[82] Maurice Leblanc was the creator of Arsène Lupin, a character who appears very briefly (and rather artificially) in a late version of [Leblanc 1916], and Gaston Leroux was the creator both of Rouletabille (*The Mystery of the yellow room*) and Chéri-Bibi (an enormous best-seller in France in 1913). These two writers were extremely popular in France at the beginning of the 20th century.

[83] Noël, petit Noël, n'allez pas chez eux, n'allez plus jamais chez eux, punissez-les! Vengez les enfants de France!

> War to be lacerated, it's for the pleasure of a few ringleaders that we could easily count; that if whole nations go to slaughter marshalled in armies in order that the gold-striped caste may write their princely names in history, so that other gilded people of the same rank can contrive more business, and expand in the way of employees and shops—and we shall see, as soon as we open our eyes, that the divisions between mankind are not what we thought, and those one did believe in are not divisions[84]. [Barbusse 1916]

it seems that this condemnation—of war rather than of Germany—was not much heard. Propaganda was stronger (notice that the French expression "bourrage de crâne" (brainwashing) was used in Barbusse's book for the first time). The realistic

> One believes he dies for his country. He dies for some manufacturers[85],

of Anatole France, would not be heard until 1922. As for the *Craonne Song*[86], it was forbidden after it was sung by the mutineers in 1917, and it would remain forbidden... until 1974.

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[84] "Après tout, pourquoi fait-on la guerre?" Pourquoi, on n'en sait rien; mais pour qui, on peut le dire. On sera bien forcé de voir que si chaque nation apporte à l'Idole de la guerre la chair fraîche de quinze cent jeunes gens à égorger chaque jour, c'est pour le plaisir de quelques meneurs qu'on pourrait compter; que les peuples entiers vont à la boucherie, rangés en troupeaux d'armées, pour qu'une caste galonnée d'or écrive ses noms de princes dans l'Histoire; pour que des gens dorés aussi, qui font partie de la même gradaille, brassent plus d'affaires — pour des questions de personnes et des questions de boutiques. — Et on verra, dès qu'on ouvrira les yeux que les séparations qui se trouvent entre les hommes ne sont pas celles qu'on croit, et que celles qu'on croit ne sont pas.

[85] On croit mourir pour la patrie. On meurt pour des industriels

[86] *La Chanson de Craonne* is a song which described the life of the soldiers at the front, the chorus of which says:

> Farewell life, farewell love,
> Farewell all women
> It is over, it is forever
> With this infamous war
> It is at Craonne on the plateau
> That we shall leave our skin
> We are all sentenced
> We are the sacrificed.
> [Adieu la vie, adieu l'amour,
> Adieu toutes les femmes
> C'est bien fini, c'est pour toujours
> De cette guerre infâme
> C'est à Craonne sur le plateau
> Qu'on doit laisser sa peau
> Car nous sommes tous condamnés
> C'est nous les sacrifiés.]

This anti-German atmosphere was not at all lightened among our gentlemen of the Academy of Sciences. Moreover, it would last long after the war and we shall never stop noticing its consequences.

⋆

But, for now, we are in 1915. On March 15th 1915, the Academy of Sciences expelled its correspondent members who signed the "Manifesto of the Ninety-Three", or "Appeal of the German intellectuals to the civilised nations" dated October 4th 1914, a text which, to tell the truth, was rather calm, and which protested against the accusation of barbarity made against Germany after, in particular, the invasion of Belgium (six thousand civilians killed in August-September 1914). This exclusion was a very exceptional measure. In all the history of the Academy of Sciences, from its creation to the present day, there have been only three waves of "exclusions": Carnot and Monge were expelled from the Academy of Sciences at the time of the Restoration of the Monarchy, by a royal order in 1816 (by the political authorities); our German intellectuals are expelled by the Academicians themselves; there would also be the invalidation of the election of Georges Claude, a rather too visible collaborationist (who had been a big contributor to the war effort in 1914–18), at the time of the Liberation in 1944.

But let us go back to 1915 and let President Perrier [1915, p. 805] speak (once again); he "explains" to us the matter:

> Barbarity was spoken of, but conscious barbarity changes its name: it is called crime, and crime does not stop being crime when it is committed by crowned heads, when it becomes collective, when it is moreover disciplined. This is why the Academy of Sciences crossed off its lists, on March 15th 1915, the signatories of the sorry manifesto in which the German intellectuals tried to defend the cruelty and the treachery committed by their compatriots and inspired by those whom they serve: the chemist von Bayer [*sic*], from Munich, foreign associate member, and three correspondents: the mathematician Felix Klein[87], from Göttingen, the chemist Emil Fischer, from Berlin; the anatomist Waleyer, also from Berlin[88]

[87] Felix Klein was the only mathematician among the 93. According to tradition, he was asked by phone to sign the text and had no opportunity to read it (see for instance [James 2002, p. 228]—I don't know a more direct source).

[88] The chemist Adolf von Baeyer (1835–1917), Nobel Prize in 1905, was the one who, among other work, discovered tear gas. Emil Fischer (1852–1919), who was his assistant at Strasbourg, was also awarded the Nobel Prize in chemistry and in 1902.

> [...] With this gesture, the Academy wanted to stigmatise those who despise the moral values that were passed on to us by the generations who, during long centuries, lived, suffered, loved and thought on our soil[89]. [90]

Soon after this exclusion, the daily paper *le Figaro* published (on April 21st and 25th and May 9th, 18th and 26th 1915) a series of papers on the "Bluff of German science", written by distinguished scientists (among whom we shall not be surprised to find our Perrier). Still in 1915, on October 4th, Picard presented to the Academy his pamphlet *The history of science and the claims of German science* [*L'histoire des sciences et les prétentions de la science allemande*][91] with these words:

[89] It is not true, fortunately, that the Academy of Sciences excluded most of its German members, as can be read in [Lehto 1998, p. 16], and it is even less true that this happened *after* the election of Picard as Permanent Secretary, as the writing of the text under consideration implies. After the exclusion of the four signatories of the manifesto of the Ninety-Three, the "state of the Academy", published in the *Comptes rendus* in January 1916, attests the attendance of two German associates (including Dedekind), of fourteen German correspondents (including Schwarz, Max Noether and Hilbert) and of two Austrian correspondents.

[90] On a parlé de barbarie, mais la barbarie consciente change de nom: elle s'appelle le crime, et le crime ne cesse pas d'être le crime quand il est commis par des têtes couronnées, quand il devient collectif, surtout quand il est discipliné. C'est pourquoi l'Académie des sciences a rayé de ses listes, le 15 mars 1915, les signataires du triste manifeste où les intellectuels allemands ont essayé de défendre les cruautés et les félonies commises par leurs compatriotes et inspirées par eux à ceux qui les servent: le chimiste von Bayer [*sic*], de Munich, associé étranger, et trois correspondants: le mathematician Felix Klein, de Göttingue; le chimiste Emil Fischer, de Berlin; l'anatomiste Waldeyer, également de Berlin. [...] Par son geste l'Académie a voulu stigmatiser les contempteurs des conceptions morales que nous ont léguées les générations qui ont, durant de longs siècles, vécu, souffert, aimé et pensé sur notre sol.

[91] The anti-German feelings of Picard were deep and long-lasting. They would show themselves once again in the boycott of Einstein by the Academy of Sciences when Langevin, a convinced internationalist, would invite him to lecture at the Collège de France in March-April 1922. Borel, Appell, Cartan among any others, would warmly welcome the physicist, as Camille Marbo recounts [1968, p. 193].

The anti-German feelings of Permanent Secretary Picard against Einstein played an important role in this boycott, they have to be added in this case to a professed anti-Semitism and to a hatred of the Human Rights League and of left-wing people, including Langevin, Perrin and Hadamard, as his correspondence with Lacroix shows (archives of the Academy of Sciences, letters dated August 6th 1921, August 8th 1923, August 9th 1925, November 13th 1926...).

Coming back to Germans in general, let us quote another letter to Lacroix, dated April 17th 1922, in which Picard mentions a German colleague under the name of "kraut Hecker" [Boche Hecker] (Picard file, archives of the Academy of Sciences).

See also Note 36 in Chapter VI.

> Except for a specifically historical part, I emphasise in this study the often very formal nature of scientific German writings. This nature, in which a singular notion of reality and truth, and a sort of contempt for common sense sometimes appear, can, I believe, be linked to Kant subjectivism and formalism, and to the philosophical systems that more or less directly derive from them. The tendency to systematise everything is common in the German spirit. It can even be found in the most practical views, up to the concept of organisation, the new requirements that Germany would like, for its greatest profit, to impose on the world[92].[93]

And it was at the end of the same year 1915, on December 27th, that President Perrier delivered the customary speech during the annual public session of the Academy, an interminable speech of nineteen printed pages, several excerpts of which have already been quoted here, a speech permeated with a patriotic hatred that readers have probably noticed... and from which we extract again a few well-chosen expressions: German felony, Germany, form now on separated from the whole civilised world, Germany's crimes, Germany who deserved all the curses, what Germany calls its Kultur, monstrous Germany [la félonie germanique, la Germanie, séparée désormais de tout le monde civilisé, les crimes de l'Allemagne, qui a mérité toutes les malédictions, ce qu'elle appelle sa Kultur, la monstrueuse Allemagne][94]...

From December 1915 to March 1916, the Belgian mathematician Charles de la Vallée Poussin[95], from Louvain, was invited to the Collège de France,

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[92] Would it not be worthwhile to do a comparative study of this text by Picard with the "types" in the Nazi journal *Deutsche Mathematik* of Bieberbach?

We are very far from the repeated assertions of Picard's father-in-law, Charles Hermite, in his correspondence, thirty years earlier, of his admiration for German mathematicians, with whom neither the war nor the political disagreements could prevent him from collaborating (see [Dugac 1984 ; 1985 ; 1989 ; Lampe 1916], and Note 64 of Chapter II).

[93] En dehors d'une partie plus particulièrement historique, j'insiste dans cette étude sur le caractère souvent si formel des écrits scientifiques allemands. Ce caractère, où apparaissent parfois une notion singulière du réel et du vrai, et une sorte de mépris pour le sens commun, peut, je crois, être rattaché au subjectivisme et au formalisme kantiens, et aux systèmes philosophiques qui en dérivent plus ou moins directement. La tendance à tout systématiser est habituelle à l'esprit germanique. On la retrouve même dans les vues les plus pratiques, jusque dans le concept d'organisation, nouvel Impératif que l'Allemagne voudrait, pour son plus grand profit, imposer au monde

[94] The final goal of Perrier's speech was to denounce the drop in the birth rate, alcoholism and the class struggle, as was to be expected from an address which was slightly contemptuous of women workers (see the excerpt quoted on page 6), which did not prevent it from ending with a very catholic "love each other"...

[95] Charles de la Vallée Poussin is well-known, as a mathematician, because of his contribution to the prime number theorem. He would be the first president of the new International Mathematical Union, the structure that would organise the "International" congress of Mathematicians at Strasbourg in 1920. A picture of

an "expression of sympathy after the cruel acts of violence Belgium suffered" [témoignage de sympathie après les cruelles violences dont la Belgique a été victime], as he says in the preface of his book [1916]. His passing through Paris is mentioned in a letter from Lebesgue [1991, p. 330] to Borel: Lebesgue worried of the audience of de la Vallée Poussin's lectures (very few mathematicians were present in Paris, because of the war), which he would have to attend... to learn what the Lebesgue integral is.

On March 13th 1916, the Academy of Sciences elected Charles de la Vallée Poussin as a corespondent member, in replacement of Felix Klein. It is hard to not see in this election an answer to the

> It is not true that our troops brutally destroyed Louvain

in the Manifesto of the Ninety-Three.

At that time, the list of members of the SMF was published in the *Bulletin*, with the words:

> Because of the present war, the Council of the French Mathematical Society decided to suspend the relations of the Society with its members who belong to enemy nations; as a consequence, their names do not show up on the list below[96].

After the war, this statement would be replaced by the following:

> In its session of January 14th 1920, considering that the relations of the Society with those among its members who belong to enemy nations were suspended during the war, the Assembly of the French Mathematical Society decided that these relations could only be resumed after a formal request from the above mentioned members, which request would be submitted to the vote of the Council; consequently, the names of these members do not show up in the list above[97].

This text would still appear in the *Bulletin*, with the list of members, until January 1930.

Let us add that French people were not alone in being violently anti-German after the war. See for instance the excerpts of articles that appeared in the British journal *Nature* and that are quoted in [Dauben 1980].

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de la Vallée Poussin, with Julia, taken during the (more) international congress of 1928 can be found on page 210.

96 En raison de l'état de guerre actuel, le Conseil de la Société mathématique de France a décidé de suspendre les relations de la Société avec ceux de ses membres qui appartiennent aux nations ennemies; en conséquence, les noms de ces membres ne figurent pas sur la liste ci-dessous.

97 Dans la séance du 14 janvier 1920, l'Assemblée générale de la Société mathématique de France, considérant que les relations de la Société avec ceux de ses membres qui appartiennent aux nations ennemies ont été suspendues pendant la guerre, a décidé que ces relations ne pourraient être reprises qu'à la suite d'une demande formelle des membres susvisés, demande qui serait soumise au vote du Conseil; en conséquence, les noms de ces membres ne figurent pas sur la liste ci-dessous.

It should nevertheless be noticed that, delivering in his turn the speech during the annual session on January 18th 1916, Jordan who mentioned, of course, German barbarity[98]... but relatively briefly,

> Without speaking of the glorious success of our armies, the crimes that our enemies multiply are the omen of their defeat. They dare to speak of freedom, of liberation, while on each of their borders an oppressed nation moans; while whole populations are deported to slavery, and they prepare to enlist them by force in their armies. Who could believe in the final success of what they have undertaken, which seeks to erase twenty centuries of Christianity to take us back to the regime of the Babylonian monarchies. It is in vain that they will appeal to their "Old German God", the bloody idol forged by their pride. We leave them this God. Ours does not know old age and is not the prerogative of a people; but this is a King of justice, and with his help we shall overcome [Jordan 1916][99].

did not then forget, when he recalled the Academicians who had deceased that year, to devote a few lines to pay tribute to the German mathematician "who died full of years", Richard Dedekind, correspondent of the Academy since 1900 and foreign associate since 1910 (and who had not signed the Manifesto of the Ninety-Three)[100].

Seven of the award-winners of the Prizes awarded during this public session, among whom was René Gateaux, were killed at the front.

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98 According to [Sirinelli 1992], even in the 20's, students applying for the Grandes Écoles would have to treat subjects like:

> Should war be waged in a humane or in a barbaric way?
> How should a people that uses science as the arm of barbarity be judged?

99 Sans parler des glorieux succès de nos armées, les crimes multipliés de nos ennemis sont le présage de leur défaite. Ils osent parler de liberté, d'affranchissement, lorsque sur chacune de leurs frontières gémit une nation opprimée; lorsque des populations entières sont déportées en esclavage, et qu'ils s'apprêtent à les enrôler de force dans leurs armées. Qui pourrait croire au succès final de leur entreprise, qui prétend effacer vingt siècles de christianisme pour nous ramener au régime des monarchies de Babylone. Ils invoqueront en vain leur "Vieux Dieu Allemand", sanglante idole que s'est forgée leur orgueil. Nous leur laissons ce Dieu-là. Le nôtre ne connaît pas la vieillesse et n'est pas l'apanage d'un peuple; mais c'est un Roi de justice, et avec son aide nous vaincrons.

100 In the middle of the war, on May 12th 1917, the German mathematician David Hilbert (who had not signed the Manifesto either) delivered a speech on Gaston Darboux (who had died two months earlier) to the Academy of Sciences of Göttingen, this speech would be translated into French and appear in [Hilbert 1920], a warm academic speech, the conclusion of which was about the influence that Darboux' ideas had on Felix Klein's efforts (regarding teaching mathematics) and about his role in the International association of scientific academies. The publication by*Acta mathematica* was obviously not by pure chance—regarding the role Mittag-Leffler wanted "his" journal to play after the war, see [Dauben 1980].

I.4 Iteration, a few definitions and notation

This is a good place to return, at last, to mathematics. In the following chapters, we shall try to explain this mathematics while telling chronologically the story of the publication of Fatou's and Julia's works on iteration.

Let us start by reminding the readers of a few definitions and by establishing the notation. We are dealing with holomorphic functions defined on open subsets of $\mathbf{C}$, and most often with rational fractions, namely with analytic functions on the Riemann sphere $\widehat{\mathbf{C}} = \mathbf{P}_1 = \mathbf{P}_1(\mathbf{C}) = \mathbf{C} \cup \{\infty\}$, although many results still hold true in the case of an entire function having an essential singularity at ∞. As the authors did it at that time, we shall often speak of rational fractions from $\mathbf{C}$ to $\mathbf{C}$, considering the point at infinity as an ordinary point and not worrying about the poles of the fractions under consideration. Such a fraction will in general be denoted by R and k will be its degree as a map from $\mathbf{P}_1$ to $\mathbf{P}_1$, that is, the number of points in $R^{-1}(a)$ (counted with multiplicity) for $a \in \mathbf{P}_1$, or

$$k = \sup(\deg P, \deg Q) \text{ if } R = \frac{P}{Q} \text{ is irreducible}$$

(P and Q are polynomials). In the example where we apply Newton's method to find the roots of a polynomial f of degree k, we iterate the rational fraction

$$R(z) = z - \frac{f(z)}{f'(z)}$$

which also has degree k.

Almost always (but not always, to iterate an isomorphism is also tempting, think of a rotation...), we assume that $k \geq 2$. The n^{th} iterate $R \circ R \circ \cdots \circ R$ (n times) of R is denoted R^n. This is a rational fraction of degree k^n.

The periodic points for R are the fixed points for some R^n. The period of a periodic point a is the smallest integer n such that $R^n(a) = a$. The n distinct points $a, R(a), \ldots, R^{n-1}(a)$ constitute what is called a periodic cycle.

The *multiplier* of the fixed point z is the derivative of R at z. Note that, if

$$z_1 = R(z), \ldots, z_{n-1} = R^{n-1}(z),$$

then

$$(R^n)'(z) = R'(z_{n-1}) \cdots R'(z_1) R'(z),$$

so that, if z is periodic of period n, we have

$$(R^n)'(z) = (R^n)'(z_1) = \cdots = (R^n)'(z_{n-1}) :$$

all the points of a cycle have the same multiplier.

Some fixed points of R or of its iterates will be[101] called *repelling*, they are the ones for which

$$R^n(z) = z \text{ and } |(R^n)'(z)| > 1.$$

Following Julia, let us denote by E the (countable) set of all the repelling points and by E' its derived set, that is, the set of its accumulation points. We do not discuss here the question of whether E indeed contains some points, for this see Remark III.1.2.

Attracting fixed points will be those whose multiplier has absolute value strictly less than 1. Since the work of Kœnigs[102] around 1880, it has been known that every attracting fixed point has a neighbourhood on which the sequence R^n converges to a constant (equal to the fixed point).

Indifferent fixed points are those such that the absolute value of their multiplier is 1.

If ∞ is a fixed point, we take it to 0 by $w = 1/z$, this conjugates R to

$$S(w) = \frac{1}{R\left(\dfrac{1}{w}\right)},$$

so that the multiplier is

$$s = S'(0) = \lim_{w \to 0} \frac{S(w)}{w} = \lim_{|z| \to \infty} \frac{z}{R(z)}.$$

Thus for instance, if R is a polynomial of degree $k \geq 2$, the point ∞ is always an attracting fixed point (it is even *super-attracting*, that is, $s = 0$).

Assume that a is an attracting fixed point of R. As $|R'(a)| < 1$, there exist real numbers $r > 0$ and $\sigma < 1$ such that

$$|z| < r \Rightarrow |R(z) - a| < \sigma\, |z - a|\,.$$

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[101] We use the future here because the terminology will be invented as the story goes along.

[102] Gabriel Kœnigs (1858–1931) would be elected to the Academy of Sciences, in the Section of mechanics on March 18th 1918. Regarding him and his mathematical connection with Darboux, see [Alexander 1995]. He would be the secretary of the IMU from 1920 to his death in 1931, a position he is supposed to have not taken very seriously (according to [Lehto 1998]). He would also be, with Paul Appell, Émile Borel, Jacques Hadamard, Jean Perrin and a few others, one of the members of the Honour committee of the Rationalist union [Union rationaliste], founded in 1930 with Langevin as vice-president. Neither of the obituaries [de Launay 1931 ; Buhl 1931a] mentions either the IMU or the Rationalist union. However, it is not absolutely true that nothing can be found in the archives of the Academy of Sciences on his activity at the IMU: in a letter to Villat (Villat collection 61J) dated January 19th 1920, Kœnigs wrote that he had given the statutes of the IMU to the printer.

Thus

$$|R^n(z) - a| \leq \sigma^n \, |z - a|$$

and the sequence R^n converges uniformly to the constant function equal to a on the disc.

The set of points z of the plane such that $R^n(z)$ tends to a is the attraction basin (or domain) of the fixed point a. This basin may have infinitely many connected components. The one containing a is the immediate basin (or domain) of a. This notion is easily generalised to the case of an attracting cycle.

Let us mention, since the word will show up, that what we call a fixed point was often called, at that time, a "double" point.

Example I.4.1. The first example is, of course, the simplest one, that of the function $R(z) = z^2$. The fixed points of R are 0 and ∞ and are attracting (even super-attracting). In the figure, the iterates of the points in the (grey) disc converge to 0, those of the outer (white) points converge to ∞.

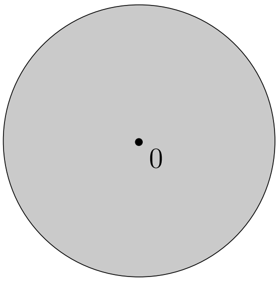

Fig. I.1. The set E' for $z \mapsto z^2$

The fixed points of R^n are the solutions of the equation

$$z^{2^n} - z = 0.$$

The origin is a (super-attracting) fixed point. If $n \geq 2$, all the other solutions are repelling fixed points, hence

$$E = \left\{ z \in \mathbf{C} \mid z^{2^n - 1} = 1 \right\} \subset S^1.$$

This set is dense in the circle, so that $E' = S^1$ is the whole circle. It divides $\mathbf{P}_1(\mathbf{C})$ into two connected components (attraction basins or domains), the sequence R^n converges to a constant function on each component (the constant is equal to the attracting point which the component contains).

This example was well known by all the "iterators" in history, long before the point at which we began our account. It was explained by our authors, for instance in [Julia 1918f, p. 103] and [Montel 1927, p. 228]. It is related to Newton's method: the polynomial $R(z) = z^2$ is conjugated with

$$S(w) = \frac{1}{2}\left(w + \frac{1}{w}\right) \text{ by } z = \frac{w-1}{w+1}$$

and S is the rational fraction

$$S(w) = w - \frac{w^2 - 1}{2w}$$

given by Newton's method for finding the roots of $w^2 - 1$. As the study of R shows, the iterates of the points in the half-plane $\text{Re}(w) > 0$ converge to the root $+1$, those of the half-plane $\text{Re}(w) < 0$ to the root -1... an ideal situation, which is specific to degree 2. As was shown by Schröder[103] in a paper [Schröder 1871] we shall have the opportunity to mention again, if α and β are the (distinct) roots of a degree 2 polynomial, the bisector of the segment $\alpha\beta$ divides $\mathbf{C}$ into two open half-planes and Newton's method converges to one of the roots, whatever point in the half-plane of this root we start from.

Example I.4.2. The second example is one of those that show up in Pierre Fatou's Note [1906d] in 1906. This is the rational fraction

$$Z = R(z) = \frac{z^2}{z^2 + 2}.$$

Since more recent work of Douady, Hubbard, and others, popularised the examples of quadratic polynomials, it is nicer for today's readers to use the polynomial

$$V = P(v) = v^2 + 2$$

which is conjugated to R by $v = 2/z$ (and by $V = 2/Z$, as one would have said at that time).

The set E' is then a Cantor set (one of the first examples of Cantor sets in dimension 2) which is sometimes called a "dust" and rather hard to visualise. It looks like the one in the figure, which corresponds to $z \mapsto z^2 + 1$ but that Arnaud Chéritat suggested that I use because it is a little bit more visible.

[103] The reduction of the resolution of a degree-2 equation to the iteration of $z \mapsto z^2$ can also be found in the note [Cayley 1890] of Cayley... the conclusion of which is the sentence

> I hope to apply this theory to the case of a cubic equation but the computations in this case are far more difficult. [J'espère appliquer cette théorie au cas d'une équation cubique mais les calculs sont beaucoup plus difficiles.]

As we shall see, quite apart from computation, the situation is much more intricate in higher degrees. See for instance Figure II.4.

Fig. I.2. The set E' for $z \mapsto z^2 + 1$

The terminology "Cantor set" was not as commonly used at that time as it is nowadays. Fatou proved that this set is perfect, everywhere discontinuous, and invariant under R. The iterates of all the points of $\mathbf{C}$, except those in E', converge to the fixed point. To be quite precise, Fatou did not define (in his Note [1906d]) the set we call E' (and that he denotes E) exactly as we just did (derived set of the set of repelling points), but rather as the limit of a sequence of sets, each included in the following one. The point 0 is an attracting fixed point, it thus attracts all sufficiently small discs. Let us choose a large enough $r > 0$, such that the disc of radius r contains all the critical points of the inverse mapping of R. The inverse image of the circle C of radius r then comprises of k disjoint closed curves such that the restriction of R to each of them is a diffeomorphism onto C. Fatou calls E_n the set of points z such that

$$|z| > r,\ |R(z)| > r, \ldots,$$
$$\ldots,\ \left|R^{n-1}(z)\right| > r,\ |R^n(z)| \geq r \text{ and } \left|R^{n+1}(z)\right| < r$$

(it would seem more natural to put-non strict inequalities everywhere). See Figure I.3, in which the sets E_n for $n = 0, 1, 2$ and $k = 2$ are very schematically depicted. We then have $E_{n+1} \subset E_n$ and it is easy to check that $\cap_n E_n$ is the set E' we are interested in. We note here that Fatou, as a specialist of the Lebesgue integral, was familiar with infinite intersections.

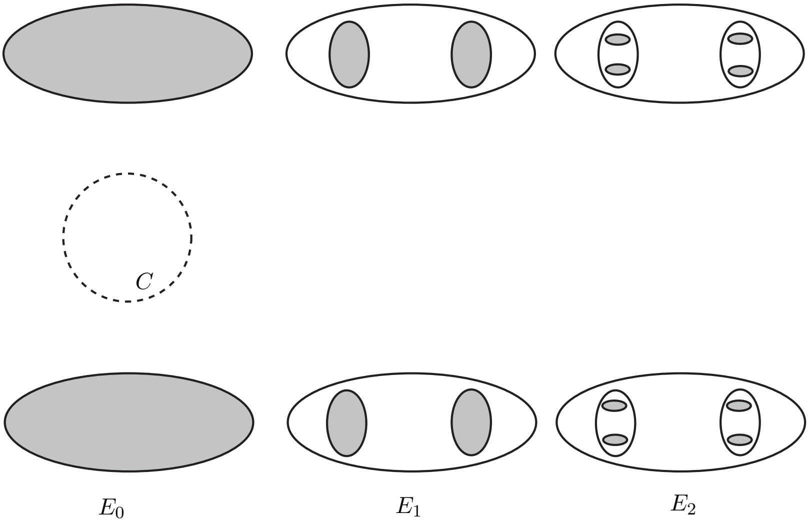

Fig. I.3. A schematic view of the sets E_0, E_1, E_2

Example I.4.3. In his Note [1906d], Fatou also considered[104] the polynomial

$$R(z) = \frac{z + z^2}{2}.$$

The fixed points are 0 and ∞ (attracting) and 1 (repelling). The domains of convergence to 0 and ∞ are separated by lines which are not analytic.

For modern readers: the function is conjugated with

$$r(u) = u^2 + \frac{1}{8},$$

the polynomial has a unique attracting fixed point and the set E' is a slightly deformed circle (with dimples), a not very differentiable Jordan curve which has no tangent at any point. See Figure I.4[105].

[104] In the notice he would write in 1921, Fatou would say:

> I also showed cases in which the substitution has two attracting double points the respective domains of which are connected, simply connected and separated by a non-analytic curve, [J'ai également indiqué des cas où la substitution présente deux points doubles attractifs dont les domaines respectifs sont d'un seul tenant simplement connexe et séparés par une courbe non analytique,]

And Hadamard, in the report we have already quoted:

> Starting from this simple case however, the strangest singularities show up.

Fatou file, archives of the Academy of Sciences.

[105] In this figure, as in Figure I.1 and in most of the other figures in this book, the set E' is the boundary of the grey part.

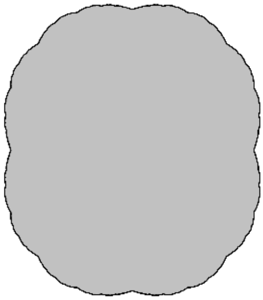

Fig. I.4. The set E' for $R(z) = \dfrac{z+z^2}{2}$

In addition to what we shall cite here, see also Douady's talk [1983] at the Bourbaki seminar, a very beautiful modern introduction to the subject (although already old), and the more recent paper of Yoccoz [1999] and books of Milnor [2006a], Berteloot and Mayer [2001] and of Tan Lei [2000].

I.5 Normal families

One of the main objects investigated in the work of Fatou and Julia which we are discussing here is what is nowadays called the "Julia set" of the function R which is defined, since Fatou [1920a], as the set of points at which the sequence of iterates R^n of R is not normal. The idea to use the notion of a normal sequence, or family, appeared, as we shall see, while Fatou and Julia had already begun to work on the iteration of rational fractions. It stimulated their work, which leaped forward.

Let us remind the readers that a family of holomorphic functions on an open subset $U \subset \mathbf{P}_1$ taking their values in $\mathbf{P}_1$ is said to be *normal*, a notion that was invented by Paul Montel at the beginning of the century (to make things simpler, we refer here to a later book [Montel 1927, p. 32]), if, from any infinite sequence of functions in this family, a sub-sequence that converges uniformly on U can be extracted (in modern terms, this is a relatively compact subset of the space $\mathcal{O}(U)$ of holomorphic maps from U to $\mathbf{P}_1$, endowed with the compact open topology)[106]. In this case, the limit is automatically a

[106] As it was well known at the time of our story, sets of continuous functions (contrary to sets of points in $\mathbf{R}^n$) do not have the property that the compact sets are the closed bounded subsets. The notion of equicontinuity of Arzelà and Ascoli gives the compactness. It was the work of Montel in complex analysis that showed

holomorphic function (that may be constantly equal to ∞). A family is normal at a point if there exists a neighbourhood of the point on which it is normal.

Examples I.5.1.

(1) We start these examples with a counter-example: consider the function $f(z) = e^z$ and the sequence (f_n) defined by $f_n(z) = f(nz)$. At a purely imaginary point iy_0, the sequence (f_n) is not normal: on any disc centred at this point, we have

$$\lim_{n\to+\infty} |f_n(z)| = \begin{cases} 0 & \text{if } \operatorname{Re}(z) < 0 \\ 1 & \text{if } \operatorname{Re}(z) = 0 \\ +\infty & \text{if } \operatorname{Re}(z) > 0, \end{cases}$$

so that no sub-sequence of (f_n) can converge uniformly on such a disc.

(2) Back to iteration. At an attracting fixed point a of the rational fraction R, the sequence of iterates R^n is a normal family, since it converges uniformly, on a disc centred at a, to the constant function equal to a.

(3) In Example I.4.1, an open disc centred at a point of the circle contains points at which $R^n(z)$ converges to ∞ and others at which it converges to 0. Thus the family R^n is not normal at any point of the circle (and it is normal everywhere else). In this example, it can be seen that the set E', defined as the derived set of the set of repelling fixed points of the R^n (being perfect, it is also its closure), happens to coincide with the set of points at which the sequence R^n is not normal. We shall see that this property is fairly general.

(4) The repelling fixed points of R (and its iterates) are always points at which the sequence of iterates is not normal: if $R(a) = a$ with $|R'(a)| > 1$, since $(R^n)'(a) = R'(a)^n$, the sequence of derivatives of the R^n has no sub-sequence that converges in a neighbourhood of a, hence R^n has no uniformly convergent sub-sequence.

(5) Application to Fuchsian and Kleinian groups. One can also consider the set of elements of a discrete group Γ of Möbius transformations

$$z \longmapsto \frac{az+b}{cz+d}, \quad ad - bc \neq 0$$

(a Kleinian group) as a family of holomorphic maps from $\mathbf{P}_1(\mathbf{C})$ to $\mathbf{P}_1(\mathbf{C})$. The set of points where this family is not normal is called the limit set of Γ, this is also the set of the accumulation points of the fixed points of the group. For instance, if the group is that of the Möbius transformations R such that

$$\frac{R(z)-\alpha}{R(z)-\beta} = k^n \frac{z-\alpha}{z-\beta} \text{ with } k \neq 1,$$

the limit set consists of the two fixed points α and β. See the book (by Fatou) [Appell et al. 1930, § 31–33]. The fact that this situation is analogous with

how this notion could be useful. The readers will certainly have understood that we allowed ourselves an anachronism: the word "compact".

that of iteration was already mentioned in Fatou's 1906 work on iteration[107], it would also be noticed by Julia. We shall come back to this (on page 112 and in §IV.5.b).

The property that a family is normal at a point is clearly a local property. The fundamental result on normal families is the following theorem [Montel 1927, p. 21]:

Theorem. *If holomorphic functions defined on an open set U are uniformly bounded on U (with a uniform bound), they constitute a normal family.*

In modern terms: in the space $\mathcal{O}(U)$, the compact sets are the closed and bounded subsets.

As an application of the conformal mapping theorem, Montel [1912] also showed the following normality criterion:

Theorem (of Montel). *A family $\mathcal{F}$ of analytic functions on an open set U which has only two exceptional values*[108] *is normal.*

This is because the functions in the family $\mathcal{F}$ take their values in the complement of two points in $\mathbf{C}$, an open set which is conformally equivalent to a bounded open subset. This theorem allows us, for instance, to prove the celebrated theorem of Picard (first proved in 1879, see [Picard 1879]).

Theorem (Picard's first theorem). *A non-constant entire function takes all values, except possibly one.*

Let us assume, Montel says, that f avoids two values. We want to apply a theorem about sequences of functions to the investigation of the single function f. We must thus construct a sequence of functions, the properties of this sequence giving some information on the properties of the function. The sequence (f_n) defined by

$$f_n(z) = f(nz)$$

takes the same values as f, so that it is normal (according to Montel's theorem). Hence it is bounded on a disc, hence f is bounded on $\mathbf{C}$, and it must be constant, thanks to Liouville's theorem.

There is a whole host of Picard theorems, to the proof of which Montel's theorem applies, for instance the following.

---

[107] Poincaré [1883] himself considered the limit set, defined as the set of points at which the action of the group is not properly discontinuous (without the notion of a normal family). This is the line L that we have already met in Note 14 and that will show up again in §IV.5.b.

[108] Let us remind the readers that what is called an "exceptional value" is a complex number a that is not a value, more precisely:

$$\forall f \in \mathcal{F}, \quad a \notin f(U).$$

Theorem (Picard's second theorem). *Let f be a holomorphic function on the punctured disc*

$$D_0 = \{z \in \mathbf{C} \mid 0 < |z| < \varepsilon\}.$$

If f has an essential singularity at 0*, it takes all complex values, except maybe one.*

The argument is analogous, using this time the sequence (f_n), which, if f avoided two values, would be uniformly bounded (but then f would have a removable singularity at 0) or would tend to the constant function equal to ∞ (then f would have a pole at 0), thus 0 would not be an essential singularity[109].

Montel's theorem, as one can see, establishes a parallel between the investigation of the points at which a family is not normal and the investigation of the essential singular points of a single function[110]. See also §§ IV.3 and VI.3. For a complete panorama of the theorems of Picard and their variants, see [Segal 2008].

Coming back to iteration itself. It was not as the set of points at which the sequence is not normal that the Julia set was defined, at least by Julia, but starting from the set E of repelling periodic points of R. See above and next chapter.

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109 This is a good place to mention that the short article [Dieudonné 1990] devoted to Montel by the *Dictionary of scientific biography* (DSB) is a very efficient introduction to normal families.

110 Here is a comment made by Émile Borel (probably in 1937, at the latest in 1947) on this application of normal families [Borel 1966, p. 46]:

> [...] in this application, it is necessary to tackle the study of a single function by a method, the principle of which introduces an infinity of functions. In all the other applications of the theory of normal families, an infinite sequence of functions is naturally present. Here it was necessary to construct it from scratch. Paul Montel was able to deduce from a unique function a family of functions in which the properties of the initial function are collectively reflected. [dans cette application, il faut aborder l'étude d'une fonction unique par une méthode dont le principe même introduit une infinité de fonctions. Dans toutes les autres applications de la théorie des familles normales, une suite infinie de fonctions se présente naturellement. Ici, il a été nécessaire de la construire de toutes pièces. Paul Montel est arrivé à déduire d'une fonction unique une famille de fonctions sur laquelle se reflètent collectivement les propriétés de la fonction initiale.]

Among the infinite sequences of functions that appear naturally, Borel was certainly thinking of the family of the R^n from iteration theory.

I.6 Relation to functional equations

One may wonder why so many of the works devoted to the question of iteration which we have cited contain the words "functional equations" in their title. We shall come back later to the reappearance of functional equations in Fatou's titles (see Note 55 in Chapter II).

The fact remains that the investigation of some functional equations is naturally related to that of iteration. It was in a paper [Schröder 1871] on iteration that Schröder[111] considered the question of replacing the function R by a conjugate $S = \psi^{-1} \circ R \circ \psi$ to give it a simpler form, trying in particular

$$\psi(sw) = R(\psi(w))$$

or, equivalently,

$$F(R(z)) = sF(z) \text{ with } w = F(z) \text{ and } F = \psi^{-1}.$$

This has been called, since that paper, a "Schröder equation" (R and s are given and we look for F).

The work of Kœnigs—which we have already qualified as a prehistory of the subject—deals with functional equations. The connection with iteration is very clearly explained in Paul Montel's book [1927]. Let us recall briefly what he explains there. We try to solve Schröder's equation

$$F(R(z)) = sF(z)$$

where F is the unknown, a holomorphic function we have to determine, defined on an open set we also have to determine.

Let us assume that $a \in \mathbf{C}$ is an attracting (but not super-attracting) fixed point[112] of R, which is simple in the sense that we have, in a neighbourhood of a,

$$R(z) = a + s(z-a) + \sum_{k \geq 0} a_k (z-a)^{k+2},$$

hence $s = R'(a)$ is the multiplier of R at a, and our assumption that a is attracting but not super-attracting says that $0 < |s| < 1$. For $r > 0$ small enough, we have, on the disc $D(a, r)$

$$|R(z) - a| < \sigma\,|z-a| \text{ for some } \sigma < 1$$

and also

$$|R(z) - a - s(z-a)| < A\,|z-a|^2 \text{ for some } A > 0.$$

[111] Ernst Schröder (1841–1902) was a prolific German mathematician, influenced by Cantor, and, in an area far from iteration, the author of a proof of the so-called Cantor-Bernstein Theorem (there exists a bijection from A to B if and only if there exists an injection from A into B and an injection from B into A). Other aspects of the resolution of Schröder's equation can be found in [Alexander 1994].

[112] We assume here that the fixed point is not the point at infinity.

We set

$$F_n(z) = \frac{R^n(z) - a}{s^n}$$

and prove that the sequence of holomorphic functions F_n converges to a holomorphic function F on $D(a, r)$ that is a solution of Schröder's equation. Put $z_0 = z$ and $z_n = R(z_{n-1})$ for $n \geq 1$. We then have

$$F_n(z) = \frac{R(z_{n-1}) - a}{s^n} = (z - a) \prod_{k=0}^{n-1} \frac{R(z_k) - a}{s(z_k - a)}.$$

The function F_n being written as a product, the convergence of the sequence follows from that of the infinite product of the terms $(R(z_k) - a)/s(z_k - a)$, which in turn is equivalent to that of the series with general term

$$\frac{R(z_k) - a}{s(z_k - a)} - 1 = \frac{R(z_k) - a - s(z_k - a)}{s(z_k - a)}.$$

We have

$$\left| \frac{R(z_k) - a}{s(z_k - a)} - 1 \right| < \frac{A\,|z_k - a|^2}{|s|\,|z_k - a|} = \frac{A}{|s|}\,|z_k - a| = u_k.$$

But the series with general (positive) term u_k converges since we have

$$\frac{u_{k+1}}{u_k} = \left| \frac{z_{k+1} - a}{z_k - a} \right| = \left| \frac{R(z_k) - a}{z_k - a} \right| < \sigma < 1.$$

The sequence F_n indeed converges to a holomorphic function F and we have

$$F_n(R(z)) = \frac{R^{n+1}(z) - a}{s^n} = sF_{n+1}(z)$$

so that the limit F is indeed a solution of Schröder's equation.

Let us also observe that $F(a) = 0$ and $F'(a) = 1$, so that F is locally invertible in a neighbourhood of a. On the other hand, applying the result to a determination of R^{-1} near a (we have not used the fact that R is rational), we get the same result when $|s| > 1$. What we have obtained is a theorem of Kœnigs [1884 ; 1885], which we formulate here as a linearisation statement:

Theorem. *Let a be a fixed point of R with multiplier s (with $|s| \neq 0, 1$). There exists an invertible holomorphic function F, defined in a neighbourhood of a, such that*

$$F(a) = 0 \text{ and } F \circ R \circ F^{-1}(w) = sw.$$

Remark. The inverse mapping θ (the existence of which was proved by Poincaré [1890] for $|s| > 1$) is thus a solution of

$$R(\theta(u)) = \theta(su) \text{ and } \theta(0) = a.$$

The function θ extends to the whole of $\mathbf{C}$, while F can be mutli-valued. See Examples II.1.2 and II.2.1 below.

In the case of a super-attracting fixed point (the $s = 0$ case), a theorem of Böttcher [1904a; 1904b; 1904c][113] shows that R is conjugated, in a neighbourhood of the fixed point, to a power function. The functional equation solved by Böttcher is

$$F(R(z)) = aF(z)^k.$$

This theorem is the essential tool in the study, for instance, of the attraction basin of the point at infinity when R is a polynomial.

The additive form of Schröder's equation,

$$F(R(z)) = R(z) + a$$

called Abel's equation[114], is useful in understanding what happens near indifferent fixed points (the fixed points whose multiplier s has absolute value 1), and would be investigated, in particular, by Fatou, in his study of what happens near parabolic fixed points (those whose multiplier is a root of unity).

[113] The articles of Böttcher (a Polish mathematician) that we cite here were published in Russian in a Kazan journal and it is not certain that our protagonists would have had the opportunity to read them. Fatou would write that

> The existence of this function [the function which conjugates R to a "power" function] seems to have been proven for the first time by M. Böttcher [Fatou 1919b, p. 189] [L'existence de cette fonction [celle qui conjugue R à la fonction puissance] semble avoir été démontrée pour la première fois par M. Böttcher]

but he would not cite Böttcher's papers. He might have known of their existence by the review published by the *Jahrbuch über die Fortschritte der Mathematik*. Unless if I am mistaken, Julia would mention Böttcher only in [Julia 1923, p. 145], also without a reference. I have not seen these papers myself and I cite them from the *Jahrbuch*. Ritt would mention and use "un théorème de L. Boettcher" in his Note [1918], and, moreover, he would publish a proof in his paper [1920]. It is possible that he, Ritt, read the papers: according to [Lorch 1951], he learned "to read mathematical Russian before that became fashionable".

[114] Abel devoted a few papers to functional equations. One of them [Abel 1881, Mémoire VI] was published in Volume II of his Complete Works in 1881. The paper was not published during Abel's lifetime. What might have been only a fragment appeared in Holmboe's edition (in Norwegian) of his works in 1839, but we shall never know, since some of Abel's manuscripts were later burned. It was then reproduced in the 1881 edition in French. To cut a long story short, the paper is called "Determination of a function by means of an equation with a single variable" and starts very abruptly with

> Given the function fx, find the function φx by the equation
>
> $$\varphi x + 1 = \varphi(fx).$$

The article is three and a half pages long (in which Abel transforms the functional equation to a difference equation) and the reasons why he was interested into this particular equation are not explained.

Abel's and Schröder's functional equations were the subject of several papers that are rather forgotten nowadays and that are too far from our subject to cite here. See [Alexander 1994, Chapter 2].

II

The Great Prize of Mathematical Sciences

In 1917 and 1918, the *Comptes rendus* of the Academy of Sciences published a series of Notes, related to the subject of the Great Prize. The authors were Fatou, Julia, Lattès, Ritt (and Montel). Julia also sent sealed envelopes [plis cachetés][1].

At the end of 1917, the memoirs in the running for the Great Prize were received by the Academy. In 1918, more *Comptes rendus* Notes were published, while the Committees of the Academy of Sciences that were in charge of choosing the award winners began to meet.

In this chapter, we present the chronology of the publication of the Notes related to iteration and of the decisions of the Academy of Sciences in 1917 and 1918. Because this story is not independent of its historical context and because it took place in the atmosphere evoked in § I.3, a few stages in these two years of war will also be recalled.

Remark. As we shall see, the efficiency and the speed with which the *Comptes rendus* were used as a means of communication between mathematicians are remarkable and very impressive.

[1] A sealed envelope is, as one can imagine from its name, a text contained in an envelope, closed with sealing wax; it is not supposed to be published immediately, but ensures its author for priority of a result. Unless the author requests otherwise, it cannot be opened for one hundred years (see the famous example, or rather counter-example, of the opening of Doeblin's sealed envelope in [Kahane 2006b]). The way Julia used the sealed envelope in our story is the classic, usual way—there were more particular ways in which sealed envelopes were used by authors who were forbidden to publish their results, during the German Occupation of France (see our article [2009a]). On the number of sealed envelopes received during such and such a period, and the way this reflected the political events of the time, see [Berthon 1986].

M. Audin, *Fatou, Julia, Montel: The Great Prize of Mathematical Sciences of 1918, and Beyond*, Lecture Notes in Mathematics 2014, DOI 10.1007/978-3-642-17854-2_3,

II.1 Year 1917

In March, the mathematician Paul Painlevé became War Minister (the President of the Republic was Raymond Poincaré, the mathematician's cousin). On April 2nd, Émile Picard[2] was unanimously elected Permanent Secretary[3] of the Academy of Sciences (to replace Gaston Darboux, who died on February 23rd).

On January 2nd, February 26th, March 19th, April 10th and 23rd, the Academy of Sciences published Notes by Julia on quadratic and non-quadratic forms related to the subject of his thesis (several such Notes had already been published during the previous year). According to the notice Julia wrote later for his election at the Academy of Sciences in 1934 (this notice is published in [Julia 1968]),

> This work occupied the spare time in the hospital during the year 1916[4].

He had already finished writing the two hundred and ninety-three pages of his thesis, since the report on it[5] written by Georges Humbert was dated May 24th. He had also constituted the committee: Picard would chair it and Humbert would be a referee, but he needed a third examiner and, as he was afraid Borel would be too busy, he asked Lebesgue to be this examiner (letter to Borel[6] of May 29th 1917).

May 21th

War continued. Soldiers began to refuse to be slaughtered for nothing. Painlevé named Pétain as commander-in-chief and Foch as chief-of-staff. Mutinies broke out. The War Ministry sent a scientific mission to the United States and this was announced in the *Comptes rendus* just before Fatou's Note we discuss below.

Pierre Fatou

A Note by Pierre Fatou [1917b], presented at the May 14th session, appeared[7]. The author was interested, as he had been in 1906, in the determination of

[2] The enormous authority of Émile Picard in the mathematical and more general scientific community throughout several decades (at least from 1917 to his death in 1941) will be evident from time to time in this book. See also page 207.

[3] Picard, who lost a son and a daughter in 1915, sent his candidacy on writing paper with black edging, on March 25th (biographical file of Picard, archives of the Academy of Sciences).

[4] Ce travail [...] a occupé [ses] loisirs d'hôpital pendant l'année 1916.

[5] All information about Julia's thesis (date, reports) were given to us by Juliette Leloup.

[6] Borel Fund, archives of the Academy of Sciences.

[7] A Note was presented to the Academy during a session. It could be printed one or several weeks later, for instance because the author had not corrected the proofs immediately.

the convergence domains corresponding to the various limit points associated with rational fractions. He announced that he knew how to solve the iteration problem, which he qualified as "very difficult" in general, in the special case of rational fractions that map a given disc into itself. Here is the classification he obtained. There are three possibilities (to clarify the exposition, we include the simplest possible examples):

– Either: R has two attracting fixed points that are conjugated with respect to the circle, and $k-1$ repelling fixed points, all located on the circle; the interior of the circle converges to one of the attracting fixed points and the exterior to the other. This is the situation we met in Example I.4.1 where the two fixed points were 0 and ∞, conjugated with respect to the circle.

– Or: R has a unique attracting fixed point[8] (and also k other fixed points that are repelling) which is located on the circle, and the iterates of all the points (except for a discontinuous perfect set contained in the circle) converge to this fixed point. Regarding the "circle" as the real axis (a case to which Fatou reduces for his computation), this is for instance the case for

$$R(z) = 2z - \frac{1}{z},$$

where the point at infinity is the only attracting fixed point, the repelling cycles are located on the real axis and constitute the discontinuous perfect set in question.

– Or: R has a unique fixed point of multiplier $s = +1$, a double or triple root of $R(z) = z$, located on the circle, and all the iterates of all the points converge to this fixed point, except perhaps those on the circle[9] (there are $k-1$ or $k-2$ repelling fixed points, all located on the circle). This is the case (again, the real axis is the "circle") for

$$R(z) = z - \frac{1}{z},$$

where the point at infinity is the unique fixed point, an "indifferent" fixed point. The iterates of the points of each of the two open discs (half-planes) converge to ∞. The only points of the x-axis that converge to this point are its antecedents (a countable subset of $\mathbf{R}$).

June 4th

Gaston Julia

A sealed envelope of Julia was recorded under the number 8401. The small envelope[10] that housed the manuscript bare his name and address: Gaston Julia,

[8] Note that the term "attracting" did not yet exist and that Fatou still called these points "limit points"; they are indeed points which are limits.

[9] Nothing is said about the points on the circle.

[10] For detail on the material, physical, aspects of the sealed envelopes under consideration, see the session files, at the archives of the Academy of Sciences.

45 rue d'Ulm[11]. It contained eleven pages, torn from a notebook, filled with Julia's neat, fine handwriting, in purple ink (with a lot of crossings-out), under the title "On point transformations" [Sur les transformations ponctuelles][12].

We thus see that Julia had already obtained his first results on iteration. He nevertheless continued working on the theory of forms, publishing two Notes on this subject, on June 11th and 25th. He wrote to a friend[13], on July 5th [1970, p. 2]:

> I did quite a lot of work last year during the time left to me after violent headaches, frequent dressings and operations. I wrote a thesis that I will defend, I think, next November [actually December 12th, see below]. Having finished the thesis, I did something else, from which I expect more, and which I will only publish after a few months, when I will have gone into it more deeply.
> In the meantime, I have looked at a few small problems, from which I will write a few papers[14]..

This "something else" is certainly the beginning of his work on iteration and the "more" he is expecting may be the Great Prize. It is not impossible that Georges Humbert, Émile Picard or others encouraged Julia to compete.

Paul Montel

The Note [Montel 1917a] published by Montel at that date was to inspire Fatou and Julia. Julia would write in his Note [1917] about his first sealed envelope:

> At that time, I was not aware of M. Montel's works. His Note dated June 4th 1917 drew my attention. I then studied them in a reprint M. Montel was kind enough to send me[15].

[11] This is the address of the ENS. Julia was no longer a student there but he still lived at 45 rue d'Ulm, at Hospital 103, between the surgery he had to undergo, which took place a few hundred metres from there, at the military hospital of Val de Grâce (see [Julia 1970, p. 381]). Marguerite Borel, the wife of the scientific director of the ENS, was a nurse at Hospital 103, so that the injured former students of this school find themselves "at home". The address on the envelope was not written by Julia himself but by the person who registered it at the Academy.

[12] See also Note 42.

[13] Julia's correspondent was a son-in-law of Poincaré. In the rest of his letter, Julia therefore mentioned the relationship between his work and that of Poincaré.

[14] J'ai fait assez de travail l'an dernier pendant les loisirs que me laissaient de violents maux de tête, des pansements fréquents et des opérations; j'ai rédigé une thèse que je soutiendrai je pense en novembre prochain. La thèse terminée, j'ai fait autre chose dont j'attends mieux et que je publierai dans quelques mois seulement, quand j'aurai assez creusé. / Entre temps, j'ai fignolé quelques petites questions dont je ferai quelques articles [...]

[15] À cette époque, j'ignorais les travaux de M. Montel. Mon attention sur eux fut attirée par sa Note du 4 juin 1917. Je les étudiai à ce moment dans un tirage à part que M. Montel voulut bien m'envoyer.

Montel used his theory of normal families to investigate the conformal mapping of a simply connected open subset of **C** the boundary of which is not an analytic arc. This was what would give Fatou and Julia the idea of using this same theory to investigate the intricate sets they saw occurring in their work on iteration.

⋆

On August 4th 1917, Émile Borel wrote the preface for his book [1917], lectures at the ENS, which were written up by Julia; the book should have appeared in 1914, but its publication was delayed by the war: at the time of the mobilisation, on August 1st 1914, Julia had finished the first three chapters and, as he wrote to Borel[16], he had only two problems to solve to finish the fourth chapter. Julia finished writing at the end of May 1917 and he corrected the proofs after "being gloriously wounded in January 1915", Borel writes,

> [...] despite courageously endured suffering, and at the same time carrying on remarkable personal work; I highly appreciate the value of the assistance of this young scientist, on whom we can rely to perpetuate the French mathematical tradition[17].

This shows, on the one hand the exceptional capacity of Julia, and on the other hand the weight of the expectations his "fathers" placed on him.

August 27th

The Academy of Sciences published a Note on the supposed influence of the cannonade on rainfall. Julia deposited a second sealed envelope (number 8431)[18], comprising seven pages, "On rational substitutions (2nd Note)" [Sur les substitutions rationnelles (2e note)].

On September 13th, Painlevé was nominated Prime Minister.

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[16] Julia also wrote Bôcher's book [1917], a book published in 1917, but based on lectures given in 1913–14 (the American mathematician Maxime Bôcher spent three months at the Sorbonne, on an exchange with Harvard which began in November 1913 [Eisele 1971]). Julia finished writing the book before the war: there was no mention either of him or of his wounds in the preface. In his letters to Borel in August 1914, he fretted about this book. For Julia's letters to Borel, see Borel Fund, archives of the Academy of Sciences.

[17] [...] malgré des souffrances courageusement supportées et en même temps qu'il poursuivait de remarquables travaux personnels; j'apprécie hautement la valeur du concours de ce jeune savant sur qui l'on peut compter pour perpétuer les traditions mathématiques françaises

[18] The envelopes which contained the manuscripts were signed by the person who registered the sealed envelope and who seems here to have been Lacroix, the other Permanent Secretary.

September 17$^{\text{th}}$

Julia deposited a third sealed envelope (number 8438), comprising four pages, "On rational substitutions" [Sur les substitutions rationnelles].

On November 17$^{\text{th}}$, Clemenceau replaced Painlevé as the Prime Minister.

December 10$^{\text{th}}$

The Academy of Sciences held its annual public session[19]. The President d'Arsonval being absent, it was Perrier who read his very sober speech (in which he mentioned that it was not he who would have the pleasure of "welcoming the dawn of the definitive victory" [saluer l'aurore de la définitive victoire]).

Julia came to receive the Bordin Prize, awarded for his thesis, on the subject (on which the Academy received only two manuscripts):

> To improve, in some important point, the arithmetic theory of non-quadratic forms.

The length of the report which was devoted to this work[20] and which was published in the corresponding volume of the *Comptes rendus* was rather impressive. It was none other than the report Georges Humbert wrote on the thesis in May. Julia perhaps took advantage of his presence there to deposit his fourth and last sealed envelope (number 8466), four pages (one of which is a correction to an assertion made in the second envelope), "On the iteration of rational functions $z_1 = \varphi(z)$" [Sur l'itération des fonctions rationnelles $z_1 = \varphi(z)$].

An interlude on Julia's thesis is essential here.

Digression (Julia's thesis). On December 12$^{\text{th}}$ (in his CV [1970, p. 289], Julia mentions November, but the dates on the manuscript, on the reports on the thesis and on the defence agree: it is indeed December 12$^{\text{th}}$), Julia defended his thesis "Study of non-quadratic binary forms with real or complex indeterminates, or with conjugated indeterminates" [Étude sur les formes binaires non quadratiques à indéterminées réelles ou complexes, ou à indéterminées conjuguées][21].

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[19] As usual, the public session included a speech by the President (such as the ones we met earlier, [Perrier 1915 ; Jordan 1916 ; Painlevé 1918c]), with the list of all the Academicians who had passed away during the year, with a few words on each, and the awarding of the Prizes—the award-winners usually being present.

[20] Julia's thesis itself is exceptionally long (293 pages).

[21] Later, Julia perhaps considered his thesis and the publications associated with it as being marginal in his work, and he downplayed them in Volume 5 of his Complete Works. We have mentioned that his first paper [1913] was already a classical work of complex analysis. Julia was an analyst and he showed it.

At that time, the official title "thesis advisor" did not exist. It is clear that the subject on which Julia worked for his thesis was inspired by Hermite's work, so that a former student of Julia at École polytechnique believed he remembered in [Montbrial 2003]:

> In Geometry, we were all in one lecture hall with Gaston Julia. In his lectures, this severely injured soldier of the First World War did not do any mathematics but rather recounted, every year, the same how he did his thesis with the illustrious Charles Hermite[22].

The illustrious Charles Hermite died in 1901, so that we can imagine that Julia was not well understood (Montbrial was a student on this course in 1963, when Julia was seventy and his voice had become very low). What is certain is that at the time he prepared his thesis, Julia spoke of mathematics a lot with Georges Humbert and also with Émile Picard. In a speech he gave in 1950, Albert Châtelet [Châtelet 1950, p. 145] addressed Julia, speaking of "your master Georges Humbert". It is also certain that the lectures of Georges Humbert in 1916 were very important to Julia[23]. And, even if the use of the term "master" at that time was very varied, it indeed seems that Humbert played in Julia's thesis a role which is very close to that of today's supervisor. Julia actually said:

> I was delighted by these beautiful geometric interpretations of arithmetic facts. They suggested to me the thesis I was to defend one year later and which strongly interested Humbert. Every week, after his lecture (which took place form 12.30 to 14.30), I would explain my recent discoveries and he would invariably urge me to write them up. To supervise me more closely, he would invite me to his house in Normandy, where he lived almost permanently and he would jokingly require every day, a certain number of pages and theorems. Then we would go for a walk in the countryside. Sitting on an embankment, or at the corner of a beech forest, he would explain to me his own work and, impromptu, a lot of questions I had ignored completely, my

[22] En géométrie, nous eûmes droit à un amphi unique de Gaston Julia. Dans cet amphi, naturellement, ce grand blessé de la première guerre mondiale ne faisait pas de mathématiques, mais il racontait tous les ans la même histoire, sur les conditions dans laquelle il avait fait sa thèse avec l'illustre Charles Hermite.

[23] In the obituary [Borel 1922], Borel mentioned in particular the subjects of seven series of lectures given by Humbert at Collège de France, following notes given to him by Gaston Julia, who attended all Humbert's courses from 1914 to 1920 (except that of 1915) and he writes:

> Humbert gave the true measure of his qualities as a professor, he trained students and exerted an important influence on the orientation of mathematical studies in France. [Humbert a donné la vraie mesure de ses qualités de professeur, formé des élèves et exercé une influence importante sur l'orientation des études mathématiques en France.]

See also Paul Lévy's souvenirs on Humbert's lectures at École polytechnique and Collège de France in his book [1970, p. 37].

> development having been more analytic than arithmetic or geometric, while Humbert was more an algebraist or geometer than an analyst[24].

See also [Desforge 1979]. However, it is not impossible that the Peccot course given by Châtelet [1913] himself in 1912 (the notes of which were written by Vidil) influenced the choice of the subject of Julia's thesis.

In any case, the committee consisted, as we said, of Émile Picard, chair, Georges Humbert, referee and Henri Lebesgue, examiner. The cause of the delay between the time when Georges Humbert wrote his report (May) and the defence (December) was possibly the surgery Julia had to undergo. We know for certain, for instance, that he had a cartilege transplant in June [Julia 1970, p. 1]. It is also possible that Picard preferred to await the proclamation of the Bordin Prize, in order to make the defence more significant.

What is likely, is that the defence was more than a mathematical event. To prove this, let us quote here what Picard said, which can be found in the report on the defence[25]:

> Sir, the work you presented as a thesis has been honoured the day before yesterday by the Academy [we are indeed on December 12$^{\text{th}}$]. One will read in the next issue of the *Comptes rendus* the report written by M. Humbert. I will not repeat how much we appreciated your remarkable work, the second and third parts of which demonstrate a real inventiveness. We also appreciate the moral qualities you showed in the last three years. You had the pious thought to dedicate your work to the memory of your fellows from École normale who fell on the battlefield. You were more fortunate than so many young professors, whose heroic death will remain the glory of the École normale. But you have been cruelly distressed, and it is between numerous operations that you found the energy to devote yourself to mathematical research. After the tragic days in which we are living, France will need, more than ever, men of talent and of character. We rely on you, Sir, for the future. After this first

24 [Ces belles interprétations géométriques de faits arithmétiques m'enchantèrent. Elles me suggérèrent la thèse que je devais soutenir un an plus tard et qui intéressa vivement Humbert. Chaque semaine après le cours (qui avait lieu de 12$^{\text{h}}$30 à 14$^{\text{h}}$30), je lui exposai [*sic*] mes récentes trouvailles et il me pressait invariablement de les rédiger. Pour me surveiller plus étroitement, il m'invitait dans la propriété normande où il vivait presque en permanence et exigeait chaque jour, en plaisantant, un certain nombre de pages et de théorèmes. Puis nous allions nous promener dans la campagne. Assis sur un talus, au coin d'un champ ou d'une hêtraie, il m'expliquait ses propres travaux et, impromptu, de proche en proche, un tas de questions que j'ignorais totalement, car ma formation avait été plus analyste qu'arithmétique ou géométrique tandis qu'Humbert était plutôt algébriste et géomètre qu'analyste.] This is an excerpt of a paper [Julia X] by Julia a copy of which (a cutting in a journal) we found in the Humbert file at the archives of the Collège de France, but the origin of which we were unable to find (in the rest of this article, one learns Julia and Humbert singing Wagnerian arias in chorus in the countryside).

25 Reference AJ/16/5542 at National Archives. And many thanks again to Juliette Leloup...

> work, further will come, which you already have in progress. For today[26], I am happy to tell you that the Faculty confers on you the grade of doctor with distinction, and we add to this our strong and heratfelt congratulations[27].

The atmosphere in which our story continued to unfold could not be better described.

December 17th

This is the day when the Academicians were read a telegram by Vito Volterra[28] who thanks for having been elected foreign associate member in these words[29]:

> = WISHING TO EXPRESS TO FELLOW MEMBERS MY DEEP GRATITUDE FOR THE VERY GREAT HONOUR GIVEN BY THE FAMOUS ACADEMY I WANT TO TELL THAT I AM ESPECIALLY MOVED IN THIS MOMENT OF HISTORY WHEN IN A BROTHERLY OUTBURST YOUR NOBLE HEROIC COUNTRY HAS SENT ITS ARMY TO FIGHT WITH OURS = VITO VOLTERRA[30]

Although this was not the subject of any prize, proofs of Fermat's theorem regularly arrived at the Academy of Sciences. One of them, this very day, in fact. But, above all, this was the day when the history of iteration really started.

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[26] The work Julia would submit for the Great Prize a few days later had thus already been announced. Knowing the rest of the story, it is hard not to hear the "for today" as a promise (?).

[27] Monsieur, le travail que vous avez présenté comme thèse a été couronné avant-hier par l'Académie [nous sommes bien le 12 décembre]. On lira dans le prochain numéro des *Comptes rendus* le rapport fait à ce sujet par M. Humbert. Je ne redirai pas combien nous avons apprécié votre remarquable travail, dont la deuxième et la troisième partie témoignent d'un véritable esprit d'invention. Nous apprécions aussi les qualités morales dont vous avez fait preuve depuis trois ans. Vous avez eu la pieuse pensée de dédier votre travail à la mémoire de vos camarades de l'École normale tombés au champ d'honneur. Vous avez été plus heureux que tant de jeunes professeurs, dont la mort héroïque restera la gloire de l'École normale. Mais vous avez été cruellement éprouvé, et c'est entre de nombreuses opérations que vous avez eu l'énergie de vous livrer à des recherches mathématiques. Après les heures tragiques que nous vivons, la France aura besoin plus que jamais d'hommes de talent et de caractère. Nous comptons sur vous, monsieur, pour l'avenir. Après ce premier travail, en viendront d'autres que vous avez déjà sur le chantier. Pour aujourd'hui, je suis heureux de vous dire que la Faculté vous confère le grade de docteur avec la mention très honorable, et nous y joignons nos vives et sympathiques félicitations.

[28] Italy formed, in 1917, an *Uffizio Inventioni*, chaired by Vito Volterra. On Voterra's activity during the war, see [Goodstein 2007].

[29] Session file, archives of the Academy of Sciences.

[30] En vous priant vouloir exprimer confrères ma profonde reconnaissance très fier grand honneur célèbre académie a voulu me rendre je tiens vous dire que j'en suis particulièrement ému dans ce moment historique où par un élan fraternel votre noble héroïque pays envoie son armée se battre à côté de la nôtre = Vito Volterra

Pierre Fatou

And the history started with the publication of another Note by Pierre Fatou [1917c], in which he announced a whole series of results.

– Still looking at the boundary of the domain of attraction of an attracting fixed point a, Fatou defined a set D_a as the set of the points z in the plane such that, in a neighbourhood of z, the sequence f^n converges uniformly to the constant function equal to a. He claimed that the boundary of D_a, which he called F (probably because this is a frontier[31]) is a perfect set (if the function f is not linear) and above all, and this was the great novelty, *that at a point of F, the sequence f^n cannot be normal*. In any disc centred at $z \in F$, any of the functions f^n takes all values, except at most two, which are called exceptional values. These exceptional values exist only if (up to conjugacy), f is a polynomial or a power function ($z^{\pm k}$, the exceptional values being then 0 and ∞).

– If there are more than two limit points or cycles, only one of the domains of convergence D_a is connected, the others have infinitely many connected components. See Example II.1.1.

– Fatou also showed that the set F is the derived set of $\cup_{n \geq 0} R^{-n}(z)$ (for any point z except the exceptional ones), and that in F in general there are repelling fixed points[32] in F (see the precise statement in Remark III.1.2). All this being established, the set called E' in I.4, the derived set of the set of repelling fixed points of R^n, is identical to the set F of points at which the sequence R^n is not normal.

– For every limit cycle $a_1, \ldots, a_n$, at least one of the domains D_{a_i} contains a critical point of the function f^{-1} (an application of normal families)[33].

– He made connections with his 1906 Note [1906d] and he gave new examples.

Example II.1.1. Fatou gave the example of $R(z) = z^2 - 1$. The attracting point is ∞. The domain D_∞ (its basin of attraction) is connected. The points 0 and -1 are interchanged by R and they constitute a cycle of order 2, which is the only attracting cycle. The domains D_0 and D_{-1} have infinitely many connected simply connected components[34], as Figure II.1 suggests. There are points of D_∞ which "penetrate" between the small domains constituting D_0 and D_{-1}.

Apparently, this image (the left part of Figure II.1, drawn by Arnaud Chéritat) evokes the domes of San-Marco basilica in Venice together with

[31] Slightly ironically, what is nowadays called the Julia set and is often denoted J, the points of which will soon be called "J-points", was indeed invented by the discreet Fatou under the name F.

[32] The term "repelling" [répulsif] still did not exist.

[33] The same is true of the critical points of f, an application of Schwarz' lemma, see [Douady 1983]. One or the other of these properties implies that there are only finitely many attracting cycles.

[34] A "simply connected" "area" is an area which is delimited by a single contour.

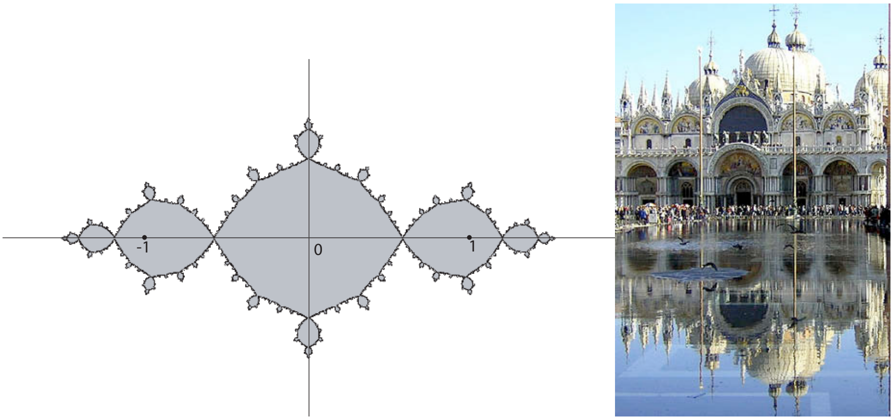

Fig. II.1. The set F for $R(z) = z^2 - 1$ (the basilica)

their reflection in the water on an "acqua alta" day [Mandelbrot 1982, p. 185]; anyway, it is nowadays called the "basilica" by specialists in complex analysis.

Example II.1.2. Let R be the polynomial[35] defined by $R(\cos u) = \cos(2u)$, namely

$$R(z) = 2z^2 - 1, \text{ which is conjugated with } P(z) = z^2 - 2.$$

The only attracting fixed point is ∞ and $D_\infty = \mathbf{C} - [-1, 1]$. Here $\theta(u) = \cos u$ is the Poincaré function we defined in § I.6.

Furthermore, $R^n(z) = z$ with $z = \cos u$ if and only if $\cos(2^n u) = \cos u$, that is, if and only if

$$u = \frac{2k\pi}{2^n \pm 1}.$$

All these points are repelling. The corresponding values of $\cos u$ are dense in $[-1, 1]$. Hence E' is the interval $[-1, 1]$. As in the case of Example I.4.1, the set E' (or F) is a "linear continuum" [un "continu linéaire"][36].

[35] Fatou considers more generally the polynomials defined by $R(\cos u) = \cos(2ku)$ and by $R(\sin u) = \sin(2k + 1)u$ (Tchebychev's polynomials).

[36] It does not seem to me that there exists a precise definition of the expression "linear continuum" anywhere in the papers under consideration. The interval that appears here is certainly a linear continuum... continuum certainly meaning connected while linear means a "curve"—this expression often coexists with that of "superficial continuum", but for dimension 2 this time. See also Note 17 in Chapter III.

December 24th

On this Christmas Eve, the Academy received a letter from Gaston Julia, that would become, one week later, his Note [1917]. This is the same day that Julia deposited his manuscript for the Great Prize competition at the secretary's office of the Academy of Sciences [Julia 1917, note 2]. He brought the letter at the same time. He claimed in this letter that the results announced by Fatou in the Note [1917c] are contained in his sealed envelopes and required that they be opened. The envelopes were opened during the session (the empty envelopes are retained in the file of this session) and the Academy chose Georges Humbert to study them. Coincidentally, during the same session, Hadamard communicated a Note by Hardy and Littlewood [1917], in which the authors generalised a theorem from Fatou's thesis [1906c] (on Fourier series)—the *Comptes rendus* also published a Note on cannon sound at high speed.

December 31st

On this day the Academy of Sciences played its role of accreditation of scientific discoveries and of their priority, and the *Comptes rendus* published a short report by Georges Humbert [1917]:

> The comparison of the texts of the two Notes[37] with the text of the four sealed envelopes shows that the claim of M. Julia is well based: his envelopes contain, among other things, all the statements for which he claims priority; they appear there, either in the terms of one Note or the other, or in equivalent terms, and sometimes in a more general or extensive form[38].

Gaston Julia

Gaston Julia's letter is thus published as a Note [1917]. Here is most of its first paragraph[39]:

> I have just read with interest M. Fatou's Note that was published in the *Compte rendu* of December 17th 1917. The essential results it contains I wrote down myself earlier in a series of four sealed envelopes [...] On opening these envelopes, the Academy will be able to record that the results given by

[37] The two Notes mentioned by Humbert are [Fatou 1917c] and [Julia 1917].

[38] La comparaison du texte des deux Notes avec celui des quatre plis montre que l'assertion de M. Julia est fondée: ses plis renferment bien, parmi d'autres, tous les énoncés dont il revendique la priorité; ils y figurent, soit dans les termes de l'une ou de l'autre Note, soit en des termes équivalents, parfois aussi sous une forme plus générale ou plus étendue.

[39] This letter arrived at the Academy of Sciences on December 24th. The publication of the *Comptes rendus* was thus extremely rapid. Unfortunately, the manuscript was kept by its author and cannot thus be found at the archives of the Academy of Sciences.

> M. Fatou, up to notation and examples, are written there, with an indication on the methods employed, one of which uses, an amazing coincidence, precisely the results of M. Montel on normal sequences of analytic functions used by M. Fatou. The Academy will judge, with regard to the methods and also the results, to whom priority should be attributed. I add that my four sealed envelopes contain more results than those stated by M. Fatou, and other methods. And, in relation to the results stated by M. Fatou, some further details may be added[40].

There were, it is said (see [Alexander 1994, p. 115]), some comment on the expression "amazing coincidence", that it should be taken rather as a sign that Julia was very angry than as an insinuation. Julia did not accuse Fatou of stealing his results. The coincidence was not that both of them were working on the subject: there was a competition and Fatou was known to have done previous work on the subject, so that this was not unexpected; rather the coincidence was that both of them thought of taking advantage of Montel's normal families. It is true that Julia's tone is a little arrogant—but surely that is the familiar self-importance of the brilliant ENS student, perhaps strengthened by the feeling that much (everything?) was due to him, because of the atrocious wounds he had suffered. We shall see below what should be thought of his claim that his results were more precise than Fatou's.

With regard to Julia's claim, Humbert and the Academy of Sciences decided to grant him priority. They might nevertheless have noticed, that, when Julia writes, in his Note

> If I decided to deposit these envelopes, it is because, on May 21st 1917, M. Fatou made known, in the *Comptes rendus*, some of the results I had already obtained. Hence, as early as June 4th, I made known [...][41]

he admitted priority to Fatou[42]. However, nobody seems to have noticed it.

40 Je viens de lire avec intérêt la Note de M. Fatou publiée dans le *Compte rendu* du 17 décembre 1917. Les résultats essentiels qu'elle contient, je les ai consignés moi-même antérieurement dans une série de quatre plis cachetés [...] L'Académie pourra, en ouvrant ces quatre plis, se rendre compte que les résultats que donne M. Fatou, aux notations et aux exemples près, s'y trouvent avec l'indication brève des méthodes suivies dont l'une, coïncidence curieuse, utilise précisément les résultats de M. Montel sur les suites normales de fonctions analytiques qu'utilise M. Fatou. L'Académie estimera, à la fois quant aux méthodes et quant aux résultats, à qui doit être attribuée la priorité. J'ajoute que mes quatre plis contiennent d'autres résultats en sus de ceux indiqués par M. Fatou, et d'autres méthodes. Et relativement aux résultats énoncés par M. Fatou, il y a des précisions nouvelles qu'on peut énoncer.

41 Si j'ai pris la décision de déposer ces plis, c'est qu'à la date du 21 mai 1917, M. Fatou faisait connaître dans les *Comptes rendus* quelques-uns des résultats auxquels j'étais parvenu. Aussi, dès le 4 juin, je faisais connaître [...]

42 This could also explain the physical appearance of Julia's manuscripts: they are drafts, with crossings out, written on pages torn from a notebook. He certainly wanted to submit something, as soon as possible, after [Fatou 1917b] had been published in the *Comptes rendus*.

The fact remains that Julia repeated, in his note, the results stated by Fatou and that he explained his treatment of each of them:

– He had proved that there exists a critical point for f^{-1}, but in a *restricted* domain (he did not give the definition).

– He had also looked for the possible number of fixed points, in a less precise way than Fatou had[43], but he had improved this result since then.

– After that, there had been Montel's Note [1917a] and he had started using normal families. He had introduced the set Fatou called F while he had called it E', had investigated its properties and had proved those announced by Fatou, forgetting the case of the complex exponential (because, he said, of concerns related to his thesis).

– He had also found examples with the same properties as these of Fatou (Example II.1.1 above)... and he had even found that of $R(z) = z^2 - 2$ (which is, as Julia confirmed it, conjugated with Example II.1.2 related to $\theta(u) = \cos u$).

End of 1917

The manuscripts in competition for the prize had been deposited by the end of 1917 (if the deadline does not seem to appear anywhere in the *Comptes rendus*, it is written on Pincherle's[44] manuscript to which we shall return below: December 31st 1917). In any case, as we have seen it, Julia said in his Note [1917] that he had deposited his on December 24th. We shall see that, when the Committee examined them, there were three of them.

II.2 Year 1918

Throughout the year 1918, even though the competitors had already sent their manuscripts, the protagonists of our story proceeded to submit *Comptes rendus* Notes, repeating or not results contained in their Memoirs. This year, the president of the Academy of Sciences was Paul Painlevé (who no longer had a political position).

January 7th 1918

Accordingly it was Painlevé's role to give the speech, "taking possession of the chair" [en prenant possession du fauteuil] (using the Academy jargon), during the first session of the year. This speech gives an important place to war related research:

[43] There is a mistake in his statement, see Remark III.1.2.

[44] File of prizes, archives of the Academy of Sciences.

> [...] If I cast my eyes around this room, on our colleagues who were appointed to head the major departments of the National defence, I see (I quote them at random, and how long the list would be, were it complete) an astronomer who proved himself a tenacious and inventive artilleryman, physicists who contributed to the development of military applications of WTS; chemists who, in the gas war, increased our means of protection and attack; a mathematician, a geodesist the computations of whom were used to detect and destroy enemy batteries. You encouraged or rewarded much work the results of which were kept secret. Your students, many of them already masters, the youngest at the front, the others in the universities, the arsenals, the factories, efficiently attacked all the new problems raised by the war, on the land and at sea. Fifteen months ago, one of our senior chiefs spent a whole day visiting our laboratories of pure science, which spontaneously devoted themselves to National defence, and he did not conceal the feelings of admiration that this scientific mobilisation inspired in him. His sharp eye discerned the truth and the intricacy of these researches, the perseverance involved in starting from initial attempts and ending at the mass production of practical instruments; the wonderful output from limited resources, obtained by the disinterested zeal of all, from the innovators to the more modest contributors. [...] [Painlevé 1918a][45].

[45] [...] Si je jette les yeux dans cette salle, à côté de ceux de nos confrères que leurs fonctions mêmes ont placées à la tête des grands services de la Défense nationale, j'aperçois (je cite au hasard, et combien l'énumération serait longue si elle était complète) tel astronome qui s'est révélé artilleur inventif et tenace, tels physiciens qui ont contribué à développer les applications militaires de la T. S. F.; tels chimistes qui, dans la guerre des gaz, ont accru nos moyens de protection et d'attaque; tel mathématicien, tel géodésien dont les calculs ont servi à repérer et à détruire les batteries ennemies. Vous avez encouragé ou récompensé de nombreux travaux dont les résultats ont dû être tenus secrets. Vos élèves, dont beaucoup sont déjà des maîtres, les plus jeunes au front, les autres dans les universités, dans les arsenaux, dans les usines, se sont attaqués efficacement à tous les problèmes nouveaux qu'a soulevés la guerre sur terre et sur mer. Il y a quinze mois, un de nos grands chefs employait une journée entière à visiter des laboratoires de science pure, qui spontanément s'étaient consacrés à la Défense nationale, et il ne dissimulait pas les sentiments d'admiration que lui inspirait cette mobilisation scientifique; son œil aigu d'observateur avait discerné la vérité et la délicatesse des recherches, leur ténacité allant des premiers tâtonnements jusqu'à la réalisation en série des instruments pratiques; le merveilleux rendement de ressources bien restreintes, obtenu grâce à l'ardeur désintéressée de tous, des initiateurs come des collaborateurs les plus modestes. [...]

Samuel Lattès

In accordance with the motivational statement accompanying the subject of the Great Prize, Lattès[46], using the existence (proved by Fatou [1917c])[47] of a repelling fixed point (for a rational fraction with distinct fixed points), based his Note [1918a] on the investigation of the Poincaré function denoted θ (see § I.6 for the definition of θ). He stated his version of the iteration problem:

> To determine the derived set E' of the set E of consequents of an arbitrary point z.

And he proposed to investigate it, using the properties of the function θ, particularly in the case of functions θ for which s is an integer, for instance a trigonometric function (we have seen that $\theta = \cos$ for the rational fraction of Example II.1.2 and Lattès also mentions $\theta = \tan$ for $s = 2$ and the rational fraction which expresses $\tan(2u)$ in terms of $\tan u$) or an elliptic function, such as the Weierstrass $\wp$-function in the example above[48]. These examples are the natural cases where a duplication formula (such as $\cos(2w) = 2\cos^2 w - 1$) giving $\theta(2w)$ as a rational fraction of $\theta(w)$ is known. They had appeared, as such, in the paper of Schröder [1871] — long before mathematicians would be interested in the investigation of the set E'.

Example II.2.1. Lattès[49] considers the rational fraction

$$R(z) = \frac{(z^2+1)^2}{4z(z^2-1)}.$$

---

[46] Lattès' manuscript, kept in the session file at the archives of the Academy of Sciences, is written in green ink, in beautiful large handwriting.

[47] This all happened very quickly: Lattès used a Note which had appeared three weeks before, he was in Toulouse, so one could not expect him to mention Julia's [1917] Note of December 31st. On the other hand, he used the notation E', as Julia did.

[48] The cases of the power functions (as in Example I.4.1), of Tchebychev polynomials (as in Example II.1.2) and of elliptic functions (as here) are cases for which the Poincaré function is periodic (or even doubly periodic). For a more general and modern study, see [Milnor 2006b].

[49] Lebesgue thought that Samuel Lattès' work was undervalued; he had already written in 1910, in his lively and non-conventional style:

> He had carried out interesting, *useful*, *individual* [underlined] work, but his thesis is known only by Hadamard, who is not as impressed by it as by that of Fréchet. [Il a fait des travaux intéressants, *utiles*, *personnels* [souligné], seulement sa thèse n'est connue que de Hadamard, qui n'en est pas épaté comme de celle de Fréchet.] [Lebesgue 1991, p. 251].

In addition to his own work, this mathematician from Nice and of Italian origin translated the book [Burali-Forti & Marcolongo 1910] to which he added a supplement on quaternion systems.

The Weierstrass $\wp$ function associated with the elliptic curve with equation $y^2 = 4z(z^2 - 1)$ satisfies the duplication formula

$$\wp(2u) = \frac{(\wp(u)^2 + 1)^2}{4\wp(u)(\wp(u)^2 - 1)},$$

in other words, $\wp$ is the function θ of Poincaré for R with $s = 2$. The consequents of $z = \wp(u)$ are thus

$$z_n = \wp(2^n u).$$

To determine the closure of the sequence of consequents of a given point z is then a question of arithmetic.

⋆

On the same day, the Academy of Sciences discussed, in secret committee (see Note 16 page 7) the proliferation of foreign words used by some young scientists in their papers; Picard complained that the ephemerides of the "little planets[50]" [asteroids] were computed by the Germans[51].

January 14th

Gaston Julia

In his Note [1918a] (passed in the January 7th session, as was that of Lattès [1918a], but which was published the 14th), Julia considered, as Lattès did, the set of consequents $\{z_1, \ldots, z_n, \ldots\}$ of a point z and its derived set. The fundamental problem he studied is how a limit point of this set depends on the initial value z of the sequence.

– He thus considered the non-empty countable set E of repelling points (using here the same notation as in § I.4).

– He announced that the derived set E' is perfect.

– The structure of the set E' is the same as that of all its subsets, in the sense that E' is everywhere discontinuous or everywhere a linear continuum, or everywhere a superficial continuum[52] (and he proved that in this case, if E' has non-empty interior, it is the whole plane $\mathbf{C}$—in other words, if there is a point in $\mathbf{C}$ that is not in E', then E' has an empty interior). He did not give any example (simply because he could not find one [Julia 1918f, p. 105]).

– When E' is not the whole plane $\mathbf{C}$, Julia investigated, finally, the regions of the plane bounded by E'.

---

[50] The annual report of the Paris Observatory for 1916 indeed mentioned that, owing to the lack of ephemerides, the observation of asteroids had been deferred.

[51] Secret committee register, archives of the Academy of Sciences.

[52] See Note 36.

January 28th

Samuel Lattès

In the Note [1918b], passed during the January 21st session, Lattès began his investigation of rational fractions with two variables[53]

$$(z_1, z_2) \longmapsto (R_1(z_1, z_2), R_2(z_1, z_2)).$$

He considered the "parametric" question, that is with a function θ, as in § I.6, and he looked at the case where there is a point (z_1, z_2) at which the two multipliers[54] s_i are such that $|s_i| > 1$. He studies in particular the case of a Cremona transformation (the case where R_1 and R_2 are polynomials).

There was a small error in this Note. This was pointed out, a few years later, by Fatou [1924b] (see also our note 41 in Chapter V).

Gaston Julia

In his Note [1918b], Julia seized the example Lattès had just published [1918a] (here Example II.2.1) and pointed out that, in this example, the set E' is "identical to the whole plane", that is the whole of $\mathbf{P}_1$: choose $u = 2\omega(v + iw)$ with v and w two rational numbers with a periodic expansion in base 2 with period n; then $2^n u \equiv u$ modulo the lattice; u is then a repelling fixed point of R^n and, of course, such u are dense in the period parallelogram.

He then investigated the regions of the plane bounded by his set E', when the latter is not the whole plane.

February 4th

Pierre Fatou

The *Comptes rendus* published the Note [Fatou 1918a][55], in which Fatou showed that the boundary of an invariant simply connected open set containing an attracting fixed point can be an analytic arc only if this open set is a disc (and its boundary a circle!).

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53 As the title of his thesis showed (see page 23), Lattès was a specialist in functional equations for functions of two variables, a subject on which he published several papers.

54 To define the multipliers s_1 and s_2 at a fixed point, one changes coordinates in a neighbourhood of this fixed point (mapped to 0) so that the transformation reads

$$\begin{pmatrix} z_1 \\ z_2 \end{pmatrix} \mapsto \begin{pmatrix} s_1 z_1 + F_1(z_1, z_2) \\ s_2 z_2 + F_2(z_1, z_2) \end{pmatrix},$$

where F_1 and F_2 have no degree 1 term. Samuel Lattès had already studied the case where $|s_1|$ and $|s_2|$ are both < 1 in [Lattès 1911].

55 Pierre Fatou, who did not submit a memoir for the Great Prize, henceforth entitled his work "On functional equations", although the two Notes he published

March 4th

The Brest-Litovsk peace between Soviet Russia on the one hand and the "central empires", Germany, Austria-Hungary, Turkey and Bulgaria on the other, was signed the day before.

Joseph Fels Ritt

At the moment when two million American soldiers left or prepared to leave for Europe, we witness the appearance of an American in our story[56], Joseph Fels Ritt (1893–1951), who would become one of the founders of differential algebra and who was already a specialist in differential equations. He had also obtained results on iteration, some of which was presented to the *American Mathematical Society...* on December 27th 1917[57], which had an intimate relation with the theorems Fatou recently published, and that would appear, he thought, in the *Bulletin of the* AMS. He thus sent his note [1918] to the *Comptes rendus*; in this note, he presented those of his "theorems that do not seem to be identical to those of M. Fatou and some others that he believed to be completely new[58]". He used in particular the Poincaré function near a repelling fixed point. He gave some properties of the function θ, thanks to which he obtained rather strong results: he proved, by virtue of transfinite

before Julia's burst of anger had been called "On rational substitutions". Fatou still worked and published his results, but distanced himself, by his titles, from the subject of the competition. It seems to me that the assessment of [Alexander 1994, p. 124], according to which the title "functional equations" has a nineteenth century flavour, is a misinterpretation and, in any case, does not take into account the fact that Fatou changed his titles as time went on.

56 Other Americans had already contributed to the story of the subject (or allied subjects), especially Edward Kasner (with whom Ritt had prepared his thesis) and George Pfeiffer, but it does not seem that our protagonists knew their work. See § IV.4.

57 The twenty-fourth annual meeting of the Society took place on December 27th and 28th in New York. Ritt presented his work *"On the iteration of rational functions"*; the secretary of the session took notes, so that an abstract appears in Volume 24 of *Bulletin* (page 272). The corresponding paper would never appear. The fact that Fatou and Julia announced analogous results, together with the publication of the Note [Ritt 1918], may have dissuaded Ritt, or the *Bulletin of the American mathematical society*, from publishing it. In 1920, he would publish in the *Transactions* of the AMS [1920] the results of his that did not appear in the memoirs of Fatou and Julia,

> *I wish to present here that small part of my work which has not been covered by the other writers, postponing until after the appearance of their memoirs the publication of such of my other results as may still be of interest.*

58 théorèmes qui ne semblent pas identiques à ceux de M. Fatou et quelques autres qui [lui] paraissent être entièrement nouveaux

ordinals, that near a fixed point of R, there are infinitely many fixed points of R^n. He too showed that the set of accumulation points of the antecedents of a given point contains a perfect set in which all the antecedents of the repelling points are contained (one of the results announced by Lattès [1918a]). In the case where R is a polynomial, he elucidated a dichotomy[59] in terms of the basin of attraction of ∞:

– either all the orbits of the critical points of R are bounded (and the basin of attraction of ∞ in $\mathbf{P}_1$ is then connected and simply connected),

– or one of the orbits of a critical point tends to ∞ and the basin is then multiply connected.

Similarly to Lattès, he did not use normal families. Among the results he used is Böttcher's theorem, which is, as we have noted, the essential tool for the study of the neighbourhood of ∞ in the case of a polynomial.

It is in this Note that the word "repelling" appeared, in the form "point de répulsion", together with "point d'attraction", as perusal of the mentioned papers shows (this was confirmed by Ritt himself in his article [1920]). The efficient terminology attracting/repelling is thus due (in French) to an American[60]! Good news to comfort our Academicians, who were very nervous about the excess of foreign words in scientific publications (see the secret committee of January 7th, mentioned above). On the other hand, Ritt also called a fixed point at which the derivative is 1 a "point of circulation" [point de circulation], but this less fortunate terminology was not kept by posterity...

March 18th

Gabriel Kœnigs, one of the forerunners of iteration, was elected at the Academy of Sciences in the mechanics section (to replace Henri Léauté).

March 25th

The German offensives started again and Paris was bombed two days before by the *Pariser Kanonen.*

Samuel Lattès

Following Ritt's Note [1918], Lattès returned in his own Note [1918c] to the use of the Poincaré function. According to Picard's theorem, the entire function

[59] This dichotomy between the case where the Julia set is connected and the case where it has a non-countable infinity of connected components is classical today (and given here in modern terms)—but this is not the Julia set that Ritt described.

[60] It would need a double translation to become as efficient in English: Ritt wrote [1920] *point of attraction*, *point of repulsion*, for what is today called an *attracting point*, a *repelling point.*

θ takes all possible values except at most two, α and β. Lattès related this to Fatou's and Julia's approach: if z is a point on the boundary of a domain of attraction, the $R^n(z)$ take all values except at most two, and these are the same α and β.

The Note [Lattès 1918c] was published under the title "On the iteration of *ir*rational fractions" [Sur l'itération des fractions *ir*rationnelles], a misprint in the title... it was followed by an *erratum*. There was already an *erratum* for a very visible misprint (which did not come from the manuscript) in a formula in Note [Lattès 1918a]. Apparently, Lattès did not correct his proofs very carefully.

April 15th

Gaston Julia

In this Note [1918c], transmitted the previous week, Julia gave examples of sets E' for Fatou's [1918a] Note of February 4th. He presented in this Note some results contained in his memoir for the Great Prize. In particular the fact that the domains of convergence to an attracting fixed point (he used the terminology "limit point with regular convergence" [point limite à convergence régulière] but not very confidently, so that he mentioned "attracting point" [point d'attraction] in a footnote) are multiply connected only when their order of connectivity is infinite, namely, they are either simply connected (as for a disc), doubly connected (as for an annulus) or their fundamental group has infinitely many generators. He gave three examples. The first one was that of Fatou [1906d], the one in Example I.4.3, for which he claimed that the lines separating the domains of convergence (Fatou had pointed out that these lines were not analytic) constitute a Jordan curve. The other two are described explicitly below.

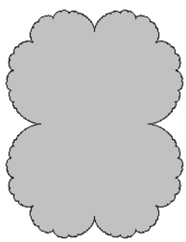

Fig. II.2. A cauliflower, $R(z) = z^2 + \dfrac{1}{4}$

Example II.2.2.

This is a perturbation of a simple example, with two attracting points symmetric with respect to a circle (as in the case of $r(z) = z^2$, our Example I.4.1, where these two points are 0 and ∞). Julia claimed that he could perturb r to R in such a way that R has two attracting fixed points close to the initial attracting points, a closed simple curve (a Jordan curve) close to a circle... but which has a tangent at no point of E. For the modern reader: the set E' is intermediate between Figure I.1 (the circle) and Figure I.2 (a dust), similar to the one on Figure I.4 (a circle with dimples) or what is today called (following Douady) a "cauliflower" and which is shown on Figure II.2.

Example II.2.3 (... with digressions). The rational fraction is

$$R(z) = \frac{-z^3 + 3z}{2}.$$

Julia indicated the shape of the curve separating the attraction basins of the points 1 and -1, at which $R'(1) = R'(-1) = 0$ (which are today called super-attracting points). The third finite fixed point is 0 and this is repelling. Julia presented an iterative construction leading to a curve like the one occurring in this example, with the property of having no tangent at any point; this curve is analogous to the one created by von Koch [1906][61] (the memoir contains the reference, but this is not the case of this Note, although the triangles used in the construction seem to have been inspired by von Koch): start from two equilateral triangles, append six other equilateral triangles which are eight times smaller, and so on[62], as in Figure II.3.

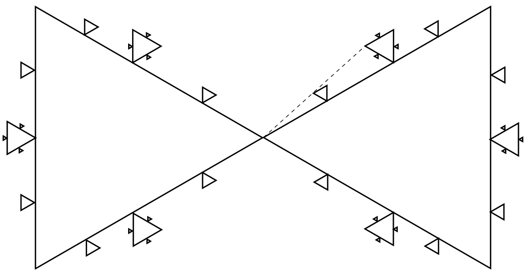

Fig. II.3.

[61] The totally discontinuous sets of iteration, the von Koch curve and soon the triadic Cantor set... not to mention that the Sierpinski triangle had appeared [1915], in the *Comptes rendus*, a short time before.

[62] One aside remark that we cannot resist including among these questions of priority: Paul Lévy says in his memoirs [1970] that he discovered von Koch's curve at age sixteen, in 1902 (four years before its publication [von Koch 1906]). See also Note 79 in Chapter VI.

It was definitely still necessary at the time to reiterate examples of what were considered, not that long ago, monstrosities. We are not so far from the oft-quoted "epistolary stupid remark of Hermite" [épistolaire bêtise d'Hermite],

> I turn away with dread and horror from this appalling plague, continuous functions with no derivatives[63]

(in 1893, see [Baillaud & Bourguet 1905b]) that Darboux was still repeating insistently around 1906, according to [Lebesgue 1991, p. 136]. In defence of Hermite, let us mention a more tactful way of expressing, his horror indeed, but also his "submissiveness", in a letter to du Bois-Reymond dated May 17th 1885 [Lampe 1916][64]:

> But I don't know what inertial force keeps me in a happier time, now far from us, in the golden age of analysis, when the existence of the derivative was never an issue. So it is not without a significant effort, a great trouble, I confess, that I obey the requirements, the hard necessities of the present time in terms of rigour. [Mais je ne sais quelle force d'inertie me retient dans un temps plus heureux et maintenant loin de nous, dans l'âge d'or de l'analyse, où jamais l'existence de la dérivée ne faisait une question, de sorte que ce n'est pas sans un sérieux effort, une grande gêne, je vous l'avouerai, que je me plie aux exigences, aux dures nécessités du temps présent en fait de rigueur.]

Julia was to point out later (the example takes eighteen pages of his memoir [1918f]), that the iteration of this rational fraction is related to using Newton's method to find the roots of the polynomial $z(z^2-1)$. The rational fraction R investigated here is conjugated (by $w = 1/z$) to

$$S(w) = \frac{2w^3}{3w^2-1} = w - \frac{w(w^2-1)}{3w^2-1}$$

given by Newton's method for finding the roots of $w(w^2-1)$. The fixed points ∞, 1 and -1 of R correspond to the fixed points 0, 1 and -1 of S (this is why they are super-attracting, as is always the case with Newton's method for a polynomial with simple roots). In principle, iterating the rational fraction R should "give" the roots of the polynomial. Yes, but... starting from which point? The attraction basins of 1 and -1 comprise the points whose iterates converge to these roots. One sees here (Figure II.4) that the convergence of Newton's method is more intricate than what the study of the degree-2 case could suggest (see Note 103 in Chapter I).

63 Je me détourne avec effroi et horreur de cette plaie lamentable des fonctions continues qui n'ont pas de dérivée

64 It is certainly not by pure chance that these letters written in the 1880s were published, in the middle of the war, in 1916 and in Germany: they are full of Hermite's expression of his faith in scientific co-operation rising above the divisions between France and Germany..

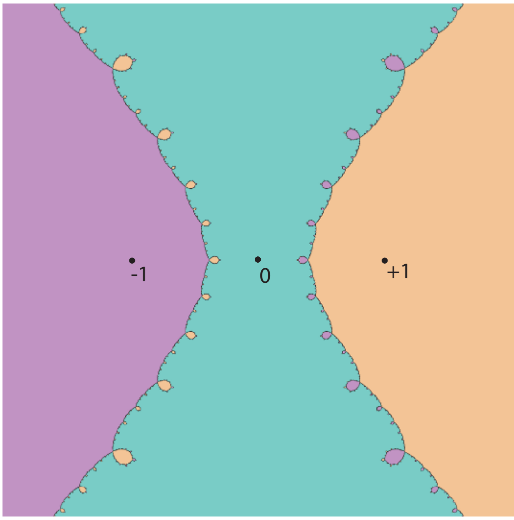

Fig. II.4. Newton's method for $z(z^2 - 1)$

Here is a further coincidence: during the same session of April 15$^{\text{th}}$, Georges Humbert presented a note [1918] in which he extended (to the case of non-definite quadratic forms) a theorem from "an extremely remarkable Note" [une Note extrêmement remarquable] of Fatou [1906a].

In May, Samuel Lattès became sick with a typhoid fever. On July 5$^{\text{th}}$, he died, aged 45[65].

Samuel Lattès (1873–1918)

[65] For more information on Samuel Lattès' life, death and work, see [Montel 1919 ; Buhl 1921].

July 8th

Meeting in secret committee, the Academy started to decide whom it would award. On this day, it decided to give the Francœur Prize to Paul Montel.

On July 18th, the second "battle of the Marne" began.

September

Alliance victories.

October 28th

The meeting of the Academicians in secret committee discussed the scientific organisations that it would be interesting to revive among the allies[66].

The secret committee meeting proceeded to hear the reports on the Prizes. It decided to award Fatou a 2 000 F Prize (from the Henri Becquerel Foundation) "for his work on the theory of series and the iteration of rational fractions" and to award Lattès part of the Gegner Prize (financial aid of 2 000 F for his family).

The members of the "Institut" (namely, the union of all the academies) who were kept in Lille during the occupation of the city by the German army[67] protested against the abuses.

It is touching to notice that, whether in wartime or not, the Academy of Sciences received its quota of letters from madmen—and on the same day, there was a perfumier of Perthuis (Vaucluse, Provence) who was anxious about a memoir on squaring the circle that he had sent a few weeks before.

---

[66] A meeting took place in London from October 9th to 11th, during which the scientific Academies of the allies made some resolutions about future relations with German scientists [Painlevé 1918c, p. 802]. It was nearly the armistice... and the scientific boycott of Germany had already been decided.

The October meeting was followed by another one, in Paris from November 26th to 29th and then a third, in Brussels, from July 18th to 28th (less than a month after the Versailles treaty had been signed, on June 28th), which created the "International Research council" (under the presidency of Picard). The conclusions of these meetings can be found in [EnsMath 1920] for the London and Paris meetings and in [EnsMath 1921] for that in Brussels. See what Picard himself said in his speech [1921] at the Strasbourg Congress.

[67] The city of Lille was occupied by the German army from October 12th 1914 to October 17th 1918. For a brief history of this terrible period, see [Pierrard 1992].

November 4th

The day before, Austria-Hungary had signed an armistice with the allies.

The meeting of the Academy of Sciences in secret committee had two parts. One of them was devoted to hearing the reports that would lead to the election of Foch (this was indeed Marshal Foch, the head of the "État-major général") as "free" Academician. The war was more than present. During the second part of the meeting, the Academicians proceeded to award the Prizes.

> M. Émile Picard gave the report whose conclusion was the awarding of the Prize to M. Julia, a "very honourable mention" being proposed for M. Lattès' work. Adopted[68].

Yet Julia's memoir contained a mistake which rendered it, let us say, incomplete (see Remark III.1.2) and which he corrected before its publication.

The commission which examined the memoirs (its composition was identical to that of the one which awarded the Bordin Prize to Julia one year earlier) was composed of Jordan, Appell, Painlevé, Hadamard, Boussinesq, Lecornu; the referees were Émile Picard and Georges Humbert. Notice that Gabriel Kœnigs was not a member, but it had been constituted before his election.

November 11th

At the very same moment, as Painlevé [1918c] would say three weeks later, as the armistice was signed, Marshal Foch was elected a member of the Academy of Sciences. This was the day when Madame Lattès thanked the Academy for the award given by it to her husband's work.

Digression (Jeanne Lattès). Samuel Lattès married, in 1910, one of his students at Montpellier, Jeanne Ferrier. They had a baby daughter (see [Buhl 1921]). Holding both a mathematics and a physics license, Jeanne Lattès taught in the boys' secondary school [lycée de garçons] in Tarbes. With a Carnegie grant, she became, in 1921, a collaborator of Marie Curie at the Radium Institute. In 1924, in a Note [Lacassagne & Lattès 1924][69] with Lacassagne, she invented a technique allowing the detection of an injected radioactive element and making its location more precise (according to [Latarjet 1979], taking into account the number of works which ensued from this Note, this proved to be one of the most important discoveries in biology in the 20th century). In 1926, she defended her thesis [Lattès 1926]. In 1930, after health alerts, she left the Radium Institute for the Henri Poincaré Institute (next

[68] M. Émile Picard dépose le rapport sur le Grand prix des Sciences mathématiques qui conclut à l'attribution du prix à M. Julia, une mention très honorable serait faite au travail de M. Lattès. Adopté.

[69] Either in testimony of her faithfulness to her husband's memory or in feminine modesty, either by the rules of the *Comptes rendus* or by herself, her Notes are signed Lattès (first name), J. Samuel (given name).

door) as an assistant in probability computations with Émile Borel, and she stayed there until her retirement in 1958. See the obituary [Latarjet 1979] and a beautiful picture of Jeanne Lattès, at work, in the garden of the Radium Institute, in [Devaux 2004].

November 18th

The President (Painlevé) welcomed Foch to the Academy of Sciences with these words:

> One hundred and twenty two years ago[70] the Academy of Sciences welcomed the victor of Arcole and Rivoli; today, to repeat the terse words you used yesterday to address the soldiers of the Republic, we greet in you, Marshal, the victor of the greatest battle in the History and the victorious defender of the noblest of all causes: liberty for the world[71]. [Painlevé 1918b]

On this session, there is an account written by René Garnier[72] fifty years later. René Garnier happened to come to the Academy of Sciences, the same day, to give a Note for Appell to transmit, and he remembered, with emotion, the solemn entrance of Foch, in uniform, surrounded by Painlevé and Picard.

December 2nd

This was the day of the annual public session. It was opened by the President's speech — fortunately, the President who had the long-awaited pleasure of celebrating the victory was a true politician. We have already quoted on page 29 the passage from this speech about the sacrifice of young scientists. After Picard's theories on the "German spirit" in the German science, it is reassuring to see the quality of the arguments increase [Painlevé 1918c, p. 800]:

> [...] because there does not exist French geometry or German geometry; there is only geometry. But, just as the same iron can be used either to harvest or to kill, inflexible human reason can be used either for the most generous ends or for the most appalling infamies. Scientific culture, fiercely pursuing a goal of immediate utility, of sordid wealth, or of oppressive domination, degrades the soul rather than elevating it above itself. It leads to a kind of scholarly barbarity, of organised cruelty, that, for its adepts, seems like a wild religion, all crimes of which are sacred and before which all infidels must kneel[73].

[70] One hundred and twenty one years before, Napoléon Bonaparte was elected to the Academy of Sciences ("5 nivôse an VI" or December 25th 1797).

[71] Il y a cent vingt-deux ans, l'Academie des sciences accueillait dans son sein le vainqueur d'Arcole et de Rivoli; aujourd'hui, pour reprendre les paroles lapidaires que vous adressiez hier aux soldats de la République, nous saluons en vous, monsieur le Maréchal, le vainqueur de la plus grande bataille de l'Histoire et le défenseur victorieux de la plus noble des causes: la liberté du monde.

[72] Garnier file, archives of the Academy of Sciences.

[73] [...] car il n'y a point une géométrie française et une géométrie allemande; il y a une géométrie. Mais, ainsi que le même fer peut servir à moissonner ou à tuer,

The point was not that the Germans thought in a different, Germanic way, but that their political leaders had given a bad role to science. He continued thanking the Americans and then considering the future of scientific relations [Painlevé 1918c, p. 802]:

> [...] It is not enough that Prussian militarism is brought down and rendered ineffectual: we need the German mentality to be transformed. As long as Germany will not really give up its bloody ideal of oppression, of plundering and of violence; as long as it will not become aware of and have horror for its crimes, no reconciliation will be possible, even for scientific collaboration, between Germany and mankind[74].

He then cited all the Academicians who died during the year, after which he returned to participation in the war effort and announced what the organisation of research in France should be, in a resolutely political (and modern) conclusion to the speech.

Next he invited the Permanent Secretary to read the list of winners. We have already quoted the beginning of the report on the Great Prize. This very long report (almost four pages), explained in detail the contributions of Lattès and Julia, it then recalled the "quarrel" with Fatou (which did not happen) in these terms:

> As his research progressed, M. Julia wrote his results in sealed envelopes, sent to the Academy; after the deposit of the last envelope, in December 1917, a known geometer, M. Fatou, to whom the theory of iteration already owed interesting and novel results, published almost the same results in the *Comptes rendus*, that he himself had obtained, also using the properties of the normal families of M. Montel: this is not the first time in the history of science that two top scientists have arrived simultaneously at the same discovery using the same approach[75].

l'inflexible raison humaine peut être employée aux fins les plus généreuses ou aux plus abominables forfaits. La culture scientifique, âprement poursuivie dans un but d'utilisation immédiate, de lucre sordide ou de domination oppressive, dégrade l'âme au lieu de l'élever au-dessus d'elle-même. Elle aboutit à une sorte de barbarie savante, de cruauté organisée qui prend pour ses adeptes l'aspect d'une religion sauvage, dont tous les crimes sont sacrés et devant qui les infidèles doivent plier les genoux.

74 [...] il ne suffit pas que le militarisme prussien soit abattu et momentanément réduit à l'impuissance: il faut que la mentalité allemande soit transformée. Tant que l'Allemagne n'aura pas renoncé au fond d'elle-même à son idéal sanglant d'oppression, de rapines et de violences; tant qu'elle n'aura pas pris conscience et horreur de ses crimes, il n'y aura pas de réconciliation possible, fût-ce pour une collaboration scientifique, entre elle et l'humanité.

75 Au fur et à mesure de sa recherche, M. Julia avait consigné ces résultats dans des plis cachetés, déposés à l'Académie; postérieurement au dépôt du dernier pli, et en décembre 1917, un géomètre connu, M. Fatou, auquel la théorie de l'itération devait déjà d'intéressants progrès dans une voie nouvelle, énonçait aux *Comptes rendus* la plus grande partie des mêmes résultats, qu'il avait obtenus de son côté

He ended with praise for the mathematical qualities of Julia's memoir and the decision to award him the Prize and to give a "very honourable" mention to Lattès' work[76]. As we said, Fatou was awarded 2 000 F from the Becquerel Foundation, the Francœur Prize being awarded to Montel "for his work on sequences of analytic functions". All our protagonists—that is, all our French protagonists—were thus rewarded.

The Academy received only three memoirs. Those of Julia and Lattès, and also that of the Italian mathematician Salvatore Pincherle (who was not named in Picard's report).

On Salvatore Pincherle

Salvatore Pincherle (1853–1936), the author of the third memoir, was one of the founders of functional analysis. He studied with, among others, Weierstrass. At the time of which we are speaking, he was professor of infinitesimal analysis in Bologna. He sent his memoir "On the iteration of the substitution $x^2 - a$" under the motto *La verità, che tante ci sublima,...* Dante[77]". His own contributions to the iteration problem constituted a very marginal part of his work. As the title indicates, his memoir consisted in the study of the transformations $R(z) = P(z)/Q(z)$ of degree 2, that reduce, by a Möbius transformation, to $R(z) = z^2 - a$ (these are the quadratic polynomials which would, much later, produce the Mandelbrot set). He considered only the case where a is a (strictly) positive real number, but he made a rather precise investigation of the "Julia set" (he did not refer to it by this name, of course, and he denoted it $\mathcal{Z}$) according to the value of a with respect to 2. The six parts of his memoir were called:

- I Preliminaries [Préliminaires],
- II $a > 2$,
- III $a = 2$,
- IV $a < 2$,

et par la même méthode, en utilisant, lui aussi, les propriétés des suites normales de M. Montel: ce n'est pas la première fois, dans l'histoire de la Science, qu'on aura vu deux savants de valeur arriver en même temps, par la même marche, à une même découverte.

[76] Let us quote once again Hadamard's report on Fatou:

> It [the Academy of Sciences] thereby obtained the first rate works of M. Julia and also of M. Lattès, which were a great success for French mathematical science, showing, right after the war, that it kept its vitality and its power.

[77] Prizes file, archives of the Academy of Sciences. In this kind of competition, the rule was to send an anonymous memoir, identified only by a motto, together with a second envelope, with the same motto, containing the name of the author. We do not know the mottoes used by Julia and Lattès.

– V Domains of regular or irregular convergence [Domaines de convergence régulière et irrégulière],

– VI Functional equations [Équations fonctionnelles].

It would be unfair to say that this work consisted only of examples—although Pincherle himself repeated, in his conclusion, that he had only investigated a very special case. These results were the subject of several Notes to the Italian Academies. See his list of publications [1925]. He was also the author of the article on functional equations and operations in the German *Encyclopädie* (and also in the French edition) which was a slightly outmoded paper, § 33 to 38 of which were devoted to the iteration problem.

Salvatore Pincherle was also the first director of the Italian mathematical Union in 1921 and the President of the International Congress of mathematicians of Bologna in 1928. This was not an easy task, see our page 209, as he had to give a speech there, of which, to remind the readers of the political context, we quote here a few lines [Pincherle 1929a]:

> The exceptional Man, whom it is Italy's fortune to have come to the fore to direct its destinity approved our conduct; the Congress had his support [...][78]

Mussolini was, indeed, one of the Presidents of the Congress.

Why did Fatou not compete?

We have seen that Fatou did not send a memoir. However, his two Notes [1917b; 1917c] were only the visible part of an iceberg that would be published in three parts in 1919–1920, in all about three hundred pages, showing that he intended to compete for the Great Prize.

The report of the Prize commission stated almost explicitly: had Fatou competed, the Academy would have shared the Prize. It is also certain that a Great Prize of mathematical sciences would have increased his chances of obtaining a position as a mathematician.

As for the reasons why he eventually decided not to compete, leaving the road clear for Julia, we can only make guesses. The 2 000 F of the Becquerel Foundation were given to him for "his work on the theory of series and the iteration of rational fractions"... which is very close to the subject of the Great Prize. Did somebody ask him not to compete? Did he withdraw from participation after Julia's Note [Julia 1917] of December? It was also considered that the fact that he did not make the war, which made more than one unfit for service feel guilty, could have held Fatou back. He was perhaps simply sick, as Hadamard says in the 1921 report we have already quoted several times:

> [his] state of health prevented him from taking part in due time in the competition.

[78] L'Homme exceptionnel que la fortune de l'Italie a fait surgir pour qu'il en dirige les destinées a approuvé notre ligne de conduite; le Congrès a eu son appui [...]

He nevertheless was able to send Notes published December 17th 1917 and February 4th 1918. Hence it could not have been a serious illness! The most probable explanation is that he decided not to compete because the competition did not suit him—as well as the approach taken by his competitor. It seems that the change he made to his titles, that we mentioned in note 55, points in the same direction. See the light shed on Fatou's personality in Chapter V.

December 23rd

Pierre Fatou

The prizes were announced and Fatou did not compete, but this did not prevent him from continuing to work. He published another Note [1918b][79] in which he proved that, when the complement of our set E', or F, has two connected components, the domains of attraction of two attracting fixed points, the curve which separates them has no tangent at any point (unless this is a circle). If one of the fixed points becomes[80] indifferent, that is, as Fatou pointed out[81], of multiplier $s = +1$, the curve acquires a cusp point, which is the only point at which it has a tangent.

This same year, Julia published two more *Comptes rendus* Notes [1918d ; 1918e], the latter immediately following [Fatou 1918b], but on quite different topics.

Communication?

An interesting question, to which we shall return (in § V.8), would be to know whether, during this period, Fatou and Julia spoke together of their mathematical work (a sub-question would be: where could they have met?).

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[79] While all the Notes on iteration of Fatou, Julia and Lattès in 1917–18, and that of Montel as well, were published without the name of the presenter, this one was presented by Georges Humbert.

[80] The word "becomes" [devient] is indeed in the text. Notice the dynamical aspect, the "variation of a parameter" facet of this way of thinking.

[81] It seems that the terminology "indifferent" was invented in this Note, in the restrictive sense $s = +1$ (today this is called a parabolic fixed point).

III

The memoirs

Julia's memoir was published, but not immediately as we shall see, as the paper [1918f] in what was still called "Jordan's Journal"[1]. Samuel Lattès' memoir was never published, yet, according to [Buhl 1921, p. 13], it was to be published in the *Annales de l'École normale supérieure* in 1921 or 1922 "thanks to the care of M. Émile Picard" (it seems that nobody knows today what happened to the manuscript[2]). Pincherle published his examples. Pierre Fatou divided his memoir into three parts, the three first chapters [1919b], the fourth and fifth [1920a], then the final ones [Fatou 1920b].

The publication of the prize-winning memoir [mémoire couronné][3], of a work which was also given by the Academy of Sciences a kind of priority certificate, in [Humbert 1917], goes without saying. The publication of Fatou's three hundred pages, simultaneously with (or right after) that of Julia's memoir, proves once again, not only the quality of his results, but also the esteem his mathematician colleagues had for his work: after all, many of the results in [Fatou 1919b ; 1920a ; b] were already labelled as "theorems of Julia".

How, by whom, was the choice of the journal which published Julia's paper (the *Journal de mathématiques pures et appliquées*) made? Perhaps this was the normal place to publish a prize-winning memoir, a "mémoire couronné".

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[1] The official name of the journal was *Journal de mathématiques pures et appliquées*, founded in 1836 and written by Liouville and thus called "Liouville's Journal" [Journal de Liouville]... then, as was the case for "Crelle's Journal", it was given the name of its editor, which was Jordan in 1918.

[2] The file of the Prizes of 1918 in the archives of the Academy of Sciences contains only the fifty-four small pages of Pincherle's manuscript—together with the statements that Georges Humbert borrowed Julia's manuscript on January 14th 1919 and Lattès' one on February 3rd 1919.

[3] The French expression "mémoire couronné", crowned memoir, should be put in quotation marks. We shall see (in Chapter VI) that our protagonists would still use it years later, "M. Julia's 'mémoire couronné'" and even, "my 'mémoire couronné'".

M. Audin, *Fatou, Julia, Montel: The Great Prize of Mathematical Sciences of 1918, and Beyond*, Lecture Notes in Mathematics 2014, DOI 10.1007/978-3-642-17854-2_4,

The choice of the *Bulletin de la Société mathématique de France* for Fatou was natural, given his involvement in the life of the SMF (see Chapter V). Notice also that Lebesgue[4], with whom Fatou worked for his thesis, and who had always proved his independence from Picard (see page 207), was in 1919 the President of the SMF, and that Montel, a close friend of Pierre Fatou, was the secretary (and as such published the *Bulletin*) in 1919 and 1920. The Academy of Sciences was no longer any more, at that time, the only authority to recognise the quality and the originality of a mathematical work.

Regarding the way Fatou published his memoir, we have two letters he wrote to Montel[5]:

> I willingly accept the solution you suggest, because I also believe that it is better to start this publication straight away. However I will try to give you Ch. VI and VII shortly, even if I must then give you some additional notes later on.
> I noticed that Julia has started the publication of his memoir on entire functions and that he has announced the one on iteration; the latter has been with the printer for almost a year; I think he had to revise it a lot, since in the past I mentioned to him some mistakes in his C.R. Notes. It is not very easy to finalise all this and he must have understood that, despite his haste to publish[6].

The second letter is dated January 6$^{\text{th}}$ 1920:

> I gave my memoir (Ch VI and VII) to your janitor on Friday[7]. I hope it is not lost.

[4] This is a good place to bemoan the quarrel between Borel and Lebesgue during the war, because it caused the end of their correspondence, so that the certainly sharp opinion Lebesgue must have expressed on our story has not managed to reach us.

[5] Montel collection, archives of the Academy of Sciences. The first letter is a card and dated 1919 (most of Fatou's letters were not dated, but Julia's paper he is speaking of is [Julia 1919c], the first of a series of three on entire functions, and it appeared in the April 1919 issue of the *Annales* of the ENS, his memoir on iteration had been with the printers for a year). Fatou made fun of Julia's impatience to publish in another letter to Montel (see page 149). Note also that Ritt, who was waiting for the publication of Julia's memoir to publish his own paper [1920] in *Transactions*, confirms in Note ‡ of the first page of this paper that [Julia 1918f] appeared one year late.

[6] J'accepte volontiers la solution que tu me proposes, car je crois aussi qu'il vaut mieux commencer dès maintenant cette publication. Je tâcherai cependant de te remettre les ch. VI et VII dans un délai assez rapproché, quitte à te donner plus tard quelques notes supplémentaires.

J'ai vu que Julia a commencé la publication de son mémoire sur les fonctions entières et qu'il annonce celui relatif à l'itération; voilà près d'un an que ce dernier est à l'impression; je pense qu'il a eu à le remanier pas mal, car je lui ai autrefois signalé des fautes qui apparaissaient dans ses notes des C.R. Tout cela n'est pas très facile à mettre au point et il dû s'en apercevoir malgré sa hâte de publier.

[7] Namely January 2$^{\text{nd}}$ 1920. For the complete text of these letters, see the Appendix.

Let us come to the description of the papers under consideration. It is remarkable that these mathematical papers are written in a way that shows so well the differences of approach, of personality and of behaviour between the two authors, even if, as we have mentioned, their studies meant that they had benefited from a common corpus of knowledge.

The first of Fatou's [1919b] three papers starts with giving a clear historical background of the subject, mentioning Kœnigs' work, and the relation with Picard's and Schottky's theorems. He explains he only used Montel's work:

> I then understood that the recent research on analytic functions with exceptional values allowed one to attack the problem in its whole generality; the properties of functions we alluded to are mainly due to MM. Landau and Schottky and are extensions of M. Picard's theorem on entire functions. M. P. Montel, applying to them the notion, due to him, of normal sequences of analytic functions, obtained elegant results that are very convenient for applications. In our research, we[8] have used exclusively propositions of M. Montel[9].

And it is only at the very end of the third page that he comes explicitly (and quite simply) to the question of priority:

> In this research, we have met several authors who obtained, at the same time and independently, results analogous to ours, in particular MM. Ritt, Lattès, and especially M. G. Julia, whose remarkable research is the object of a not yet published memoir[10], awarded a prize by the Academy of Sciences and the results of which were summarised by the author in various communications [Fatou 1919b, p. 163–164][11].

[8] At this point in his introduction, Fatou, who was using "I" [je], begins to use "we". This might show that this part, which is the one in which he has to place his work, unavoidably, with respect to Julia's, was rethought and rewritten.

[9] J'ai ensuite reconnu que les recherches récentes relatives aux fonctions analytiques qui admettent des valeurs exceptionnelles permettaient d'aborder le problème dans toute sa généralité; les propriétés des fonctions auxquelles nous faisons allusion sont dues principalement à MM. Landau et Schottky, et sont l'extension des théorèmes de M. Picard sur les fonctions entières. M. P. Montel, en leur appliquant la notion de suite normale de fonctions analytiques qui lui est due, est parvenu à des résultats élégants et d'un emploi très commode dans les applications. Nous avons fait exclusivement usage dans nos recherches des propositions de M. Montel.

[10] The article [Julia 1918f] appeared in the 1918 volume of the *Journal de mathématiques pures et appliquées*, [Fatou 1919b] in the 1919 volume of the *Bulletin de la Société mathématique de France*, but, as we have already mentioned, the publication of Julia's memoir was very late and they might have appeared simultaneously.

[11] Nous nous sommes rencontrés dans ces recherches avec plusieurs auteurs qui sont parvenus en même temps et indépendamment à des résultats analogues aux nôtres, notamment MM. Ritt, Lattès et surtout M. G. Julia, dont les remarquables recherches ont fait l'objet d'un Mémoire non encore publié, couronné par

The beginning of the "crowned memoir" is more aggressive. Julia does not lose any time either in the history of the subject or in paying tribute to his predecessors (it is even almost the opposite):

> The Memoir I submit to the judgement of the Academy is devoted to the study of the iteration of a rational fraction, $z_1 = \varphi(z)$, in the whole z plane. On this subject, there were only, except for the local study, two Notes of M. Fatou in the *Comptes rendus de l'Académie des sciences* of October 15$^{\text{th}}$ 1906 and May 21$^{\text{st}}$ 1917. The very interesting results he gives are only examples, using *particular* properties of the function he studies. It will be seen that these examples were the simplest one could imagine. I tried, in this Memoir, taking any fraction $\varphi(z)$, to give properties which are independent of certain of its distinctive characteristics. I wanted to go from the general to the particular, and I gave examples only to show the *realisation* of possibilities that were revealed to me by an analysis which is severe and sparing. The Academy will decide whether I have succeeded [Julia 1918f][12].

He proceeds to go straight to the point in presenting his results.

III.1 Julia's memoir

Here is a short summary of the paper [Julia 1918f]. It seems that this is the very text he sent on December 24$^{\text{th}}$ 1917, simply updated by footnotes and an "Additional note" at the end of the paper—even if Fatou (in the letter to Montel quoted above) thought that Julia was seriously revising his memoir. He explains what he intends to do:

> *a point z being given, all the consequents of which $z_1, z_2, \ldots, z_n, \ldots$ constitute a set e, what are the properties of the derived set e' of the set e*[13].

If $\zeta(z)$ is a point of accumulation of this sequence of iterates, where can z vary so that ζ is an analytic function? Examples of [Fatou 1906d] showed that

l'Académie des sciences, et dont les résultats ont été résumés par l'auteur dans diverses communications.

[12] Le Mémoire que je soumets au jugement de l'Académie est consacré à l'étude de l'itération d'une fraction rationnelle, $z_1 = \varphi(z)$, dans tout le plan des z. Il n'y avait sur ce sujet, hors l'étude locale, que deux Notes de M. Fatou aux *Comptes rendus de l'Académie des sciences* du 15 octobre 1906 et du 21 mai 1917. Les résultats qu'il donne, fort intéressants, ne sont que des exemples qui utilisent des propriétés *particulières* de la fonction à étudier. On verra que ces exemples étaient les plus simples qu'on pût imaginer. J'ai tâché, dans ce Mémoire, prenant une fraction $\varphi(z)$ quelconque, de donner des propriétés qui fussent indépendantes de telle ou telle de ses particularités. J'ai voulu descendre du général au particulier, et je n'ai donné des exemples que pour montrer la *réalisation* des possibilités qu'une analyse, stricte et avare d'hypothèses *a priori*, me révélait. L'Académie estimera si j'ai réussi.

[13] *un point z étant donné dont tous les conséquents $z_1, z_2, \ldots, z_n, \ldots$ forment un ensemble e, quelles sont les propriétés de l'ensemble e', dérivé de l'ensemble e.*

the set of singular points contains all the repelling fixed points of one of the iterates R^n. Thus Julia decided to investigate the set E of these repelling fixed points

$$E = \{z \in \mathbf{C} \mid \exists\, n \geq 1 \text{ with } R^n(z) = z \text{ and } |(R^n)'(z)| > 1\}\,.$$

After the introduction, the paper contains twenty-seven pages of preliminaries, recalling notions on point sets (it is Zoretti's paper [1912] in the *Encyclopédie* which is given by Julia as a reference) and in complex analysis, in which some variants of the Schwarz Lemma are given (in particular the case of a function from a disc to itself which fixes a point of the boundary, rather than an interior point as in Schwarz's case) that allowed him to recover the results of the Note [Fatou 1917b] on the iteration of functions that preserve a disc[14].

Thanks to Montel's work on normal sequences, in a neighbourhood of any point of E, the iterates R^n take all values except at most two[15]. Julia considers here the derived set E' of the set E of repelling fixed points. He states and proves a "fundamental theorem" on the way the iterated images of a neighbourhood of a repelling fixed point cover the plane: either there are two exceptional points (and R is conjugated to $(z-a)^k$ or to $1/(z-a)^k$, says Julia[16]), or there is one exceptional point (and R is conjugated to a polynomial), or (this is the general case) the images cover the whole plane.

Then Julia proves that the derived set E' contains E, that E' can be discontinuous, a linear continuum (with Fatou's examples $z^k/(z^k+2)$ in the first case and the z^2 example in the second case) or a superficial continuum[17] in which case $E' = \mathbf{C}$, but he has no example (he mentions the example of Lattès which appeared after he had submitted his memoir, in a footnote to page 106). He also proves that E' is perfect and shows

> how the *structure of* E' *is the same*[18] *in all its subsets*[19].

---

[14] Julia must have realised later that the result was interesting per se, so that he published it again in [Julia 1920a] (without mentioning it had already been published). Nevanlinna would write [1961, p. 356]:

> This work was about an important and elegant extension of the classical lemma of Schwarz that can now be found in all the modern books on the theory of functions under the name of Julia's lemma, [Il s'agissait dans ce travail d'une importante et élégante extension du lemme classique de Schwarz que l'on trouve maintenant dans tous les traités modernes de théorie des fonctions sous le titre de Lemme de Julia]

He himself generalised this lemma of Julia in his paper [1922]. See a statement on page 101.

[15] Here, we are close to Picard's theorem. See § I.5.

[16] It is thus conjugated to $z^{\pm k}$ as well.

[17] If the precise definition of a linear continuum is not completely clear (see Note 36 in Chapter II), a "superficial continuum" is here a subset with non-empty interior.

[18] A weak version of the self-similarity property that will later define fractals.

[19] comment *la structure de* E' *est la même dans toutes ses parties.*

He proves, finally, that the sequence R^n is normal outside E', and this gives him the expected result: outside E', ζ is an analytic function of z. This is the end of the first part.

In the next two parts, he investigates the "geometric properties" of E', in particular the question of the connectivity, both of E' and of its complement.

Assuming R or one of the iterates R^n has an attracting fixed point, he proves that each of these attracting fixed points[20] is in the interior of a convex region the boundary of which is included in E' and that he calls the "immediate domain of convergence" [domaine immédiat de convergence] of the point under consideration. Each of these regions contains a critical point of R^{-1}.

But, in general, the total domain of convergence of this point comprises infinitely many connected components and its boundary is the whole set E'.

He also proves, for the example of

$$R(z) = \frac{-z^3 + 3z}{2}$$

that the successive antecedents of most of the points converge to the points of E' (this is a general property, it would later be used to enable computers to construct Julia sets, see Remark III.1.1).

Julia proves that the complement of E' has one, two, or infinitely many connected components, and that, if it has two connected components, the limit of R^n on each of them is a constant (as in the case of Example I.4.1, namely that of the function $z \mapsto z^2$). This third part contains a long series of examples which are presented and motivated with pedagogic skill and explained in great detail. Julia studies in particular the problem of the convergence of Newton's method for the polynomials $f(z) = z(z^2 - 1)$ and $f(z) = z^3 - a^3$ (see Note 103 in Chapter I and page 81). If $k > 2$, the complement of E' for $R(z) = z - \dfrac{f(z)}{f'(z)}$ has infinitely many connected components; thus for at most one root of f, the attraction domain is connected, the attraction basins of the other roots all have infinitely many connected components. In Figure II.4, one sees that the attraction basin of the root 0 is connected, but those of the roots -1 and 1 are not.

Julia also constructs a polynomial,

$$R(z) = A\left(\frac{z^5}{5} - 2a\frac{z^4}{4} + a^2\frac{z^3}{3}\right)$$

(for suitable values of A and a), which is especially designed to have a set E' with infinitely many connected components which are not single points.

[20] Julia uses the terminology "circular group" [groupe circulaire] for an orbit and "limit circular group" [groupe circulaire limite] when the points of this orbit are attracting. It seems nevertheless that at that time the word group was no longer synonymous with "set".

In the case of the example of [Fatou 1906d] (our Example I.4.3) where

$$R(z) = \frac{z+z^2}{2},$$

he proves that the set E' is a Jordan curve[21].

In the fourth and final part of the memoir, Julia studies convergence in the case of an indifferent fixed point ζ with $R'(\zeta) = e^{i\theta}$ in the case where $\theta/2\pi \in \mathbf{Q}$ (the multiplier is a root of unity, this is a parabolic fixed point, in the modern terminology). As Leau[22] did [1897], he reduces to the case where $\theta = 0$ and he says that he will reprove Leau's result, using the version of the Schwarz Lemma he gave in the preliminaries. This result is, in Julia's words, that the point ζ

> can be surrounded by a small circle [disc] containing points the consequents of which tend to ζ and points the successive antecedents of which, defined by the branch of the inverse of φ [this is our R] which equals ζ at ζ, also tend to ζ[23].

What he proves is slightly more precise, but there is no other statement. He studies first the case where the expansion of R is $z+az^2+\cdots$ with $a \neq 0$, then the general case in which he constructs loops [des boucles] around ζ, which are invariant under R or R^{-1}. He investigates the examples of $R(z) = z + z^2$ and $R(z) = z + z^3$. See, in the summary of Fatou's memoirs below, the star drawn by Fatou.

He did not progress much, he says, in the case where θ is not commensurable with 2π, he just remarks that the fixed point could only be a centre, or be in E', but he did not find an example[24].

Remark III.1.1. The property noticed by Julia and noted here in reference to the example of $R(z) = (-z^3 + 3z)/2$ allows one to construct the points of E': one starts from $a \in E'$ and the set of its antecedents is dense in E'. In [Julia 1918f, p. 50], one can read the beautiful sentence:

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[21] In the article [Julia X] already mentioned above in reference to Humbert, Julia also says:

> I was happy enough to show to Jordan one of the first examples, not constructed purposely, of a Jordan curve which appears naturally as a boundary in a simple problem. [J'eus la joie de fournir à Jordan un des premiers exemples, non fabriqués exprès, de courbe de Jordan se présentant naturellement, comme frontière dans un problème simple.]

[22] Kœnigs' report on Léopold Leau's thesis, who can be considered as his student, a thesis defended in 1897, can be found in [Gispert 1991, p. 368]. Léopold Leau was also one of the founders of the Esperanto language.

[23] pouvait être entouré d'un petit cercle contenant des points dont les *conséquents* tendaient vers ζ et des points dont les *antécédents* successifs, définis par la branche de l'inverse de φ qui égale ζ en ζ, tendaient aussi vers ζ.

[24] Julia would soon commit an error on this subject (see § IV.4). He announces it (not as an error!) in a footnote on page 222.

> We had to see clearly the geometric properties of this set E', which is theoretically well-defined from E, the point by point construction of which requires the solution of a countable infinity of algebraic equations[25].

The method indeed allows a computer to draw the Julia set E'. One can marvel that our protagonists, who never saw a Julia or a Fatou set, were nevertheless able to understand all the complexity of the situation. It is remarkable that their published papers contain no figure depicting the set E'. We are no longer discussing the question of the usefulness of a computer to study iteration, but the amazing disappearance of the figures which the authors must have drawn. For instance, in the manuscripts contained in Julia's sealed envelopes, which are more or less drafts, there are some hand-drawn figures, which are very clear[26].

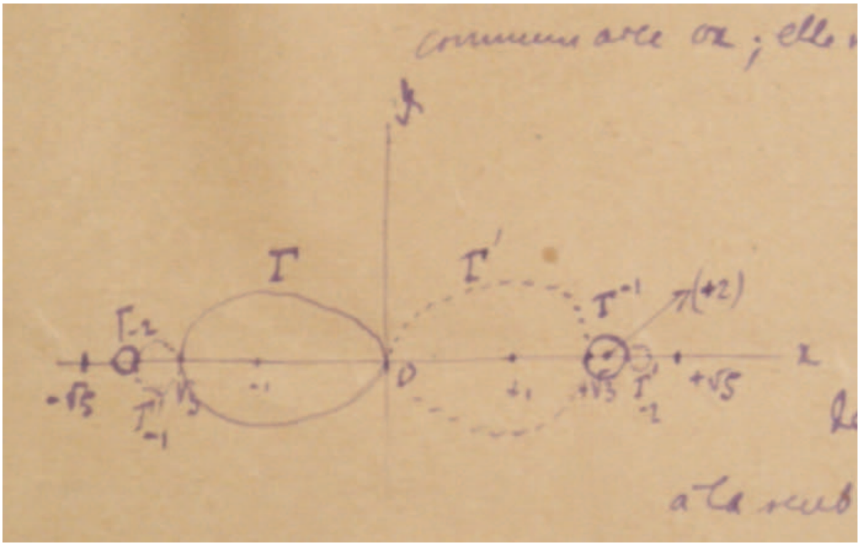

Fig. III.1. A Julia set... drawn by Julia

[25] Cet ensemble E' qui, théoriquement, est bien défini, à l'aide de E dont la construction point par point exige déjà la résolution d'une infinité dénombrable d'équations algébriques, il fallait voir d'une façon nette ses propriétés géométriques.

[26] People often think, and sometimes write (here [Chabert 1990, p. 360]):

> Unfortunately, *and with good reason* [emphasised by me], his text [Julia's memoir] contains no figures, we had to wait for computers before visualising [...] [Malheureusement, *et pour cause*, son texte ne comporte pas de figures, il a fallu attendre les ordinateurs pour visualiser [...]]

...but they are wrong! See also Remarks III.2.1 and IV.5.1.

One can see here (Figure III.1) a photograph of one of these figures. In the quoted passage of Julia, he was speaking of the rational fraction

$$R(z) = \frac{-z^3 + 3z}{2}$$

of Example II.2.3. This set E' is the image by $z \mapsto 1/z$ of the one shown in Figure II.4. Underneath (Figure III.2) is the same set E', produced by Arnaud Chéritat and his computer. As Julia explains in detail in this article, the fixed point ∞ is attracting, the other fixed points are 0 (repelling) and -1 and 1 (super-attracting, the function is odd so that there is a symmetry), the attraction basin of ∞ contains all the points such that $|z| > 3$, together with the imaginary axis (except 0), the two limit points of E' on the real axis are the points $\pm\sqrt{5}$ (they are marked on the hand-drawn figure and they constitute a repelling cycle of order 2), and the set E' is *a curve with everywhere dense multiple points* [*une courbe ayant des points multiples denses partout sur elle-même*].

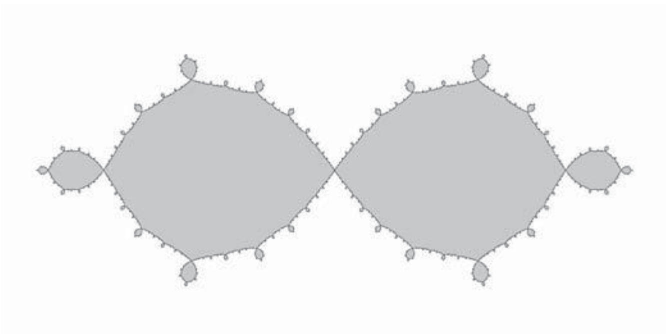

Fig. III.2.

The figure, which Julia needed to draw, is replaced, in his published paper, by the example looking like the famous example of von Koch [Julia 1918f, p. 170] already shown here in Figure II.3:

> It is hard to imagine precisely what this continuum E' can be. But it is not impossible to get an idea by a constructive procedure, which I will now explain and which will show that there is no impossibility for instance that a simply connected area $\mathcal{R}_\infty$ could be limited by a linear continuum E' having everywhere dense multiple points, a continuum that divides the plane into infinitely many regions, each of them being adjacent to $\mathcal{R}_\infty$ along its whole boundary; the boundary of every small region being a simple curve[27].

[27] Il est assez difficile de se représenter exactement ce que peut être ce continu E'. Mais il n'est pas impossible de s'en faire une idée à l'aide d'un processus constructif, que je vais maintenant exposer et qui montrera qu'il n'y a nulle impossibilité

Remark III.1.2 (Existence of repelling fixed points). In his memoir, Julia claims that all the rational fractions have repelling fixed points (hence E is not empty). He uses this fact, of course:

> In order to not reason in emptiness, it matters first to prove that E always contains points[28] [Julia 1918f, p. 84].

Unfortunately, this is not true. Julia does not say how he noticed his error. This is very likely one of the mistakes Fatou pointed out to him[29]. The polynomial $R(z) = z + z^2$, the two fixed points of which are ∞ (super-attracting) and 0 (indifferent[30]) provides a counter-example. In the Note [Fatou 1917c] where he announced all his results, Fatou wrote that this was the case "in general". The correct result is indeed:

Theorem. *If the algebraic equation $R(z) = z$ has a double root a, then $R'(a) = 1$. If all the roots are simple, then there is at least one at which $|R'(a)| > 1$.*

There is a proof in § III.2.

In an appendix to his memoir, Julia explains first that his result is wrong, then how the proofs can be modified so that they work nevertheless: more intricate arguments can be used to show that an "indifferent" fixed point a with $R'(a) = 1$ is the limit of a sequence u_n, where u_n is a repelling fixed point of R^n.

Strangely, this error is not mentioned in [Alexander 1994].

To conclude both this remark and the previous one, note that there is a difficult question here since, even if people did not know it at that time, the indifferent points a with $R'(a) = e^{i\theta}$ and $\theta/2\pi$ a diophantine number are *not* in E' (see Note 24 and § IV.4).

Remark III.1.3 (Functional equations). Julia decided that functional equations were "off the subject[31]":

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par exemple à ce qu'une aire $\mathcal{R}_\infty$, simplement connexe, soit limitée par un continu linéaire E' ayant des points multiples partout denses sur lui-même, continu qui divise le plan en une infinité de régions dont chacune touche $\mathcal{R}_\infty$ par toute sa frontière; la frontière de chaque petite région étant d'ailleurs une courbe simple.

28 Pour ne pas raisonner dans le vide, il importe d'abord de prouver que E contient toujours des points.

29 See Fatou's letter to Montel we have already quoted on page 92. See also on page 217 Montel's letter to Paul Lévy dated July 9th 1965 and the remark that goes with it.

30 In this case, the multiplier at 0 is 1. This indifferent point is said to be *parabolic*. At a parabolic point, the family R^n is not normal, these points will thus belong to the Julia set... as defined by Fatou.

31 This does not quite correspond to Fatou now giving his articles this title, but it is close.

> I could have applied the results I obtain here to the functional equations that are well-known since M. Kœnigs' and his successors' work. This was away from the main subject of this memoir, which is iteration itself: I might come back later to the unsettled questions[32].

As we said in § I.6, a solution of Schröder's equation (and, to some extend, of Abel's) is what we today call a "normal form" in a neighbourhood of the fixed points. For today's dynamicists, they clearly belong to any study of iteration. We shall see that Fatou was fully aware of this. Julia's remark that we have just quoted is puzzling with regard to the way he considered this question. He perhaps just wanted to say that he did not have enough time. He would come back to this in two papers, a little bit later, see [Julia 1923 ; 1924b][33]. In the second paper, he comes back to Schröder's and Abel's equations from a slightly different viewpoint.

Digression (Julia's lemma). Neither the memoir [Julia 1918f] nor the article [Julia 1920a] really contains a statement of Julia's lemma which is an extension of Schwarz's lemma. Here is the statement that can be found in Bieberbach's[34] [1927] book:

Lemma. *Let f be an analytic function on the unit disc $|z| < 1$ such that $|f(z)| < 1$ for all z. Assume that*

$$\lim_{z \to 1} f(z) = 1 \textit{ and } f'(1) = \alpha \in \mathbf{R}$$

(the limit being taken on the segments inside the disc). Then, on any disc of diameter d internally tangent to the unit disc, the function f takes its values in a disc, also internally tangent to the unit disc and of diameter $2\alpha d/(2 + d(\alpha - 1))$.

III.2 The (three) memoir(s) of Fatou

Let us repeat[35], the three memoirs of Fatou on iteration are called "On functional equations". The biggest difference with Julia's viewpoint is, it seems

[32] J'aurais pu aussi appliquer les résultats obtenus ici aux équations fonctionnelles bien connues depuis les travaux de M. Kœnigs et de ses successeurs. Cela sortait de l'objet propre de ce Mémoire qui est l'étude de l'itération elle-même: peut-être reviendrai-je ultérieurement sur les questions laissées ainsi en suspens.

[33] In [Julia 1923], he considers, rather, a generalisation of Poincaré's equation

$$G(R_1(z)) = R_2(G(z)) \text{ with } R_1(0) = 0 \text{ and } \left|R'(0)\right| > 1$$

(the case of Poincaré is when $R_1(z) = sz$). Julia shows that in general the points of the Julia set of R_1 are essential singularities of the solutions.

[34] We have chosen this formulation because it is almost contemporary with the original papers [Julia 1918f ; 1920a].

[35] Because we have already mentionned it, in Note 55 of Chapter II and on page 89.

to me, the following: Fatou *defined* the set $\mathcal{F}$ he would study[36] as the set of points at which the sequence of iterates of the function R is not normal. This global definition is of course the modern definition of the "Julia set" of the function R. It is also, he says, the derived set of the boundary points of the domains of convergence, a perfect set. Thanks to Montel's theorems, he proves that there are finitely many attracting and indifferent cycles. He also shows that "in very extensive cases" [dans des cas très étendus], the curves that limit the domains of convergence have no tangent at any point, or at a countable infinity of points. He proves all the results he announced in [Fatou 1917c][37]. Let us look more precisely at the way he progresses through his seven chapters.

The first chapter is devoted to proving the theorem about fixed points which is the subject of Remark III.1.2. Since the proof is very nice, it would be a shame not to give it here (in the same notation as in § I.4, even if it is not exactly the same as Fatou's).

The first claim is clear: if $R(z) - z = (z-a)^2 S(z)$, where a is not a pole of S, then $R'(a) = 1$. The second is proved with the help of an elementary but elegant argument. Let us first conjugate R with a Möbius transformation, so that ∞ is not a fixed point. Thus

$$R(z) = \frac{P(z)}{Q(z)} \text{ with } \deg Q = k \geq \deg P$$

and $R(z) = z$ is a degree-$(k+1)$ algebraic equation.

If the zeroes of $R(z) - z$ are simple, the same is true of the poles of its inverse $1/(R(z) - z)$. The residue at such a pole a is $1/(R'(a) - 1)$, so that

$$\frac{1}{R(z) - z} = \sum_{\{a \mid R(a) = a\}} \frac{A(a)}{z - a} \text{ with } A(a) = \frac{1}{R'(a) - 1}.$$

Expanding the two members in powers of $1/z$ and equating coefficients, we find that $\sum A(a) = -1$. We thus have

$$\sum_{\{a \mid R(a) = a\}} \frac{1}{R'(a) - 1} + 1 = 0.$$

This relation was stated in the Note [Fatou 1917c] which was the cause of Julia's ire. In the list of what Fatou announced there, although Julia already

[36] The F (for "frontier") of Note [Fatou 1917c] becomes an $\mathcal{F}$ in the memoirs.

[37] Let us quote again Hadamard's 1921 report:

> Not only should we not forget that these conquests find their origin in M. Fatou's initiative [namely, Note [Fatou 1906d]], but that the latter, whose state of health prevented him from taking part in due time in the competition, obtained slightly later, often by simpler methods, more decisive results than the previous ones.

knew it, one can read [Julia 1917]: "I gave the relation [...]" [Je donnais la relation [...]].

A geometric argument allows us to conclude: the map $u \mapsto 1/(1-u)$ sends the unit disc $|u| \leq 1$ onto the half-plane $\mathrm{Re}(v) \geq \frac{1}{2}$. If σ denotes the image of $R'(a)$, we thus have $\sum \sigma = 1$. The barycentre of the points σ is the point $1/(k+1)$ ($k+1$ is the degree of the algebraic equation $R(z) - z = 0$), which is outside this half-plane (since the degree k is at least 2), hence the same is true of at least one of the points σ and thus at least one of the $R'(a)$ is outside the unit disc.

He proves that there are infinitely many repelling or parabolic cycles and announces that there are only finitely many attracting cycles—but that he would prove it later in the paper (using normal families). As he says in his introduction [Fatou 1919b, p. 164],

> Although this result is much less precise than the one which follows from applying general theorems on sequences of analytic functions, the elementary way in which it is obtained deserves to get the attention, and the method could lead to other results[38].

He also proves that, in general[39], every point has infinitely many antecedents. He defines what he calls a completely invariant domain (that is, one such that $R(U) \subset U$ and $R^{-1}(U) \subset U$) and he proves the result on the critical points of R^{-1} that he had announced, namely the fact that every invariant simply connected domain contains $k-1$ critical points of R^{-1}.

He then considers the attracting and super-attracting fixed points (in the latter case, he uses the function given by Böttcher's theorem). He studies the invariant analytic curves in the neighbourhood of such a point. He eventually looks at the indifferent double points for which $R'(z) = 1$ (what we call parabolic fixed points)[40]. In the neighbourhood of such a fixed point, that we assume to be at 0, we have

$$R(z) = z + az^{n+1} + o(z^{n+1}) \text{ for some } n \geq 1 \text{ and some } a \neq 0.$$

[38] Bien que ce résultat soit beaucoup moins précis que celui qui résulte de l'application des théorèmes généraux sur les suites de fonctions analytiques, la manière élémentaire dont il est obtenu mérite d'attirer l'attention, et la méthode pourrait peut-être conduire à d'autres résultats.

[39] Except if R is conjugated to a polynomial (then ∞ has a unique antecedent) or to Az^k or A/z^k... a result about which Ritt would complain [1920], because it appeared in his Note [1918] and Fatou did not quote him. It seems that, although he adopted its terminology, Fatou did not quote [Ritt 1918].

[40] As we said (in Note 81 of Chapter II), it was probably Fatou who invented the word "indifferent", which has a more general meaning nowadays. The fixed points whose multiplier is a root of unity are also called parabolic. This case reduces to that of fixed points of multiplier 1. In the survey [Cremer 1925], which we shall have the opportunity to mention again, Cremer uses *"indifferent"* (without translating) when $|R'(z)| = 1$ and *"rational indifferent"* when $R'(z)$ is a root of unity.

Going back to Leau's results [1897] (already mentioned above), he proves the existence of a "star" relative to such a fixed point. It seems that people today prefer the flower[41] that this result contains: namely, inside an arbitrarily small disc centred at the fixed point, n simply connected open sets D_i $(1 \leq i \leq n)$ such that

$$R(\overline{D}_i) \subset D_i \cup \{0\} \text{ and } \bigcap_{m \geq 0} R^m(\overline{D}_i) = \{0\}$$

(these open sets are called "attracting") and n repelling open sets D'_i (analogous definition), alternating around 0 and such that their union is a punctured neighbourhood of 0. It must be said that the result proven here appears more clearly than in Julia's memoir.

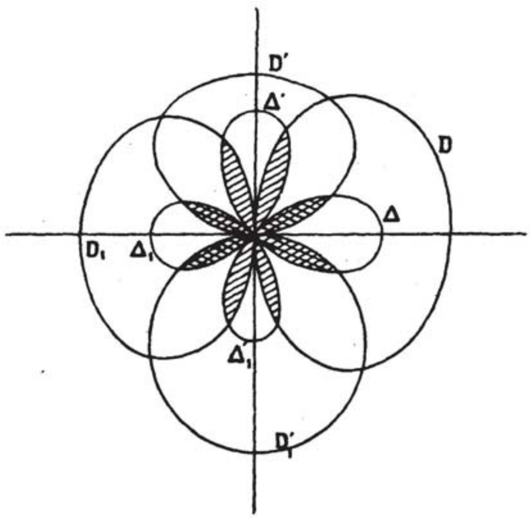

Fig. III.3. A star, drawn by Fatou in the case where $n = 2$

The star is cross-hatched in Fatou's figures, one of which is reproduced here (Figure III.3). The petals are the open sets D and D_1 (attracting), D' and D'_1 (repelling), on which $R^n(z)$ (respectively $R^{-n}(z)$) converges to 0. On the Δ's, the convergence is uniform. On the branches of the star, for instance $\Delta \cap D'$, R^n uniformly converges to 0 and R^{-n} converges to 0 (but not necessarily uniformly). The rays around which the petals of the flower are arranged are spanned by the v such that $nav^n = \mp 1$. If $nav^n = -1$, the

[41] It would be interesting to determine who introduced the terminology "flower" that can be found in modern works (flower is not used in [Leau 1897]). Fatou the astronomer named the star but not the flower!

direction is attracting[42] in the sense that, for any orbit $R^m(z)$ tending to 0,

$$\lim_{m\to+\infty} \sqrt[n]{m}\, R^m(z) = v \text{ for one of these directions } v.$$

Fatou then proves the existence and the properties of a solution of Abel's[43] equation. In each branch of the star, there is a uniform solution of Abel's equation

$$F(R(z)) = F(z) + a$$

such that

$$F(z) = \frac{1}{z} + O\Big(\log\left|\frac{1}{z}\right|\Big).$$

This is what is today called a Fatou coordinate (one should be careful with this terminology: the Fatou "coordinate" is defined on an attracting petal, not on a neighbourhood of the fixed point). He mentions the example of

$$R(z) = z + \frac{1}{z} \text{ in the neighbourhood of } \infty$$

(conjugated with $w/(1+w^2)$ at 0, hence an example with $n = 2$).

Like Julia, he says he knows very little about the case where $R'(z) = e^{i\theta}$ when θ is not commensurable with 2π. To complete this second chapter, he proves... that the derived set E' of the consequents of a point is not empty. He then gives a list of examples. It is remarkable that he considers families of examples with a parameter, $z \mapsto z^2 + a$ (for $a = 5$, $a = -5/4$), $z \mapsto z^d + a$ (for general a and d), for which he proves that, if $|a|$ is large enough, the "Julia" set is totally discontinuous[44].

Chapter III is devoted to substitutions with a fundamental circle, that is, as in the Note [Fatou 1917b], "those which map the inside and the circumference of a circle[45] to themselves and consequently also the outside of the

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[42] We use here the notation of [Milnor 2006a, § 10] rather than that of Fatou.

[43] Fatou presented his results on Abel's equation during the session of the SMF of November 26th 1919 [Fatou 1919a].

[44] The Mandelbrot set is here within reach. See § IV.5.a.

[45] Let us point out that the word "circle" [cercle] denotes what we today call a disc (what we call a circle was called a "circumference" [circonférence]). According to [Cartan 1979/80], it was Bourbaki who, much later, would impose the distinctive terminology ball/sphere, disc/circle.

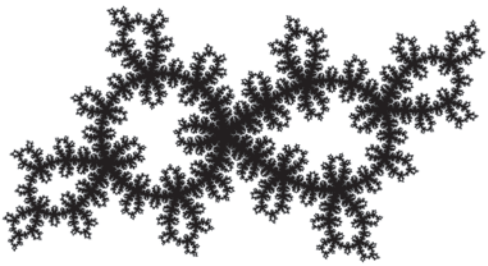
It is not enough that the transformation preserves the circle, as is the case of

$$R(z) = \mu z^2 \frac{z-3}{3z-1} \text{ with } \mu = \exp(2i\pi\theta) \text{ and } \theta = 0.386351337$$

for instance (see the figure, once again due to Arnaud Chéritat), which preserves the unit circle but also sends the interior of the disc onto the whole of $\mathbf{P}_1$. The Julia set of R is rather complicated.

circle[46]". Fatou also considers there the case of a substitution which preserves a circle and exchanges its inside and outside (then R^2 satisfies the previous properties). In this chapter, Fatou remarks in particular that, for the substitution $z \mapsto z^3$, starting from $z_0 = e^{i\theta_0}$ where θ_0 is a well-chosen element of the triadic Cantor set[47] the derived set E' of the z^{3^n} is also a Cantor set (in the circle)—this is probably the first explicit mention of the Cantor set (contained in a circle)[48] in this theory! It is a few lines later (page 243) that he asks the question of the measure of his set with empty interior.

He comes back to the examples $R(z) = z^2 + 5$, $R(z) = z^d + a$ ($|a|$ large) and also considers $z/(z^m + 2)$ and $z^2/(z^2 + 2)$ (an example of [Fatou 1906d], our Example I.4.2), in which the limit set has infinitely many connected components, and he proves that these components can be numbered (if one dares to put it like that) by a non-countable set.



[46] celles qui transforment respectivement en eux-mêmes l'intérieur et la circonférence d'un cercle et par conséquent aussi l'extérieur du cercle

[47] It seems that the very first appearance of what we today call a "Cantor set" was due to H. J. S. Smith [1874], who is better known for his work in number theory. The triadic Cantor set was born slightly later, in 1883. This was already available to French-speaking readers in [Cantor 1884].

[48] Let us avoid an anachronism: at the time we are discussing here, there existed a unique "Cantor set", this was the triadic set we have just mentioned, the set of real numbers between 0 and 1 an expansion in base-3 of which contains only the digits 0 and 2. Nowadays, a "Cantor" is any perfect totally discontinuous compact set—homeomorphic, like Cantor's original set, to the product space $\{0, 1\}^{\mathbf{N}}$. It would be a pity not to mention here the more or less contemporary construction by Louis Antoine (1888–1971) [1921], another "gueule cassée", of a Cantor set embedded in 3-dimensional space, "Antoine's necklace": this is a closed chain (a necklace) each link of which is again a closed chain, each link of which... a closed totally discontinuous set the complement of which is not simply connected. For more information on Louis Antoine, see the obituary devoted to him by his friend Gaston Julia [1971].

Up to this point, everything has been treated in an "elementary" way, namely without using the new technique of normal families. This brings us to the end of [Fatou 1919b].

Normal families show up in Chapter IV, where the perfect set $\mathcal{F}$ is defined and its main properties are investigated: the set $\mathcal{F}$ for R is the same as for R^n, it is completely invariant, all the points of the repelling cycles are in $\mathcal{F}$ together with all the indifferent fixed points with $R'(z) = e^{2i\pi p/q}$... $\mathcal{F}$ is perfect, it has the same structure in all its parts (if it contains a continuous curve, it is continuous, and so on), if $\mathcal{F}$ has non-empty interior, then it is the whole of $\mathbf{P}_1$ (Lattès' example), $\mathcal{F}$ is compact, if at every point of $\mathcal{F}$, $|R'(z)| > k$ (the degree of R), then $\mathcal{F}$ is totally discontinuous and has zero length[49], any point of $\mathcal{F}$ is a limit of periodic points, any point of $\mathcal{F}$ is a limit of antecedents of an arbitrary point in the plane, except at most two exceptional points (the property which, as we mentioned on page 96, can be used to construct Julia sets), $\mathcal{F}$ divides the plane into one, two (in this case the limit functions are constant), or infinitely many connected components. The question of the "accessibility" of the boundary points is raised. As announced in the first paper [Fatou 1919b], he shows that the number of attracting cycles is less than the number of critical points of R^{-1} (and the same is true of the parabolic points)[50].

Here is another question raised in this paper (E_c denotes the set of the consequents of the critical points of R^{-1} and E'_c is its derived set):

> I point out [...] the interest there would be in looking for necessary and sufficient conditions such that the set $\mathcal{F}$ varies continuously[51], from the point of view of the position of the points and from the point of view of the connectivity of the domains into which it divides the plane, when the coefficients of $R(z)$ vary. It seems, and it can be seen in some examples, that the discontinuity happens for the values of the coefficients for which $\mathcal{F}$ contains points of $E_c + E'_c$. [Fatou 1920a, p. 73][52]

In the next chapter, the domains of convergence and their connectivity properties are investigated. Another question, to which he thought the answer was negative, without being able to prove it, was: when $\mathcal{F}$ is everywhere

[49] The set $\mathcal{F}$ is the intersection of sets E_n like those Fatou considered in the Note [Fatou 1906d] (see page 49 and Figure I.3). The assumption on the multiplier guarantees that the sum of the lengths of the (rectifiable) curves bounding E_n tends to 0 when n tends to infinity.

[50] He noted, in passing, that any attracting periodic point is the limit of iterates of critical points of R^{-1}.

[51] Here, it is Hausdorff's distance and topology that would be needed. See § IV.2.

[52] Je signale [...] l'intérêt qu'il y aurait à rechercher les conditions nécessaires et suffisantes pour que l'ensemble $\mathcal{F}$ varie d'une manière continue, tant au point de vue de la position des points qu'au point de vue de la connexion des domaines dans lesquels il divise le plan lorsqu'on fait varier les coefficients de $R(z)$. Il paraît bien et l'on peut le constater sur des exemples que la discontinuité a lieu pour les valeurs des coefficients telles que $\mathcal{F}$ contient des points de $E_c + E'_c$.

discontinuous, can it contain some critical points of R^{-1}? He then studies a rather long list of examples,

$$R(z) = az + z^3, \text{ with } a \in [1,2], \quad R(z) = \frac{z}{2+z+z^2},$$

where $\mathcal{F}$ is perfect and discontinuous,

$$R(z) = \frac{1}{4\sqrt{2}}(z^4+1) + z,$$

where $\mathcal{F}$ contains infinitely many distinct continuums, as is the case for

$$R(z) = z(z+1)(z+2) \text{ and } R(z) = z(z^2-a) \text{ with } |a| > 1,$$

and $R(z) = (2z^2+1)/3z^2$ (what Newton's method gives for $z^3 - 1$), $R(z) = Kz - \frac{1}{z}$, $R(z) = z^2 - 1$...

This brings us to the end of the article [Fatou 1920a][53].

In Chapter VI, Fatou studies the (non-) analytic and (non-) differentiable properties of the curves limiting these domains. We have seen (Example I.4.3) that

$$R(z) = \frac{z+z^2}{2}$$

gives rise to an $\mathcal{F}$ which is a Jordan curve with no tangent at any point. This is the case for the boundary of the immediate basin of an attracting fixed point of R if it contains no limit of consequents of critical points of the function R^{-1}. In other cases, a curve with tangents at only a countable infinity of points only can be obtained (for example with $R(z) = z + z^2$).

He also proves that, if $\mathcal{F}$ contains an isolated arc of an analytic curve, then $\mathcal{F}$ is an arc of a circle (Julia points out in a footnote to page 114 of [Julia 1918f] that this result kills one of his remarks), but it may contain non-isolated analytic arcs: he gives the example of $z \mapsto cz - \frac{1}{z^3}$, where $\mathcal{F}$ contains for instance the whole real axis[54]. Eventually, these techniques give results on the uniform transcendent solutions of the functional equations of Schröder and Abel, showing in particular that Schröder's function is defined and analytic

[53] Here (that is, in the article [Fatou 1920a, p. 83]), Fatou uses in a proof, in passing, the "gap" [écart] between two curves C and C', the definition of which he recalls in a footnote, and which is $\max(d, d')$, where

$$d = \inf_{m \in C} d(m, C') \text{ et } d' = \inf_{m' \in C'} d(m', C).$$

This distance, which is indeed that of Hausdorff, is only a tool (in the investigation of the boundary of the immediate basin of an attracting fixed point) and does not seem to be operational for the continuity question asked above.

[54] In $\mathbf{P}_1$, lines are circles.

on the immediate domain of the fixed point under consideration—but highly singular on its boundary (all the points of which are accumulation points of its zeros). This brings us to the end of the third paper [Fatou 1920b].

Remark III.2.1 (Figures). Like those of Julia, Fatou's papers contain figures, for instance the picture of the star we have seen above (our Figure III.3), but there are also others. It is remarkable that, except for the figure constructed from triangles by Julia (Figure II.3 of Chapter II), neither of them chose to draw the sets E' or $\mathcal{F}$ they were considering—although, as we have seen, at least one figure was drawn in one of Julia's drafts (here Figure III.1). Even without having seen Fatou's drafts, it is hard to believe that he drew no pictures. He suggests, as Julia does, an iterative construction giving an idea of what the set $\mathcal{F}$ looks like, still in Example II.2.3, but starting from tangent circles rather than from triangles [Fatou 1920a, p. 86]:

> One can get an idea of the set $\mathcal{F}$ from the next example, which is analogous: consider two externally tangent circumferences; draw two other circumferences, externally tangent to the first two and completely exterior to one another; then draw four circumferences externally tangent respectively to the four circumferences already drawn, but exterior to each other and so on in such a way that the radii of these circumferences tend to zero and that each point of one of them is the limit of contact points; the set of all these circumferences and their limit points constitutes a curve with infinitely many double points and which is analogous to our $\mathcal{F}$; the circumferences must only be replaced by Jordan curves without double points which, as we shall see in the next chapter, have no tangent at any point[55].

This is a long sentence which is probably the description of a good picture drawn by the author on his drafts and that would not have been very hard to reproduce in a published paper... The figure that this passage of [Fatou 1920a] suggests looks like the one drawn by Norbert Steinmetz in [1993, p. 135], under the name of "synthetic Julia set" and that we show here (Figure III.4). This is the set $\mathcal{F}$ for $R(z) = az + z^3$ (with a real in $[1, 2]$). For $a = 3/2$, this is conjugated by a similarity to the $(-z^3 + 3z)/2$ of our Example II.2.3.

In other words, the four figures (II.3, III.1, III.2, and III.4) represent the same thing.

[55] On peut se faire une idée de l'ensemble $\mathcal{F}$ par l'exemple suivant qui est analogue: considérons deux circonférences tangentes entre elles extérieurement; traçons deux autres circonférences tangentes extérieurement aux deux premières et complètement extérieures l'une à l'autre; traçons ensuite quatre circonférences tangentes extérieurement respectivement aux quatre circonférences déjà tracées, mais extérieures les unes aux autres et ainsi de suite de manière que les rayons de ces circonférences successivement tracées tendent vers zéro et que chaque point de l'une d'elles soit limite de points de contacts; l'ensemble de toutes ces circonférences et de leurs points limites forme une courbe ayant une infinité de points doubles qui est analogue à notre $\mathcal{F}$; les circonférences doivent seulement être remplacées par des courbes de Jordan sans points doubles qui, comme nous le verrons au Chapitre suivant, n'ont de tangentes en aucun point.

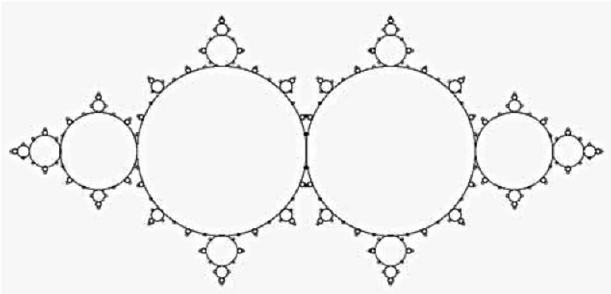


Fig. III.4.

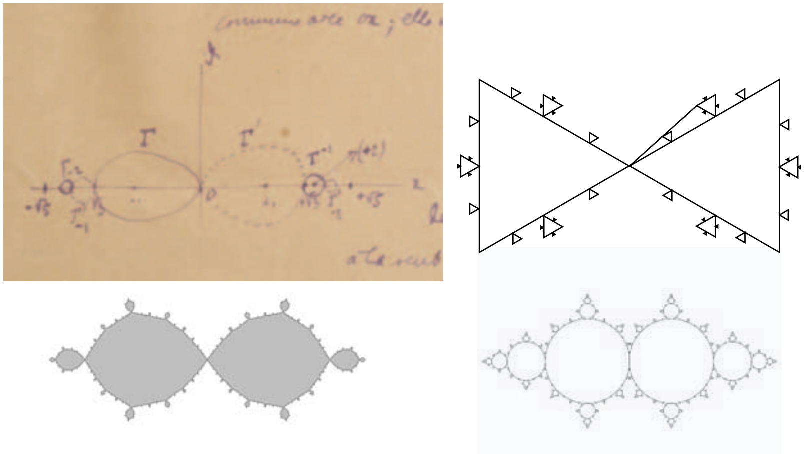


Fig. III.5. Four representations of the same Julia set

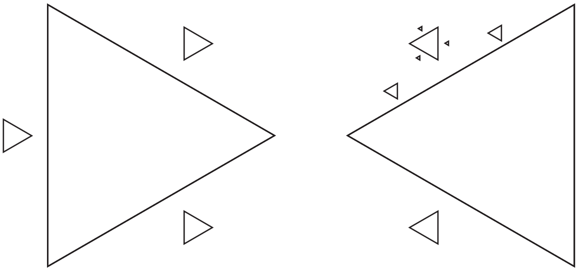


Fig. III.6. A picture by Cremer in 1925

The work of Julia and Fatou on iteration was the subject of a seminar at the University of Berlin[56] on which Cremer [1925] wrote a report. In this

[56] This was organised by Ehrard Schmidt and Ludwig Bieberbach, with the participation for instance of Brauer, Reidemeister and Hopf.

remarkable paper, he reproduced Julia's picture with the triangles... and he added a variant of it, which we reproduce here in Figure III.6. The Julia set, for instance of the polynomial $R(z) = z^3 + 1.55z^2$ looks like this. Regarding pictures, see also Remark IV.5.1.

III.3 Comments (in the first person)

It is completely obvious that the work we are discussing is first-rate, both Julia's and Fatou's. However, I do not hide it: I found the three articles of Fatou more pleasant and more lucid. This is one of the reasons why they happen to occupy more space in this text. It goes without saying that the way I read these texts is not the way they were read in their time—nor is it the way Ahlfors read them (see the Note on page 2).

In the unavoidable comparison—we are discussing only the way in which the papers are written—let us recall that playing against Julia were his youth (twenty-four in 1917) and thus his lack of mathematical maturity, and also the fact that he had to write very quickly to give his memoir in time. Let us remind the readers that Julia's memoir was submitted as early as December 24^{th} 1917, so that in particular, he could have used neither Lattès' examples nor Ritt's terminology. Too much self-confidence and some assumptions also led to errors, mathematical mistakes and errors of judgement.

I do not agree with the opinion expressed by Alexander [1994, p. 124] according to which Julia expresses himself more precisely than this. I sometimes find it very difficult to understand what exactly he is proving, namely to identify the statements, although some of them are indeed presented as theorems. For instance the nice generalisation of Schwarz's lemma mentioned above (the one which allows him to recover Fatou's results on rational fractions with a fundamental circle) is not expressed as a proposition[57]. The frequent use of italics in the text [Julia 1918f], to emphasise or to write certain statements, do not make it easier to read. It would not be wrong to say, taking Julia's other research papers into account, that he was not a modernist[58].

We shall see (in § V.4) that Lebesgue criticised the fact that Fatou, in his thesis, did not explain well enough what he was doing. He clearly made progress after that time. These texts of Fatou are very modern for 21^{th} century readers: he explains what he is doing, he extracts precise statements (written in italics), uses recent but efficient notation such as the small oh (o) of Landau[59], he adopted the attracting/repelling terminology (one of the advantages

[57] Neither in [Julia 1918f], nor even in [Julia 1920a]... this is why we had to call on Bieberbach (page 101).

[58] Hadamard concluded the report we have quoted by saying that Fatou was innovative.

[59] Perhaps not invented by Edmund Landau himself, the small oh (o) notation was popularised by him.

of not having submitted his manuscript at the end of 1917, Julia was encumbered with “regular convergence limit points” [points limites à convergence régulière] and so on), he uses notions of the future, such as the “curves that are identical from the *Analysis situs* point of view” [courbes identiques du point de vue de l’*Analysis situs*], and Fuchsian groups (which Julia indeed noticed were related to the subject but decided [1918f, p. 106] were not directly relevant[60]), mentions the triadic Cantor set, makes a beautiful remark on von Koch’s curve, which was purposely created to have no tangent at any point, unlike the examples of sets E' which show up naturally in mathematics, raises interesting questions, such as the arithmetical properties of the multipliers, the continuity of $\mathcal{F}$ with respect to R, and the question of the measure of these sets with empty interior [1919b, p. 243], a question which was only solved (in the case of a quadratic polynomial) very recently by Xavier Buff and Arnaud Chéritat [2005][61].

On the other hand, Fatou’s style is cordial, it is the style of a deep and thoughtful mathematician. We have already met some examples, we shall see some others. Here is another (to be compared with the quotation of Julia contained in Remark III.1.3). The beginning of the first memoir (all of them are called “On functional equations”) contains the beautiful sentence (and above all a beautiful adjective):

> But the first researches on these [functional] equations, unrelated and without clear method, all belong to the domain of delightful mathematics[62].

With Kœnigs, it started to become interesting mathematics, and the works that followed went in the same direction. It seems clear that Fatou saw, in the resolution of Schröder’s and Abel’s equations, more than a recreational question.

Still, it does not seem false to say that the mathematical corpus into which Julia’s work fits is complex analysis (à la Picard) while Fatou, by measure theory, hitches onto the train of the beginnings of general topology (see also the comments on the article [Fatou 1923a] in § V.5).

III.4 To summarise

In 1918, it was known that the set of points where the sequence R^n is not normal is also that of the accumulation points of the set of repelling periodic points.

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[60] Regarding this question, see also § IV.5.b.

[61] Another prize of the Academy of Sciences, the Prize Le Conte, rewarded their work on this question of Fatou in 2006.

[62] Mais les premières recherches relatives à ces équations, recherches sans lien entre elles et sans méthode bien définie, appartenaient pour la plupart au domaine des mathématiques délectables.

It was known that this set is empty only if R (assumed to be non-constant) is conjugated with a rotation ($z \mapsto \lambda z$ with $|\lambda| = 1$), and that it consists of a single point only if R has degree 1 but is not conjugated to a rotation ($z \mapsto \lambda z$ with $|\lambda| \neq 1$ or $z \mapsto z + b$).

Otherwise, that is, if R is not a Möbius transformation (namely if $k \geq 2$), it is infinite and contains all the points of the repelling cycles. It is then perfect, and it has empty interior if it is not the whole of $\mathbf{P}_1$. It is invariant under R. It can be discontinuous or a continuum, it can even have an uncountable infinity of connected components without being discontinuous[63], but it is homogeneous, in the sense that it is everywhere either totally discontinuous or a continuum.

It can be connected or not. If it contains an analytic arc, it is an arc of a circle (this includes segments of lines and circles). If it is not itself an arc of a circle, it contains no isolated arc of curve with a tangent at every point.

Its complement has one, two or infinitely many connected components (that would be called "Fatou components" and that are also called "stable" components by some authors). In the case where R is a polynomial, the bounded components are simply connected[64].

Fatou–Julia, again

The Fatou–Julia story does not actually stop with the publication of the memoirs of the two mathematicians. Both continued to work on the subject, each in his own groove of course, so that the *Comptes rendus* could publish

– on October 10th 1921, a Note of Fatou [1921a] in which he announced a whole series of results on permutable rational fractions... and two weeks later,

– on October 24th 1921, a Note of Julia [1921b] which ends with a footnote the contents of which will not surprise the readers:

> The results of paragraphs 2° to 5° of Part II were also obtained, independently of me, by M. P. Fatou, who has just explained them in a Note in the last *Comptes rendus*[65].

The question of permutable rational fractions is natural, for instance because two permutable rational fractions have the same "Julia set". Note that there would also be, after the publication of Julia's paper [1922], an article by

[63] This intermediate case does not occur for quadratic polynomials. Figure III.6 shows schematically an example of this kind (for a polynomial of degree 3).

[64] In the examples we have presented here, the Fatou set

– is connected in Examples I.4.2 and II.1.2,
– has two connected components in Examples I.4.1 and II.2.2,
– has infinitely many connected components in Examples II.1.1 and II.2.3.

[65] Les résultats des paragraphes 2° à 5° de la partie II ont été également obtenus, et indépendamment de moi, par M. P. Fatou, qui vient de les exposer dans une Note aux derniers *Comptes rendus*.

Ritt [1923], which continued his work by an algebraic method. Julia would say in his academic notice [Julia 1968, p. 7]:

> Some algebraic research by M. Ritt establishes that the solutions given in my work on the permutability of rational fractions are the only possible ones[66].

But let us come back to October 24th 1921: on the same day, two pages later, a second Note of Fatou [1921b] appears, in which he studies the group of algebraic substitutions $g : \mathbf{P}_1 \to \mathbf{P}_1$ such that $R^n \circ g = R^n$ (for some n). Resolutely modern as always, Fatou makes the terminology precise in a footnote it would be a shame not to quote:

> We say "finite or infinite group" instead of "group containing a finite or an infinite number of operations"; "linear group" instead of "finite or infinite group of first degree substitutions"[67].

[66] Des recherches algébriques de M. Ritt enfin établissent que les solutions fournies dans mon travail sur la permutabilité des fractions rationnelles sont les seules possibles.

[67] Nous disons "groupe fini ou infini" au lieu de "groupe contenant un nombre fini ou une infinité dénombrable d'opérations"; "groupe linéaire" au lieu de "groupe fini ou infini de substitutions du 1er degré".

IV

After Fatou and Julia

The question could be: why did a subject with so bright a beginning suddenly stop? Two very different mathematicians developed a passion and proved the same results... And it (almost) disappeared for sixty years, and then flowered again.

IV.1 Stop

It seems indeed that the subject stopped, at the very moment when it started to develop and, moreover, when suitable new tools were beginning to be set up.

Gaston Julia was a brilliant and very prolific mathematician[1]. It seems that what interested him most, in the subject of iteration (not mentioning the competitive aspect), was the relation with Picard's theorem (see § I.5 and, below, § IV.3). Oddly, he had no students who became interested in the subject, even though he was surrounded by young people, at the École polytechnique where he taught from 1919 as "répétiteur" (coach) of Hadamard, at the ENS where he was appointed "maître de conférences" (senior lecturer) as early as 1919, and at the Sorbonne where he taught from 1920 and where he participated in the setting-up, in the 30's, of the "séminaire Julia"[2] to which almost all the brilliant young mathematicians of the time contributed.

[1] The list of publications of Gaston Julia, at the beginning of his Complete Works, although not exhaustive (omitting, for example, [Julia 1943]) comprises 157 research articles (84 up until 1929), including a large number of *Comptes rendus* Notes. There are also 30 books (many of them are lecture notes written by members of the audience) or editions of books and about sixty other publications, in particular speeches.

[2] Regarding the Julia Seminar, see page 195 and our article [Audin 2010].

M. Audin, *Fatou, Julia, Montel: The Great Prize of Mathematical Sciences of 1918, and Beyond*, Lecture Notes in Mathematics 2014, DOI 10.1007/978-3-642-17854-2_5,

It seems, moreover, that Julia had no (doctoral) students at all at that time[3]. Dubreil thanked him for having "guided and stimulated his research"[4], but he was an algebraist. In his text on Julia's work on complex analysis [Julia 1968, p. 98], Hervé speaks of "all the questions on which M. Julia stimulated research by the beauty of his own results"[5], which is very vague, especially since he contents himself with mentioning Milloux's thesis [1924] which was a work on the values of a function near an essential singularity and not on iteration[6]. Julia might have been a "problem-killer" but he was probably not a "ideas-sower"[8]. This is not how he thought of himself. Indeed he said in 1961 [1970, p. 384]:

> Besides, I could never finish all this work, because, as soon it was published, other mathematicians began to work on it while, passing to different ideas, I developed and produced other results. Certainly, pioneer work suited me better than methodical and thorough exploitation of results[9].

Pierre Fatou, assistant-astronomer at the Paris Observatory, had to work a lot... as an assistant-astronomer, measuring visual double stars, he thus had less time than Julia and, above all, he had less contact with young mathematicians. It is therefore not very surprising that he had no students[10]. Besides, he died rather young. See Chapter V.

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[3] Much later, he was the advisor, in particular, of Jacques Dixmier. See the speech of the latter in [Julia 1970, p. 331].

[4] guidé et stimulé ses recherches

[5] toutes les questions sur lesquelles M. Julia stimula la recherche par la beauté de ses propres résultats

[6] The refinements of Picard's theorem already belonged to the past... It is remarkable that, during the very first meeting of what would become the reforming group of mathematics, on January 14th 1935, the first concrete negative suggestion, put forward by the analyst Mandelbrojt, was to deal "as little as possible with entire functions"[7] (digitised Bourbaki archives, document `delta_002`).

[8] We reproduce these two terms, [tombeur de problèmes, semeur d'idées] from a text written by Jean-Pierre Kahane for the Academy of Sciences [2006a] after Adrien Douady's death—Douady was, without a doubt, both.

[9] Je n'ai d'ailleurs jamais pu terminer tous ces travaux, parce que, aussitôt publiés, d'autres mathematiciens se sont mis à les travailler et à les exploiter, pendant que, passant à un autre ordre d'idées, je mûrissais et produisais d'autres résultats. Décidément, le travail du pionnier me convenait mieux que l'exploitation méthodique et complète des résultats.

[10] According to [Bloch 1931], Fatou looked for and found collaborators on the astronomy questions he was interested in—double stars, stability of orbits. Just before his death, he was planning programmes for training astronomers who would *not only* be calculators. We shall see the case of Rose Bonnet in Chapter V. Léon Bloch does not mention mathematical students.

IV.2 Hausdorff distance (1914) and dimension (1919)

This section is a continuation of the digression into general topology on page 18. Hausdorff distance is, as we have said, the proper language with which to ask Fatou's question mentioned on page 107, a question which remained dormant until the 1990s. This is a good opportunity to tackle the question of how mathematicians, especially in France, received Hausdorff's work.

Hausdorff distance

Let us first consider the notion of Hausdorff distance—a distance on a set of subsets (the compact subsets) of a metric space—already mentioned on page 107 and which is defined in [Hausdorff 1914, p. 293] (and will occupy more space in the section called *Mengenräume* in the 1927 edition of the same book).

Felix Hausdorff (1868–1942)

The introduction [Purkert 2002] in Hausdorff's Complete Works contains a detailed study, not only of the history of general topology before the publication of [Hausdorff 1914] (mentioned here on page 18), but also of the way this book was received (especially in Poland and in Russia). Whereas Hausdorff's influence on Bourbaki is known (and acknowledged by this author's associates), its spreading in France during the period following WW I is less clear. For instance, there was indeed a review of his book in French, but it was done by the Swiss journal *L'Enseignement mathématique*, and the book was not reported at all in the *Bulletin des sciences mathématiques* (edited by Picard).

Following the articles [Taylor 1982; 1985], the paper [Siegmund-Schultze 2005] gives some interesting hints on the way Hausdorff's book was received in France and, above all, on the relation between this book and Fréchet's work for instance. I will add a remark to this. The 1914 edition of Hausdorff's book does not seem to be present today in any French mathematical library[11] (there is a copy in the National Library of France (Bibliothèque nationale de France)). It happens of course that books disappear but, given the atmosphere described in § I.3, it is quite possible that this one never arrived[12]. Except perhaps in Strasbourg, which was a German city in 1914 and where, after all, Fréchet was from 1919. Fréchet confirms my impression, saying that he was aware of this book only after the war [1921, p. 367], and referring to

> works that came to my knowledge only very recently[13],

i.e. in Strasbourg. The fact that the book is no longer on the shelves of the Strasbourg library (and the 1927 neither is) suggests the possibility that it disappeared during the relocation of the library during WWII (from Strasbourg to Clermont-Ferrand in 1939, then back to Strasbourg in 1941, eventually to Tübingen, Oberwolfach and back once more) or worse, that it was destroyed during the annexation of Alsace to the *Reich* because its author was Jewish. In any case, even the 1935 edition of the IRMA library at Strasbourg was bought after WW II.

An example of the ignorance of Hausdorff's work, which is related to our main topic, is given by the book [Montel 1927] that we shall quote. Montel begins his exposition by recalling facts on point sets, for which he still refers to Jordan's book [1893], to the 1914 edition of that of de la Vallée Poussin [1921][14] and to that of Schoenflies [1913], a pre-war German refer-

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[11] The historical notes of Chapters I and II of the general topology book of Bourbaki quote the 1914 edition (and this, as early as its first edition in 1940), the 1927 edition was quoted by de Possel (who had the reputation of being a bookworm [rat de bibliothèque] [Dubreil 1982]) in his talk on general topology at the Julia Seminar on November 12th 1934. There is a copy of the 1927 edition in the IHP library (and this was indeed used, since we know for instance, thanks to the lending register, that it was borrowed by Élie Cartan in November 1933).

[12] If a book that was published in Germany in 1914 seemed to have never arrived in the French bookshops, it might have been possible, in 1919, to buy an older German book (see Note 53).

[13] travaux qui ne sont parvenus à ma connaissance que tout récemment

[14] The fourth edition of the book [de la Vallée Poussin 1921] begins with a foreword written by the author in December 1920 and which takes us back to the context of the beginning of our story:

ence. This book of Montel appeared in the same year as the second edition of that of Hausdorff and Montel must still not have known of it[15].

See in § IV.5.c a few answers that the use of Hausdorff's distance allows us to give to Fatou's question quoted on page 107.

Hausdorff dimension

Another notion which could have made the subject progress is that of Hausdorff dimension, which was invented by Hausdorff precisely in a paper [1919] published in 1919, but which did not have a big impact at that time in France: there was not much time between the moment when French mathematicians stopped boycotting German mathematicians and the moment when Nazi policy decided that Hausdorff could no longer work[16]...

Unlike in the case of Hausdorff distance, there are very few indications of how the article [Hausdorff 1919], in which Hausdorff defined the dimension which was given his name, was received in France. It is remarkable that neither the notion nor the article was mentioned in Bourbaki's topology book. It does not seem either that Louis Antoine, for instance, who, just after Fatou and Julia invented, for his thesis (which he defended in Strasbourg in 1921) one of the first fractals, ever used or mentioned it.

It would be interesting to study the diffusion of the notion of Hausdorff dimension and its use by mathematicians. Notice that Hausdorff himself mentions neither his article [1919] nor the notion in the second edition of his book, which appeared in 1927. Hausdorff dimension was known and used in France by Bouligand (who was a close relation of Fréchet), who reviewed various notions of dimension in his books [1932] and [1935]... these notions seem to be completely disconnected from Julia sets. Apparently, nobody had the idea, in the thirties, to compute the Hausdorff dimension of the Julia sets[17] (yet Bouligand mentions the work of Denjoy, Montel and Lebesgue, together with Antoine's thesis).

> The third edition of this book disappeared in the flames in August 1914, during the tragic events which marked the passing of the Germans through Louvain. [La troisième édition de cet Ouvrage a disparu dans les flammes en août 1914, lors des événements tragiques qui marquèrent le passage des Allemands à Louvain.]

[15] We do not know whether he owned a copy of this book, but we know for sure that Julia, for instance, had one, which is today on the shelves of the library of the CIRM.

[16] Felix Hausdorff, his wife and his sister-in-law committed suicide in January 1942 to avoid deportation. On the life of Felix Hausdorff, a likeable figure, a mathematician who was also, under the penname Paul Mongré, a writer, see [Segal 2003, p. 455–461].

[17] Gaston Julia owned copies of these books, which do not seem to have inspired him (Julia collection, library of the CIRM).

One could also look at the comments of S. D. Chatterji in [Hausdorff 2001, p. 44–54] and the relations with the notions of capacity in the book of Kahane and Salem [1963].

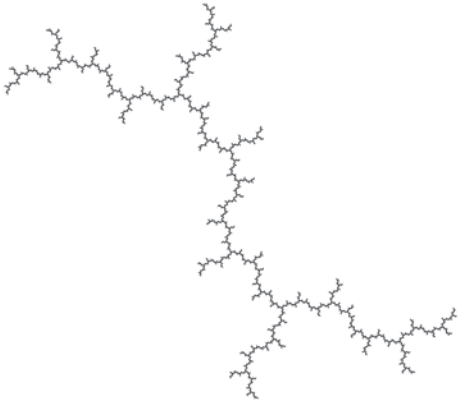

Fig. IV.1. A dendrite, the Julia set of $z^2 + i$

Computing the Hausdorff dimension of such and such a Julia set is nowadays a quite common activity. For instance, the Hausdorff dimension of the Julia set of $z \mapsto z^2 + i$ (a "dendrite", represented here in Figure IV.1, the Julia and Fatou sets are both connected) is about 1.2. We shall mention other appearances of this notion in § IV.5.c.

IV.3 Irregular points, *J*-points, *O*-points (1925–1927)

In 1927, the *Collection de monographies sur la théorie des fonctions* (chaired by Émile Borel, published by Gauthier-Villars) published a book by Paul Montel [1927], in which he reviewed the theory of normal families, which he created, and the applications of this theory, either due to him or due to others. This book is still a very beautiful introduction to the subject(s). In particular, Chapter VIII, which is devoted to iteration.

In the meantime (and even before the question of permutability which was the object of his Note [1921b] and his paper [1922]), Julia worked on results related to the famous theorems of Picard, which were the subject of seven *Comptes rendus* Notes (which appeared in Volume 168 and will not be quoted more precisely here), of three articles in the *Annales de l'École* *normale supérieure* [1919c ; 1920b ; 1921a], and of the Peccot course he gave in 1920[18]. This was written down by Paul Flamant[19] and became, a few years later, the book [Julia 1924a]. For convenience, we will quote this book rather than the original papers.

The famous theorems of Picard under consideration here are those we stated in § I.5. The refinements due to Julia [1924a] are the following, respectively for the first and the second:

Theorem. *If f is a non-constant entire function, there exists a ray such that, in any sufficiently small angle of which it is a bisector, the function takes all values except maybe one.*

Theorem. *If f has an essential singularity at z_0, for all $a \in \mathbf{C}$ (except maybe an exceptional value), the equation $f(z) = a$ has infinitely many solutions in a neighbourhood of z_0.*

For instance, as we have seen in § I.5, for the entire function $f(z) = e^z$, the rays in question are the two rays with origin at 0 of the imaginary axis. We shall see Montel giving a proof of the first of these two theorems on page 221.

In 1925, Alexandre Ostrowski[20] also wrote an article [1925] on the subject. For a family of functions $\mathcal{F}$, he introduced the notion of "J-points", those at which the family is not normal (in the case where $\mathcal{F} = \{R^n\}$, the J-points are those of the Julia set).

Ostrowski called "J-lines" the lines supporting the half-lines defined by Julia, a terminology which was also used by Montel in his book [1927, p. 82] two years later[21]. The results he proved use, like those of Julia and in an essential way, the theory of normal families.

[18] This was said again and again, many brilliant young mathematicians died during the war... this might be a reason why Julia could give two Peccot courses, in 1918 and in 1920 (even if he was not the first to do so, see page 233). This is Paul Lévy who gave that of 1919—a course related to Gateaux' work (see [Mazliak 2007]).

[19] Paul Flamant (1892–1940), who entered the ENS in 1913 as the top student, was also wounded and even taken prisoner during the war. He spent almost four years in captivity, after which he passed the agrégation (again as the top student) in 1919. After this period, his health was very fragile. At the beginning of WWII, in 1939, a professor at the University of Strasbourg and a captain in the reserves, he tried, neither to be declared unfit for service, nor to get a quiet post and ended up in the damp bunkers of the Maginot line. This is where he had a relapse, which led to his death. See [Sartre 1948].

[20] Alexander Ostrowski (1893–1986) was a mathematician of Ukrainian origin. He studied in Germany with Hensel, then with Hilbert and Landau, and he contributed to Hilbert's 13$^{\text{th}}$ problem in 1920 (he was then a *Privatdozent* in Göttingen). Then he was professor in Basel for almost all his life, including the Second World War.

[21] There is a review by Bieberbach of Ostrowski's paper in the *Jahrbuch über die Fortschritte der Mathematik* in which he mentions Julia points, and even Julia sequences, and so on.

There is no direct relation between J-points and J-lines, in the sense that the J-points are defined for a family of functions $\mathcal{F}$ and the J-lines for a single function f. But there are links, of course: for a function f with an essential singularity at infinity, the J-points of the sequence $f_n(z) = f(z/n)$ span the J-lines. See page 221.

Let us return to the definition of J-points. As we have said, the property of being *normal* is a local property. Hence, if a family $\mathcal{F}$ is not normal on the open set U, there must exist a point $z_0 \in U$ such that the family is not normal, on any neighbourhood of z_0. Hence, for every sufficiently small $r > 0$, there exists a sequence $S_r \subset \mathcal{F}$ such that no sub-sequence converges on the disc $D(z_0, r)$.

The sequence in question depends, *a priori* on the specific r, a subtlety which escaped Julia and which Ostrowski pointed out in his paper. Of course, if the family $\mathcal{F}$ is a sequence, as is the case in iteration problems, this remark is vacuous. Besides, as Ostrowski proved (but it is not absolutely trivial), one can indeed assume that the sequence does not depend on r.

Using a few quantifiers, the point z_0 is a J-point if

$$\forall\, r > 0, \quad \exists\, S_r \subset \mathcal{F}...$$

such that no sub-sequence of S_r converges uniformly in $D(z_0, r)$. In his book [1927, p. 37], Montel defines the notion of O-points: the point z_0 is an O-point if

$$\exists\, S \subset \mathcal{F}, \quad \forall\, r > 0...$$

no sub-sequence of S uniformly converges in the discs $D(z_0, r)$. And he gives Ostrowski's proof of the fact that J-points are indeed O-points (the converse is clear!). He thus decides to call these points "irregular"—as he had done for a long time when the family is a sequence [Montel 1907]. And he also remarks that, as he proved in his thesis [1907] in the case of a sequence, the irregular points of a family which is bounded at every point "constitute a perfect non-dense set, continuous and in one piece with the boundary of the domain" [forment un ensemble parfait non dense, continu et d'un seul tenant avec la frontière du domaine] [Montel 1907, p. 39].

There are two reasons why we have spent so long on this question:

– the J-objects foreshadowed, and even prepared the way for the name "Julia set",

– the discussion of irregular points *vs* J-points was far from over (see Chapter VI).

But let us return to iteration.

IV.4 The centre problem (1932–1942), Cremer points and Siegel discs

This concerns the local study of R near a fixed point (that we assume to be 0) at which $|R'(0)| = 1$. The question is whether R is linearisable, that is, whether there exists a neighbourhood of 0 on which R is conjugated with a linear map—here a rotation. One needs to solve Schröder's equation. It is said that 0 is a "centre". It is clear that a centre is a fixed point belonging to the Fatou set (while the parabolic fixed points belong to the Julia set).

Even before the work of Fatou and Julia, Kasner [1913] conjectured that the answer is yes, in other words that the linearisation is always possible[22]. Then Pfeiffer [1917] gave counter-examples, showing that there exists an analytic function

$$R(z) = sz + \cdots \text{ with } |s| = 1 \text{ and } s^n \neq 1$$

for which Schröder's equation has no solution[23] which is analytic at the origin with a non-zero derivative at that point (and he applied this result to the geometry problem posed by Kasner)[24].

Julia himself, in a Note [1919b] published later than his memoir, proved that, if $k = \deg R \geq 2$, the answer was always no, and that the indifferent fixed points are all in the Julia set:

> In my Note of January 28th 1918[25], I left the question of whether, for a rational substitution $z_1 = R(z)$, an invariant point $\zeta = R(\zeta)$ at which $R'(\zeta) = e^{i\theta}$, θ being incommensurable with 2π, could be a *centre*. Today I am able to settle this question negatively: *for a rational substitution, there is no centre, any point $\zeta = R(\zeta)$ where $|R'(\zeta)| = 1$ is a point of the perfect set I called E'.*[26]

Fatou [1920a, p. 58] clearly did not believe this and pointed out that this result of Julia was stated without proof[27]. Siegel would write [1942]:

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22 The question was formulated by Kasner in terms of the conformal invariants of the angle between two plane curves.

23 As Siegel explains very well at the beginning of his article [1942], there is always a formal solution, the question is whether it converges or not.

24 It is not obvious that Fatou and Julia knew these American works, that they do not quote.

25 This is the Note [Julia 1918a].

26 Dans ma note du 28 janvier 1918, j'ai laissé en suspens la question de savoir si, pour une substitution rationnelle $z_1 = R(z)$, un point invariant $\zeta = R(\zeta)$ où l'on aurait $R'(\zeta) = e^{i\theta}$, θ étant incommensurable à 2π, pouvait être un *centre*. Je suis en mesure aujourd'hui de trancher cette question par la négative: *pour une substitution rationnelle, il n'y a pas de centre, tout point $\zeta = R(\zeta)$ où $|R'(\zeta)| = 1$ est un point de l'ensemble parfait que j'ai appelé E'.*

27 This was another tactful understatement of Fatou: there was actually a summary of the proof (*eine Beweisandeutung*, Cremer [1927] would say) in [Julia 1919b], containing a gap, recognisable by a "it is easily proved that" [on démontre aisément que].

> *Julia tried to prove the erroneous hypothesis that the Schröder series is always divergent if $f(z) - a_1 z$ is a rational function not identically zero*[28].

Writing $R'(0) = e^{i\theta}$, we now know that the answer depends on the arithmetic properties of $\theta/2\pi$:

– If $\theta/2\pi \in \mathbf{Q}$, one reduces to the case where $\theta = 0$. In this case,

$$R(z) = z + \alpha z^{n+1} + \cdots \text{ with } \alpha \neq 0 \text{ for some } n \geq 2.$$

We have seen (in § III.2) that Leau's study [1897], completed by Fatou [1919b], described the dynamics in a neighbourhood of the fixed point with the help of a star (and a flower).

– Cremer[29] [1927 ; 1932] proved that, if[30]

$$\liminf_{n\to\infty} |s^n - 1|^{1/\log M_n(r)} = 0, \text{ where } M_n(r) = \sup_{|z|\leq r} |R^n(z)|,$$

then Schröder's equation has no solution in a neighbourhood of a and, in particular, the rational fraction is not linearisable. The corresponding s's are very well approximated by the roots of unity, or the θ's by the rationals. This subset of the unit circle has zero measure in this circle and, at these points, linearisation is not possible.

– If $\log |s^n - 1| = O(\log n)$, in other words if $\theta/2\pi \notin \mathbf{Q}$ is a diophantine irrational number, namely badly approximated by the rationals in the sense that there exist real positive numbers C and m such that, for all integers p and q ($q > 0$), one has

$$\left|\frac{\theta}{2\pi} - \frac{p}{q}\right| > \frac{C}{q^m}$$

(for instance if $\theta/2\pi$ is algebraic)[31], then the set of these irrationals has full measure and Siegel showed [1942] that the formal solution of Schröder's equation converges and thus that R is indeed linearisable. In particular, in this

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[28] The a_1 used by Siegel is our multiplier s.

[29] Hubert Cremer was a student of Bieberbach. We have already seen that he wrote the survey [Cremer 1925] and drew Figure III.6. Interesting information on this mathematician can be found in the book [Segal 2003].

[30] The condition is equivalent to the fact that the sequence $\sqrt[k^n]{1/|s^n - 1|}$ is not bounded when $n \to +\infty$.

[31] Liouville proved in 1884 that the algebraic numbers are diophantine (this is how he was able to give the very first examples of transcendental numbers) and that the exponent m can be chosen to be the degree of the algebraic number. Siegel himself improved this statement (one can choose $m = 2\sqrt{d} + \varepsilon$) in 1929: if x is algebraic of degree d, there exists a finite number of rationals p/q such that

$$\left|x - \frac{p}{q}\right| \leq \frac{1}{q^{2\sqrt{d}}}.$$

Later (in 1955), Roth would prove that one can even choose $m = 2 + \varepsilon$—a result which would earn him the Fields medal in 1958.

case, the fixed point is a centre (this is why this result is called the centre theorem); a disc on which R can be linearised is called a Siegel disc[32] and such a disc isolates the fixed point from the Julia set E', showing that the result announced by Julia in [1919b] was wrong.

– Much later, Rüssmann [1967], then Bruno [1971 ; 1972], would find a weaker arithmetic condition[33]; then Yoccoz [1995] would prove, in 1987, that this was optimal by showing non-linearisable cases[34].

Digression (Denjoy's theorems). At the time when Cremer published the articles [1927 ; 1932], the question of conjugacy to a rotation seems to have been rather fashionable: it was in 1932 that Arnaud Denjoy [1932] proved his famous theorem—the $\mathcal{C}^2$ diffeomorphisms of the circle with an irrational rotation number are conjugated with rotations. And, as we are speaking of iteration, and of Denjoy, let us point out that the latter is also the author, improving a proof of Julius Wolff [1926], of a theorem in iteration theory [Denjoy 1926], but in a disc (in this case, the hyperbolic case, the Julia set is empty). This is the so-called Denjoy-Wolff theorem—for a holomorphic transformation f of the unit disc which is not conjugated with a rotation, the $f^n(z)$ converge to a point of the circle which is independent of z. Regarding Arnaud Denjoy's life and work, see [Denjoy 1975].

Remark. Thanks to Yoccoz, we today know how to prove the existence of Siegel discs, a result that Fatou did not succeed in proving, using two results of this same Fatou, the radial extension theorem (related to the "Fatou lemma" of integration theory, see § V.5 for statements of these two results) and the fact that every point of an attracting cycle attracts a critical point of R^{-1} (see Note 50 in Chapter III).

Digression (About Carl Ludwig Siegel). Let us return to the First World War. Mobilised in 1917, Siegel could not bear the army. André Weil says in his book [1992, p. 127] that Siegel told him he deserted. The word might be exaggerated. However, according to Davenport [1985][35], Edmund Landau's

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32 Regarding Siegel discs and those other rotation domains, the Herman annuli, see Douady's talk [1987] at the Bourbaki seminar. Regarding the "centre problem of Siegel", see also [Pérez Marco 1992].

33 Write s as a continuous fraction and call p_n/q_n the n^{th} reduced fraction. Rüssmann and Bruno proved that if

$$\sum_n \frac{\log q_{n+1}}{q_n} < +\infty,$$

then R is linearisable.

34 The polynomial $z^2 + sz$, with multiplier $s = e^{i\theta}$ such that the series $\sum_n \log q_{n+1}/q_n$ diverges, is not linearisable in a neighbourhood of 0 [Yoccoz 1995].

35 The article [1985] "of Davenport" quoted here is actually an article by Mrs Davenport, based around letters written to her by her husband and notes he took in the course of discussions with Siegel during a visit to Göttingen in 1966.

father, who was a physician and a neighbour of the Siegels in Berlin, helped him to be cured and discharged.

He wrote a thesis, with Landau, in fact, as supervisor. He was professor in Frankfurt from 1922 in an exceptional atmosphere (see his moving description of this golden age in his article [1978])—until Hitler took power. He spent the year 1935 in Princeton, then went back to Frankfurt, left for Göttingen in 1938, then went to Princeton in 1940 where he stayed until 1951 when he returned to Göttingen.

Whether this information be relevant here or not, let us mention also that Siegel and Julia were quite friendly in the thirties. Siegel went to Paris in May 1937, he gave a series of three talks in Julia's Seminar, then Julia visited him in Frankfurt and they went together to the celebrations for the bicentenary of Göttingen[36]—a genuine Nazi ceremony. The incompatible political choices of Hasse and Siegel separated them[37]. The same reasons probably also separated Siegel and Julia. Siegel would nevertheless be one of the personalities belonging to the committee of the scientific jubilee of Julia in 1961—but not Hasse[38].

IV.5 Holomorphic dynamics

Following the results of Julia, Fatou and Siegel mentioned above, and starting in the 1980's, there began the development of what we today call complex (or holomorphic) dynamical systems[39].

It would of course not be absolutely correct to say that nobody worked on iteration anymore before the 1980's explosion. We shall mention in § IV.5.b the

[36] In a postcard to Hasse dated June 14th 1937, Julia wrote:

> I do not know the precise time I will arrive in Göttingen on June 25th. It depends on which train Siegel, with whom I will travel from Frankfurt to Göttingen, will take. Siegel will tell you the time, as he certainly knows it [...] [Je ne connais pas l'heure exacte à laquelle j'arriverai à Göttingen le 25 juin. Elle dépend du train que prendra Siegel, avec qui je dois faire le voyage de Francfort à Göttingen. Siegel vous indiquera l'heure en question, car il la connaît certainement [...]]

(Nachlaß Hasse, Göttingen University).

[37] Still from Davenport [1985], it happened that Siegel, in the 1920's, expressed, while Hasse was present, his horror of the war and the fact that his military duties almost killed him. Hasse then expressed the exactly opposite opinion: his military service in the Navy much improved his health and he had the best memories of the war.

[38] Regarding Hasse and Julia, see [Audin & Schappacher 2010].

[39] An indication of the recognition of the existence of a sub-subject is given by the subject classification of the *American mathematical society*. This classification contains, or contained:

work of Sullivan, on the Kleinian group side. Let us mention briefly here the results of Noel Baker[40] on the iteration of *entire* functions (a subject Fatou attacked in 1926 and in the article [Fatou 1926], in which he asked many difficult questions, see page 168). A complete history of iteration (which this book is not) should of course take all these works into account.

IV.5.a The Mandelbrot set (around 1980)

The idea now is to consider a family of rational fractions and the behaviour of their "Julia sets" and "Fatou components" as they vary in the family. In the example of the family of degree-2 polynomials, conjugated with polynomials $R(z) = z^2 + c$, $c \in \mathbf{C}$, the subset M of the space of parameters c for which the Julia set is connected is called the "Mandelbrot set". This is also the set of c such that the orbit of the critical point 0 of R, that is, the sequence $R^n(0)$, is bounded (as it is in Examples I.4.1 and II.1.1 but not in Example I.4.2 nor in Figure I.2). When c is outside the Mandelbrot set, the Julia set is totally discontinuous (and perfect).

In general, the Julia set is the victim of a severe dichotomy: either it is connected, or it has infinitely many connected components. In Chapter III, we saw Julia and Fatou[41] giving many examples of this. In the case of quadratic polynomials and of the Mandelbrot set considered here, the only possibility for the Julia set to have infinitely many components is when these components are points: the Julia set of $z \mapsto z^2 + c$ is

– connected if c is in the Mandelbrot set,
– a Cantor set otherwise.

In his famous book [1982, p. 182], Mandelbrot[42] attributes to Fatou in his 1906 Note [1906d] the idea of considering the transformation $z \mapsto \lambda z(1 - z)$ while letting the parameter λ vary in $\mathbf{R}$. Yet, there is nothing like that in this Note. Although, as we have seen, there are indeed examples with parameters in Fatou's memoirs.

For more information on the Mandelbrot set, a beautiful topic, but not the subject of this work, see [Tan 2000].

– from 1980, the sub-sub-subject 30D05, *Functional equations in the complex domain, iteration and composition of analytic functions* in category 37, *Functions of a complex variable*,

– from 1991, the sub-sub-subject 32H50, *Iteration problems* in category 32, *Several complex variables and analytic spaces*,

– from 1991 to 1999, the sub-sub-subject 58F23, *Holomorphic dynamics* in category 58, *Global analysis, analysis on manifolds*,

– and at last, since 2000, the sub-sub-subject 37F, *Complex dynamical systems*, in the new category 37, *Dynamical systems and ergodic theory.*

40 See a description of this work in [Rippon 2005].

41 This dichotomy was also sensed by Ritt [1918], see page 78.

42 Benoît Mandelbrot was a student at the École polytechnique where he took courses given by Paul Lévy and by Gaston Julia.

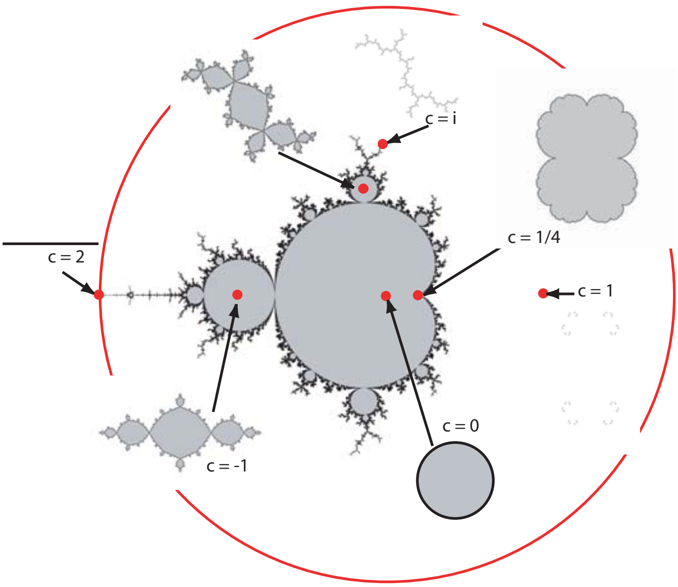

Fig. IV.2. The Mandelbrot set together with some associated Julia sets

IV.5.b From Kleinian groups to the question of wandering components ((1883–)1906–1980)

We have already mentioned, following Fatou, the relations, or more precisely the analogies, between the family R^n of iterates of a rational fraction R and that of the elements of a discrete subgroup of $\mathrm{PSL}(2;\mathbf{C})$ (see pages 52 and 112—and, for Poincaré's influence, Note 14 in Chapter I). The conclusion of the Note [1906d] said:

> So, simple functional equations lead to the introduction of uniform transcendents with discontinuous perfect sets of singularities, or non analytic cuts. Besides, one knows that analogous facts already appear in the theory of automorphic functions[43].

Again in 1921, it was another Fatou Note [1921d] which began with[44]:

> The theory of Kleinian groups and research on iteration of rational fractions have shown this fact that curves without tangents and everywhere

43 Ainsi, des équations fonctionnelles simples conduisent à introduire des transcendantes uniformes possédant des ensembles parfaits discontinus de singularités, ou des coupures non analytiques. On sait d'ailleurs que des faits analogues se présentent déjà dans la théorie des fonctions automorphes.

44 We shall also see that he mentioned singular lines of Kleinian functions in a letter to Montel, on page 264.

> discontinuous perfect sets[45] can show up, in particular as boundaries of existence domains of uniform functions, in problems with simple statements where all the data are analytic[46].

This property was noticed by Hadamard, a great connoisseur of Poincaré, in his 1921 report (see page 251), and was developed and explained in the book (of Fatou, let us repeat) [Appell et al. 1930][47].

Later, it was a theorem of Ahlfors, claiming that the quotient Ω/Γ of the normality set Ω by the group Γ is of finite type, which gave Sullivan the idea for his non-wandering theorem [Sullivan 1985], which asserts that every component of the Fatou set is, eventually, periodic (that is, a suitable iterate of this component is periodic): applying the rational fraction to a Fatou component never condemns it to wander for ever.

This question is sometimes attributed to Fatou and Julia (but we were unable to find where, and even if, one or the other explicitly asked it). Sullivan's proof uses quasi-conformal homeomorphisms (invented and used as early as in the thirties by Ahlfors and Teichmüller). Not content with closing this open problem, Sullivan gave a new proof of Ahlfors' theorem together with a whole dictionary relating iteration theory and Kleinian groups (see [Sullivan 1985]), the non-wandering theorem being a "translation" of Ahlfors' theorem[48].

Remark IV.5.1 (Figures (continuation)). It is remarkable that there exist so few images of limit sets drawn before the computer age. Poincaré, who did not balk at a beautiful figure, included no images of limit sets in his memoir [1883] on Kleinian groups. We have already quoted (Note 14 in Chapter I) some comments of Poincaré on the line L of the points where the Kleinian group action is not properly discontinuous—the set of points of non-normality, the limit set. Let us add here another sentence of Poincaré, coming from the same article:

> We see that the plane decomposes into two open sets D and D', the first one being covered by the polygon R_0 and its transforms, the second one by

[45] For these sets, it often happens that a curve containing them can have no tangent at any point of the set. [Pour ces ensembles il arrive souvent qu'une courbe qui les contient ne peut avoir de tangente en aucun point de l'ensemble.] Note of Pierre Fatou.

[46] La théorie des groupes kleinéens et les recherches relatives à l'itération des fonctions rationnelles ont mis en évidence ce fait que des courbes sans tangentes et des ensembles parfaits partout discontinus peuvent s'introduire, notamment comme frontières de domaines d'existence de fonctions uniformes, dans des problèmes à énoncés simples où toutes les données sont analytiques.

[47] The boundary in question in the above quotation would be called Φ in Fatou's book, certainly by analogy with the set F or $\mathfrak{F}$ of his works on iteration.

[48] A conjecture of Ahlfors, which succumbed recently to the the attacks of a series of mathematicians (see [Calegari & Gabai 2006] and the review of this paper in *Mathematical reviews*), asserts that the locus of non-normality of a finite type Kleinian group is either the whole of $\mathbf{P}_1$ or has zero measure. The theorem of Buff and Chéritat [2005] is thus one of the first discrepancies in this dictionary.

> the polygon R'_0 and its transforms. These domains are separated by a line L, if it can be called a line[49].

This was in 1883, ten years *before* Hermite expressed his dread and horror (page 81). It is possible that Poincaré did not dare to depict L, after all he hesitated to call it a line. It seems that the first published figures of limit sets appeared in 1897 in the book [Fricke & Klein 1897] (which contains numerous beautiful geometric pictures). We reproduce one of them here (Figure IV.3).

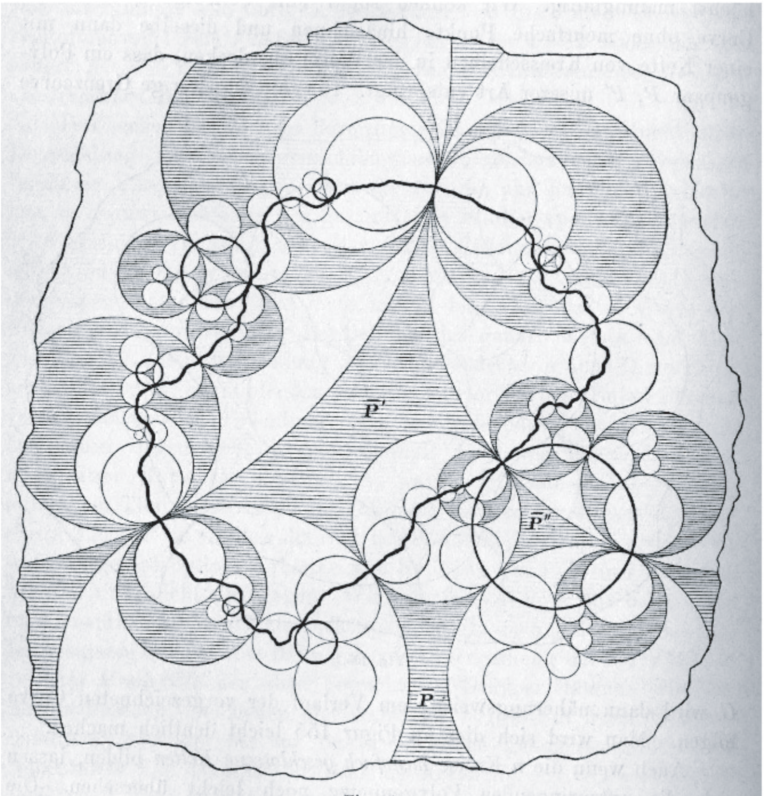

Fig. 156.

Fig. IV.3. Figure 156 of [Fricke & Klein 1897]...

This figure has a history. Firstly because it eventually appeared in Poincaré's Complete Works (!): when Garnier, in 1954, gave a talk on Poincaré's work, during the celebration of his centenary, he felt the need

[49] On voit que le plan se décompose en deux ouverts D et D', le premier est recouvert par le polygone R_0 et ses transformés, le second par le polygone R'_0 et ses transformés. Ces domaines sont séparés par une ligne L, si l'on peut appeler ça une ligne.

to show limit sets and thus included this figure and its neighbours in his article [Garnier 1954] (in Volume 11 of Poincaré's Works)[50]. Secondly because it was the object of a criticism by Mandelbrot [1983]:

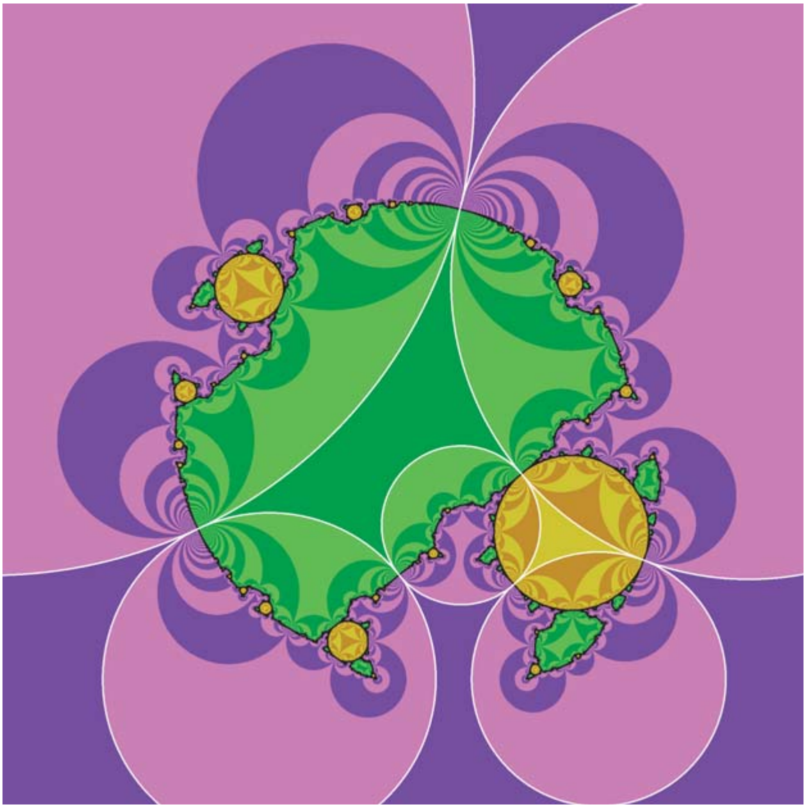

Fig. IV.4. ... and that drawn by Arnaud Chéritat and his computer

> It now seems that Figure 6 [our Figure IV.3] was drawn by a hapless draftsman (legend has it that he was an engineering student in Fricke's class), who had been instructed how to determine a few points of L exactly, and was then left to draw "some very wiggly and complicated curve" passing through these points. As Fricke did not know what to expect, the draftsman received no explicit directions.

It seems quite remarkable that Fricke, Klein, and the illustrator[51] were able, in 1897, to conceive of a curve containing so much of the true limit set

[50] The mention, in the French edition of the present book, that Garnier had not written that he had borrowed the figures from [Fricke & Klein 1897] was erroneous.

[51] Fricke, himself a student of Klein, was teaching engineering students in Braunschweig. One will have noticed that Mandelbrot is ironic about the hapless student

(the very wiggly curve together with the circles drawn in bold). What this figure missed is what would make the limit set invariant, that is, the small "copies" of the wiggly curve attached to the bold tangent circles, and so on.

The computer used by Mandelbrot's collaborator would, of course[52], do much better in 1983. Figure IV.4 was produced by Arnaud Chéritat, who did even better in 2008; it shows

– the five circles defining the inversions which generate the group (in white),

– the three polygons these circles define: a quadrilateral (dark green), a triangle (light orange) and a pentagon (dark purple),

– their respective images by the elements of the group, in green, orange and purple,

– the limit set (in black) is the boundary between the colours.

Let us add that Gaston Julia owned a copy of the book [Fricke & Klein 1897], whih he bought, according to the hand-written inscription "Gaston Julia, October 1919"[53], after he had written his memoir on iteration (this does not mean that he had not read the book before). He annotated a few pages of this book (kept in the library of the CIRM), but unfortunately did not leave us any comment (even in a margin) on Figure 156.

IV.5.c Does the Julia set depend continuously on the rational fraction?

Here is a reformulation of the question in [Fatou 1920a, p. 73] (see page 107). Consider a family

$$\mathcal{R} : X \times \mathbf{P}_1(\mathbf{C}) \longrightarrow \mathbf{P}_1(\mathbf{C})$$

of rational fractions (X is a complex algebraic variety, for instance $X = \mathbf{C}$ and $\mathcal{R}(t,z) = z^2 + t$). The question would be to investigate the bifurcation locus $B(\mathcal{R})$, the set of all t in the neighbourhood of which the Julia set $J(R_t)$ does not depend continuously on t. The continuity is defined by the Hausdorff distance on the compact subsets of $\mathbf{C}$.

---

and his teacher, but refrains from mentioning (and hence from criticising) Klein. While commenting on another figure in the same Fricke and Klein book, the authors of [Mumford et al. 2002] would write:

> Figure 6.4 [Figure 145 in [Fricke & Klein 1897]] was drawn by one of Klein's students, by a happy chance a gifted draftsman, whose beautiful pictures were not to be improved until the advent of modern computer graphics a century later.

This is an excellent opportunity to direct the readers to this beautiful book and to its no less beautiful figures.

[52] I confess I only understood the figure in [Mandelbrot 1983] (which shows the limit set without the circles which gave rise to it) when I saw Figure IV.4.

[53] This shows that it was possible, in Paris, in 1919, to buy a German book—and even a book by Klein—published before the war.

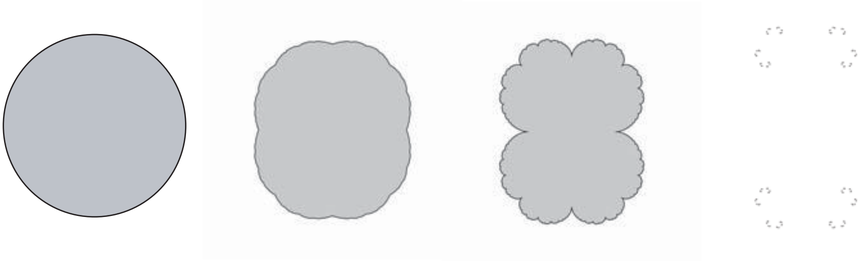

Fig. IV.5. The semicontinuity of J for $R(z) = z^2 + c$, $c \in \mathbf{R}$, $c \in [0, 1]$

Douady studies the question very precisely for the quadratic polynomials in a very accessible article [2009]. He proves there in particular that the map

$$R \longmapsto J(R)$$

is lower semicontinuous, while the analogue for the "filled Julia set" (the complement of the attraction basin of the point at infinity, the set in grey in the figures) is upper semicontinuous. These two properties play an important role in the construction by Buff and Chéritat of Julia sets of positive measure.

In the example where $\mathcal{R}(t, z) = z^2 + t$, $B(\mathcal{R})$ is the boundary of the Mandelbrot set (see § IV.5.a). It is known that $B(\mathcal{R})$ is, in general, closed, nowhere dense, and that this is also the set of t in the neighbourhood of which the number of attracting cycles of R_t is not locally constant. See [Douady 2009 ; McMullen 2000] and the references contained in these papers.

Beyond the computation of the Hausdorff dimension of such and such a Julia set, let us mention for instance that the Hausdorff dimension of the bifurcation set $B(\mathcal{R})$ as defined above is (when $B(\mathcal{R})$ is not empty) the dimension of the parameter space X. This is for instance 2 in the case of $z^2 + t$, see [McMullen 2000], and this is also the dimension of the Julia set $J(R_t)$ for t in a dense subset of $B(\mathcal{R})$.

V

On Pierre Fatou

My interest in this story came from the Gaston Julia side. Even if no biography has been devoted to him, there is much to read about his life. An image of his childhood and his brilliant schooling, then of his second birth (in his own words) after his 1915 war wound, an image which was polished by himself, is given by the many speeches he delivered or that were addressed to him through the 20^{th} century (see [Julia 1970]). What he said, what he did not say, where he said it, when he said it, who addressed him, who did not address him: independently of other less public sources, his official biography is quite well-informed (however, see also § VI.4).

It was reading the mathematical papers of Pierre Fatou, together with understanding the way he played his role in the Great Prize story in 1917–1918, that made me want to know more about this mathematician. As we have already noticed, he was a discreet but self-confident, self-confident but cordial, protagonist of the story of iteration.

There is very little public information on Pierre Fatou's biography[1]. The published sources used here are the two obituaries of Chazy [1932] and Bloch [1931], the entry [Nathan 1971] in the *Dictionary of scientific biography* together with the speech given by Mineur [1929] during the funeral of Pierre Fatou. The letters of Lebesgue [1991] to Borel, already quoted several times in this book, contain invaluable information. The information about the life of the Society in the *Bulletin de la Société mathématique de France* also proved very useful.

In addition to the published sources, we use here information from the registers of births and deaths (both for Pierre Fatou and Florian La Porte),

[1] David Aubin gave me some of these references together with a document that can be read below. Gladys Sérieyx sent me the article from the local newspaper *le Nouvelliste du Morbihan* containing Mineur's speech, a published source... but one that would have been hard to unearth without her help. There is much to learn about the context of Fatou's work at the Observatory in reading the thesis [Saint-Martin 2008].

M. Audin, *Fatou, Julia, Montel: The Great Prize of Mathematical Sciences of 1918, and Beyond*, Lecture Notes in Mathematics 2014, DOI 10.1007/978-3-642-17854-2_6,

the annual reports of the Paris Observatory, and some documents from the archives of the Academy of Sciences.

The generous and extensive help given by Pierre Fatou's family has been unvaluable. It is remarkable that this mathematician, who died childless already a long time ago (in 1929), has a large family of cousins and great-nephews, several of whom are very attached to the history of their family and in particular to that of "Pierre Fatou the astronomer", as they call him, who, as we shall see, was a singularity in this family. Of the information these relatives gave me, much belongs to oral family tradition, but there is also a text written by Robert Fatou (1895–1981), a nephew of Pierre Fatou who knew him[2]. He was preparing for the entrance exam for the École navale (Naval School) as a student of the lycée Saint-Louis and Pierre Fatou was his "correspondent":

> around the year 1911 [...] I was invited to come with him on Sundays, either to the concert, or for silent walks in the Paris streets. This gave me moderate pleasure[3]

(the young man was sixteen...). He saw him again several times around 1920 and had later the good idea of writing a few pages of memories concerning this uncle (which, unfortunately, he did not date). This moving document is one of the important sources of this chapter. Much can be learned also, very simply, by reading the announcement of the death of Pierre Fatou's mother (with thanks again to Gladys Sérieyx).

A clarification: as the readers will notice, this embryo of a biography was not designed to be an academic eulogy.

V.1 Childhood and youth of Fatou

Pierre Fatou was born[4] in Lorient on February 28$^{\text{th}}$ 1878. He was the fourth and last child of Prosper Ernest Fatou and Louise Eulalie Courbet, after Louis, born in 1867, Ernestine, born in 1869, and Jeanne. In Pierre Fatou's family, at that time and even today, many men were naval officers. The birth certificate

[2] The few pages of memories of Robert Fatou were gathered together with copies of published documents sent to him by Suzanne Débarbat in 1992–93, by a cousin of Pierre Fatou, Admiral Alain Fatou, who sent me this set of documents, fifteen years later.

Alain Fatou is a grandson of the physician uncle of Pierre Fatou who will be mentioned below.

[3] aux environs de l'année 1911 [...] j'étais invité à l'accompagner le dimanche, soit au concert, soit dans des promenades silencieuses à travers les rues de Paris. Ce qui me procurait un plaisir modéré

[4] According to a family tradition, all the Fatous are descended from a pharmacist from Quimper, Ambroise Fatou, born in the Palace of Versailles in 1786.

of "our" Fatou tells that his father, aged fifty-six[5], was a retired commander [capitaine de frégate], and that his mother, aged thirty-three, was a housewife [sans profession]. Pierre Fatou's parents married on July 25th 1866. A brother of his father was also a commander, another brother was pharmacist, and a third brother was a physician. The latter might have helped deliver the child; in any case he accompanied the father, as a witness, for the registration of the birth.

This photograph was taken around 1885. It shows an unidentified young man (a cousin?), Pierre Fatou's mother, his father Ernest Fatou, his elder brother Louis, and his sister Ernestine, sitting on chairs (from left to right), his sister Jeanne with her doll, and Pierre Fatou himself, sitting on the ground.

We know almost nothing about Pierre Fatou's childhood, except that

[5] According to the genealogical documents of the Fatou family, Prosper Ernest Fatou (who was called Ernest) was born in 1832, which made him forty-six in 1878. Probably a mistake of the town clerk.

> Early on his ears had the opportunity to become familiar with the classics, since one of his sisters and his brother played the piano, the first skilfully, the second fiercely[6]

as Robert Fatou, who was the son of this fierce pianist, writes.

Moreover, his father died in 1891, when Pierre Fatou was thirteen.

Pierre Fatou studied at the Lorient lycée, where he was, at sixteen, the pupil of the philosopher Alain, who was still called Émile Chartier and was then a young teacher (he was only ten years older than his pupil). It is not impossible that Fatou later kept in contact with his former teacher; in any case Alain was aware of the evolution of his career when he remembered in his book [1936, Chapter Lorient][7]:

> I had during those years a very simple and modest pupil, who was a mathematical genius. I taught him somehow or other the philosophy of these things; he understood all that easily and never made any objection. He taught me a lot about the ways of the eagle; since I had observed a baby eagle. He died a few years ago, a calculator at the Paris Observatory, and the author of a thesis which was understood maybe by two men in the world. He was called *Fatou*. According to my opinion, he died from mathematical boredom. And he is not the only one[8].

After the lycée at Lorient, Pierre Fatou went to Paris. He was a pupil at the Collège Stanislas from 1894 to 1897, in "elementary mathematics" and "special mathematics" [mathématiques élémentaires, mathématiques spéciales]. The list of prizewinners of the Collège Stanislas tells us that Pierre Fatou, a pupil in the "special mathematics" class, was awarded the first prize in mathematics in 1896–97. He was then a pupil, in the "blue section", of Charles Biehler, a renowned teacher[9].

---

[6] De bonne heure ses oreilles avaient pu se familiariser avec les classiques, car l'une de ses sœurs et son frère jouaient du piano, la première avec art, le second avec acharnement

[7] The philosopher Alain taught at Lorient from 1893 to 1900. The text quoted here was written in 1936. It anticipates slightly what follows in this story, as it suggests a first version of Pierre Fatou's death.

[8] J'eus en ces années-là un élève tout simple et modeste, qui était un génie mathématicien. Je lui enseignai vaille que vaille la philosophie de ces choses; il comprenait aisément tout cela et ne faisait jamais d'objection. Il m'apprit beaucoup sur ces méthodes d'aigle; car j'observai l'aiglon encore petit. Il est mort il y a quelques années calculateur à l'Observatoire de Paris, et auteur d'une thèse qui fut comprise peut-être de deux hommes dans le monde. Il s'appelait *Fatou*. Selon mon avis, il est mort de l'ennui mathématique. Et il n'est pas le seul.

[9] Charles Biehler was the author of a thesis he defended in 1879 and of several papers. At that time, the Collège Stanislas was run by Marianist priests and Biehler was himself a Marianist. He knew Hermite well (his thesis is dedicated "to my master M. Charles Hermite") and even Mittag-Leffler, as shown by his appearance here and there in their correspondence, for instance the mention of "Mr Biehler, the director of studies at the Collège Stanislas whom you know" [Mr.

Pierre Fatou at 17 in the uniform of the Collège Stanislas

Pierre Fatou was then a student at the lycée Saint-Louis (according to [Bloch 1931]) and he entered the ENS in 1898.

V.2 What do we know of Pierre Fatou?

One will find in this section what a sociologist would probably call a psycho-sociological habitus, a description of the background of Fatou's life and, from rather scant sources, an attempt to give an idea of his character, his tastes and his lifestyle.

Pierre Fatou came from a catholic Breton background, from a family in which there were (and there still are) sailors—several of them went through the Naval school [École navale], which means that they did at least some

Biehler, le directeur d'études au Collège Stanislas que vous connaissez] in a letter from Hermite to Mittag-Leffler dated February 18th 1892 [Dugac 1989]. Here is the conclusion of Hermite's report on Biehler's thesis [Gispert 1991, p. 329]:

> This thesis is the work of a teacher of special mathematics who is devoted to his pupils and who dedicates a distinguished analytical talent to educate them to a suitable standard in higner algebra of his time. [Cette thèse est l'œuvre d'un professeur de mathématiques spéciales dévoué à ses élèves, et qui consacre avec succès un talent distingué d'analyste à les initier dans la mesure convenable à l'algèbre supérieure de son époque.]

scientific study; we saw or we shall see that Picard, Julia, and even Montel were examiners for the entrance exam of this school—but also some physicians, pharmacists (from the "beginning", see Note 4), magistrates, up to a member of the Council of State... and even a general (in the Army), Georges Fatou, a first cousin of Fatou.

A middle-class family, then, maybe not very rich but cultured—we have seen that some used to play the piano in Pierre Fatou's family; we shall come back to his own musical tastes. However, Pierre Fatou, in such a family, looked like an (isolated) singularity and this for at least two reasons. Because he had not the physical abilities of these sailors; his nephew Robert Fatou confesses he felt

> some pity for this weak-looking uncle, who never played football, who maybe could not even swim[10].

Above all because he went through the ENS; one can imagine the respect, or the inferiority complex, that the pupils (or the former pupils) of the Naval School preparatory classes may have feel towards their schoolfellows of the "special mathematics" classes and above all towards those who entered the École normale supérieure. This is what his nephew says:

> Within a family in which no doubt many were unable to understand him, or even to suspect the extent of his intelligence, people were often surprised by his appearance, by the scruffiness both of his outfit and of his accommodation, by his complete indifference to social rituals as well as to the way he spoke, which was dull and even hesitant, as long as the subject did not touch upon the fibres of his curiosity.
> Of his passion for mathematics, of course, none of his relatives was able to speak. One would confidently attribute to him exceptional scientific knowledge, because of his degrees and of the brilliant way he passed through the École normale. His relatives felt proud of this, but some of them would have preferred that Pierre Fatou had a more modest, less abstract, thus more accessible, intellectual knowledge, under a less eccentric exterior more in accordance with the traditions of his background[11].

[10] un peu de pitié pour cet oncle d'apparence débile, n'ayant jamais joué au football, ne sachant peut-être même pas nager.

[11] Au sein d'une famille dont sans doute bien des membres étaient incapables de le comprendre, mais même de soupçonner l'étendue de son intelligence, on s'étonnait facilement de son apparence extérieure, du négligé de sa tenue vestimentaire ou de celle de son logement, de son indifférence totale aux rites mondains comme au ton de sa conversation, laquelle restait terne, lente et même hésitante, aussi longtemps que le sujet traité n'effleurait pas les fibres de sa curiosité.

De sa passion pour les mathématiques, bien entendu personne parmi ses proches n'était à même de l'entretenir. On lui accordait de confiance des connaissances scientifiques exceptionnelles, en raison de ses diplômes et des conditions brillantes de son passage à l'École normale. Les siens en étaient très flattés, mais certains eussent sans doute préféré que Pierre Fatou eût été nanti d'un bagage intellectuel plus modeste, moins abstrait, partant plus accessible, mais sous des dehors moins originaux plus conformes aux traditions de son milieu.

Besides, it is known that he was not appreciated by everybody, that some relatives did not find him very likeable, perhaps because of his both reserved and ironic character, which could be seen as haughtiness: Michel Fatou reported that he once left a party at some cousins', pretending to be freezing cold and declaring that the atmosphere was "icy!"

However, Pierre Fatou was close to his family, to his brother Admiral Louis Fatou, to his cousin General Georges Fatou, and his family remembered the fact that Georges and Pierre Fatou used to see and think highly of each other (according to Michel Fatou, a grandson of Georges Fatou). It is also remembered in the family, that he happened to take his cousin Marguerite (an aunt of Michel Fatou) to the opera, in the twenties.

His links with and his seeing of people in his family must have been particularly important because Pierre Fatou, who was single, lived for several years with his mother. The announcement of his mother's death says indeed that she died in her Paris residence, 172 boulevard Montparnasse, which is the address[12] that Pierre Fatou used to give to the SMF from 1906. Thus, Pierre Fatou lived with his mother (this could have been the reason why he moved in 1905) until her death in 1911. The nephew does not mention the mother (his grandmother) in his memories, but he probably arrived in Paris for the start of the school year in 1911, while she died on April 8$^{\text{th}}$. In the absence of other information, one can imagine that, after having married off her daughters, Pierre Fatou's mother came to Paris to take care of her single son, perhaps at a time when he was ill enough to have applied for sick leave in 1906 (see page 156).

Even after his mother's death, the single Pierre Fatou did not live absolutely alone since, as his nephew tells us, he owned a parrot (which perhaps arrived in Paris with Pierre Fatou's mother, as it was an inheritance from Fatou's father, who brought it back from a trip to Brazil) who sang a drinking song. Given that Ernest Fatou died in 1891, the parrot must have been a respectable age! Let us add that an oral family tradition has it that the room Fatou used as an office was locked and that the cleaning lady was forbidden to enter it.

In addition to his family, Pierre Fatou had friends, and even a group of friends, friendships which were forged during his years as a student at the École normale supérieure. As we have said (page 23), Pierre Fatou acquired at the ENS and at the Sorbonne a mathematical knowledge (courses of Borel, Picard, Goursat, Appell,...) which gave him a common scientific grounding with his fellow mathematicians (in particular Montel, Fréchet, Julia,...). But

[12] Near the Port-Royal metro station, practically at the corner with the avenue de l'Observatoire, very close to Fatou's workplace (as Suzanne Débarbat pointed out to me, an astronomer making night-time observations had to live in the area). Before that, in 1905, he lived a little bit farther from the Observatory, at 15 rue des Ursulines, as we also learn from the list of members of the SMF, which was published every year in the *Bulletin* of this society.

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172 boulevard Montparnasse
(December 26th 2007)

he also acquired, at the ENS, long-lasting friendships. There he met the (future) physicist Eugène Bloch, who entered the school one year before him, and his elder brother Léon Bloch, who became his friends[13]. We shall also quote[14] some letters from Fatou to Fréchet and above all to Montel.

Léon Bloch [1931] tells us that Fatou and he belonged to a group of friends, [une "cohorte"] which used to meet regularly, always in the same café (and from "ancient times"). The friends would discuss, for instance, politics[15]. It seems that the Bloch brothers belonged to a rather "left-wing" ideology (one can add here as many quotation marks as one wants: what was the left-wing in France, between say the Dreyfus affair and the Popular Front, is not quite clear); the same was perhaps true of Pierre Fatou. His activity as a "trade-unionist", which we shall mention below, is an indication which points in the same direction.

[13] Léon Bloch, who entered the ENS as a philosophy student in 1894 (the same year as Paul Langevin, Henri Lebesgue and Paul Montel), himself became a physicist. He wrote in particular an article reviewing the state of quantum physics at the end of the war in 1918. Assistant at the Faculty of sciences, he gave several series of talks on the structure of atoms and modern physics at the IHP from 1928 to 1933, some of which led to articles in Volumes 1 and 2 of the *Annales de l'Institut Henri Poincaré*.

[14] In fragments in this chapter, in their integrality in the Appendix.

[15] The fact that Fatou was interested in politics is confirmed by the memories of his nephew, according to whom he was also interested in sociology.

It should be noticed that this trade union activity took place among astronomers, a setting in which it is not quite obvious that Fatou made friends; except as the president or even as a simple member of the AAPSOF, whom he associated with at the Observatory, we do not know.

On the other hand, we shall see that Pierre Fatou had other friends, Léon Guillet and his family, for instance (see Note 170), but we know not how they met nor to which group we should link them (in the case of Léon Guillet, neither to mathematicians, nor to astronomers, nor to the family, nor to students of the ENS...).

Outside his professional (or semi-professional, at the SMF or at the AAPSOF) activities as a mathematician or as an astronomer, we know that Pierre Fatou was a great lover of music. We said that he used to go to the opera. He even travelled quite far:

> Musical passion led him, when he was quite young, to make the pilgrimage to Bayreuth[16] where he knew the historic evenings of the performance of the Ring Cycle. In those last years, he was little by little conquered by the tragic charm of Russian music, by the bright colour of the Spanish, by all this exoticism, so moving that it is sometimes unfair to our French music[17] [Bloch 1931].

Robert Fatou (the nephew) states:

> Although he never "learned" music, Pierre Fatou certainly found in it a huge pleasure [...] [He] had the gift of being able to read music and he savoured it. He used to buy the scores of his favourite composers, the German romantics first, then the Russians for whom he conceived a great admiration. [...] In concert, he used to follow the interpretation carefully on his staves and, back home, he "heard" again his favourite arias while reading the pages of his notebooks. Sometimes, when his memory was unfaithful, he helped himself with a piano or viola[18] to remember a melody. Then a smile would brighten his face, showing his deep satisfaction[19].

[16] According to his nephew Robert, Fatou even went several times to Bayreuth.

[17] La passion musicale l'avait conduit tout jeune à faire le pèlerinage de Bayreuth où il a connu les soirées historiques qui réalisèrent la Tétralogie. Dans ces dernières années, il avait été peu à peu gagné au charme tragique de la musique russe, à l'éclatante couleur des Espagnols, à tout cet exotisme si prenant qu'il rend parfois injuste pour notre musique française.

[18] Here are mentioned a piano and a viola. According to Henry Fatou, Pierre Fatou was an excellent violinist. Another relative tells that he played an instrument but he cannot say which. The trumpet one can see on the family photograph on page 137 does not seem to be a very serious option.

[19] Bien que n'ayant jamais "appris" la musique, Pierre Fatou y trouvait certainement un immense plaisir [...]. [Il] avait le don de lire la musique et il la savourait. Il achetait les partitions de ses auteurs favoris, les romantiques allemands d'abord, puis les Russes pour lesquels il avait conçu une très grande admiration. [...] Au concert, il suivait attentivement des yeux sur les portées l'exécution des morceaux et rentré chez lui il "entendait" à nouveau ses airs préférés en parcourant les pages

Michel Fatou's father used to say that Pierre Fatou was (or pretended he was?) the best French specialist in Russian music. His taste for music went with a (more surprising) taste for dance. This was not a secret for his friends:

> We all remember that, at the age of twenty, he could be met every evening at the concert or at the dance, and that part of his thesis was conceived at Bullier[20]. [Bloch 1931][21]

It seems that his family was not aware of this taste, since his nephew Robert was surprised, around 1920 (this was thus a long-lasting interest for Fatou, who was no longer twenty), to see his uncle taking tango lessons[22], in Luna Park, with a young lady who he carefully points out was chaperoned by her father and was only a teacher for Fatou. The family seems to have been very surprised when Robert Fatou mentioned this activity of "Pierre Fatou the astronomer". Pierre Fatou himself confirms this long lasting-taste at the end of a letter to his friend Montel[23]:

> December 6th 1920
> My dear Montel
>
> In the last few days, I have done a little work on certain uniform functions and I have written a note on it, which I first thought of giving to the C.R. But, since this exceeded noticeably the authorised limits, in order not to have to write another brief and then a detailed article, I have found it convenient to give you this note for the BSM[24], adding to it a few applications, details of computations and of proofs. Besides, this mode of exposition has in itself some advantages and one might have more chance of being read this way. Please be so kind as to give all this to Galbrun[25].
> If you go to the Société mathématique next Wednesday, which I cannot do myself, my evening being busy, I would be very grateful if you could read out

de ses cahiers. Parfois, si sa mémoire lui était infidèle, il s'aidait d'un piano ou d'un alto pour retrouver une mélodie. Alors un sourire éclairait son visage, témoignant de son intense satisfaction.

[20] The Bullier dance [Bal Bullier] was located in 174 boulevard du Montparnasse... literally the next-door for Fatou who, let us recall, lived at number 172.

[21] Nous nous rappelons tous qu'à l'âge de vingt ans, on le rencontrait chaque soir au concert ou au bal, et qu'une partie de sa thèse fut conçue à Bullier.

[22] In 1920, tango must have been "the" fashionable dance. A little remark: the love of music is rather traditional among mathematicians; this is well-known and nobody is surprised by it. Less well-known, but also classical today, is the interest in tango shown by many mathematicians.

[23] The original French version of the letters to Montel and/or Fréchet quoted in this chapter can be found in the Appendix.

[24] This is the *Bulletin* of the SMF, which was called the "société mathématique" at that time.

[25] Henri Galbrun defended in 1912 a thesis on finite difference equations; he would become later a specialist in actuarial mathematics. He was, in 1920, together with Montel, a secretary of the SMF.

> this note[26], of course without the final explanatory notes and even skipping some passages in § II if necessary. If this bothers you, or if you think that it will bother everybody, you can also avoid it.
> Yours sincerely
>
> [signed] P. Fatou
>
> Note for the history of mathematics[27]: § III was thought up at Bullier.
> Supplementary note: it is not useful to read the previous note to the SM.

Music and dance, but this was not all: Pierre Fatou was also an amateur photographer. This activity, Bloch says,

> gave us artistic souvenir photo albums of a surprising delicacy[28] [Bloch 1931, p. 57].

This opinion was not shared by his nephew[29]:

> Most of his innumerable pictures showed only the elegant ladies on the avenue du Bois or the young dressmakers on boulevard Saint-Michel who, neither the first nor the latter, were anything picturesque. As for the landscapes he brought from his holidays, they were usually of a lesser quality than the banal illustrated postcards[30].

Let us mention also the way he spent his holidays. He liked mountain trekking:

> Living ten months a year[31] a completely sedentary life, he was still curious of the world and he liked to travel. He often described to me his ex-

[26] The communication "On a problem in the general theory of analytic functions: at which set of points does an analytic function vanish on a singular line?" [Sur un problème de la théorie générale des fonctions analytiques: en quel ensemble de points une fonction analytique devient-elle nulle sur une ligne singulière?] was indeed put forward at the SMF, but on December 22nd 1920, which was not exactly "next Wednesday" (this would have been December 8th) but the following session. The section "Society life" of the *Bulletin* of the SMF tells that the corresponding paper appeared in the *Bulletin des sciences mathématiques* and gives the reference [Fatou 1921c].

[27] Eighty-eight years after the recipient Montel, Florence Greffe and I have probably been the first to read this note. Taking the risk of not maintaining the desired distance with the object of this study, I confess that I was moved by this message sent to us by Fatou.

[28] nous a valu des albums de souvenirs photo d'un art d'une délicatesse surprenante.

[29] It seems that the photo albums of Pierre Fatou have disappeared, so that we cannot form our own opinion.

[30] La plupart de ses innombrables clichés ne représentaient que les élégantes qui se pavanent sur l'avenue du Bois ou les trottins du boulevard Saint-Michel qui, pas plus les unes que les autres ne représentaient rien de pittoresque. Quant aux paysages recueillis au cours de ses vacances, ils restaient généralement d'une qualité inférieure à celle de banales cartes illustrées.

[31] The Paris astronomers had one and a half months holiday [Saint-Martin 2008, p. 218]. Moreover, Pierre Fatou often took observations during the summer months. He thus certainly did not take two months vacation every year.

> cursions in Tyrol or in the Italian lakes region, and his narrative always presented original viewpoints, even on the best-known subjects[32]

his nephew tells us, and Bloch confirms:

> he sometimes had incredible sudden bursts of energy, which enabled him to walk for ten hours in the Tyrol mountains or along the Brittany beaches[33]. [Bloch 1931]

For Fatou remained a Breton, who went every summer

> to ask of the air full of the scent of wrack the boost which would put him back on his feet for a year[34]. [Bloch 1931]

Fatou liked mathematics, astronomy, Brittany, photography, solitude and silence, dance, and, above all, music. A thoughtful, reserved, silent and discreet—even taciturn—man (Lebesgue [1991] said that Fatou was "rather withdrawn and shy" [assez renfermé et timide] (January 16$^{\text{th}}$ 1906)), not very healthy, perhaps neurasthenic, a dancer, musician, photographer, pipe smoker[35], Pierre Fatou seems to have been able to show plenty of imagination.

To complete this rather pointillist portrait[36] of Pierre Fatou, let us quote again the obituary [Bloch 1931] devoted to him by his friend Léon Bloch, a kind of "intellectual biography", to which we shall add some additional information.

> Cordial and discreet, he would sit down without disturbing anybody, in the heart of our group, and he would not interrupt the conversation for any reason. Whether we spoke of art, science, politics, literature or music, he would keep an obstinate silence, which was neither disdainful nor distant, but in which a subtle neighbour might have seen some reserve. Should we ask for his opinion, he would let us wait for his answer several seconds, and we would have to prick up our ears to catch the sentence he would give with his sonorous voice. The phrase he uttered was always different from the one we expected. For the banal aspect of the question, which we thought we had turned to see all its faces, he would substitute a new aspect which immediately was seen to be the only true one. The angle at which Fatou

[32] Menant dix mois par an une existence absolument sédentaire, il n'en conservait pas moins la curiosité du monde et le goût des voyages. Il m'a souvent décrit ses excursions dans le Tyrol ou dans la région des lacs italiens, et ses relations présentaient toujours des vues originales, même sur les sujets les plus rebattus

[33] il avait parfois des sursauts d'énergie incroyables, qui lui permettaient de faire des marches de dix heures dans les montagnes du Tyrol ou le long des grèves bretonnes.

[34] demander à l'air chargé de la senteur des goémons le coup de fouet qui le remettrait d'aplomb pour un an.

[35] According to an oral family tradition.

[36] This is more because of lack of information than a stylistic choice.

> looked at problems was always the one we missed, the one which penetrated the heart of the puzzle[37]. [Bloch 1931, p. 53]

We have seen that, when very young, Fatou's nephew did not appreciate his uncle's silence. Later, he would understand it better:

> around 1920, when I was a lieutenant[38], I sometimes spent a few days leave in the capital, where then I began to distinguish, under the extreme eccentricity of my relative, a kind heart together with a calm frankness and a freedom of judgement which neither a convention nor the opinion of anybody could hold back.
> Since however his natural delicacy forbad him to hurt his fellow man, it happened that he kept silent rather than exhibiting feelings contrary to those of his interlocutors[39].

One could compare these opinions with the elegant and pleasant way Fatou expressed himself in a few quotations from his articles:

> This study was carried out by M. Leau in his thesis; we are going to resume his analysis in a different way and to complete in many points the results obtained by this distinguished geometer[40]. [Fatou 1919b, p. 191]

Or again:

---

[37] Cordial et discret, il s'asseyait sans déranger personne au cœur de notre cohorte, et pour rien au monde, il n'eût interrompu la conversation déjà commencée. Qu'il s'agît d'art, de science, de politique, de littérature ou de musique, il gardait volontiers un obstiné silence, lequel n'était ni dédaigneux ni distant, mais où pourtant le voisin subtil eût pu voir quelquefois une réserve. Venait-on à lui demander son avis, sa réponse se faisait attendre plusieurs secondes, et nous devions tendre l'oreille pour recueillir la sentence qu'il laissait choir d'une voix caverneuse. La phrase proférée était toujours différente de celle qui était attendue. À l'aspect banal de la question, que nous pensions avoir retournée sous toutes ses faces, venait se substituer un aspect nouveau qui apparaissait immédiatement comme le seul vrai. L'angle sous lequel Fatou envisageait les problèmes était toujours celui qui nous avait échappé, celui qui pénétrait au cœur de l'énigme.

[38] There will not be any digression describing ranks in the French navy in this text. In 1920, Robert Fatou was twenty-five and was a lieutenant [enseigne de vaisseau]. Later a captain, he would command the school cruiser Jeanne-d'Arc, so that he would be known by several generations of (younger) naval officers—including several Fatou cousins.

[39] vers 1920, quand j'étais enseigne de vaisseau, il m'arrivait de passer quelques jours de permission dans la capitale, où alors je commençai à distinguer, sous l'extrême originalité de mon parent, un cœur d'or assorti d'une franchise imperturbable et d'une liberté de jugement qui ne se laissait entraver par aucune convention, pas plus que par les opinions de quiconque. Comme néanmoins sa délicatesse naturelle lui interdisait de froisser son prochain, il lui arrivait de se taire plutôt que d'afficher des sentiments contraires à ceux de ses interlocuteurs.

[40] Cette étude a été faite par M. Leau dans sa Thèse; nous allons reprendre son analyse sous une forme différente et compléter sur beaucoup de points les résultats obtenus par cet éminent géomètre.

> This case was also studied by Lattès in his already-quoted article in the *Bulletin de la Société mathématique* in an entirely correct way, although he announced later inaccurate results on this topic[41]. [Fatou 1924b, p. 76][42]

The way he addressed his friend Montel, to point out some errors in certain of his published papers, is remarkable as well:

> My dear Montel[43]
>
> I accept without any reservation the 2nd paragraph[44] on p. 41 but I have some concern about what immediately precedes it (p. 41, lines 6 to 12): "Since there is a curve ending at z_0 on which $f(z)$ has limit z_0 we deduce that $f(z)$ has limit z_0 on any curve interior to (d) ending at z_0 and fulfilling the conditions of § 23...
> I return to the conclusions of § 23 and I read: Let L be a curve, interior to the angle AOB, tangent at O to OA and having at this point a non-zero radius of curvature...
> But here, we have no reason to assume that the curve ℓ has a radius of curvature, or even a tangent whether distinct or not from the tangent to the circle. Besides, § 23 envisages curves that are closer and closer to the boundary and what we are interested in here, are rather the chords of the circumference.
> It is to the preceding § 22 that we should go back and, actually, this § 22 confirms *more or less* your claim; but not quite since, I repeat, the argument of the limit of $z - z_0$ may take all the values between $\frac{\pi}{2}$ and $\frac{3\pi}{2}$ (fig) so that the curve L (p. 34) would be tangent neither to OA nor to OB, and nor would it be interior to an angle $< AOB$.
> However, I notice, going back to the proof of § 22 that there would probably be few things to change to make this fit. This works if the [illegible] λ_n uniformly converge to the segment A_2A_3; this also works a fortiori (§ 11) if the limit points of the λ_n all are interior to Δ'_1. I thus think that this works in the intermediate cases. Hence there is no error in the principle, as I thought at first, but only a carelessness in the detail which we mention especially because your proofs are, in general, perfectly precise and clear.
> Your § 22 thus misses the proof of the following theorem which, at first sight, seems to need only a few lines (however, it would take some thought): "If $f(x)$ tends to α on any curve ending at 0 and interior to AOB, $f(x)$ uniformly tends to α in any sector $A'OB'$ which is completely interior to AOB". Until this is done, some doubt may remain over the conclusions of p. 41.

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[41] *C. R. Acad. Sc.*, t. 166, 1918, p. 51. The two functions satisfying the equations (27) below cease in general to exist for $s' = s^p$, contrary to the claim contained in this Note (footnote by Pierre Fatou) [Les deux fonctions vérifiant les équations (27) ci-après, cessent en général d'exister pour $s' = s^p$, contrairement à l'affirmation contenue dans cette Note].

[42] Ce cas a été étudié également par Lattès dans son article déjà cité du *Bulletin de la Société mathématique*, et d'une manière entièrement correcte, bien qu'il ait indiqué postérieurement des résultats inexacts à ce sujet.

[43] Montel collection, archives of the Academy of Sciences. See the Appendix for all Fatou's letters kept by Montel.

[44] The paper in question is [Montel 1917b].

But he was nevertheless capable of being ironic:

> Tuesday [Jan. 6^{th} 1920]
> My dear Montel
>
> If instead of making the substitutions of the group $(z|k^{\pm n}z)$ act on z, you use the substitutions $(z|z + p\omega + q\omega')$ where p and q denote all the integers and (ω, ω') any two numbers, and you consider the functions
>
> $$F_{pq}(z) = F(z + p\omega + q\omega')$$
>
> where F is an entire or a meromorphic function, you must obtain new and interesting results. It seems that Julia did not think of that, otherwise he would have published it urbi et orbi.

Here is another passage of [Bloch 1931] on Fatou's behaviour during their friendly meetings:

> Like all true thinkers, he would never express his opinion in a form that could hurt others by showing a lack of clear-sightedness. Fatou would not speak in aphorisms, as geometers and fools do[45].

Pierre Fatou

However tempting it may be to do so, we do not absolutely need to think here of a particular geometer, the tendency to express oneself in aphorisms being widespread among mathematicians. Of which particular geometer Bloch was thinking is not clear—neither is the exact meaning he gave to this word.

[45] Comme tous les penseurs de la vraie race, il ne formulait jamais son opinion sous une forme qui pût blesser autrui en découvrant un manque de clairvoyance. Fatou ne parlait point par aphorismes, à la façon des géomètres ou des sots.

One could regard Fatou as a geometer—especially having read the first chapter "Discontinuous groups of linear substitutions" [Les groupes discontinus de substitutions linéaires], of his book [Appell et al. 1930].

Léon Bloch even tells us what Fatou thought of official success (he does not mention the Great Prize of mathematical sciences of 1918, but nothing forbids us from thinking of it):

> Success, I mean official and crowned success, which might be the most natural dream in the world, the only conceivable one, the only one worthy of chasing, remained for Fatou, until his last day, the ideal of mediocre people[46]. How he would have been saddened by hearing men of merit ranking personal value according to the titles of social hierarchy, from the poor prizewinner of the Academy to the buxom Nobel Prize. For Fatou, the only success was that of the defeated contradiction. To pose a problem well, to recognise its affinity with other problems, to understand well the link between them, this was the only game he felt was worthy be played[47]. [Bloch 1931, p. 54]

Robert Fatou confirms:

> To fortune, and honours as well, he was indifferent and I never heard him make discreet allusions to the thinness of his purse, except when he deplored not being able to help people needier than him[48].

To end this section devoted to Pierre Fatou's personality, let us mention his "indestructible uprightness" [indéfectible droiture]:

> Everything in him was clear and neat, devoid of intrigue. He had, neither bias, nor jealousy, nor hatred, and he only despised two things, lying and favouritism. However surprising it might seem, a character of this calibre can have enemies. It is hard to accept that a free spirit judges men and things

[46] In addition to the Prize of the Henri Becquerel foundation that was awarded to him by the Academy of Sciences in 1918, let us note, among the official honours received by Pierre Fatou, his nomination as "Officier d'Académie" on July 14th 1911, "Officier de l'Instruction publique" on August 21st 1919 (these were decorations awarded by the Ministry of Education, see page 37), and as "chevalier de la Légion d'Honneur" on October 1st 1923 (Pasteur promotion).

[47] Le succès, j'entends le succès officiel et couronné, qui est peut-être le rêve le plus naturel du monde, le seul concevable, le seul digne d'être suivi, demeura pour Fatou jusqu'à son dernier jour l'idéal des médiocres Comme il eût été attristé d'entendre des hommes de mérite classer les valeurs personnelles selon les titres de la hiérarchie sociale, depuis le pauvre lauréat de l'Institut de France jusqu'au plantureux Prix Nobel. Pour Fatou, il n'y avait d'autre succès que celui de la contradiction vaincue. Bien poser un problème, bien reconnaître ses affinités avec d'autres problèmes, bien saisir le nœud qui les lie, telle est la seule partie qui lui parût digne d'être jouée.

[48] La fortune, autant que les honneurs, lui étaient bien indifférente et je ne l'ai jamais entendu faire de discrètes allusions à la maigreur de sa bourse que pour déplorer de ne pas pouvoir en faire profiter de plus nécessiteux que lui.

> on their own value, independently of any political tactics, of any religious or social traditions[49]. [Bloch 1931]

We shall see that Fatou indeed had some enemies, at the Observatory at the beginning (see § V.3), but not only there, as the slowness of his career, all his life long, would show.

Fatou's health

It is essential, because it would play an important role in his professional life, to insert here a little information about Pierre Fatou's health.

Pierre Fatou was neurasthenic, Lebesgue said (see the letter of January 16th 1906 quoted on page 155). Neurasthenia seems to have been quite common among mathematicians at that time. In addition to Fatou we have seen mention of the neurasthenia of Baire, who was seriously sick, and even of Lattès (page 32). As for understanding what the word meant... Melancholy perhaps. A mental depression certainly—which the working conditions of Fatou in the first years of the century can explain in his case.

Mineur [1929] thought that Pierre Fatou's health "did not allow him to become a sailor[50]". He was actually a frail man with a very fragile health,

> All his life was a struggle against difficult digestion, heart palpitations and insomnia[51] [Bloch 1931],

and this was probably the reason why he was not called up, although he was only thirty-six in 1914. Chazy[52] confirms:

> Fatou's health was often precarious, and the observations put his physical resistance to a severe test; but he was very conscientious in all the tasks that were entrusted to him, in particular in the redaction of the observations, in the discussion of instrumental constants[53]. [Chazy 1932]

[49] Tout en lui était clair et net, dépourvu de traverses et d'intrigue. Il n'avait ni parti pris, ni jalousie, ni haines, et ne méprisait que deux choses au monde, le mensonge et le favoritisme. Quelque surprenant que cela paraisse, un caractère de cette trempe peut avoir des ennemis. On admet difficilement qu'un esprit libre juge des hommes et des choses sur leur seule valeur, indépendamment de toute tactique politique, de toute tradition religieuse ou sociale.

[50] ne lui avait pas permis d'embrasser la carrière de marin

[51] Toute sa vie fut une lutte contre les digestions difficiles, les palpitations de cœur et les insomnies.

[52] Jean Chazy (1882–1955), who was awarded (together with Boutroux and Garnier) the Great Prize of mathematical sciences in 1912 (on algebraic differential equations), was also a specialist in the motion of celestial bodies. He studied in particular the three body problem and the advance of the perihelion of Mercury and its explanation by the theory of relativity.

[53] La santé de Fatou était souvent précaire, et les observations soumettaient sa résistance physique à de rudes épreuves; mais il apportait une grande conscience à toutes les tâches qui lui étaient confiées, notamment dans la réduction [rédaction?] des observations, à la discussion des constantes instrumentales.

The annual report of the Paris Observatory for 1929 would note, in the section devoted to Fatou's death:

> His health, unfortunately, was always a little fragile; it limited to some extend the effort he would have wanted to devote to pure astronomic observation, which he loved, and in which he showed a reliable and enlightened ability[54].

However, his nephew Robert Fatou writes that Pierre Fatou had never in

> all his life, thought it useful to consult a physician[55].

This information was confirmed (after Fatou's death) by a letter from Louis Fatou (see page 187). The nephew might not have been aware that Pierre Fatou was sick because:

> He would seldom speak about himself, and even less about his bodily miseries, which he used to treat with a smile or disdain, and almost always simply keep to himself, with that courage made of modesty that only the Bretons have. Thus we would find normal, almost natural, the air of tiredness which, year after year, would show more deeply on his face. Those who saw Fatou again after many months always found he looked unwell, and had a sullen and emaciated appearance[56]. [Bloch 1931]

The difference between the physical appearance of the Pierre Fatou on the photo of the astronomers which is reproduced here in Figures V.1 and V.2 and the more emaciated Fatou who can be seen in Figure V.3 is indeed striking. We shall see below (in § V.9) that Pierre Fatou probably suffered from a stomach ulcer.

V.3 Continuation of Fatou's career

Let us return to the chronology, which was interrupted on page 139 in 1898, at the moment when Fatou entered the ENS. He passed the agrégation exam in 1901, in second place, and he was immediately hired at the Paris Observatory, first as a trainee-astronomer, then as an assistant-astronomer starting from January 1904 (in the position vacated by Hamy who was promoted to titular-astronomer [astronome-titulaire]). One can sometimes read (in [Nathan 1971],

[54] Sa santé malheureusement avait toujours été un peu chancelante; elle limitait dans une certaine mesure l'effort qu'il aurait voulu consacrer à la pure observation astronomique, qu'il aimait, et dans laquelle il témoignait d'une habileté sûre et éclairée.

[55] sa vie durant, jugé utile de consulter un médecin.

[56] Il parlait rarement de lui-même, moins encore de ses misères corporelles, qu'il avait l'habitude de traiter par le sourire ou par le dédain, presque toujours de taire simplement, avec ce courage fait de pudeur qui appartient aux seuls Bretons. Aussi en arrivions-nous à trouver normal, presque naturel, cet air de fatigue qui, d'année en année se marquait plus profondément sur son visage. Ceux qui revoyaient Fatou à de longs mois de distance lui trouvaient toujours mauvaise mine, l'aspect morose et décharné.

then for instance in [Alexander 1994] or on the St Andrews website) that it was because there were no positions as mathematicians in Paris[57] that he accepted a position as assistant-astronomer (actually, as a trainee to begin with). The reasons why Fatou would have wanted to stay in Paris are not given in these texts. Besides, this is not exactly what Léon Bloch says[58]:

> Fatou entered astronomy, so to speak, by pure chance. He owes this to the initiative of Tannery[59], who wanted to make the Observatory benefit from the presence of one of his best "agrégés", who would soon become one our best Doctors. At that time, the Paris Observatory was no longer the Observatory of Arago and Le Verrier. Recruiting workers was difficult, finding talents was almost unknown. Some punctual and quiet civil servants were doing their job, more or less as a head of division at the Seine prefecture can do his. Tannery understood that this institution could be given some lustre and originality. In proposing to attach to it a mathematician like Fatou, he was sure to safeguard at least theoretical astronomy, in all the applications where it touches analysis[60]. [Bloch 1931]

The work of an astronomer was indeed, at the beginning of the 20$^{\text{th}}$ century, service work (weather forecast, time, map of the sky) and not research work[61]. Some "punctual and quiet civil servants" are seen on the photograph reproduced in Figure V.1 (one should notice the astronomer's hats, which emphasise the pen-pusher aspect).

At the beginning, Fatou probably worked more on his thesis than on his astronomical tasks.

[57] Most of the mathematicians mentioned in this text started their career teaching in secondary schools, often in the provinces.

[58] For a possible reason for this choice of the Observatory, a position without teaching duties, see also § V.7 below.

[59] At the time when Fatou was a student there, Jules Tannery was the assistant director of the ENS, the post which Borel later took.

[60] Fatou est entré dans l'Astronomie, pour ainsi dire, par hasard. Il le doit à l'initiative de Tannery, qui voulait faire bénéficier l'Observatoire de la présence d'un de ses meilleurs agrégés, lequel devait bientôt être un de nos meilleurs docteurs. En ce temps, l'Observatoire de Paris n'était plus l'Observatoire d'Arago et de Le Verrier. Le recrutement des vocations y était difficile, celui des talents y était presque inconnu. De ponctuels et calmes fonctionnaires y faisaient correctement leur métier, à peu près comme peut faire le sien un chef de division à la Préfecture de la Seine. Tannery comprit qu'il fallait rendre à cette institution un peu de lustre et d'originalité. En proposant d'y attacher un mathématicien comme Fatou, il était sûr de sauvegarder au moins l'Astronomie théorique, dans toutes les applications par où elle touche à l'analyse.

[61] Pierre Fatou is not the only mathematician known to have earned his living as an employee in an observatory; let us mention, in the previous generation and in Leiden, the case of Thomas Stieltjes. The two activities, mathematician and astronomer were not absolutely disjoint: in 1890 and while he was a professor at Toulouse, Stieltjes went with Benjamin Baillaud, then the director of the Toulouse Observatory, in his inspection trip to the Pic du Midi [Baillaud & Bourguet 1905b, p. 79].

> Indeed, during his first years as a trainee, Fatou remained the elite mathematician who would end up in 1907 publishing his famous thesis on trigonometric series[62]. [Bloch 1931]

This was not liked by everybody since, in 1906, people complained about him during a meeting of the council of the Observatory[63]:

> M. Fatou proved to be very active and zealous before his nomination. Student without salary in November 1901, he was, on the suggestion of the Director[64], promoted "aide-astronome" on January 1st 1904 and, three months later, he became assistant-astronomer; he thus had an exceptional promotion and we thought he was an excellent acquisition. Unfortunately this hope was entirely thwarted. Not only has this civil servant produced almost nothing during these two years, but his very limited contribution to the work has been a real hindrance for his colleagues[65]. It is because of him that the publication of the volume of observations for 1902 was delayed by more than six months [...] An abnormal and disturbing situation is established that cannot last[66].

In the same period, recriminations of a similar kind were made against Salet and even against Nordmann, who also applied for long leaves for health reasons [Saint-Martin 2008, pp. 197 and 202]; both were involved, like Fatou, in personal research. This is what Bloch comments:

> To tell the truth, the daily observation work, which Fatou did with perfectly seriously, was not his passion: for him, it was the framework, not the ultimate goal of science. We should not be surprised if, in the rather simple-minded environment into which this student of the ENS was thrown, his care for theory caused some setbacks at the beginning[67].

62 De fait, pendant ses premières années de stage, Fatou resta le mathématicien d'élite qui devait aboutir en 1907 à la publication de sa célèbre thèse sur les Séries trigonométriques.

63 Archives nationales, F/17.3722, quoted by Philippe Véron [2004] and Arnaud Saint-Martin [Saint-Martin 2008]. Thanks again to David Aubin.

64 The director of the Paris Observatory was, in 1906, Maurice Lœwy. It was Benjamin Baillaud who succeeded him.

65 The annual reports of the Observatory are less virulent!

66 M. Fatou [...] s'était montré très actif et plein de zèle avant sa nomination. Élève sans traitement en Novembre 1901, il a été, sur la proposition de Monsieur le Directeur, promu aide astronome au 1er janvier 1904 et, trois mois après, il devenait astronome-adjoint; il a donc obtenu un avancement exceptionnel et on pensait avoir fait en lui une excellente acquisition. Malheureusement cet espoir a été entièrement déçu. Non seulement ce fonctionnaire n'a presque rien produit pendant ces deux ans, mais sa participation si restreinte aux travaux a été une véritable entrave pour ses collaborateurs. C'est à cause de lui que la publication du volume des observations de 1902 a été retardée de plus de six mois [...] Il s'est établi ainsi une situation anormale et troublante qui ne peut se prolonger.

67 À vrai dire, la besogne d'observation quotidienne, que Fatou exécutait avec une parfaite conscience, ne le passionnait pas: elle constituait pour lui l'ossature, non

Lebesgue [1991, p. 134] explained the affair to Borel as it unfolded (on January 16^{th} 1906):

> Fatou has written me, but you might already know, that Lœwy is trying to get rid of him, banking on his bad state of health which, Lœwy says, will always prevent him from doing astronomy. For this reason, Lœwy considers Fatou to be more suited to teaching. According to Fatou, Lœwy's idea would be to create, by Fatou's departure, a position for a certain Ebert[68] of whom Fatou makes the criticism, in a somewhat nationalistic way, that he has been recently naturalised[69].
>
> I believe, but do not bank on this because I have no serious reason to believe it, that Fatou can do without a salary at the moment. If I am not mistaken, there would be no disadvantage for Fatou, on the contrary, to be given leave to treat his neurasthenia, which seems rather acute. I believe that Fatou will see no reason why he should not be given leave, but what he wishes, and it seems to me that this is quite natural, is that his rights for later be preserved and that a physical inability to fulfil the duties of his position, which might be temporary, would not be considered as a sufficient reason to reject him from the Observatory.
>
> Fatou has written to me that he has spoken of all this to Tannery, who promised to take care of it. If you had the opportunity to talk with Tannery or to any person who could have an excuse to intervene, I would be grateful. Besides, I assume that Fatou has told you about this, although it is not certain since he is rather withdrawn and shy[70].

Then, at the beginning of February 1906:

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pas la fin dernière de la science. Ne nous étonnons pas si, dans le milieu un peu primaire où ce normalien était jeté, son souci de théorie lui valut d'abord quelques déboires.

68 There was indeed, at that time, at the Paris Observatory, a "volunteer-assistant" [assistant-volontaire] called Wilhelm Ebert, who worked in collaboration on a catalogue of stars. In 1905, he got a position as assistant-astronomer in Nice, but he came back to Paris in 1906, still as a volunteer-assistant. It is thus very likely that he would have been very happy to get an assistant-astronomer position in Paris. This information, contradictory to that given by [Lebesgue 1991, note 1095], come from the annual reports of the Observatory, in which we also learn that Ebert and Fatou collaborated, later, in 1908, in the study of the photographic meridian instrument of Lippmann.

69 Maurice Lœwy, with whom is seems to be quite clear that Fatou had atrocious relations, was himself of Austrian origin, naturalised French in 1868, a possible explanation of the "nationalist" comment on Ebert.

70 Fatou m'écrit, ce dont vous êtes peut-être au courant, que Lœwy cherche à se débarrasser de lui se basant sur son mauvais état de santé qui, dit Lœwy, l'empêchera toujours de faire de l'astronomie. Pour cette raison, Lœwy considère Fatou comme plus propre à faire de l'enseignement. D'après Fatou, l'idée de Lœwy serait de créer par le départ de Fatou un poste pour un certain Ebert auquel Fatou fait le reproche, quelque peu nationaliste, d'être récemment naturalisé.

Je crois, mais ne vous basez pas trop sur cela car je n'ai pas de sérieuses raisons de [le] croire, que Fatou peut momentanément se passer d'un traitement. Si je ne me trompe pas, il n'y aurait pas d'inconvénient pour Fatou, au contraire, à ce

> As for Fatou, I believe that a solution is about to be reached; he has applied for leave[71] [Lebesgue 1991, p. 137].

If the annual report of the Observatory does not explicitly mention a leave given to Fatou in that year, one can see that he took observations at night with the Great meridian instrument in January, then from August to December, and this confirms that he took a few months leave (at most six, from February to July).

He probably already had some problems before 1906, since, on April 7th 1905, Lebesgue [1991, p. 107] wrote, again to Borel:

> Thanks a lot for what you did for Fatou; I believe indeed that he was careless, but I think that there are in his state of health some extenuating circumstances and it would have been excessive to castigate him[72].

Let us remind the readers that the letters from Borel to Lebesgue have not been kept. On the other hand, the only letter we know from Fatou to Borel is just a mathematical proof (see § V.5). We thus do not know what Borel did for Fatou.

As many astronomers did, Pierre Fatou participated in the observation of the transit of Mercury on November 14th 1907. The library of the Observatory holds a photograph of a group of astronomers, taken on the terrace on this occasion (Figure V.1) on which Fatou appears (it is easier to distinguish the various people in Figure V.2).

Digression (The astronomers in the picture). Let us digress here to quickly describe the people in the picture taken on the occasion of the transit of Mercury, a digression that will help us to understand the atmosphere in which the non-mathematical part of Fatou's life took place. According to the hand-written notes, these people are, from left to right

– Benes; this is Ladislav Benes, a Czech (hence, at that time Austrian) astronomer (and later a soldier) who was a "free student" at the Observatory from July 1907 (and who stayed there until December 1907),

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qu'on lui donne un congé lui permettant de soigner sa neurasthénie, assez aiguë paraît-il. Je crois que Fatou ne verra aucun inconvénient à ce qu'on lui donne un congé mais ce qu'il désire, il me semble et cela me paraît trop naturel, c'est que ses droits pour plus tard soient réservés et qu'une incapacité physique de remplir les obligations de son poste, qui n'est peut-être que momentanée, ne soit pas considérée comme une raison suffisante de le rejeter hors de l'Observatoire.

Fatou m'écrit avoir parlé de tout cela à Tannery qui a promis de s'en occuper. Si vous avez l'occasion d'en parler à Tannery ou à quelque personne qui ait prétexte à intervenir, je vous en serais reconnaissant. Je suppose que Fatou vous a d'ailleurs mis au courant; cela n'est cependant pas certain car Fatou est assez renfermé et timide.

[71] Pour Fatou, je crois qu'une solution est prête d'intervenir; il a demandé un congé.

[72] Merci beaucoup de ce que vous avez fait pour Fatou; je crois bien qu'il a été négligent, mais je pense qu'il y a dans son état de santé des circonstances atténuantes et qu'il eût été exagéré qu'on l'exécutât.

Fig. V.1. Transit of Mercury, November 14$^{\text{th}}$ 1907

– Bocquet [*sic*]; this is certainly Félix Boquet, who was then the head of the time department,

– A. Chatelu; André Chatelu, a scientific employee (a calculator) born in 1873,

– J. Chatelu; Jules Chatelu, also a scientific employee, born in 1870,

– Rabioulle; Émile Rabioulle, also a calculator, at that time, who would be killed in war on September 21$^{\text{st}}$ 1914 as a second lieutenant in the infantry,

– Simonin; Martial Simonin, who would be a titular-astronomer at the Paris Observatory from 1911 (but in 1907 must still have been in Nice), we shall see Fatou resigning himself to be a subordinate of Simonin in a letter to Montel on page 271,

– Fayet; Gaston Fayet, a self-taught (who came to the Observatory as a calculator, passed the baccalaureate then did a thesis), a specialist in comets, later the director of the Nice Observatory,

– Renan; Henri Renan, a specialist in meridian observations, who had an easier career, the ENS, student astronomer, assistant-astronomer, titular-astronomer in 1906,

– Gonnessiat; François Gonnessiat, just visiting, as an assistant-astronomer, the Paris Observatory from November 1$^{\text{st}}$ to December 1$^{\text{st}}$

1907 between his positions as the director at Quito (before) and Algiers (after), just enough time to watch Mercury passing and to pose for the photo,

Fig. V.2. Transit of Mercury, detail

– Fatou, with hat and walking stick,

– Brandicourt; Charles Brandicourt, with the reputation of a low-calibre civil servant[73],

– Viennet; Éloi Viennet, one of the members of the first board of the AAPSOF.

All this information comes from Philippe Véron's dictionary [2004].

[73] Let us quote, again to show the atmosphere, two assessments (reproduced from [Véron 2004]) made of Brandicourt by two directors of the Observatory, first Lœwy, in 1905:

> No scientific titles. An employee of goodwill, but of average value [N'a pas de titres scientifiques. Employé de bonne volonté, mais d'une valeur moyenne],

then Baillaud, in 1911:

> Very good observer, conscientious calculator, must however finish his career in the category in which he is. [Très bon observateur, calculateur consciencieux, doit cependant terminer sa carrière dans la catégorie où il se trouve.]

But let us return to Pierre Fatou. In an Observatory there are plenty of minor and humdrum tasks that are allocated, among others, to the young recruits[74]. At that time, as we have said, the Observatory played an important public service role (to give the exact and legal time, for instance). Nothing there to stir up the appreciation of or the desire for science of a Fatou. Besides, he was, in the first years, often absent for health reasons, as the annual reports mention. Over time, one can pick up in these reports facts showing that Fatou was employed in various observations to determine the coordinates of the reference stars for the sky map (he made, for instance, 2207 observations for this in 1903), in sending the time to the provinces (in 1905), in other observations with such and such an instrument, with such and such a colleague, nothing under his responsibility, until the war, when he found himself doing alone what five people had been doing until then.

Whether the person complaining about him above was indeed one of Fatou's enemies, or the way in which Fatou and his colleagues conceived his role and his work changed, the fact remains that, when he died in 1929, he had only friends at the Observatory (this is what can be read in the annual report of this institution for 1929 and no obituary would have failed to point it out) and his work was acknowledged and appreciated.

It seems indeed that, later, Fatou did his work seriously, and not only because he was taking actual observations and measurements. Even non-specialists in astronomy may think that what he had to do, from 1923, in the equatorial of the west tower, was more rewarding (see § V.6). Besides, no more "repeated" absences were reported. This could be a sign of the evolution of the role of astronomer, from positional astronomy and services to more physical observations, during this period (see [Saint-Martin 2008]).

Let us mention another piece of information given by Lebesgue [1991, p. 118]: it had been suggested "in times past" [jadis] (this was in a letter dated August 22nd 1905, the "times past" were thus not very long ago!) to Fatou that he should go to Quito Observatory[75], which would have advanced his career rapidly on his return, but Fatou refused.

Fatou as a trade-unionist

In 1909, several astronomers founded the "Friendly union of scientific employees of the French Observatories" [*Association amicale des personnels scientifiques des Observatoires français*] (AAPSOF). This was not, strictly speaking,

[74] This was a reason why the scientific staff of the Paris Observatory was more feminine than that of the Faculty of sciences. At that time, the word "calculatrice" (French for pocket calculator, a feminine word) denoted a female employee making computations.

[75] The director of the Quito Observatory was, from 1900 to 1906, a French astronomer, François Gonnessiat (whom we have seen in the transit of Mercury photo).

a trade union (the so-called 1884 Waldeck-Rousseau law, which authorised trade unions in France was still forbidding them for civil servants), but it played the same role (see [Saint-Martin 2008]). In 1911, Pierre Fatou was the president of this association—and this shows his investment in his professional life.

V.4 Fatou's thesis

Fatou was working on his thesis while Lebesgue gave his Peccot course on trigonometric series and Lebesgue quoted Fatou's work several times in his book [1906]. Lebesgue showed a great delicacy towards Fatou. He wrote to Borel on January 29th 1905, about the preparation of this book:

> I can give the manuscript before January 1st 1906. I do not want to dawdle on this subject, which, as you think, is beginning to lose the attraction of the novelty.
> But there is a small difficulty. Fatou passed on to me a certain number of results which are more or less related to Fourier series; I would like Fatou to write and defend his thesis before the publication of my lessons. The questions he is dealing with are sometimes rather close to the ones I am discussing so that those who judge superficially would not be able to distinguish clearly what belongs to Fatou[76]. And you know how easily one attributes the credit for things found by others to those whose names we already know.
> I have just written to Fatou and I hope that in giving him, as I have, January 1906 as the date when I shall submit the manuscript, I will make him hurry a little. I now admit that I would have been better defending my thesis earlier[77], I want to try to prevent him from acting as I did. If you have an opportunity to talk to him, you could maybe influence him in the same direction[78] [Lebesgue 1991, p. 93].

[76] Lebesgue thought highly of Fatou. To all that appears here, let us add that he systematically asked Borel to send him the books of his collection.

[77] Let us remind the readers that Lebesgue's thesis was only defended in 1902. Fatou immediately used it.

[78] Je puis donner le manuscrit avant le premier janvier 1906. Je ne tiens pas à trop traîner sur ce sujet qui, comme vous le pensez, commence à perdre pour moi l'attrait de la nouveauté.

Seulement il y a une petite difficulté. Fatou m'a communiqué un certain nombre de résultats qui se rapportent de près ou de loin aux séries de Fourier; je voudrais bien que Fatou se décide à rédiger et soutenir sa thèse avant l'apparition de mes Leçons. Les questions dont il s'occupe sont assez voisines parfois de celles que je traite pour que ceux qui jugent superficiellement ne distinguent pas bien ce qui est à Fatou. Et vous savez avec quelle facilité on attribue à ceux dont on connaît déjà le nom le mérite des propriétés trouvées par d'autres.

Je viens d'écrire à Fatou et j'espère qu'en lui indiquant, comme je l'ai fait, pour janvier 1906 le dépôt de mon manuscrit, je vais le faire se dépêcher un peu. Je reconnais maintenant que j'aurais mieux fait de passer ma thèse plus tôt, je veux

Fatou only defended his thesis[79] in 1907, on February 14th according to [Lebesgue 1991, note 393], a date which is confirmed by the 1921 Notice[80] in which Fatou wrote that he became a Doctor in February 1907 and by a letter he sent to Fréchet the evening after his defence and that he dated "Thursday evening", February 14th 1907 being indeed a Thursday (see this letter on page 258). This could seem contradictory to the fact that the thesis was published in the 1906 volume of *Acta mathematica*. We have seen that, in January 1905, Lebesgue (and maybe Borel also) urged him to write. He did so, since, as Lebesgue wrote on June 16th 1905,

> Fatou told me that his thesis has been written; he often talked to me about it, I hope that all in all this will be a good thesis[81] [Lebesgue 1991, p. 112][82],

and, two days later,

> I am happy to learn that, at first sight, you find Fatou's thesis interesting. We often spoke together and I don't think we interfere much. He is concerned mainly with the Poisson integral and with a Taylor series on its circle of convergence, things with which I don't deal. Sometimes we tend to the same goal: to represent the most general function at the largest possible number of points. Fatou gets there by the Poisson integral and I by Fejér summation; our statements and our methods are different [...]
> Fatou announced to me a result which seems important for applications; it is the extension of the Parseval equality to unbounded functions. It is so hard to attack these blasted functions and in the applications [...] it is such large reduction of generality and interest to make the assumptions necessary to have only bounded functions that I attach to such a result greater value than it seems to deserve[83].

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essayer de l'empêcher d'agir comme moi. Si vous avez l'occasion de lui causer, vous pourriez peut-être agir sur lui dans le même sens.

79 The members of the board of examiners for Fatou's thesis were Paul Appell (chair), Paul Painlevé and Émile Borel (examiners). Probably Lebesgue, who was in the provinces at that time and was not a professor at the Sorbonne, was not able to participate. The thesis is dedicated "À Messieurs Émile Borel et Henri Lebesgue". The second thesis was on the periodic solutions of the equations of the motion of systems.

80 Fatou file, archives of the Academy of Sciences. The date given in [Gispert 1991] (together with the report on the thesis which is indeed dated February 14th) is July 1st.

81 Fatou m'a dit que sa thèse était rédigée; il m'en avait souvent parlé, j'espère qu'au total ça fera une bonne thèse.

82 It is not correct to say that Lebesgue *supervised* [Alexander 1994, p. 88] Fatou's thesis. Firstly because, as we have said, the notion of a thesis supervisor did not exist at that time in France. In this special case, Lebesgue's comment that "he often talked to me about it" clearly shows the way in which things happened.

83 Je suis heureux d'apprendre qu'à première vue vous jugiez intéressante la thèse de Fatou. Nous avons causé souvent ensemble et je ne pense pas que nous interférions beaucoup. Il s'occupe surtout de l'intégrale de Poisson et d'une série de Taylor sur son cercle de convergence, ce dont je ne m'occupe pas. Parfois nous tendons

Then Lebesgue carefully read the thesis. On February 26th 1906, he gave a vague opinion on the way it was written:

> In general, it seems to me that Fatou does not highlights very well the interest of the questions he deals with and the results he obtains [...] Fatou seems also to quite readily suppose that everybody knows or sees the same thing as he does and gives rather few explanations[84] [Lebesgue 1991, p. 138][85].

He proposed, at the beginning of April 1906, a long list of corrections (that would be found in [Lebesgue 1991, pp. 145–148]) that Fatou would, or would not, make. In the meantime, as we have seen, Fatou applied for leave.

Because of his health problems, for his difficulties at the Observatory, or for other reasons, this lasted for another year. The careful readers will have nevertheless noticed that Fatou was not inactive during that, since his Note [1906d], of which we have already spoken at length above, was published on October 15th.

On November 29th 1906, Fatou went to the secretary's office at the Faculty of sciences to see whether the report on Montel's thesis had arrived. Then he wrote him a letter which shows well his state of mind during this wait:

> My dear friend,
>
> I went this morning to the secretary's office at the Faculty of sciences, but Guillet told me that Painlevé had not brought back your manuscript; the truth is that there is quite some disorder in his office and that he had to move several theses which were piled up higgledy-piggledy on his desk before he gave me the assurance that it was still in Painlevé's hands. You had better speak to Painlevé to find out what is happening. I must tell you however that your thesis was not, on the register of the secretary, among those which have been brought back[86].

à un même but: représenter une fonction la plus quelconque en le plus de points possible. Fatou y arrive par l'intégrale de Poisson, moi par la sommation de Fejer [Fejér]; nos énoncés et nos méthodes sont différentes. [...]

Fatou m'a annoncé un résultat qui me paraît important dans les applications; c'est l'extension de l'égalité de Parseval aux fonctions non bornées. Il est si difficile d'attaquer ces maudites fonctions et dans les applications [...] c'est une telle diminution de généralité et d'intérêt que de faire les suppositions nécessaires pour qu'on n'ait que des fonctions bornées que j'attache à un tel résultat plus de prix qu'il n'en paraît mériter.

84 D'une manière générale il me semble que Fatou met assez mal en lumière l'intérêt des questions dont il s'occupe et des résultats qu'il obtient [...] Fatou me semble admettre aussi assez volontiers que tout le monde sait ou voit la même chose que lui et il donne parfois assez peu d'explications.

85 Rare are the theses for which such a criticism cannot be made! We have seen that Fatou's abilities as a writer greatly improved later.

86 According to [Gispert 1991, p. 399], Montel defended his thesis on May 25th 1907 (but Painlevé's report is not dated).

As for me, I am not yet sure of the date of my defence, since I have not received proofs since September. It has been going on so long that I have grown tired of thinking about it, and this wait does not perturb me at all[87].
I continue working from time to time on iteration. Incidentally, I obtained a result on Taylor series which I communicated to the Société mathématique the other day and which Hadamard liked[88]. Here is what it is about.
Assume a Taylor series

$$(1) \qquad u + u_1 x + \cdots + u_n x^n + \cdots$$

the coefficients of which are determined by the induction law

$$u_{n+1} = f(u_n)$$

(f an analytic function). Without being more specific we cannot say anything, since any Taylor series can be obtained this way.
But let us assume that the substitution $(u \sim f(u))$ has a regular limit point, that is, a point which is a root α of the equation

$$u - f(u) = 0 \text{ with } \left|f'(\alpha)\right| < 1$$

(f holomorphic at α). If the initial value u lies in the domain of convergence relative to the point α, the series (1) represents a meromorphic function which is the *quotient of two entire functions of genus zero and order zero*, the entire function in the denominator being

$$(q = f'(\alpha)) \qquad (1-x)(1-qx)(1-q^2x)\cdots(1-q^nx)\cdots$$

a function that we meet in the theory of elliptic functions.
This result is rather curious, as you see, as it gives a precise result on the analytic extension of a large class of Taylor series given by the law of their coefficients, this law being in general not analytic (that is, not of the form $a_n = \varphi(n)$ where φ is a given analytic function.
[...]
If you want me to go and see Painlevé for you, send me a note.
Most sincerely yours, and see you soon
P. Fatou[89]

---

87 Surprisingly, from the point of view of chronology, on the copies (printed in Sweden, as reprints of the paper [Fatou 1906c], on November 23rd 1906) of Fatou's thesis is the note (and moreover the date):

Vu et approuvé [Seen and approved]
Paris, le 4 juillet 1905
le Doyen de la Faculté
des sciences
Paul Appell

(at that date, Fatou's thesis had not been reviewed).

88 Fatou presented "on certain Taylor series" on November 22nd 1906. It was Hadamard, as the president of the society, who chaired the session.

89 Archives of the Academy of Sciences, Montel collection. See the complete text of this letter in the Appendix.

V.5 Fatou as a mathematician

Even if it is true that Pierre Fatou's scientific activity took place in a time when astronomy was undergoing deep changes, even if it was possibly because Tannery anticipated these changes that he was hired at the Observatory, it is nevertheless true that, during almost all his career, Fatou engaged in his two activities, as a mathematician and as an astronomer, in an almost disconnected way, and that this caused him some problems at the beginning, as we have seen. This is the reason why we present here these two scientific lives in two distinct sections.

Let us start with mathematics and with another excerpt of Hadamard's 1921 report, the very beginning this time:

> M. Fatou, assistant-astronomer at the Paris Observatory, worked on the most difficult and lofty areas of current mathematics and, each time, he gave the studies he approached a fruitful and important impetus.

The thesis etc.

The question which Fatou considered in his thesis is both natural and ancient: the study of a power series on its circle of convergence. The method was new: the Lebesgue integral. Fatou himself says in the notice on his work which he wrote in 1921:

> I noticed that the progress made in the study of integration and derivation of functions of real variables by the important work of M. Lebesgue allowed one to answer some of these questions[90] [...] more precisely and more generally than it had been done previously[91].

Painlevé comments in his report on the thesis [Gispert 1991, p. 397]:

> But the application of these notions to the problems which arose in the theories investigated by M. Fatou was not without its difficulties which he overcame with ingenuity and talent[92].

His work was immediately used and developed. Fatou left his name to a lemma in integration theory, the "Fatou lemma[93]", which asserts that, *if* f_n

[90] These were about the behaviour of a Taylor series on its circle of convergence.

[91] J'ai remarqué que les progrès réalisés dans l'étude de l'intégration et de la dérivation des fonctions de variables réelles par les importants travaux de M. Lebesgue permettaient de répondre à certaines de ces questions [...] avec plus de précision et de généralité qu'on n'avait pu le faire précédemment

[92] Mais l'application de ces notions aux problèmes qui s'étaient posés dans les théories étudiées par M. Fatou, n'était pas sans présenter des difficultés qu'il a surmontées avec ingéniosité et talent [...]

[93] This lemma is the reason why all mathematicians know Fatou's name. Anny Fatou told me that a mathematics teacher asked her if she was from "Fatou lemma's family". As for Michel Fatou, he remembers examiners who kept the hardest questions for him once they had understood that he was a relative of this lemma.

is a sequence of non-negative measurable functions, then

$$\int \liminf f_n \, d\mu \leq \liminf \int f_n \, d\mu.$$

One of his theorems, *if f is a bounded holomorphic function on the open disc $|z| < 1$, there exists a bounded function $f^\star$ on the unit circle which is defined almost everywhere by the formula*

$$f^\star(e^{it}) = \lim_{r\to 1} f(re^{it}),$$

was one of the first applications of the Lebesgue integral to the study of holomorphic functions, says Rudin in his book [1966, p. 265] as does Segal in his [2008, p. 301][94]. The result is slightly more general: one can replace the holomorphic function by a harmonic function...

It has been since Pierre Fatou's thesis [1906c, p. 350] that the equality

$$\sum |a_n|^2 = \frac{1}{2\pi} \int |f(t)|^2 \, dt$$

for a function $f(t) = \sum a_n e^{2i\pi nt}$ has been called "the Parseval formula", a formula which Lebesgue has already spoken of (in the letter quoted on page 161), which Fatou proved for what we today call L^2-functions, a result much praised[95].

One of the very first papers Fatou published is the Note [Fatou 1904b], in which he investigates the relations between the real and imaginary parts of a Taylor series in its disc of convergence. The Notes [Fatou 1905b ; 1906b] are announcements of some results of the thesis.

What precedes suggests that the evaluation of Fatou's work prior to iteration made by Alexander in his book [1994, p. 89–90] is somewhat lightweight[96]. In the present text, we have also seen that Fatou's work (besides that on iteration) inspired the Notes [Hardy & Littlewood 1917 ; Humbert 1918] which have already appeared in this story without disturbing its chronology. Let us also mention the exchange of letters between Hurwitz and Pólya [1916] and

[94] This result was already the subject of a section in the chapter on bounded functions of Bieberbach's book [1927] from which we have already quoted the "lemma of Julia" on page 101.

[95] The history book [Kline 1972], which appeared before iteration came back into fashion, has no entry "iteration" (and not even an entry "Julia") in its index—but it quotes Fatou for this Parseval formula.

[96] It is true that the main source of this evaluation is the publication list of Fatou as it is given in [Poggendorff 1922] (!), with, moreover, an omission, as there are quoted not two but three papers between Fatou's thesis and 1916, namely [Fatou 1910 ; 1913a ; d]—see our more complete bibliography.

the articles of Marcel Riesz [1916a; 1916b][97]. There exists another "Fatou lemma" (again in his thesis [Fatou 1906c], announced in [Fatou 1904a], his very first paper), the one which says that if all the coefficients of the Taylor series at 0 of a rational fraction are rational, then the poles of this fraction are inverses of algebraic integers, a lemma which gave birth to the notion of "a Fatou ring".

It is possible that health problems, difficulties at the Observatory, the defence of his thesis, and then the return to his activities as an astronomer at the Observatory, prevented Fatou from devoting a lot of time to mathematics, and especially from continuing and writing up his work on iteration. For this intermediate period, we have to mention a little paper [1910] in which he considers at the same time two of his favourite objects, a power (Taylor) series

$$\sum u_n z^n, \text{ with } u_n = R^n(u)$$

where R is a rational fraction with an attracting fixed point of multiplier s, and for the sum of which he describes the singularities (there is a pole at each s^{-n}). Moreover, this result was generalised by Lattès [1915] to the case where the coefficients are obtained by the iteration of functions of several variables.

Number theory

In the third Note [Fatou 1904c] published by Fatou in 1904, he studies the distribution of fractions p/q which approximate a real number a in the sense that

$$|q(p - aq)| < 1.$$

Calling $\varphi(h)$ the number of solutions for which $q < h$, he shows that

$$\varphi(h) > \frac{1}{2} \log h$$

(and not more when a is a quadratic integer). This result has applications to trigonometric series (see below in reference to the Note [Fatou 1913a]) that Fatou uses in his thesis. This research is clearly related to the only letter of Fatou kept in the Borel collection[98].

Let us recall again the "extremely remarkable" Note [Fatou 1906a] already mentioned on page 82, and quote the 1921 notice:

[97] It is thus not true, as the philosopher Alain believed, or any case said, that there were only two men who understood Fatou's thesis: in addition to Lebesgue, there are already three men here.

[98] Archives of the Academy of Sciences, Borel collection. The letter of Fatou kept in the Borel collection can very likely be dated as 1903 or 1904—before December 12$^{\text{th}}$ 1904, the date of [Fatou 1904c], in any case. Fatou thanks Borel for having sent him an article, which could be [Borel 1903a], and a Note of the *Bulletin de la Société mathématique de France*, which is, without any doubt, [Borel 1903b]. In this article, Borel made a list of the fractional parts of $\sqrt{N}$ for $1 \leq N \leq 500$ (the

> I noticed that the Dirichlet method for the computation of the number of classes of binary quadratic forms with a given determinant leads, when we apply it to definite Hermite forms, to a simple expression of the number of classes where only the prime factors of the determinant show up. The computations, summarised in a comptes-rendus Note, were communicated to me by G. Humbert who developed them in his Collège de France course, and who then investigated more extensive analogous problems[99].

Iteration and later

It is pointless to elaborate here on the monumental work on iteration which was the subject of Chapter II and § III.2. After the preliminary Note [1906d] and the three memoirs [1919b ; 1920a ; 1920b], Fatou was still left with some problems to investigate[100].

In [Fatou 1922g], Fatou studies examples of iteration of algebraic functions. The two Notes [1922d ; 1922e] announce the results on functions of two variables that are the subject of the article [1924b].

In the two articles [1923c ; 1924a] he reviews permutability.

In [1926], Fatou reviews the iteration of entire functions which are not polynomials[101]. He explains what remains in this case of the results obtained on rational fractions and investigates two examples in great detail. Let us note that the proof of the fact that the set $\mathcal{F}$ (of points at which the sequence of iterates is not normal) is not empty is not as easy as we understood it to be when we read Remark III.1.2 and § III.2. In this article, Fatou asks a certain

article mainly consists of a table) arranged in increasing order, and he noticed some gaps near the simple fractions—nothing, for instance, between 0.4941853 and 0.5202593, a gap around 1/2. It was a remark about these gaps that Fatou sent to Borel.

99 J'ai remarqué que la méthode de Dirichlet pour le calcul du nombre de classes de formes quadratiques binaires de déterminant donné, conduit, lorsqu'on l'applique aux formes d'Hermite définies, à une expression simple du nombre de classes où ne figurent que les facteurs premiers du déterminant. Les calculs, résumés dans une note des comptes-rendus [*sic*], ont été communiqués par moi à G. Humbert qui les a développés dans son cours du collège de France, et qui a étudié ensuite des problèmes analogues plus étendus.

100 In his speech during Pierre Fatou's funeral, Henri Mineur [1929] would take sides, declaring:

> To one of his most important discoveries, that is, that of iteration of rational fractions, Fatou's name is attached for ever; he was the first one who dared to attack this problem, he was also the first to have solved it. [À l'une de ses plus importantes découvertes, c'est-à-dire à celle de l'itération des fonctions rationnelles, le nom de Pierre Fatou reste indéfectiblement attaché; c'est lui qui a le premier osé aborder ce problème, il est aussi le premier à l'avoir résolu.]

101 The case of a meromorphic function with an essential singularity at infinity is much more complicated!

number of questions[102], for instance, are there entire functions whose Julia set is the whole plane **C** (as in Lattès' examples)?

To these papers, we must add the communication (of January 28th 1920) [Fatou 1921e] and the article [Fatou 1923a], in which Fatou considers a family of open sets

$$\cdots \subset D_n \subset D_{n+1} \subset \cdots$$

whose boundaries are analytic arcs, together with their union $D = \cup D_n$. Under the assumption that we reach a point of ∂D_{n+1} starting from a point of ∂D_n by an arc contained in D_{n+1} of length u_n with $\sum u_n < +\infty$, he shows that there are accessible (by arcs of finite length) points[103] on the boundary of D. As we have seen (page 107), the question of the accessibility of the boundary points of the "Fatou components" is asked in [Fatou 1920a, p. 51]. The question under consideration here indeed comes from iteration, in the case where, starting from D_0, a small disc centred at an attracting fixed point, $D_1 = R^{-1}(D_0)$ is constructed (for the branch of R^{-1} such that $D_1 \supset D_0$, and so on)[104]. The article is classified as "general topology", or more exactly as *Mengenlehre. Allgemeines und abstrakte Mengen. Punktmengen*, by Plancherel in his review for the *Jahrbuch*[105].

And what remains

Between the two focal points consisting of Fatou's thesis and his work on iteration, we must mention the article [Fatou 1921c], in which Fatou investigates the set of "zeros" on the boundary S^1 of a function f holomorphic inside the unit disc D. The announcement he gave during the December 22nd 1920 session of the SMF has the subtitle "At which set of points does an analytic function vanish on a singular line?" [En quel ensemble de points une fonction analytique devient-elle nulle sur une ligne singulière?]. More precisely, $A \in \mathbf{C} \cup \{\infty\}$ being fixed, he considers the three subsets of S^1:

$$\mathcal{E} = \left\{m \in S^1 \mid f(z) \to A \text{ on every path finishing at } m\right\},$$
$$e = \left\{m \in S^1 \mid f(z) \to A \text{ for } z = rm, r \in]0,1[\text{ and } r \to 1\right\},$$
$$E = \left\{m \in S^1 \mid f(z) \to A \text{ for some path finishing at } m\right\},$$

102 Many of them would only be answered in the period 1955–75; let us again direct the readers to the description of Noel Baker's work in [Rippon 2005].

103 A point a of the boundary of an open set U is accessible if there exists a continuous path $\gamma : [0,1[\to U$ such that $\lim_{t\to 1} \gamma(t) = a$.

104 This is not unrelated to the Note [Fatou 1921d] where the singularities of the boundary of a domain on which is defined a solution of a functional equation are observed.

105 The other mathematical papers of Fatou are, in general, classified with functions of one complex variable in the *Jahrbuch über die Fortschritte der Mathematik*, the reference journal at that time.

which satisfy, of course

$$\mathcal{E} \subset e \subset E.$$

When f is bounded (or takes all values except two, a case which reduces to that of a bounded function by conformal mapping[106]), we have $e = E$. In his article, Fatou studies all the possibilities for these three sets.

In the case where f is bounded on D, e has zero measure, a result of Marcel and Frédéric Riesz [1916]. Fatou had shown in his thesis that the complement has non-zero measure. He gives in [1923b] a "much faster and more elementary" [beaucoup plus rapide et élémentaire] proof of the result of [Riesz & Riesz 1916], a proof which is partly reproduced in the article [1921c] in question.

Fatou then shows that, in the general case, $\mathcal{E}$ always has zero measure, but that e and E may have any measure. For this he uses examples coming from his work on iteration, in particular a function which satisfies Schröder's equation for a rational fraction with a fundamental circle, a case where, for $A = \infty$, E and e have full measure equal to 2π. He then shows that E can have any measure. The general result he obtains is the following:

Let L be an arc of a rectifiable curve, f an analytic function defined on one side of L, and $A \in \mathbf{C} \cup \{\infty\}$. Let E be the set of points in L that are reached by a path on which $f(z)$ tends to A and let $\mathcal{E}$ be the set of points at which f is continuous and takes the value A. Then:

– if f is bounded or avoids only two complex numbers, E always has zero measure but cannot be countable and the same is true of $\mathcal{E}$,

– otherwise, $\mathcal{E}$ always has zero measure but E can have any measure and can, for instance, contain all the points of L.

Let us mention also [Fatou 1913a]. In his thesis, Fatou had proved:
If the series $\sum \sqrt{a_n^2 + b_n^2}$ diverges, the Fourier series

$$\sum (a_n \cos nx + b_n \sin nx)$$

is absolutely convergent in no interval, but can be absolutely convergent at a countable infinity of points,
a result in the proof of which he used the approximations he had obtained in [Fatou 1904c]. Lusin and Denjoy also gave proofs. Lusin has proved that the set of points of absolute convergence can only have measure 0 or 2π. In this article, Fatou gives a simple proof of Lusin's result.

It would be a pity not to quote here:

– [Fatou 1913d], a generalisation of the function $f^\star$ above, which he would use in his memoir on iteration [1920b],

– [Fatou 1922f], in which he studies the singular points of the inverse of a function invariant under R,

106 As we pointed out in § I.5.

– [Fatou 1924c][107], in which he studies the monodromy of a differential equation (a reappearance of Fuchsian groups; besides, this article is the only paper of Fatou which is mentioned in [Appell et al. 1930]),

– [Fatou 1925b] where the uniformisation of a multi-valued function of the same type as the inverse of a Fuchsian function is studied (this Note may have been inspired by the writing of [Appell et al. 1930]).

Appell & Goursat

Fatou also spent a lot of time revising (for its second edition) the first volume and writing the 521 pages of the second volume of the book [Appell et al. 1930]. Of this second volume, which is called, according to its cover, "Appell & Goursat-volume-2", the author, the sole author, was Pierre Fatou.

We were not able to find, on this "second edition" and this "second volume", more information than that given by Mineur and that which can be gleaned from the reading of the books themselves.

The corrections and additions made by Fatou in the first volume concern mainly the introduction, which increased from ten to thirty-three pages. He explains this at the beginning:

> We give however in the final sections of this introduction the proofs of some principles in the theory of functions that cannot be found in general in the classical books and that we shall need for the exposition of some recent research in the second volume of these lessons[108].

This concers § 8 to 16 of this new introduction, in which he describes in particular the solution to the Dirichlet problem using the Poisson formula and he uses it to prove that an analytic mapping which maps a disc into another disc and which is such that the inverse image of any point consists of n points is rational of degree n. § 15 and 16 are devoted to "sets of points" (to the Borel-Lebesgue theorem and the non-countability of the continuum (Cantor)).

Here is what Goursat[109] himself says in the introduction of the second volume:

> When the company Gauthier-Villars suggested to us that they publish a new edition of the "Theory of algebraic functions", which was sold out a

[107] Volume 52 of the *Bulletin* of the SMF, in which the article [Fatou 1924c] is published, is the fiftieth anniversary volume. It contains papers written by the cream of French mathematics... among others, in addition to Fatou, one finds Julia, Paul Lévy and Montel.

[108] Nous donnons toutefois dans les derniers paragraphes de cette introduction les démonstrations de quelques principes de la théorie des fonctions que l'on ne trouve pas en général dans les traités classiques et dont nous aurons besoin pour l'exposé de certaines recherches récentes exposées dans le 2e tome de ces leçons.

[109] In the 1920's, the mathematical Notes of Fatou in the *Comptes rendus* were presented by Édouard Goursat.

> few months ago, we felt it would be useful to complete it with at least a basic summary of the theory of Fuchsian functions. The late lamented Fatou agreed to take responsibility for this work. But, instead of writing the few chapters we expected, it is a true handbook on automorphic functions that he has left us. We are happy to have thus caused the publication of a masterly work, which the excessive modesty of the author might have prevented him from writing. Young French mathematicians will now have the opportunity to easily learn this vast theory, due to our great Henri Poincaré, and this reading will only increase the regret inspired in all those who knew and appreciated him by the premature death of Fatou, of whom science was still expecting much.
>
> The manuscript was entirely written by Fatou, who gave the very last pages to the printer a few months before his death. Our colleague M. Drach was kind enough to read the proofs of the final chapters. We address to him our great thanks; we also address them to M. Henri Mineur who played an active part in the proof correction[110].

The "excessive modesty" of a deceased author is probably a good enough reason to publish a book written by him without putting his name on the cover...

Fatou only finished writing this book a few months before his death.

> But I promise his family and his friends that the book will appear[111],

Mineur [1929] promised. Jules Drach and Henri Mineur were indeed the ones who reread the proofs.

⋆

Thinking of the remarks III.2.1 and IV.5.1 we made on figures, let us add that this book contains no image of a limit set.

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110 Lorsque la maison Gauthier-Villars nous a proposé de publier une nouvelle édition de la *Théorie des fonctions algébriques*, épuisée depuis quelques mois, il nous a semblé qu'il serait utile de la compléter par un aperçu, au moins sommaire, de la théorie des fonctions fuchsiennes. Le regretté Fatou avait bien voulu se charger de ce travail. Mais, au lieu des quelques chapitres que nous attendions de lui, c'est un véritable traité des fonctions automorphes qu'il nous a laissé. Nous sommes heureux d'avoir provoqué ainsi la publication d'une œuvre magistrale, que la trop grande modestie de l'auteur l'eût peut-être empêché d'écrire. Les jeunes mathematicians français pourront maintenant s'initier facilement à cette vaste théorie, due à notre grand Henri Poincaré, et cette lecture ne pourra qu'augmenter les regrets qu'inspire, à tous ceux qui l'ont connu et apprécié, la fin prématurée de Fatou, dont la Science attendait encore beaucoup.

Le manuscrit a été entièrement rédigé par Fatou, qui avait remis les dernières feuilles à l'imprimerie quelques mois avant sa mort. Notre confrère M. Drach a bien voulu se charger de revoir les épreuves des derniers chapitres. Nous lui adressons nos bien vifs remerciements; nous les adressons aussi à M. Henri Mineur, qui a pris une part active à la correction des épreuves.

111 Mais je donne l'assurance à sa famille et à ses amis que le livre paraîtra

V.6 Fatou as an astronomer

Pierre Fatou thus worked at the Paris Observatory from 1901 to his death, most of the time as an "assistant-astronomer". Assistant-astronomer was not such a minor position as its title would suggest. It was the equivalent, at the Observatory, of "maître de conférences" at a university (in the French system of today, a professor). There were also some trainees, a much more minor position (and, in particular, not paid), and Fatou himself began as a trainee-astronomer[112].

Observations

And Léon Bloch [1931]:

> For me, who followed his career as an astronomer from its beginning to its end, I know the rigourous correction with which he carried out his diurnal and nocturnal tasks. How many times did I hear his lamentation when he would see his meridian instrument out of order for a trifle—a micrometer lamp to replace—a damage of one day that the administrative formalities would prolong for weeks. How many times did I hear him being distressed about those astronomers who believed they had done enough for their science when they signed their report of attendance with cloudy weather[113].

In his 1928 notice[114], Fatou counted that he had done, for instance, between 1903 and 1908, five thousand eight hundred and ten observations, most of which were published in the *Annales de l'Observatoire de Paris* in 1903, 1904, 1905 and 1906.

But what did he observe? In astronomy,

> he observed comets, planets and occultation of stars by the moon, and, above all, he was interested in double stars. Recent research in the domain of stellar dynamics and statistics showed the importance of studying the motion of double stars, but this study is a protracted work [...] Fatou thus contributed to the arid work of positional astronomy which is the basis of the beautiful discoveries related to the constitution of the sidereal world[115] [Chazy 1932].

[112] The salaries of the astronomers: before the war, they were the same as those in the universities but, in 1926, they were less, as the director, Deslandres, complained in the annual report for 1927.

[113] Pour moi, qui ai suivi sa carrière d'astronomer depuis le début jusqu'à la fin, je sais avec quelle rigoureuse correction il accomplissait sa tâche diurne et nocturne. Combien de fois ai-je recueilli ses lamentations lorsqu'il voyait son instrument méridien hors d'usage pour une vétille, — une lampe de micromètre à remplacer, — avarie d'un jour que les formalités administratives prolongeaient pendant des semaines. Combien de fois l'ai-je entendu s'affliger de ces astronomer qui croient avoir suffisamment fait pour leur science quand ils ont signé par temps couvert leur rapport de présence.

[114] Archives of the Academy of Sciences, Fatou file.

[115] il observa des comètes, les planètes et des occultations d'étoiles par la Lune, et surtout s'attacha aux mesures d'étoiles doubles. Les recherches récentes dans le

The observations of double stars were the main part of his observations starting from 1923. The report says:

> By preference, he was interested in pairs the orbital motion of which could be evidenced, hence he chose, in general, bright stars[116];

and again, that of 1925 says:

> He continued observing bright and tight[117] double stars[118].

He would also compare his observations with the existing ephemeris.

He made these observations with the equatorial of the western tower... a telescope with which one could look at double stars or at what seemed to be double stars, the goal of the measurements being to determine whether these pairs were gravitationally bound or were not. The mechanism of the equatorial was old, its trapdoors and also the movement did not work very well. Fatou had to remedy this problem by adding more motor weights. In 1928, the trapdoors were repaired but not the dome.

Part of these observations of (visual) double stars were published in the *Journal des Observateurs* in Fatou's lifetime [1928d], others were recorded by Rose Bonnet[119], an "aide-astronomer" who worked with him from June 1928, in an article published later [1941]. Let us quote also the section devoted to Fatou in the annual report of the Observatory for 1929:

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domaine de la dynamique et de la statistique stellaires ont montré l'importance que présente l'étude des mouvements des étoiles doubles, mais cette étude est un travail de longue haleine [...] Fatou apporta ainsi sa contribution aux travaux ingrats de l'astronomie de position qui sont la base des belles découvertes relatives à la constitution du monde sidéral.

116 Il s'est attaché de préférence aux couples dont le mouvement orbital a pu être mis en évidence, aussi a-t-il choisi, en général, des étoiles brillantes

117 A tango dancer observes bright and tight pairs...

118 Il a continué l'observation des couples brillants et serrés d'étoiles doubles

119 On the advice of Fatou, Rose Bonnet started in 1928 some research on the statistics of double stars (the relation between their periods, their eccentricities and their masses). We know that she attended courses by Fréchet in 1928–29 (the Fréchet collection of the archives of the Academy of Sciences keeps some notebooks in which she took notes during these courses). She continued observations and mathematical work after Fatou's death. In 1937, she had the same "aide-astronome" grade when she obtained a grant from the Academy of Sciences for the publication of a memoir on double stars, for which we did not find any later reference. In addition to the publication of Fatou's results, Rose Bonnet defended a thesis [1945] on double stars, "binaries", on May 3rd 1945. She must have been a faithful colleague and friend of Pierre Fatou: she dedicated her thesis to the memory of her father, to her mother, to her husband and... "to the memory of Pierre Fatou, astronomer and mathematician". She continued to work after this thesis since the register of readers of the library at IHP records her registration in June 1948 (she was introduced by Fréchet) and that *Bulletin* of the SMF records her enrolment as a member of the society in 1955.

> at the equatorial of the western tower where he would measure double stars with very conscientious care and elsewhere, in collaboration with Miss Bonnet, perform some statistical research on the elements of their orbits[120].

And Henri Mineur's speech [1929]:

> He would constantly observe and his love for his science was such that, when he was nominated titular-astronomer, he himself continued observing, although the rules would have allowed him to refrain from it[121].

Moreover, it was Fatou who introduced Henri Mineur to observation, when the latter was trainee-astronomer, in 1922. They observed with the "meridian Bischoffsheim circle", then trained together in measuring double stars with the equatorial of the western tower.

If double stars where truly his domain, Fatou observed many other celestial bodies including, for instance, in 1924, a little planet that had just been discovered by the German astronomer Walter Baade on October 23rd and that was called "1924 TD Baade" or "Baade planet" but to which Baade then gave the more poetic name of Ganymede by which this asteroid has since been known. This "planet" was especially interesting because of its exceptionally large eccentricity, Fatou wrote in his 1927 notice. See [Fatou & Giacobini 1924], the observations of Fatou and Giacobini, which were published on November 10th.

Pierre Fatou also wrote theoretical papers on subjects related to his practice as an astronomer, on geometric optics and on celestial mechanics.

Geometric optics

Let us quote [1913c] (communicated to the SMF on March 19th 1913 [1913b]) which was inspired by "his daily occupations[122]" (Chazy says [1932]), and in which

> Fatou proves, with the help of the notion of optical path, in an elementary and intuitive way, the theorem of Thiesen which generalises the sine rule[123] [Chazy 1932].

More or less at the same time he was working on iteration, he published in 1917 a small article [1917a] in which he proved that the spherical aberration[124]

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120 à l'équatorial de l'ouest où il faisait avec un soin très consciencieux des mesures d'étoiles doubles; et, par ailleurs, en collaboration avec Mlle Bonnet, des recherches statistiques sur les éléments de leurs orbites.

121 Il observa constamment et son amour de sa science était tel que, lorsqu'il fut nommé astronomer titulaire, il continua à observer personnellement, bien que les règlements lui eussent permis de s'en abstenir.

122 ses occupations journalières

123 Fatou démontre, à l'aide de la notion de chemin optique, par une voie élémentaire et intuitive, le théorème de Thiesen généralisant la condition des sinus.

124 The (negative) spherical aberration is what makes the marginal light ray focus closer to the lens than the focus of the central rays.

of thin lenses still exists for lenses of any thickness. One can decrease the aberration by using a very thick lens, "but in proportions which are practically without interest[125]", Fatou said in his 1921 notice.

Celestial mechanics

Starting from 1921, Fatou was interested in celestial mechanics. He began with a study of the (asymptotic) motion of a planet in a medium where resistance depends on the position and the speed of the planet, and especially in the case where this resistance is proportional to the speed (Fatou also considers the case where it is proportional to the square of the speed). The planet then orbits in a spiral at a certain distance, which tends to become constant, from the attracting centre.

This is close to Fatou's interest in double stars: a double star would form when one of the two stars caught the other, which had ventured into its atmosphere, the latter colliding with the attracting star only after an infinite time. This work was the subject of the article [Fatou 1922a] of the *Bulletin astronomique*. It and its sequels were presented to the April 26th session of the SMF. Three *Comptes rendus* Notes followed, under the same title, Fatou's [Fatou 1922b], the generalisation [Chazy 1922] by Chazy a few weeks later and finally a second Note [Fatou 1922c] by Fatou. Then came the article [Fatou 1923d].

Let us cite a few more Notes: on the motion of a material point under the attraction of a flattened spheroid [1925a] (results which apply to the study of satellites), on the motion of the nodes of certain orbits [1927b ; 1928c], on the motion of the perihelion of the planets [1928a], and also the Note [1927a] which announced the future research of [1928e].

His article [1928e] took a much more prominent place than iteration in the section [Nathan 1971] that the *Dictionary of Scientific Biography*[126] devoted to Pierre Fatou. This is a truly mathematical article, about the system of two planets rotating around the sun, with a genuine application to celestial mechanics:

> [...] to compute the perturbations caused by Jupiter on the heliocentric coordinates of Neptune, there was no disadvantage in replacing the attraction of Jupiter by its mean value, namely this planet can be substituted by its Gauss ring in the computation of the secular inequalities[127] [Fatou 1928e, p. 128].

[125] mais dans des proportions qui sont pratiquement sans intérêt

[126] In this edition of DSB, there is no mention, either of Julia, or of Montel, who, by the way, were both still alive... Montel appeared later, in the supplements to this Dictionary [Dieudonné 1990]. The article on Fatou concentrates on his work in celestial mechanics. In speaking of his work on mathematics, if the word "iteration" does actually appear, it is only topic his results on Taylor series that are mentioned—iteration was not already fashionable!

[127] [...] pour calculer les perturbations produites par Jupiter sur les coordonnées héliocentriques de Neptune, il n'y avait pas d'inconvénient à remplacer l'attraction

Indeed, since Gauss, it has been standard in computation to replace the perturbing planet by its mass distributed around its orbit (Gauss ring), but without any justification except the heuristic one. Fatou thus gave a mathematical justification for this practice. His results are based on the study (announced in the Note [1928b]) of the behaviour of the solutions of a differential system

$$\frac{dx_i}{dt} = f_i\Big(x_1, x_2, \frac{t}{\varepsilon}, \varepsilon\Big), \quad i = 1, 2$$

(in which the f_is are periodic in t/ε and can be expanded in Fourier series) when ε tends to 0.

After his death, his colleagues found in his papers some completed articles. They published the Note [1929]. The most important of his works on celestial mechanics was written three months before his death. This was published posthumously [1931]; it concerns the motion of a material point in a fixed gravitational field.

V.7 Teaching and candidatures of Fatou

With regard to his participation to the Great Prize of 1918, we have already mentioned that Fatou might have wished to apply for a position of mathematician. Already in his report on the thesis, in 1907, Painlevé concluded by hoping that the young man would remain a mathematician:

> I thus conclude the acceptance [of the thesis] by expressing the wish that this will be an encouragement for M. Fatou to continue his work, of which it is legitimate to expect that it will contribute some day to the honour of French science[128] [Gispert 1991, p. 398].

According to Bloch, Fatou almost always refused to have the ambition of getting a teaching position:

> Not because he was unfit for it, as some dubious judges would clumsily try to make people believe. The opposite is obvious from the success he made of the few classes he devoted, at the School [École normale supérieure], to the agrégation candidates. But he confessed himself that teaching for an exam was not his thing. The often disciplinary role this teaching involved was not suited to his physical abilities, even less to his originality. A truly superior teaching opportunity, addressing an elite, would have suited him perfectly

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de Jupiter par sa valeur moyenne, c'est-à-dire cette planète par l'anneau de Gauss qu'on peut lui substituer dans le calcul des inégalités séculaires.

128 Je conclus donc à son acceptation [de la thèse], en exprimant le vœu que ce soit pour M. Fatou un encouragement à continuer ses travaux dont on peut légitimement espérer qu'ils contribuent un jour à honorer la science française.

> and would have honoured those who had entrusted this task to him[129] [Bloch 1931, p. 54][130].

Whatever his interest in celestial mechanics was, whatever his reticence with respect to teaching, it is acknowledged that Fatou tried to obtain a position of professor, at the Sorbonne, at the ENS and at the Collège de France. We have a few allusions to this[131] in his letters to Montel.

> I went to see Picard this morning at the end of his lecture, as I had decided. He gave me the best of welcomes and he seems to be determined to support me; I explained to him that I had some hesitation in applying for this position for health reasons; he told me that this had indeed to be considered, but that, apart from this, my place was at the Sorbonne, not at the Observatory. He told me on the other hand that Denjoy's application had some supporters, but that he did not know the opinion of some of his colleagues, in particular that of Vessiot and he advised me to see them. I am thus going to pay a visit to test the waters; I shall apply only if there is a comfortable majority in my favour and some goodwill from those who would not vote for me, so that I would be able to ask for not too hard a duty, otherwise I give up.

This letter[132] was not dated, but it was written between 1920 and 1922 (see the note 46 page 285). The issue was most probably to replace Painlevé, who had been elected deputy in Parliament, at the Sorbonne (see the appendix).

> [...][133] you can take it that I am not applying, but don't say so yet; I will take a few more days to think and I could change my mind if I was to obtain more favourable information on the particulars of the duty, but this is rather

[129] Non qu'il y fût impropre, comme s'efforçaient assez maladroitement de le faire croire quelques juges douteux. Le contraire résulte à l'évidence du succès qu'il recueillit dans les quelques conférences qu'il consacra à l'École aux candidats à l'agrégation de mathématiques, comme aussi des brillantes communications faites par lui devant la Société mathématique de France. Mais il avouait lui-même que l'enseignement d'examen n'était pas son fait. La partie scolaire, souvent disciplinaire, que cet enseignement comporte n'était pas à la mesure de ses aptitudes physiques, moins encore à celle de son originalité. Un enseignement vraiment supérieur, s'adressant à une élite, lui eût parfaitement convenu et eût honoré ceux qui lui en eussent confié la charge.

[130] Bloch, who was himself a faculty member, was not an ingenuous observer. He knew exactly what he was talking about. He was thinking of a particular position (at the ENS? at the Collège de France?). And he also thought of particular colleagues, who could have given Fatou this position. In the same way he knew exactly whom he had in mind when he mentioned the "dubious judges" or when he expressed surprise that a man like Fatou could have enemies (page 151).

[131] It is clear in these letters that what interests Fatou primarily was mathematics.

[132] Montel collection, archives of the Academy of Sciences. See the complete letter in the Appendix.

[133] Fragment of an undated letter, Montel collection, archives of the Academy of Sciences. See the appendix. Let us remind the reader that Ernest Vessiot was the scientific director of the ENS until 1935.

> unlikely since Vessiot, who is the greatest authority in these matters and who seems kindly disposed, has given me a glimpse of the duties which do not really suit me.
> On the other hand, Vessiot gave me advice that I decided to follow although this has no longer, in accordance to what precedes, any positive relevance for me, that is to print a notice. Speaking of which, I found in my notes a certain number of results, some of which might not be without value, but some of them might not be new [...][134]

In 1921, Fatou sent a notice to print[135] and he applied to the Collège de France. The minutes of the assembly of professors, on June 2nd 1921 (kept by the archives of Collège de France) say:

> Professor Hadamard revealed the qualifications of the two applicants who both appeared to him able to honour the Collège de France by their teaching, but suggested however putting M. Lebesgue in first place and M. Fatou in second. These words were supported by M. Langevin[136].

The assembly of professors followed the suggestion. Likewise, the Academy of Sciences ranked Lebesgue in first place and Fatou in second (as the *Comptes rendus* showed, on July 11th 1921). Lebesgue accepted the position which he held until the end of his life in 1941.

Fatou did not give up on obtaining a position at the Sorbonne.

> I received a letter from Lebesgue the contents of which you probably know. On the other hand, I went to see Goursat, who advised me to wait and not say yet that I am not applying.
> I return to my letter which was interrupted, in fact, by the arrival of Lebesgue, who had come to tell me on behalf of Goursat that Picard agreed with Julia that people should give me a teaching duty that would suit me and that, in those circumstances, he advised me to apply. I thus believe that this time I will not be able to shy away; on reflection, it seems to me that I can try this experiment especially if, being simply delegated at the Sorbonne to replace Painlevé, I would be on leave from the Observatory, this would allow me, I think, to return there if, for instance, after a year I see that I am not in a fit state to continue at the Sorbonne. I must clarify this point, which is very important to me[137].

If we do not know for sure whether Fatou did indeed apply on this occasion (to replace Painlevé, busy with his political career), it is known that he did in 1927. For instance, Weil thought he remembered in [1992, p. 57] (the scene took place in 1928):

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[134] And, as always, Fatou started speaking of mathematics. See the complete letter page 282.

[135] To the printer Téqui, rue de la Sablière—and not to Gauthier-Villars, the appointed printer of the Academy of Sciences.

[136] Le Professeur Hadamard expose les titres des deux candidats qui lui paraissent l'un et l'autre capables de faire honneur au Collège de France par leur enseignement et propose toutefois de mettre en 1ère ligne M. Lebesgue et en 2ème ligne M. Fatou. Ces propos sont appuyés par M. Langevin.

[137] Letter to Montel, on page 285.

> He [Lebesgue] told me to wait for him, went off to exchange a few words with Picard, and returned to me: "Do you have your manuscript?" "Yes." "Take a taxi to Monsieur Garnier's and tell him that Monsieur Picard wishes him to take charge of your thesis." Garnier had just been appointed to a post at the Sorbonne; without Picard's decisive influence, it was said, another candidate (rumor had it that this was Fatou) could and would have received the appointment.

It is certainly to the reputation of Fatou as a teacher (a reputation due in particular to Picard himself) that Picard alludes in a letter[138] to Garnier, shortly after his election at the Sorbonne, on January 20th 1928:

> My dear friend,
> I see that you have been given some work, and that perhaps an argument I gave in your favour was misused, namely that, contrary to your competitor, one could entrust you with any teaching duties[139] [...]

Two digressions would be possible here, the first one concerning the amazing success of Garnier, which will be found page 206, and the other one on the impressive power of Picard, which will be found page 207.

Fatou titular-astronomer

But let us return to Pierre Fatou and to his career as an astronomer. A position of titular-astronomer was vacant in 1927. The committee which was nominated by the Academy of Sciences (because here, too, it was the Academy of Sciences which took the decision[140]) proposed, during its meeting in secret committee on May 30th, to rank Pierre Salet in first place and Armand Lambert[141], in second, thus reversing the proposition of the Council of Observatories. A discussion then arose "on the qualifications of the two candidates and on that of M. Fatou, who was not on the list[142]". Andoyer, Deslandres and Picard gave an opinion in favour of Lambert, while Perrin, Hamy and Cotton were for Salet, and Bigourdan and Lebesgue proposed to nominate Fatou. The vote

[138] Archives of the Academy of Sciences, Garnier collection. This collection also contains a few (very friendly) letters from Picard to Garnier—in which, in general, Picard asked a favour.

[139] Mon cher ami, Je vois que l'on vous fait travailler, et que l'on a peut-être abusé d'un argument que j'avais donné en votre faveur, à savoir que, contrairement à votre concurrent, on pouvait vous confier tous les enseignements

[140] According to [Saint-Martin 2008, p. 58], the Observatory is a by-product of the Academy of Sciences.

[141] Armand Lambert was already ranked first for a position as a titular-astronomer... in 1920. It is true that he was in charge of a course at the Sorbonne. Note that this astronomer had contributed articles on spherical functions to the *Encyclopédie*.

He would be the acting director of the Observatory during the German Occupation, before he was deported and murdered in Auschwitz.

[142] sur les titres des deux candidats et sur ceux de M. Fatou, qui n'est pas sur la liste

of the Academy of Sciences in the public session one week later gave a list with Pierre Salet in first place and Fatou second.

The following year, on June 25$^{\text{th}}$ 1928, Fatou was proposed in first place for a position of titular-astronomer, which was vacant at the Observatory. The Council of Observatories presented, in first place Lambert, and in second Fatou, but the Academy of Sciences reversed the grading[143]. He was thus appointed on July 7$^{\text{th}}$ 1928.

> This is how time achieves in thirty years what merit would easily do in ten[144]. [Bloch 1931][145]

Let us quote once again the 1929 annual report of the Observatory:

> settled in his career by his recent appointment as a titular-astronomer, and freed, here, of what he called a heavy concern, he was undertaking projects of work in the quietness of a new kind of life[146].

Bloch said, as we mentioned on page 116, that Pierre Fatou was preparing to train a new generation of astronomers.

Fatou as an Academician?

In the speech he made during Pierre Fatou's burial, Henri Mineur [1929] said in particular:

> The Academy of Sciences would without doubt have called him next October[147].

It is impossible to know on what Mineur's affirmation was based. Apparently, Picard himself spoke of this election at the Academy of Sciences to the admiral Louis Fatou. The latter wrote to his cousin Étienne Fatou, shortly after Pierre Fatou's death, on September 2$^{\text{nd}}$ 1929 (concerning this letter, see also below):

143 Both in 1927 and in 1928, the proposition appeared, in the *Comptes rendus*, with a mistake in Fatou's first name: he is called "Paul Fatou". However, there was an *erratum* the first time.

144 According to the tables made by Arnaud Saint-Martin [2008, p. 165], Fatou's promotion would be typical: the average age for promotion to titular-astronomer was (in 1910) 51 (and 56% of assistant-astronomers would stay in this grade forever). A kind of promotion by seniority, therefore, for our brilliant scientist.

145 C'est ainsi que le temps arrive à faire en trente ans ce que le mérite ferait facilement en dix.

146 tranquillisé sur le sort de sa carrière par sa récente nomination comme astronomer titulaire et débarrassé de ce côté de ce qu'il appelait un lourd souci, il faisait des projets de travail dans le calme d'une sorte de vie nouvelle.

147 L'Académie des sciences devait sans nul doute l'appeler à elle, au mois d'octobre prochain.

> [...] and I learned from M. Émile Picard, through chief of official science who nevertheless took quite a long time to discover this over-modest scientist[148]—that he was sure of his election at the Academy of Sciences before the end of the year[149].

An election was indeed planned before the end of the year (in November rather than in October as Mineur had said), to replace Pierre Puiseux, in the astronomy section. There is nothing, either in the biographical file of Pierre Fatou, or in the register of secret committees, to confirm the assertions made either by Mineur in his August speech, or by Picard when he spoke with Louis Fatou. Fatou had indeed sent a new "Notice on scientific work" to the Academy of Sciences, but it is dated 1927 (and mentions only work prior to 1927) and seems to correspond rather to his application for a titular-astronomer[150]. The *Comptes rendus* contain no mention of the fact that Fatou officially announced his candidature for an election. Moreover, his name never appeared in any previous election.

One can say that the election, which was planned for the end of the year, in 1929, was much delayed (Pierre Puiseux died on September 28th 1928). On February 25th, there was already a question of moving the election closer[151]

> to stop the intrigues to which this prolonged vacancy had given birth[152].

The reason for this delay seems to be the fact that a director for the Paris Observatory had also to be chosen[153], and that this choice had a chance of

[148] There was bitterness in Louis Fatou's comment, and although he did not know the mathematicians well, he understood very well that his brother was not acknowledged for his true value.

[149] et j'ai su par M. Émile Picard, grand pontife de la Science officielle qui avait toutefois mis bien du temps à découvrir ce savant trop modeste — qu'il était assuré de son élection à l'Institut avant la fin de l'année

[150] There might have been a third version of Fatou's notice, possibly in relation to an application. This is what the page numbering used by Montel in a letter quoted below (page 229) suggests. The bibliography of [Alexander 1994] includes the mention of a 1929 notice without further detail. This version, if it indeed existed, is not in the Fatou file at the archives of the Academy of Sciences.

[151] Register of secret committees, archives of the Academy of Sciences.

[152] afin de mettre un terme aux intrigues auxquelles cette vacance trop prolongée a donné naissance

[153] Baillaud retired in 1926 and Deslandres replaced him for two years. It was Ernest Esclangon, the director of the Strasbourg Observatory, who would become the director in 1929. Ernest Esclangon was one of those who had the idea of creating the AAPSOF in 1909, he was its secretary in 1911 (when Fatou was the president); we saw (Chapter I, note 63) that he did some research on sound during the war; together with Fréchet, he was one of the "missionaries" sent to "found" the University of Strasbourg in 1919; he is also, among other things, the one who invented the speaking clock. We also note that he was a fellow pupil of Montel in the "special mathematics" class at Nice, and then, with one year difference, his fellow also at the ENS; they remained close friends.

bringing to Paris an astronomer from the provinces, who would be, at the same time, eligible to replace Puiseux (to be elected as an academician, one had to live in Paris). These men discussed the question of the date of the election again on March 4th, then on June 17th, they postponed the election to November 18th. All this seems to show a will to elect Esclangon and, for this, to wait until he had left Strasbourg. Reading Picard's letters to Lacroix[154] seems to confirm that there was no question of Fatou for this election (he wrote about the election, but never mentioned Fatou's name). On June 25th, for instance, Picard announced to his correspondent the appointment, the day before, of Esclangon as the director of the Observatory, which

> simplifies Puiseux' succession. But there is still Andoyer[155].

This could be the key to the mystery. Henri Andoyer had just died, on June 12th, leaving another vacancy in the astronomy section. It was indeed Esclangon who would get Puiseux' place. However, Mineur, who was not an academician, seemed to be, as early as August, very positive about Fatou's chances. It is likely that Picard briefly—between June 25th, the date of his letter to Lacroix, and Fatou's death on August 9th—considered electing Fatou, rather than a classical astronomer, to Andoyer's seat. Indeed, here is what he wrote to Pierre Gauja on August 22th:

> Poor Fatou died in Pornichet, where he was spending his holidays. He was a good friend of M. Guillet, who was also in Pornichet and who took care of Fatou during his last days. French astronomy is certainly in a bad state; after M. Esclangon, we must take physicists or mathematicians in the section, if we do not want M. Jules Baillaud and M. Salet[156].

The election to Puiseux's place would occur on November 25th and it was indeed Ernest Esclangon, the new director of the Observatory, who was elected. As for the next election, to replace Andoyer, it did not take place "before the end of the year", but the following year. The Academy would add the name of Charles Maurain—a geophysicist—to those presented by the astronomers and he was the one who was elected (first ballot), on May 12th 1930.

V.8 Fatou and other mathematicians

Let us now summarise what we know about the relations of Fatou and his fellow mathematicians. Even if he liked solitude, Pierre Fatou was a sociable

[154] Picard file, archives of the Academy of Sciences.

[155] simplifie la succession de Puiseux. Mais il reste Andoyer.

[156] Le pauvre Fatou est mort à Pornichet, où il passait ses vacances. C'était un grand ami de M. Guillet, qui était aussi à Pornichet et a soigné Fatou dans ses derniers jours. Décidément l'astronomie française est mal en point; on mettra, après M. Esclangon, des physiciens ou des mathématiciens dans la Section, si on ne veut pas de M. Jules Baillaud et de M. Salet.

man and he had friends (see again [Bloch 1931]). We have seen, regarding the AAPSOF, that he would participate to the social life of the astronomers. He would also participate in the social life of the mathematicians, at least in France.

International congresses

Pierre Fatou's name does not appear in the lists of participants of the international congresses that took place during his period of activity: Heidelberg in 1904 (at that time he was very young), Rome in 1908, Cambridge in 1912, Strasbourg in 1920, Toronto in 1924, Bologna in 1928. We have seen however that he liked to travel. Thus it might have been more because of his obligations as an astronomer than because of his precarious health that he did not participate in these congresses. Travelling to Toronto or Bologna may have been too expensive. Or perhaps it is simply because he was not interested in such meetings: Lebesgue did not participate either to any international congress. In contrast, Montel went to Rome, Cambridge, and Strasbourg.

Places to meet

Where did the mathematicians meet each other? They used to go to listen to lectures at the Collège de France, at the end of which they would have discussions. Paul Lévy [1970, p. 37] reported, for instance, a discussion he had with Humbert after one of his classes:

> [...] when, at the end of the third lecture, he suggested to me a subject for a thesis, I could only refuse [...] It was only in the following winter[157] that I could tell Humbert that I was working on functions of lines[158]. Fatou was present and said: "This is very nebulous; there are no precise results". I answered that I pretended to have precise results and I mentioned one of them. Humbert made a kind remark to repair Fatou's blunder[159].

They would listen to a talk, here and there; for instance, we know that Montel, Fatou and Lebesgue (among others) attended a lecture by Russell at the École des Hautes études sociales on March 22$^{\text{th}}$ 1911 [Lebesgue 1991, letter dated March 23$^{\text{rd}}$ 1911][160].

To start with, there was no seminar in Paris. That of Hadamard at the Collège de France began in 1913, and then ran from 1920 (until Hadamard's

[157] According to the context, this would take place around 1908.

[158] This would today be called functional analysis.

[159] quand, à la fin du troisième cours il me proposa un sujet de thèse sur son cours, je ne pus que refuser [...]. Ce n'est que l'hiver suivant que je pus dire à Humbert que je travaillais à une thèse sur les fonctions de lignes. Fatou, qui assistait à l'entretien, dit: "C'est bien nuageux; il n'y a guère de résultat précis." Je répondis que je prétendais avoir des résultats très précis et en indiquai un. Humbert eut un mot aimable pour réparer la gaffe de Fatou.

[160] This event was organised by the SMF, who even published the text [Russell 1911].

retirement in 1937). It would be very surprising if Fatou never gave a talk there, especially because, as we have seen it, Hadamard had a high regard for his work—but we found no evidence that he gave one. It would be even more surprising to learn that he did not attend the seminar. In any case, it is certain that Julia spoke there and it is more than probable that they met there.

Let us recall that at that time (and even long after), there was no mathematical "laboratory" and the mathematicians had no offices—they would work at home.

They would meet less, but they would write more. We have already quoted Lebesgue's [1991] and Baire's [1990] letters. They would also continue by letter a discussion begun at the end of a lecture (see for instance [Taylor & Dugac 1981]). The Borel collection at the archives of the Academy of Sciences contains only one letter of Fatou (although not dated, it si certainly of 1904), the one mentioned in § V.5. He certainly wrote to Lebesgue, but the latter did not keep his correspondence. He probably also wrote to Julia...

The sessions of the SMF

In those years, the SMF compensated the absence (the non-existence) of seminars by organising regular sessions the written traces of which we have already mentioned. After these talks as well, people would discuss. Let us listen once more to Paul Lévy, who tells us (in [1970, p. 21]) about a discussion after his talk of March 11th 1908:

> Fatou said more or less: "you seem not to know measure theory (which was true); you just have to say that the property you show is true except on a set of values of t of measure zero"[161].

It is remarkable that the only two discussions reported by Paul Lévy (more than sixty years after) and which we have repeated here involve Fatou, thus confirming the presence of this mathematician at the heart of the life of the community—and the pertinence of his interventions.

The activity of Pierre Fatou at the Société mathématique de France is acknowledged. It was important and constant. Pierre Fatou was elected as a member of the SMF on December 15th 1904, his application being presented by Désiré André[162] and Émile Borel. He immediately began to participate in

[161] Fatou avait dit à peu près: "Vous ne semblez pas connaître la théorie de la mesure (ce qui était vrai); il n'y a qu'à dire que la propriété que vous indiquez est vraie sauf sur un ensemble de valeurs de t de mesure nulle."

[162] Désiré André was the first treasurer of the SMF when this society was created in 1873 and he was its president in 1889. Hermite mentions him as one of his students in a letter to Stieltjes (dated February 13th 1886) [Baillaud & Bourguet 1905a]. This combinatorialist left his name to a symmetry principle that all of today's probabilists know. After having taught at the University of Dijon, he was

the Thursday sessions[163]. During this year 1905 alone, he made some communications on January 5th (on the singular lines of an analytic function, corresponding to his Note [1905a] of February 6th), on January 19th (on prime numbers) and on May 18th (on the character of convergence of trigonometric series)—this one was mentioned in [Lebesgue 1906, p. 62]. We have already had opportunity to mention the November 26th 1919 communication [Fatou 1919a].

He was a very active member of this society, on the governing body of which he participated without a break from 1907 until his death. He was one of the vice-secretaries from 1907 to 1917, then the archivist[164] from 1918 to 1921, and one of the vice-presidents from 1922 to 1925. He was the president in 1926. The minutes of the public session published in Volume 54 of the *Bulletin* show that Fatou attended *every* session (he was the chair) except that of May 12th 1926 (it was only when the president was not there that the session was chaired by a secretary, Chazy in this case), which was rather exceptional. Furthermore, he gave six talks himself this year.

In the council, he had often to work with Paul Montel and Paul Lévy who had the same kind of responsibility as he. It is certain that Montel was one of his friends (regarding Paul Lévy, we lack information).

There he also met Gaston Julia, who, after having been elected a member of the SMF on March 13th 1919 (presented, as we have said, by Painlevé and, no surprise here, Humbert), was elected a member of the council in 1924. Fatou, Julia and Montel were all members of the council of the SMF in 1927 and 1928.

The term of Fatou in the council was to expire in 1930, according to the statement on the situation of the society in January 1928 which was published in Volume 56 of the *Bulletin*, but we lack information on 1929 since the statement published in Volume 57 was that of 1930 in which of course Fatou does not feature. He nevertheless appeared in this volume, both in the list of previous presidents and in the minutes of the October 23rd 1929 session:

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professor of "special mathematics" at Collège Stanislas from 1885 to 1900. He was still "honorary professor" at least until 1906. It is probably because Pierre Fatou met him when he was a pupil at this institution, that he asked him to sponsor him. We have seen that Fatou was not a pupil of Désiré André, who was the teacher of the "green class" [section verte], but a pupil of Charles Biehler, in the "blue class" [section bleue]. Désiré André certainly gave him some oral examinations [colles]. And Charles Biehler was never a member of the SMF, possibly because he was a priest. Thanks again to Nicolas Lecervoisier for the information on Collège Stanislas.

163 In the 1920's and 1930's, the sessions would take place on Wednesdays.

164 An irresistible comment. The SMF had an archivist! And it had had one since its creation! This society was very concerned with the importance of its own history... To tell the truth, very few (or even fewer) archives of this society were kept, as Hélène Gispert has already complained at the beginning of her book [1991].

> M. President announces the loss just suffered by the Society in the person of M. Fatou, a previous president, and conveys to M. Fatou's family[165] the unanimous condolances of the Society[166].

This would be the only homage paid to Fatou in a French mathematical publication...

V.9 Death of Fatou

Pierre Fatou died suddenly on August 9th 1929, at 8 pm, in Pornichet where he was spending his holidays, at the villa Brise-Lames, avenue Léon-Dubas, on the coast. He was fifty-one. This information can be read on his death certificate. Pierre Fatou was certainly in a hotel, since his nephew Robert Fatou (who was in Australia at that time) thought that his uncle "passed away in a hotel room in Pornichet[167]".

It was Florian La Porte[168], a chief hydrographic engineer of the navy, living in Lorient, who declared his death to the registry office[169] the afternoon of the following day. Florian La Porte was a cousin of Fatou (their mothers were first cousins). He was certainly on vacation himself at Pornichet with his family. Some other friends of Fatou, the Guillets[170], were also staying at Pornichet and were probably the occupiers of the villa Brise-Lames, the house where Fatou died.

Bloch does not even say precisely, in what we classed intellectual biography, what was the cause of Fatou's death, mentioning only a "final and devastat-

[165] As we know, Pierre Fatou had a brother, two sisters, nephews and nieces, and numerous cousins.

[166] M. le Président fait part de la perte que vient de subir la Société en la personne de M. Fatou, ancien président, et adresse à la famille de M. Fatou les unanimes regrets de la Société.

[167] éteint dans une chambre d'hôtel de Pornichet.

[168] Florian La Porte was a former student of École polytechnique. The town of Lorient gave his name to one of its streets in 1949, as he was "the author of remarkable work and an unceasing champion of Lorient harbour [auteur de travaux remarquables et avocat incessant du port de Lorient]".

[169] Pierre Fatou is certainly not very lucky with his first name (see Note 143): on his death certificate, he appears under the name of René Joseph Louis Fatou, a mistake by an employee who read "René" for a hand-written "Pierre".

[170] Léon Guillet (1873–1946), a metallographist, was one of the inventors, at the beginning of the 20th century, of what is now called "stainless steel". He was director of the École Centrale, from 1923 to 1944, a member of the Academy of Sciences from June 22nd 1925, and was a close friend of Pierre Fatou, as we have seen Picard mention in the letter to Gauja we quoted on page 182. As already mentioned, we do not know to which network of Fatou's acquaintances Guillet belonged. He was five years older than Fatou, so they were not fellow students. In any case, Guillet was a former student of Picard at École Centrale. On Léon Guillet, see the obituary written by Élie Cartan [1946].

ing illness[171]". There are two contradictory traditions about the cause in the family:

– on the side of the nephews, it is thought that he was poisoned by a rotten sardine tin, while

– on the side of the cousins, it was protracted sunbathing, together with a stomach ulcer, which would have caused an internal bleeding and then death.

Here is what Pierre Fatou's elder brother knew, on September 2nd 1929, when he wrote to his cousin Étienne[172] the letter, a passage from which we have already quoted above:

> My dear Étienne,
>
> I was not myself able to know—I had no opportunity to meet in Pornichet the Saint-Nazaire physician (Dr. Ozzo, I believe) who treated him—(his treatment consisting besides of only two visits) to what exactly my poor brother succumbed.
>
> I noticed that he did not look well on July 12th when he left Paris and, surprised in particular by his bilious look, I suggested that he get his liver examined—naturally careless about his health as he was, and feeling no strange symptom—except the intestinal ones he was used to—he did not listen to my advice.
>
> I learned that it was only on Wednesday August 7th that he had suffered from violent black bile vomiting. This the physician diagnosed without any great effort of imagination to be "vomito negro". The latter came back on Friday—and, seeing that the patient was not suffering any more, he thought that, although his general state was far from satisfying, at least the serious crisis was over and that there was no reason to alert the family. Poor Pierre died a few hours later, very quietly as I was told—and surrounded by his La Porte relatives and his Guillet friends, care that alas I could not give him myself—since I was only called when everything was over.
>
> [...]
>
> Under the parchment-like skin of this silent, clumsy, shy, distracted astronomer—there was a good heart and, I believe, behind his forehead—a first-rate brain. All his fellows seem to be unanimous in saying so[173].

---

[171] dernière et foudroyante maladie.

[172] Étienne Fatou was a cousin (the son of a first cousin) of Louis and Pierre Fatou, a cardiologist, grandson of the pharmacist uncle. Many thanks again to Gladys Sérieyx for the transcription of this letter addressed to her father.

[173] Mon cher Étienne,

Je n'ai pas encore pu savoir moi-même — n'ayant pas eu l'occasion de rencontrer à Pornichet le médecin de St-Nazaire (Dr. Ozzo, je crois) qui l'avait soigné — (ses soins ne consistant d'ailleurs qu'en deux visites) à quoi exactement avait succombé mon malheureux frère.

Je lui avais trouvé bien mauvaise mine le 12 juillet à son départ de Paris et frappé en particulier de son teint jaune, je lui avais conseillé de se faire examiner le foie — naturellement insouciant comme il l'était de sa santé et ne ressentant d'ailleurs aucun symptôme insolite — en dehors de ceux auxquels il s'était habitué du côté de l'intestin — il n'avait fait nul cas de ma recommandation.

A medical consultation

According to Doctor Pierre Marinelli, who read in the documents we quoted the description of Fatou's symptoms and the letter from his brother, these symptoms indeed suggest a stomach ulcer. An ulcer causes bleeding which induces anaemia and thus a progressive tiredness, as noted in the accounts we have quoted. This blood is digested by the patient, leading to some intestinal problems (such as the ones we are told Pierre Fatou had) and even hepatic problems (including the bilious look noticed by Louis Fatou). When the bleeding becomes too great, the patient can no longer digest the blood and he vomits it. The "vomito negro" is often some partially digested blood. The strong bleeding also induces a drop in blood pressure and the anaemic patient quietly passes away, which fits very well with what Louis Fatou wrote in his letter[174].

Doctor Marinelli pointed out to us that, at that time, digestive bleeding caused by a stomach ulcer was an important cause of mortality. Although it is known today that a bacterium is the reason for the ulcer, stress and tobacco (and Fatou was a pipe smoker) are aggravating factors. The relation with sunbathing is less clear.

Here is the conclusion of the article [1932] by Chazy. Not being an astronomer who took observations himself, he apologised to those who did observe stars with Fatou, and wrote:

> J'ai su que le mercredi 7 août seulement, il avait été pris de violents vomissements de bile noire. Que le médecin a pu qualifier sans grand effort d'imagination de "vomito negro". Celui-ci était revenu dans la journée de vendredi — et voyant que le malade ne souffrait plus, il avait pensé que si l'état général était loin d'être satisfaisant, la crise grave était du moins conjurée et qu'il n'y avait pas lieu d'alarmer la famille. Le pauvre Pierre est mort quelques heures plus tard, tout doucement m'a-t-on affirmé — et entouré par ses parents La Porte et ses amis Guillet, des soins que hélas je n'ai pas pu moi-même lui apporter — puisque je n'ai été prévenu que lorsque tout était fini.
>
> [...]
>
> Sous la peau parcheminée de cet astronome silencieux, gauche, timide, distrait — il y avait un bien bon cœur et, je crois, derrière son front — un cerveau de premier ordre. Tous ses collègues me paraissent unanimes à lui rendre cette justice [...]

[174] It is absolutely certain, in any case, that it was not mathematical boredom that killed Pierre Fatou, as was suggested by Alain in the text we quoted above. The nephew himself did not believe that:

> Nothing [...] seems less likely, because I never had the impression that my uncle felt tired of life. He simply found, in life, joy different from those of the common run of people. [Rien [...] ne me paraît moins certain, car je n'ai jamais eu l'impression que mon oncle ait éprouvé le mal de vivre. Il avait simplement trouvé dans l'existence des joies différentes de celles du commun.]

> But may I remind you that, I too have collaborated with him: often, in conversations we started or ended at the Observatory library, we discussed, he and I, books and mathematical memoirs, and I bring a direct testimony to the sharpness of his mind, his critical sense and his broad scientific culture. Although five have years passed, Fatou's friends have forgotten nothing of the man he was, his moral elegance, his frankness, his reserve in conversation at first followed by the originality of his viewpoints, his taste in music, in the great sights of nature, his faithful love of Brittany where he would return often and where, still young, death came to catch him[175].

And, so that this full story of Pierre Fatou does not appear as a digression, let us mention how and from whom Léon and Eugène Bloch learned about the death of their friend. By chance, coming back from a mountain trek:

> At the turn of the path, a hundred metres from the village, we saw a silhouette in front of us, which was that of our friend Montel. To our cries of surprise and joy, he answered only by this sentence: "you know the news from Paris?" "What news", I asked, looking worried. "Fatou is dead". And this is how I learned the loss of my best friend, an immense disaster for French science[176].

Fatou Bequest

After Pierre Fatou's death, a few reprints (those of his thesis [1906c] and those of his articles and Notes [1910 ; 1922b ; 1922d ; 1922e ; 1924b ; 1926]), together with several books, were given to the Observatory library (Fatou bequest of October 21st 1929). His other books must have been given to the library of the ENS (there are few books, and moreover few mathematical books, in the Fatou bequest at the Observatory, but we know, having read [Lebesgue 1991], that he owned, among others, some volumes of the *Collection de monographies*), as is said in a letter of Louis Fatou to Paul Montel[177]:

[175] Mais je puis rappeler que, moi aussi, j'ai collaboré avec lui: souvent, dans des causeries commencées ou terminées à la bibliothèque de l'Observatoire, nous avons lui et moi discuté des Ouvrages et des Mémoires mathématiques, et j'apporte un témoignage direct de la pénétration de son esprit, de son sens critique et de sa vaste culture scientifique.

Malgré cinq années les amis de Fatou n'ont oublié en rien l'homme qu'il était, son élégance morale, sa franchise, la réserve d'abord de sa conversation, puis l'originalité de ses points de vue, son goût pour la musique, pour les grands spectacles de la nature, son amour fidèle de la Bretagne, où il retournait fréquemment, et où, jeune encore, la mort est venue le surprendre.

[176] Au détour du sentier, à cent pas du village, une silhouette se dessinait devant nous, c'était celle de notre ami Montel. À nos exclamations de surprise et de joie, il ne répondit que par cette phrase: "vous connaissez les nouvelles de Paris?" "Quelles nouvelles?", fis-je d'un air inquiet. "Fatou est mort". Et c'est ainsi que j'ai appris la perte de mon meilleur ami, l'immense désastre porté à la science française.

[177] Montel collection, archives of the Academy of Sciences.

Paris Hôtel de l'Avenir, 65 rue Madame 6e

Saturday, October 12th [1929 and (Littré 84-54), added by Montel]

Dear Sir

Having been too absorbed in the sad duties I am taking care of in Paris I could not tell you with all the eagerness I would have wished how I was touched by the sympathy you expressed to me in such affectionate and (for him) laudatory terms after my brother's death. Besides, I still think I may succeed in thanking you personally before I leave. For it is a great comfort to me in the hardship I am going through to hear from eminent people who gave him their esteem, their advice and their friendship. I could see when looking at the correspondence of my brother, that you were especially of their number. I know he was very grateful for this.
Please believe that I willingly accept the heritage and please accept, dear Sir, with my high consideration the expression of my faithful regards.

L. Fatou

I will certainly follow the advice M. Bloch was so kind to solicit from you regarding the best use of the scientific books I found on my brother's bookshelves.
However, I saw when I visited the Observatory that a lot of the missing books were in his office there, and since I had no time to restart the classification, I eventually asked two of his colleagues (M. Mineur and Melle Bonnet) to be so kind as to take what I had recovered from my brother's home, to reorganise the collections and then to send them to the École normale, after having kept the books they wanted as souvenirs.[178].

[178] Le samedi 12 octobre [1929 et (Littré 84-54) ajoutés par Montel]

Cher Monsieur

M'étant laissé absorber plus que de raison par les tristes devoirs dont je suis venu m'acquitter à Paris je n'ai pas pu vous dire avec tout l'empressement que j'aurais voulu y mettre combien j'ai été sensible aux sympathies qu'en termes si affectueux et élogieux pour lui vous avez eu l'aimable attention de m'exprimer à l'occasion de la mort de mon frère. Je ne désespère d'ailleurs pas de réussir avant mon départ à aller vous en remercier de vive voix. Car ce m'est un grand réconfort dans l'épreuve que je traverse d'entendre parler de lui par les hommes de haute valeur qui voulaient bien lui accorder leur estime et l'entourer de leurs conseils et de leur amitié. J'ai pu constater en dépouillant la correspondance de mon frère que vous étiez tout particulièrement de ce nombre. Je sais qu'il vous en gardait une profonde reconnaissance.

Veuillez être persuadé que j'en accepte bien volontiers l'héritage et agréer cher Monsieur avec l'assurance de ma haute considération l'expression de mes sentiments les plus cordialement dévoués.

L. Fatou

Je ne manquerai pas de me conformer à l'avis que Monsieur Bloch a eu l'amabilité d'aller solliciter de vous au sujet de la meilleure destination à donner aux ouvrages scientifiques que j'ai trouvés dans la bibliothèque de mon frère.

There was indeed a Fatou bequest to the library of the École normale supérieure, where there are, for instance, a copy of the book of Baire [1905][179], and also copies of the theses of Paul Flamant and Philippe Le Corbeiller, which once belonged to Pierre Fatou. Despite the help I was given by Odile Luguern and her colleagues, I was not able to make an exhaustive inventory of this bequest: it seems that there were no inventory registers in this library and, if the note "Fatou bequest" had possibly appeared on the index cards, it was not kept when the file was computerised.

We found no traces, either of the papers[180], or of the photo albums of Pierre Fatou. The letter shows, however, that Fatou kept his correspondence, or at least part of it.

Obituaries on Fatou

One might be surprised that the article of Chazy, which appeared in a journal dated 1932, mentioned that five years had passed since Fatou's death. This article was probably written in 1934 and the journal in which it appeared might have been late. It seems that Chazy had waited for Lebesgue. Lebesgue had spoken of writing an obituary of Fatou for the *Bulletin des sciences mathématiques*, and Chazy had given him some references (he knew Fatou's work on celestial mechanics very well). When Chazy was anxious about the publication of this obituary and asked the secretary of the Academy of Sciences, Picard asked the secretary to reply that he had not heard of it. In November 1933, Lebesgue said he was not ready and advised Chazy not to wait for him. As for Picard, he was disinclined to publish an obituary in the *Bulletin des sciences mathématiques* such a long time after Fatou's death[181]. Too early for Lebesgue, too late for Picard... Chazy wrote his paper and published it in an Astronomy journal.

Toutefois ayant constaté après une visite à l'Observatoire qu'un grand nombre des tomes qui manquaient pour certains d'entre eux se trouvaient dans son ancien bureau et manquant de temps et de moyens pour recommencer les classements que j'avais entrepris, j'ai dû me décider à prier deux de ses collègues (M[r] Mineur et M[elle] Bonnet) de vouloir bien faire prendre ce que j'ai recueilli au domicile de mon frère, reconstituer les collections et ensuite les adresser à l'École normale après avoir prélevé les livres qu'ils désireraient conserver à titre de souvenirs.

[179] I found this copy... opening all the volumes in the shelf: fortunately, the books of Borel's *Collection de monographies* are kept together in this library—besides one can find there books that belonged to other people mentioned in this book, an (other) copy of [Baire 1905] on which the names of Paul Flamant and Gaston Julia can be read, a copy of [Borel 1901] that belonged to Paul Lambert...

[180] As we have seen, Fatou's late mathematical papers have been published, probably by Henri Mineur and Rose Bonnet.

[181] Reference for this section: Chazy's correspondence with the Permanent secretary, Fatou file, archives of the Academy of Sciences.

PIERRE FATOU,

PAR M. JEAN CHAZY.

Pierre Fatou est né à Lorient le 28 février 1878. Il fut élève
à l'École Normale de 1898 à 1901, passa l'agrégation des Sciences
mathématiques en 1901. Au mois de novembre de la même année,
il fut admis comme astronome stagiaire à l'Observatoire de Paris,
qu'il ne quitta plus jusqu'à sa mort. Il devint aide-astronome en
janvier 1904, astronome adjoint en avril 1904; en 1928, au bout
de vingt-quatre ans, il fut nommé astronome titulaire. Il mourut
l'année suivante, le 10 août, à l'âge de 51 ans, à Pornichet.

Fatou participa pendant une vingtaine d'années aux travaux
d'astronomie méridienne, d'abord sous la direction de Maurice
Lœwy, puis sous celle de M. B. Baillaud : détermination des posi-
tions absolues des étoiles fondamentales, des étoiles de repère du
Catalogue photographique du Ciel, du Soleil, de la Lune et des
planètes principales. En 1908 il fut chargé, en collaboration avec
W. Ebert, de l'étude de l'instrument méridien photographique
de Lippmann. Il collabora aussi au nouveau Catalogue d'étoiles
fondamentales publié par M. Lambert.

La santé de Fatou était souvent précaire, et les observations
soumettaient sa résistance physique à de rudes épreuves; mais il
apportait une grande conscience à toutes les tâches qui lui étaient

Fig. V.3. The beginning of the article [Chazy 1932]

It seems indeed that no mathematical journal published an obituary on Pierre Fatou.

VI

History's scars—a scientific controversy... in 1965

Julia claimed priority on iteration and got it; Fatou died without knowing that he would perhaps be recognised at last for his scientific merit; German mathematicians were somehow reinstated in the international scientific community; Julia, then Montel, were elected science academicians; German science was decimated (we caught sight of this with Hausdorff's suicide and Siegel's move to the United States, where he published the article [Siegel 1942] mentioned above), and this was a prelude to the start of a new war which tried and divided French mathematicians as it tried and divided the population. After the Liberation, the community started up again, not forgetting its antagonisms but without speaking of them, so that they are today forgotten. Nevertheless, this community is not, and never was, monolithic.

Still concentrating on our surviving protagonists, Julia and Montel, we shall see that the scars caused by the resentments coming from the time of the great war and the great prize, which we discussed earlier, were still likely to reopen... half a century later.

We have seen that the first Julia set appeared in Fatou's Note [1906d], when Julia was 13, and before normal families were invented. Afterwards the Julia set was defined more generally (as the set of what Montel himself called irregular points) in the independent work of Fatou and Julia in 1917.

It is sometimes said that it was Mandelbrot who ensured immortality for Julia's name by attaching it to the set which now bears that name. It is true that it could have been called "Fatou set" and its complement "Julia set" instead of the opposite. However the terminology seems to have been established long before the invention of fractals. It was the subject of a controversy, but between Julia and Montel, more recently—but still, before the invention of fractals.

A question (that has already been raised at the end of Chapter II and invoked again in § V.8) is still unanswered: we know nothing about the relationship between Fatou and Julia. We know a little more about that between

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Julia and Montel. This is our focus here, where as in Chapters I and II, the Academy of Sciences will be the main scene of our study.

We start by examining the trajectories of Julia and Montel (between 1918 and 1965); then we consider their relationship, through, on the one hand, a scientific discussion, in 1932, of which written notes were kept, and on the other hand their confrontation in an election at the Academy of Sciences in 1934—where we cite the great influence of Émile Picard, the revolts of Henri Lebesgue and the surprising success of René Garnier. This framework being set up, we shall come to the 1965 controversy, which was induced by a text Paul Lévy had to write for the third centenary, in 1966, of the Institut de France. The feeling one might have that Julia's career and recognition had been advantaged by his status as a disabled hero, will find here explicit corroboration.

VI.1 The protagonists, from 1918 to 1965

Gaston Julia

After the 1917 Bordin Prize and the 1918 Great Prize of mathematical sciences, Julia was awarded three more prizes of the Academy of Sciences, the Francœur Prize in 1926, the Poncelet Prize in 1928 and the Petit d'Ormoy Prize in 1931. He was elected a member of the Academy of Sciences in 1934. He was already a member of the Pontifical Academy[1].

Digression (Julia at *Zentralblatt* and *Compositio*). Julia was also a member of the editorial board of *Zentralblatt für Mathematik*, from its foundation in 1931 through to the end of the war and beyond[2], even when, after the removal of Levi-Civitá, an Italian member of the board with a too Jewish name, in 1938, Neugebauer asked for and obtained the resignation of (almost) all the non-German members, Harald Bohr, Hardy, Tamarkin, Veblen, and so on, and of reviewers, like Davenport. See [Remmert 2000].

He was a member of the (over-abundant) editorial board of *Compositio mathematica*, "this fundamentally international journal" [ce périodique foncièrement international] and one of its four "administrators", from its first issue in 1935, through to beyond the republishing of the journal after the war (even after Doetsch, Feigl and Süss left, refusing to participate in a non-completely Aryan enterprise). See [van Dalen & Remmert 2006] for the history of this journal.

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[1] See § VI.4 for some remarks on the difficulty of writing a biography of Gaston Julia.

[2] On Julia's collaboration with *Zentralblatt* during the war, see [Audin & Schappacher 2010].

Digression (The Julia Seminar). Julia gave his name to a legendary seminar (see how Chevalley [1961], Dubreil [1950, p. 151] and Garnier [1978] remembered it), in which some people see a "proto-Bourbaki Seminar" [Weil 1992, p. 97]). Note however that another historic member of Bourbaki, Szolem Mandelbrojt [1985], considers that this is the Hadamard Seminar which gave birth to the Bourbaki Seminar. Furthermore, let us mention that it is not impossible that the "tea", a post-Seminar ceremony of which today's mathematicians are fond, was invented alongside this seminar. On the Julia Seminar, see our article [2010].

Julia's research activity (at least as it can be measured by his publication rhythm) died down after he took stock of complex analysis and more precisely of functions of a single complex variable in his talk [1932][3] at the International Congress at Zurich. He then (starting in 1938) devoted his work to operators on Hilbert spaces, on which he published an impressive series of *Comptes rendus* Notes, see his publication list in [Julia 1968] (this work sank into oblivion).

He was in slight trouble at the Liberation[4] in 1944, because of his sustained relation with the occupiers, because of talks he gave in Germany in 1942, and because of the publication of one of his papers [1943] in a German journal[5]. But these troubles were not very serious, resulting in a few weeks suspension (from September 21st to November 10th 1944) followed by no sanction at all, the "épuration (purge) committee" having been (unanimously) swayed by the fact that he was a "broken face"... (see [Singer 1997, p. 283]).

He then resumed his activities, being professor at the Sorbonne and at the École polytechnique, President of the Academy of Sciences in 1950, and so on. Among other ceremonies in his honour, his scientific jubilee took place with pomp in 1961 (fifty years after he entered the École normale) under the patronage of an impressive list of mathematicians, among whom one might be surprised to see the names of Hadamard and Siegel, among (many) others: mathematicians practise the esprit de corps and the "let's say no more about it".

[3] Gauthier-Villars published this talk as a separate booklet.

[4] As is remarked by Singer in his book [1997], the names of many collaborationist men (and women) in the world of literature or show business (Germaine Lubin, Céline...) are known, but this is the case of few scientists. Since Ludovic Zoretti has already appeared in this text, let us mention the singular path of this university professor, a CGT trade unionist, a pacifist who became a member of the "Rassemblement national populaire" of Déat (who passed the entrance examination for the École normale in 1914 but only entered it after having made the war, and was then a leader of SFIO (socialist party) before he founded in February 1941 the aforementioned collaborationist political organisation) and who had really serious troubles at the Liberation.

[5] This is a series of memoirs of the Prussian Academy of Sciences, a collection edited by Bieberbach, in which the few foreigners published were all nationals of the "Axis" countries, Japanese or Bulgarians—except Julia.

In addition to his work on iteration and more generally on complex analysis, the most important contribution of Julia to the mathematics of his century might have been the brilliant way in which he raised money from the former students of the École polytechnique to finance the publication (by the Academy of Sciences) of the Complete Works of Poincaré (see [Julia 1970] for information on this).

Remark. The notice prepared by Julia when he was a candidate in the election to the Academy of Sciences (reproduced in [Julia 1968]) is written so as both to highlight the author's work (this is its function) and to show how superior he was to his competitor Montel. One can read there, for instance:

> [...] logically developing the consequences of these principles that the singular points of *a single* function determine it more or less, I looked for the corresponding notion for families and I introduced in analysis the general notion of *singular points of families of functions of one or several variables*, that some prominent geometers were kind enough to name *Julia points* or J-points. This notion, which is today classic, is explained, with many or few details [...] in the monographs of MM. Montel and Valiron on normal families[6].

Many of the claims Julia would make about J-points in 1965 come from this notice.

Despite the after-effects of his injuries and the bad health he complained of, he would live to the age of eighty-five and would be buried, after a mass and a military ceremony at the Saint-Louis des Invalides church, in 1978.

Paul Montel

For detail[7] on Montel's life, see the vivid biography [Beer 1966]. He applied to the Academy of Sciences at the same time as Julia, but was only elected thereto in 1937. Obviously, the fact that Julia was preferred to him was painful for Montel (see below how the election took place)[8].

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[6] développant logiquement les conséquences de ces principes que les points singuliers d'*une* fonction la déterminent dans une mesure plus ou moins grande, j'ai recherché la notion correspondante pour les familles et j'ai introduit en Analyse la notion générale de *points singuliers des familles de fonctions d'une ou de plusieurs variables*, que d'éminents géomètres ont bien voulu appeler *points de Julia* ou points J. Ce concept, aujourd'hui classique, est exposé avec plus moins de détails [...] dans les monographies [...] de MM. Montel et Valiron sur les familles normales.

[7] Succulent detail, as the white butter pike and is Muscadet nantais, picturesque or fragrant detail, as the garlic juice his father used to cure young Montel from cholera...

[8] In the same Paul Lévy file where the letters we present here are kept, there is a letter that Montel wrote to Paul Lévy right after Garnier (who, by the way, was supported by Julia) was preferred to him on March 3rd 1952, and in which, to comfort Paul Lévy, he speaks of the "pair Garnier-Lévy" and of the "pair Julia-Montel". On Garnier, see page 206.

He exerted much influence on the young between the two wars, an influence that can be measured by the number of reports on theses he wrote (see [Mandelbrojt 1975]). According to [Beer 1966, p. 70], he supervised about twenty theses between the two wars.

Montel was a close friend of Émile and Marguerite Borel, of Albert Mathiez, of Paul Langevin and of Lebesgue, and was thus rather a "left-winger".

During the first months of the 1939–40 war, by demand of his friend Raoul Dautry, then minister of armaments, he was head of a French-British scientific mission[9]. He was the dean of the Paris Faculty of sciences from 1941 to 1946.

Digression (Dean during the Occupation). Paul Montel was the dean of the Paris Faculty of sciences from 1941 to 1946 (which year, at the age seventy, he retired), and this included the main part of the period of German occupation. He was nominated for this position by the Vichy Ministry (and his representative Jérôme Carcopino) since, under Vichy, the deans were nominated. But there was also a vote of his peers in his favour, as was the custom before Vichy (and as has always been the case)[10]. It is certain that the dean's position was hard to hold and it is not very surprising that Montel's

[9] This mission was, in particular, in charge of a common atomic policy. It was at the origin of what was called "the battle for heavy water".

[10] Montel kept (Archives of the Academy of Sciences, collection 72J) a newspaper cutting dated August 21st 1941, probably cut from Déat's daily *l'Œuvre*, which reports on his election in these words:

> The election of the dean of the Faculty of sciences was settled according to the Republican tradition. First selection: an overwhelming majority for the noble Darmois, prominent physicist and mathematician, opponent of the unsavoury Perrin, of Popular Front memory!
> Then, to succeed in having the mixed-race Montel nominated, with the help of Carco's grace (and of that of Verrier, the iconoclast), Borrel [*sic*] was sent to the most obscure parts of the learned house to gather the wishes and votes of the minor staff. [L'élection du doyen de la faculté des sciences fut réglée selon la tradition républicaine. Première sélection: majorité écrasante au noble Darmois., physicien et mathématicien éminent, adversaire du triste Perrin, de mémoire front popu! Alors, pour faire nommer Montel, sang mêlé, la grâce de Carco aidant (et celle de Verrier, iconoclaste) on envoya Borrel [*sic*] recueillir jusque dans les endroits les plus discrets de la docte maison les vœux et les voix de tout le petit personnel.]

The "noble Darmois" mentioned in this rag (I apologise for the informal term together with the inevitable partisanship) is not Georges Darmois, who had the reputation of a Resistance fighter from the very beginning, but his brother Eugène Darmois, who was committed to Vichy. The Fréchet collection at the archives of the Academy of Sciences houses a letter dated July 22nd in which Valiron reports to Fréchet the first part of this vote, the day before.

attitude was described as "ambiguous" by such a Resistant fighter as René Zazzo [Pinault 2000, p. 248 and note 35][11].

There are nevertheless various available testimonies in favour of Montel's attitude. His biographical file at the archives of the Academy of Sciences contains, for instance, the cards he sent to the Permanent Secretary to inform him of the arrest of Langevin, Borel and others, in October 1941, and the Montel collection contains notes taken about his meetings, with Carcopino or other politicians, during which Montel described these arrests. In her memoirs [1968], Camille Marbo reports that he tried (in vain) to make the Academy of Sciences take steps to free Borel and his friends. Here is what Borel himself said, a short time after, during Montel's scientific jubilee [Borel 1947]:

> I am especially grateful to you for having agreed, at a time I was forced to leave Paris to escape German police surveillance, for replacing me at the presidency of the "Help for the scientific research", and for contributing greatly, with a quiet courage, to helping professors and researchers who were persecuted by the occupier and its accomplices[12].

In the book [MathNiçois 1966], a tribute book to Montel, René Cassin writes [Cassin 1966, p. 21]:

> Throughout the enemy occupation, the great scientist, riveted to his position, jealously watches over the independence of the Sorbonne, which is constantly threatened by the occupier and its servants. All he can do to protect his colleagues, ardent in the Resistance, and to give his students a chance to escape the service of obligatory work in Germany, he achieves simply, with both firmness and flexibility, so that in high places, the Abel Bonnards[13] and other deliquescent people are furious[14].

[11] Among the documents that were kept by Montel and that are today in the collection 72J of the archives of the Academy of Sciences, are the issues of the (resistant) clandestine journal *l'Université libre* [the Free University] in which he was (sometimes vehemently) called into question.

[12] Je vous suis particulièrement reconnaissant d'avoir, à une époque où j'avais dû quitter Paris pour échapper à la surveillance de la police allemande, accepté de me suppléer à la présidence de l'Aide à la Recherche scientifique et largement contribué, avec un courage tranquille, à aider des professeurs et des chercheurs persécutés par l'occupant et ses complices.

[13] It seems that Montel was even reprimanded by his minister:

> In 1941 [...] you became dean. You were not spared the ordeal of the obnoxious Vichy. And you even have the honour of having deserved a reprimand, signed by Abel Bonnard [Roussy 1947]. [En 1941 [...] vous accédez au Décanat. Les épreuves des lois odieuses de Vichy ne vous ont pas été épargnées. Et vous avez même l'honneur de mériter un blâme, signé Abel Bonnard]

This reprimand does not seem to be among the papers kept in the Montel collection.

[14] Pendant toute la durée de l'occupation ennemie, le grand savant rivé à son poste veille jalousement sur l'indépendance de la Sorbonne menacée à tout instant par

Such "firmness and flexibility" are not absolutely contradictory with the reproach of "ambiguity" and fit well with what Dieudonné writes [1990] in his article on Montel in the DSB:

> During the German occupation he was dean of the Faculty of Science, and he was able to uphold the dignity of the French university in spite of the arrogance of the occupiers and the servility of their collaborators.

The texts we have quoted above can be placed in the category "academic praise". Nothing however forced Robert Debré, in his article [1966] in [MathNiçois 1966] to evoke somewhat more particular memories:

> And then, later[15], under the Paris Occupation, clandestine and furtive meetings were held on account of our efforts, combined with those of our friends in the Resistance. To the praises of those who admire Paul Montel, I can add testimony of his proud courage[16].

Here is a rather less recent testimony (dating back back to the jubilee [JubiléMontel 1947] on March 18$^{\text{th}}$ 1947) of what the offices of the Faculty of sciences were able to do during the Occupation [Cabannes 1947][17]:

> In October 1941, your colleagues in the Faculty of sciences had to elect their dean, the vote went to you and you had the fearsome honour of succeeding M. Maurain in the critical days of the German occupation. You had both to save the dignity of Paris University and to shield the students from the enemy.
> Your secretariat silently carried out its duty. To avoid obligatory work in Germany for the young men, they were registered as students outside the dates given by the ministerial instructions and they were given antedated certificates; people with false identity cards were registered for exams; the files of those from Alsace and Lorraine, of the Jews, were removed from the folders. The registers were sabotaged; there had never been such disorder and such carelessness. On the eve of the Liberation of Paris, in the cellars of the chemistry institute, incendiary bottles dedicated to the street fights against German tanks accumulated. Certainly, "Sir Dean" was not aware of this hidden struggle that lasted for three years, but Paul Montel knew about

l'occupant et ses serviteurs. Tout ce qu'il peut faire pour assurer la protection de ses collègues ardents à la Résistance et pour donner à ses étudiants une chance d'échapper au service du travail obligatoire, il l'accomplit simplement avec une fermeté et une souplesse dont, en haut lieu, les Abel Bonnard et autres déliquescents enragent.

[15] Before this "later" the friendship of Debré's uncle and aunt Hadamard for Montel was described.

[16] Et puis, plus tard, sous l'occupation de Paris, des rencontres clandestines et furtives avaient pour motif nos efforts, mêlés à ceux de nos amis de la Résistance. Aux éloges de ceux qui admirent Paul Montel, je puis joindre le témoignage de son fier courage.

[17] Testimonies in the same vein by which Cabannes, who succeeded Montel as a dean, might have been inspired, can be found in the Montel collection at the archives of the Academy of Sciences.

> it, approved it, and encouraged it. You found, my dear colleague, the best of rewards in being re-elected in triumph after the Liberation[18].

And indeed, the fact that Montel kept the dean's position until 1946 seems to be the most convincing testimony in his favour[19] since he lived through the period of the Liberation (and the purges [épuration]) without any trouble. When he retired in 1946, he distributed to his colleagues a text in which he said:

> I take pride in having been chosen by my colleagues in murky and difficult times. Their confidence and their support helped me to continue without our Faculty, though damaged in its freedom and in its flesh, being affected in its dignity[20].

It seems indeed that he was able to keep the position without any dishonourable behaviour.

He took over the editorship of the *Bulletin des sciences mathématiques* after Picard's death in December 1941 and he remained there until his own death in 1975. He was the director of the IHP after Borel's death, the president of a committee at UNESCO, and president of the Academy of Sciences in 1958[21].

[18] En octobre 1941, vos collègues de la Faculté des Sciences ont à élire leur doyen, les suffrages se portent sur votre nom et vous avez le redoutable honneur de succéder à M. Maurain aux heures critiques de l'occupation allemande. Il fallait à la fois sauvegarder la dignité de l'Université de Paris et soustraire les étudiants à l'ennemi.

Votre Secrétariat fait silencieusement son devoir. Pour éviter aux jeunes gens le travail obligatoire en Allemagne, on les immatricule comme étudiants en dehors des dates fixées par les instructions ministérielles et on leur délivre des certificats antidatés; on inscrit pour les examens des candidats porteurs de fausses cartes d'identité; on retire des dossiers les fiches des Alsaciens et des israélites. Les registres sont sabotés; jamais on n'avait connu tel désordre et pareille incurie. À la veille de la libération, dans les caves de l'Institut de Chimie, s'amassent les bouteilles incendiaires destinées aux combats de rues contre les chars allemands. Sans doute Monsieur le Doyen ignore-t-il cette lutte sourde qui dure depuis trois ans, mais Paul Montel la connaît, l'approuve et l'encourage. Vous avez trouvé, mon cher collègue, la meilleure des récompenses dans votre réélection triomphale au lendemain de la libération.

[19] As Jacques Roubaud told me, the fact that François Le Lionnais welcomed a paper of an author in his book [1948] is in itself a guarantee that this author was never a collaborator. This is the case of Montel with his text on the role of families of functions in analysis [Montel 1948].

[20] Je garde la fierté d'avoir été désigné par mes Collègues en des heures troubles et difficiles. Leur confiance et leur appui m'ont aidé à les traverser sans que notre Faculté, meurtrie dans sa liberté et dans sa chair, fût atteinte dans sa dignité.

[21] To make this evocation of Montel complete, we need to mention the negative and embittered comments of Lucienne Félix [Félix 2005, p. 66-67] about the friendship between Montel and Lebesgue (especially after the latter's death).

VI.2 Relations between Julia and Montel, in the 1930's

A still cordial atmosphere, 1932

In 1932, the atmosphere seemed to be still relatively cordial between Julia and Montel. That year, Julia, who was the president of the SMF, put in a lot of effort by making himself several communications to the Society in the Wednesday evening sessions. On December 14th, he even gave a lecture during one of the sessions[22]. Montel did not attend it, but Hadamard was there (besides, he made a communication on a Note by Ahlfors), and he reported to Montel what Julia said. Montel wrote to Julia on this topic[23]. He kept a draft, dated December 15th, a first response of Julia, dated the 20th, on which he wrote that he replied on the 21st, and a second response of Julia, dated of the 26th.

The mathematical question is the following: Montel had already considered in [1931a ; 1931b][24] rational fractions "with linked terms" [à termes entrelacés], namely those that can be put in the form P/Q where the roots of P and Q alternate on a circle. Using a Möbius transformation, it can be assumed that this circle is the real axis and that P and Q are real polynomials of the same degree k. Thus

$$R(z) = \frac{P(z)}{Q(z)} = A - \sum_{i=1}^{k} \frac{A_i}{z - a_i},$$

where A, $A_1, \dots, A_k$, $a_1, \dots, a_k$ are all real, and let us say that the roots a_i of Q are ordered increasingly $a_1 < \cdots < a_k$. Notice that

$$P(z) = AQ(z) - \sum_{i=1}^{k} A_i \prod_{j \neq i} (z - a_j),$$

so that

$$P(a_i) = -A_i \prod_{j \neq i} (a_i - a_j).$$

The linking assumption is thus equivalent to the statement that all the A_is have the same sign. Montel noticed in particular that this assumption is stable under composition, and in particular that the iterates of a rational fraction with linked terms are rational fractions with linked terms. He returned to these rational fractions and to the meromorphic functions that are uniform limits of such fractions in a *Comptes rendus* Note [Montel 1932] which appeared on

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[22] The section "life of the society" in the *Bulletin* gives the date of December 19th but this is a mistake: it is indeed the 14th which was a Wednesday and Montel's letter that will be mentioned is dated the 15th.

[23] For this paragraph, see the Montel file, archives of the Academy of Sciences. It is in a reprint of his article [1931b] that Montel kept these papers.

[24] Montel had, at that time, and would have, all his life, many contacts in Romania. It is thus not surprising that he would publish in a Cluj journal.

October 17th 1932, a few weeks before Julia's lecture. What Julia points out to Montel, in his December 26th letter, is that the linking assumption is also equivalent to the fact that the fraction R preserves the upper half-plane[25], which is true when all the A_is are non negative, indeed:

$$\operatorname{Im} R(z) = -\sum_i \operatorname{Im} \frac{A_i}{z - a_i} = \left(\sum \frac{A_i}{|z - a_i|^2}\right) \operatorname{Im} z.$$

The rational fractions which satisfy Montel's linking assumption are thus fractions with a fundamental circle, the iteration of which was investigated by Fatou in the Note [Fatou 1917b] (see page 60)—just before he had the idea of using Montel's normal families.

One might wonder that Julia mentioned this fact in a lecture Montel was not attending, apparently without having told him about this remark previously. But this does not prevent Montel's letter from being very cordial (this is the case, at least, of the draft he kept).

Indeed, Montel wrote in this draft:

Dec. 15th 32

My dear Julia

> M. Hadamard tells me that, in your talk of yesterday evening, you pointed out that the results I published on meromorphic functions with linked terms were obtained previously by you and Fatou. Since I have work on this question in print[26], I would like to acknowledge the necessary priorities and to avoid needless repetitions.
> I studied of the constitution [composition?] of transcendent meromorphic functions that are limits of rational fractions with linked terms. I gave in particular results on their iteration, which are not different from those for rational fractions of the same type. Is there previous work by you or by Fatou on such meromorphic functions?
> As for rational fractions themselves, I made an intrinsic study of them and gave in passing the results on their iteration. The latter have been obtained previously by Fatou in his chapter on fract. with fundamental circle and you mention them also in your crowned memoir. Are there other points in common?

With kind regards[27].

---

[25] As for the meromorphic functions which are limits of such fractions, it seemed to Julia that they were investigated by Fatou and that their iteration is not different from that of fractions with fundamental circle. It also seemed to him that Montel was using the same method (generalisation of the Schwarz Lemma) that he, Julia, used, and he gave references to his articles on the topic.

[26] Montel's paper is [Montel 1933]. It appeared in the June 1933 issue of the *Annales de l'École normale supérieure*. The note [Montel 1932] was an announcement of the results. Montel indeed referred to [Fatou 1917b].

[27] Mon cher Julia,

On this draft, Montel also wrote a list of everything that was known. More particularly, he picked up, in the "crowned memoir" [Julia 1918f], every mention of the fact that it was Fatou who studied rational fractions with fundamental circle[28]:

> In the crowned memoir on iteration, Julia attributes to Fatou the introduction of fractions with fundamental circle[29]
>
> – p. 72 "the fract. with fund. circle of M. Fatou",
> – p. 110 "the fract. that were reported by M. Fatou in a C. R. Note",
> – p. 179 "the fract. with fund. circle that M. Fatou considers in a C. R. Note"
> – p. 216 "let us return to the examples of M. Fatou in his 1917 Note".

To conclude this section, we point out that the rational fractions with linked terms are again the subject of the final chapter of Montel's last book [1957].

If the lecture of Julia, together with the care taken by Montel not to cite him in his paper [1933], allows us to imagine some tension between them, despite the rather amiable tone of this exchange of letters, it seems that the situation would deteriorate, in particular on the occasion of the election at the Academy of Sciences in 1934.

Elections at the Academy of Sciences

After Paul Painlevé's death on October 29th 1933, the Academy of Sciences prepared for his replacement.

M. Hadamard me dit que, dans votre conférence de hier soir, vous avez signalé que les résultats que j'ai publiés sur les fonctions méromorphes à termes entrelacés avaient été obtenus antérieurement par vous et par Fatou. Comme j'ai à la composition un travail sur cette question, je désire faire les restitutions de priorité nécessaires et éviter les redites.

Je me suis occupé de la constitution [composition?] des fonctions méromorphes transcendantes limites de fractions rationnelles à termes entrelacés. J'ai indiqué en particulier les résultats de l'itération de ces fonctions qui ne diffèrent pas de ceux relatifs aux fonct. rationnelles du même type. Existe-t-il sur ces fonctions méromorphes un travail antérieur de vous ou de Fatou?

En ce qui concerne les fractions rationnelles elles-mêmes, j'en ai fait une étude intrinsèque et j'ai donné incidemment les résultats de leur itération. Ces derniers ont été obtenus antérieurement par Fatou dans son chapitre sur les fract. à cercle fondamental et vous en parlez aussi dans votre mémoire couronné. Existe-t-il d'autres points communs?

Bien cordialement à vous.

28 These lines seem to have been added after Montel received Julia's response and they justify the fact that Julia is not mentioned in [Montel 1933].

29 Dans le mémoire couronné sur l'itération, Julia attribue à Fatou l'introduction des fractions à cercle fondamental

The previous election of a member in the geometry section was on March 9th 1931, when Paul Appell was replaced. The section met (secretly) on the previous Monday and ranked its candidates, in first place Élie Cartan, in joint second, Denjoy, Julia, Montel and Vessiot. The fifty-four academicians attending the session on the election day quietly followed the advice of the section: Cartan obtained 51 votes, Vessiot 2 and Montel 1. An ordinary election.

The well established ritual occurred again to replace Painlevé in 1934. On February 19th, Julia officially announced that he would be a candidate, and on February 26th, the section, namely Hadamard, Goursat, Borel, Lebesgue and Cartan[30] met secretly (for almost two hours, quite a long meeting, showing that the choice was not straightforward)[31]. Élie Cartan explained why he would vote for Montel—he also spoke in praise of Julia's work. Picard ranked Julia in first place; Lebesgue ranked Montel first. Hadamard, Goursat and Borel also spoke.

It is likely that Hadamard also voted for Montel, as a discussion which took place some time before shows. On November 14th 1932, the Academy of Sciences secretly discussed of Albert I of Monaco Prize and in particular, following Picard, asked itself which geometers it would award. Julia, Montel, Vessiot and Denjoy were mentioned. Lebesgue said that, if this was an election in the Geometry section, he would vote for Montel and Hadamard expressed the same opinion[32].

The section eventually proposed:

– in first place, Paul Montel,
– in second place, Gaston Julia,
– in third place, Arnaud Denjoy,
– in joint fourth place, Maurice Fréchet[33], René Garnier and Paul Lévy.

This time the Academicians did not follow the section since, on March 5th, the fifty-four people attending the meeting chose Julia, with 32 votes, above Montel with 21 (and one null ballot). Surely the fact that Julia was a severely wounded veteran of the great war counted in his favour. Here are a few examples of analogous situations: on May 26th 1919, for an election at the chemistry section, Hadamard explained that "he would vote for M. Béhal because of the first class role he has played in the chemical production for the war[34]; on April 4th 1921, the services given to the national defence by Borel during the

[30] Picard, since he was Permanent Secretary, was no longer a member of the section (he was replaced by Goursat).
[31] For this paragraph, register of secret committees, archives of the Academy of Sciences.
[32] Louis de Broglie was eventually awarded Albert I Prize.
[33] Fréchet was presented by Hadamard, see [Taylor 1982].
[34] il votera pour M. Béhal à cause du rôle de premier plan qu'il a joué dans les fabrications chimiques de guerre.

war were adduced in favour of his election; the three Notes published by Esclangon in 1916 (the ones mentioned in note 63 of Chapter I) would be useful for his election in 1929 [Saint-Martin 2008, p. 139]; again in 1942, when the question of replacing Lebesgue would be raised, an Academician would write to Fréchet that the fact of his being a war veteran would count in his favour (a letter quoted in [Taylor 1985, p. 365]).

Such a reversal (at least for the election of a geometer) is rather uncommon[35]. In the fifty years before, there were thirteen elections of geometers and only one in which the Academicians did not follow the advice of the section, namely, on May 19th 1919, when they closely preferred Goursat to Borel, with a quite similar result: 29 for Goursat who, according to the register of secret committees, was supported by Picard and Jordan, to 23 for Borel, supported by Humbert and Painlevé. In most cases, as in Cartan's election, the majority obtained by the lucky man was very comfortable (Darboux polled 47 votes out of 53, Appell 52 out of 53, Humbert 54 out of 58...) but there were also more difficult elections, such as that of Poincaré (who obtained 31 votes to the 24 for Mannheim, a more or less forgotten geometer) on January 31st 1887, or even that of Hadamard (36 votes to 21 for Goursat)[36] on December 9th 1912.

Montel would be elected on May 31st 1937, at the next election, to replace Goursat, with a rather triumphal vote, since the secret committee of May 24th would only need fifteen minutes to establish the list:

– in first place, Paul Montel,
– in second place, Arnaud Denjoy,

[35] During the period between the two wars, the Academy of Sciences recruited several members of the mechanics section, in a more eventful way:

– Jules Drach in 1929, who was added by the Academy to the list the section proposed, thanks to a proposition of Lebesgue (according to the registry of secret committees, June 3rd 1929), and who was elected with a comfortable majority,

– Jouguet, Villat and Louis de Broglie in 1930, 1932, 1933, without any problem,

– Alfred Caquot in 1934, who surpassed Vessiot (reversal of the proposition of the section) at the second ballot.

[36] Picard has already supported Goursat against Painlevé in 1900 (see Painlevé's letter to Mittag-Leffler of June 10th 1900 quoted in [Painlevé 1975, p. 811]). After Painlevé, Humbert and Hadamard were preferred to Goursat by the Academicians. Political divides played their role here too. In the case of Jacques Hadamard, it is likely that, a few years after the conclusion of the Dreyfus Affair, the anti-Dreyfus-faction voted against a candidate who not only was Jewish, but had been a very active Dreyfus supporter and was still a militant for the "League for Human rights" [Ligue des droits de l'Homme]. The correspondence of Picard kept by the archives of the Academy of Sciences demonstrates this clearly: Picard hated Hadamard, in particular because he was Jewish. It is hard to understand why this kind of remark does not appear with regard to this election at the Academy of Sciences in the biography [Maz'ya & Shaposhnikova 1998]—another expression of the esprit de corps.

– in joint third line, Maurice Fréchet, René Garnier, Paul Lévy and Georges Valiron,

and the result would be 51 votes for Montel, 1 for Garnier, 1 for Paul Lévy... and none for Denjoy, who would be the next to be elected[37], in 1942, long before Garnier (1952), Fréchet (1956) and Lévy (1964).

As for Garnier... The election took place on March 3rd 1952, to replace Élie Cartan who died the year before. The geometry section met (for one and a half hours) in secret committee on February 25th. Borel, Hadamard, Denjoy and Montel wanted to put Fréchet in first place, whereas Julia wanted Garnier (a precise record is kept of this story, which is quoted in [Taylor 1985, p. 366]), a thing Denjoy would call an "absurd fancy[38]" (same source)[39]. The secret committee thus ranked:

– in first place, Maurice Fréchet,
– in joint second place, René Garnier, Paul Lévy and Georges Valiron,
– on the third line, placed equal, Henri Cartan and Szolem Mandelbrojt.

And the Academy of Sciences followed Julia, without great enthusiasm, for it needed two ballots to elect Garnier. Since 1884 (we did not go back further), this was the first time a geometer was not elected at the first ballot. What Julia called a "brilliant election" was more a brilliant result for him than for Garnier.

Digression (About Garnier (and Fatou, Julia and Picard)). One might wonder about such a success for René Garnier (1887–1984), who was preferred to Fatou, according to Weil[40], for a position at the Sorbonne in 1927 at which he was elected at the suggestion of Picard, and to Maurice Fréchet and Paul Lévy for a seat at the Academy of Sciences in 1952, "after a brilliant

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37 Denjoy did not update his notice for the election to replace Goursat in 1937 because "there was unanimous support of the Academicians for Montel" [il y avait unanimité des académiciens pour Montel] (Denjoy file, archives of the Academy of Sciences).

38 caprice saugrenu

39 See also Paul Lévy's letter to Maurice Fréchet dated July 2nd 1951 in [Lévy & Fréchet 2004]:

> Julia actively campaigns against us, which is not usual at the Academy. [Julia fait une campagne active contre nous, ce qui n'est pas dans les usages de l'Académie.]

40 See page 179. Weil, who had the reputation of a malicious gossip, says, about Garnier [Weil 1992, p. 58]:

> Thus he [Garnier] was entrusted with the task of filling the report on my thesis, a task which he performed conscientiously and benevolently: he did not notice a few gaps in my proofs, but he did give me some useful advice on commas.

election I have some reasons to remember[41]", as Julia would say [1970, p. 382]. René Garnier, who was awarded the Great Prize of mathematical sciences in 1912 (together with Boutroux and Chazy) was one of the disciples of Painlevé, and the latter said (quoted by Julia in [1970, p. 281]):

> And then there is Garnier. What he does is neither easy, nor very funny, but it is solid[42].

An opinion which would, today, stop dead the career of a mathematician. Nevertheless, on this basis, Julia always supported and eventually was able to make Garnier succeed. In the story of the position considered here, Julia (who was Professor at the Sorbonne from 1925 after having been lecturer [chargé de cours] in 1920 and assistant professor [maître de conférences] in 1922) certainly pressed in favour of Garnier (who was at Poitiers[43] from 1913, firstly as an assistant-professor [maître de conférences] then as Professor) against Fatou; as he said, addressing Garnier during his jubilee [1970, p. 281]:

> I was thus lead on various occasions to support your various applications[44] [...] I was in all your campaigns for the Sorbonne[45].

No wonder Julia competed against Fatou...

This evokes the end of the obituary [1942] Szolem Mandelbrojt devoted to Picard: after a brief but impressive description of his work and of the recognition it gave him, he concluded by evoking Picard's "Olympian attitude". This was because this all-powerful figure had his executioners!

For an account of René Garnier's, career, see [Malliavin 1989]; there he is called a "convinced Christian".

Digression (About Émile Picard and his power). The quotation of Weil above, page 179, comes from a passage in his book [1992] in which he proves Picard's omnipotence. And indeed in 1928 (to limit ourselves to the

[41] après une brillante élection dont j'ai quelques raisons de me souvenir

[42] Et puis il y a Garnier. Ce qu'il a fait n'est pas facile, ni très amusant, mais c'est solide.

[43] At that time, it was possible to leave a position of Professor at Poitiers to take a position of lecturer at the Sorbonne: there were two different structures in university education, that of Paris and that of the provinces (see for instance [Lebesgue 1991, note 856]).

[44] René Garnier was also the assistant of Julia at the École polytechnique starting from 1943, see [Julia 1970, p. 282]. It is not without interest to read the dedication Garnier wrote on the copies of his books which were given to Gaston Julia. In 1940 and 1941, he wrote "To Gaston Julia, very cordially" [À Gaston Julia, très cordialement], in 1945 and 1951 "affectionate homage" [affectueux hommage], but in 1935, he was slightly more precise "To Monsieur G. Julia, with very grateful regards" [À Monsieur G. Julia, en souvenir bien reconnaissant]—he must have felt indebted to Julia (Julia collection at the library of the CIRM).

[45] J'ai été ainsi conduit en diverses occasions à soutenir vos diverses candidatures [...] Je fus de vos campagnes pour la Sorbonne.

year Weil was speaking of), Picard was a member of the French Academy (from 1924[46]), Permanent Secretary of the Academy of Sciences[47], a member of the Bureau des longitudes, a member of the Council of the Observatory (from Poincaré's death in 1912), president of the council of the École centrale, professor at the Faculty of sciences, both at the École centrale and at the École de Sèvres, director of the *Annales de l'École normale supérieure*, editor (with Appell) of the *Bulletin des sciences mathématiques* and participant in the *Journal de mathématiques pures et appliquées* which was run by Henri Villat. We have seen that he was the president of the Research Council. He was also an honorary member of the board at the SMF, a society he chaired twice, in his youth in 1884, and again in 1897. And he was the president of the "Association amicale de secours des anciens élèves de l'École normale supérieure" (association of former students of the ENS) (from 1922 until 1929) and of the "Société des Amis des Sciences". At the end of his career, his course at the Sorbonne would have been followed (more or less) by forty-six years of normaliens[48]—not including the young women of the Écoles de Sèvres [sévriennes][49]. An exceptional example of what Bourdieu[50] calls "academic capital" [capital universitaire].

[46] In a letter to Lacroix kept at the archives of the Academy of Sciences, he himself says, about his election at the "Académie française", that he was the candidate of the Catholic party.

[47] Of which he was a member since the age 33—even if he was only elected at his sixth attempt, after Jordan, Darboux, Laguerre, Halphen and eventually Poincaré.

[48] Among which were Lattès, Montel, Fatou and Julia.

[49] Annoyed by the devotion the sévriennes had to Picard (who was nicknamed "Romeo" by these young women—a nickname Lucienne Félix avoided mentioning in her speech for the centenary of Picard [Félix 1956]), Lebesgue would have said [Félix 1974, p. 10]: "Picard! Picard! come on! This is not Archimedes! [Picard! Picard! enfin, ce n'est pas Archimède]. A long time ago, he wrote to Borel [1991, p. 250]: "It seems to me that Picard is a weirdo" [Picard me semble un hurluberlu]. The reader probably understood that Lebesgue, the son of a worker (a typographer) did not belong to the same social circle as Picard, nor to his coterie. See also the digression on his revolts page 211.

[50] It is not anachronistic, and probably not inappropriate either, to quote here an analysis made by Pierre Bourdieu [1984, p. 114]:

> Here too, capital goes to capital, and holding positions that confer social weight determines and justifies holding further positions, which in turn carry the gravitas of their incumbents. [Ici aussi, le capital va au capital, et l'occupation de positions qui confèrent du poids social détermine et justifie l'occupation de nouvelles positions, elles-mêmes fortes de tout le poids de l'ensemble de leurs occupants.]

The case of the historian Pierre Renouvin (again a disabled ex-serviceman of the first World War) invoked by Bourdieu in support of this analysis recalls, *mutatis mutandis*, one generation later, the case of our Picard.

Except perhaps for Joseph Bertrand (1822–1900), see [Zerner 1991], who was not as good a mathematician and of whom Picard was a nephew by marriage (Madame Picard was the daughter of Madame Hermite, who was herself Bertrand's sister), nobody ever had as much power in this community, not even his predecessor Darboux, about the omnipotence of whom Lebesgue [1991] would complain so much.

We have seen (for instance by the pen of Louis Fatou page 180) that Picard was a "big shot"—the French word "pontife" has almost disappeared from academic vocabulary to be replaced (from around 1968) by the word "mandarin", which seems to be less global than the pontificate.

A digression, about the IMU

The "Research council" was mentioned. Another after-effect of the war was, as we have pointed out several times, the exclusion of Germany and its allies from international mathematical life, an exclusion of which some mathematicians, such as Émile Picard, were warm supporters (see for instance his speech [1921] at the Strasbourg Congress)[51].

The first international Congress to which a German delegation (chaired by Hilbert) participated was that of Bologna in 1928 (already mentioned here, page 88). This participation was not an easy thing, either from the German side (see [Segal 2003] for instance, concerning the diverging positions of Hilbert and Bieberbach) or from the French side, since Picard, as the president of the very anti-German *International Research Council*, refused to participate (see the reports on the preparation of the Congress [Pincherle 1929b]).

The 1932 Zurich Congress more or less dissolved the IMU (putting it into liquidation until the next Congress) and appointed a commission to study the general question of an international collaboration between mathematicians, a commission of which Julia was a member, and which gave its report to the Oslo Congress in 1936, acknowledging failure. Nevertheless, Julia still believed in the IMU since he wrote to Picard (letter dated February 5th 1936, already mentioned page 31)

> [...] you can respond that the International Union still exists and that there are reasons to continue to pay the usual subsidy.
> The International Commission should meet before the Oslo Congress at a date that will be fixed by M. Severi (chair), but that will certainly be prior to July, as the Congress is fixed for July 13th to 18th. Only the Oslo Congress, to which the question will be clearly posed, will decide to dissolve or to maintain the Union.
> I shall give you additional detail on the question when we see each other, if you are interested[52].

[51] Information on the IMU can be found in [Lehto 1998].

[52] vous pouvez répondre que l'Union internationale existe encore et qu'il y a lieu de continuer à verser la subvention habituelle.

Fig. VI.1. Gaston Julia and Charles de la Vallée Poussin, a picture taken by George Pólya during the dinner (in Ravenna) of the Bologna Congress in 1928

La Commission internationale qui la régit doit se réunir avant le Congrès d'Oslo, à une date que M. Severi (président) fixera, mais qui sera certainement antérieure à juillet, puisque le Congrès est fixé au 13–18 juillet prochain. Le Congrès d'Oslo seul, auquel la question sera posée de façon nette, pendra la décision de dissoudre ou de maintenir l'Union.

Je vous donnerai d'autres détails sur la question lorsque je vous verrai, si cela vous intéresse.

He reported again after Oslo and Picard would write to Lacroix in a letter dated August 29th 1936[53]:

> If the Geodesy and Geophysics Union is not doing very well, the International Union of Mathematics was buried for good at Oslo, at the end of July, during the International Congress of Mathematics. The Americans were the most fanatic for the abolition. Only France, Poland and Spain were in favour of upholding it, though Borel played a rather ambiguous role. This was Julia who recently gave me some detail on the never-ending discussions at the Oslo Congress[54].

Digression (Power of Picard... and revolts of Lebesgue). But let us return, as Lebesgue was mentioned, to the elections at the Academy of Sciences. Lucienne Félix [1974] reports, about Lebesgue:

> At the Academy [...] he declaimed against the fact that elements other that the scientific value were taken into account [...] He went so far as holding out against the Big Master and Permanent Secretary, Émile Picard, and, during some time, stopped attending the sessions[55].

Elements other than scientific value are always taken into account. Lebesgue, who wrote scores of letters to Borel to complain about this at the beginning of his career (see [Lebesgue 1991]) knew it perfectly well. Here is what he said in a letter to Montel[56] about the election of an assistant-professor at the Sorbonne in 1922:

> Math: assistant-professorship [maîtrise de conférences], 32 votes (17 for an absolute majority), 9 Denjoy, 22 Julia, 1 blank.
>
> This result is exactly the one I announced to you this morning: I said 9 ballots.
> Picard made the patriotic speech, he spoke of Julia's mutilation, of his sickbed, and so on. He spoke of those subtle studies that would have been

[53] Picard file, archives of the Academy of Sciences.

[54] Si l'union de géodésie et de géophysique ne va pas très bien, l'Union internationale de mathématiques a été enterrée définitivement à Oslo, à la fin de juillet, au Congrès international de mathématiques. Les Américains ont été les plus enragés pour la suppression. La France, la Pologne et l'Espagne étaient seules favorables au maintien, et encore Borel a-t-il joué là un rôle assez ambigu. C'est Julia qui m'a donné récemment des détails sur les interminables discussions du Congrès d'Oslo.

[55] À l'Académie [...] il s'indigna qu'on tint compte pour les élections, d'autres éléments que la valeur scientifique [...] Il alla, paraît-il, jusqu'à tenir tête au grand Maître et Secrétaire perpétuel Émile Picard, et pendant un certain temps, cessa d'aller aux Séances.

[56] Montel collection, archives of the Academy of Sciences. Lebesgue was not the kind of man to conceal differences of opinion between his colleagues. For instance, on May 31st 1923, he wrote a letter to Villat in which he made some comments on what each of the colleagues did during Villat's election as a correspondent member of the Academy of Sciences (Villat collection 61J).

> continued too far and that it was useful to halt[57]. He feigned indignation against the postponement of the baccalaureate and he dared to say that Julia had no fortune, that he had two children and that it was to feed them that he was obliged to examine at the École polytechnique rather to take care of the baccalaureate.
> There it is![58].
> H. Lebesgue

It is not clear why this would not have been the case at the Academy of Sciences. Without going back to the Dreyfus Affair and to its repercussions (see note 36), let us remember, in this story, even before Lebesgue entered the Academy of Sciences, the election in 1916 of a professor of Louvain as a "correspondent member", a replacement for Felix Klein, who was excluded because of having signed a text mentioning the war at Louvain (see page 42)[59].

Let us return to Lucienne Félix. As a true member of the community of mathematicians, she does not say exactly to what she alludes to. One cannot refrain from trying to guess. Was this the election of a geometer? If yes, there were not that many of them between that of Lebesgue in 1922 and his death in 1941 (Cartan in 1931, Julia in 1934, Montel in 1937). If Lebesgue came up against Picard, that meant that the vote was not unanimous. One can remember that Picard pressed heavily in favour of Julia's election; and so on. But it could also be an election for the mechanics section (Jules Drach, Émile Jouguet, Henri Villat, Louis de Broglie, Alfred Caquot), or for another section[60]. The answer to all these questions is given by Montel himself, rather belatedly once again, among a few light anecdotes [Montel 1966]:

> Henri Lebesgue, great mathematician of the beginning of the century, was to me the most generous, the most reliable and the most faithful friend. In 1934, I was a candidate for the Academy of Sciences. I was not elected, the vote going to a disabled ex-serviceman. Lebesgue considered that an injustice had been done and refused, for three years, to attend the sessions.

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57 Less backward looking than Hermite, a few epistolary "stupid remarks" of whom we have already quoted (pages 19 and 81), but trained in his school, Picard expressed reservations on the interest, for instance, of Baire's work (see the end of his report on the latter's thesis in [Gispert 1991, p. 375]). One can imagine that he expressed, this time, analogous reservations about Denjoy.

58 Ce résultat est exactement celui que je t'avais annoncé ce matin: j'avais dit 9 voix.

Picard a fait le laïus patriotique, il a parlé de la mutilation de Julia, de son lit de douleur, etc. Il a parlé de ces études subtiles que l'on aurait poussé trop loin et qu'il était utile d'enrayer. Il a feint l'indignation contre l'ajournement des baccalauréats et il a osé dire que Julia était sans fortune, qu'il avait deux enfants et que c'était pour les nourrir qu'il était obligé de faire des examens à l'x au lieu de faire des bachots.

Voilà !

59 The qualities of the Belgian mathematician (de la Vallée Poussin) are not in question.

60 On the elections of members at the Academy of Sciences, see above.

> He went back only in 1937, after I was elected with a large number of votes[61].

One will also find in [Lebesgue 2004, note 555][62] the following information:

> Henri Cartan told us that he has a letter of Lebesgue to Élie Cartan in which Lebesgue reprimands Cartan because he supported Julia against Montel.

The register of secret committees (see page 204) gave contrary information. Since the French original version of this book was written, we have had the opportunity of reading this letter from Lebesgue to Cartan; it was written before the election and Lebesgue "reprimands" Cartan because he was too ready to believe in what Julia told him. This letter appears here in the Appendix (page 237). Marguerite Borel, who knew him well since she had been in charge of the Hôpital 103 with him, described Élie Cartan [1968, p. 171] as a "transcendent mathematician, very brave, very gentle, but of a hesitant nature" [un mathematicien transcendant, très courageux, très doux, mais d'un caractère hésitant], and it is possible that he took some time to decide between the two candidates.

If Lebesgue rebelled and expressed himself quite explicitly both in the 1922 letter to Montel quoted above and in the letter to Élie Cartan mentioned by Henri Cartan, the reasons for this rebellion were modestly concealed[63] — by his biographer Lucienne Félix but also by others. The text by Montel, thanks to which we can identify the "elements other than scientific value" as being

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[61] Henri Lebesgue, grand mathematician du début de ce siècle; pour moi, l'ami le plus généreux, le plus sûr et le plus loyal.

En 1934, je posais ma candidature à l'Académie des Sciences. Je ne fus pas élu, les suffrages étant allés à un grand mutilé de la guerre de 14. Lebesgue considéra qu'une injustice avait été commise et refusa, pendant trois ans, de paraître aux séances.

Il n'y retourna qu'en 1937, qu'après mon élection, qui fut obtenue par un nombre élevé de voix [...]

[62] A more recent version of [Lebesgue 1991]. Apparently Henri Cartan found one by one the few letters Lebesgue wrote to his father (see note 64).

[63] This evokes the enigmatic allusion made by Laurent Schwartz in his book [1997, p. 152] to a professor of the École polytechnique who was both a collaborator and a member of the Academy of Sciences:

> Some of the professors [of École polytechnique] were collaborators. One of them did everything possible to keep the École in Occupied Paris; this same one, after Hadamard left for the United States, demanded, happily without any success, that he be excluded from the Academy of Sciences because he no longer attended the sessions. [Certains des professeurs étaient des collaborateurs. L'un d'eux fit tout pour que l'x reste dans Paris occupée; le même, après qu'Hadamard fut parti aux États-Unis, demanda, sans succès heureusement, qu'il fût exclu de l'Academie des sciences parce qu'il ne participait plus jamais aux séances.]

Julia's war wound, was written only in 1966; Henri Cartan discovered the existence of Lebesgue's letter to his father not very long ago: he did not know it at the time of the publication[64] of [Lebesgue 1991] in 1991.

We shall see that the 1965 controversy would also cause some revelations.

VI.3 The third centenary of the Institut de France

In 1966, the Institut de France (that is, the group of the five academies) celebrated its tricentenary. At this time, the Academy of Sciences published two volumes in which its sections wrote the history of these three hundred years, each for its own discipline, pointing out especially the role played by the Academicians (those who became members, those who were awarded prizes of this institution,...). The geometry section[65] entrusted Paul Lévy with this work.

During the year 1965, Paul Lévy gave his draft to several mathematicians to read and received an abundant correspondence, letters from Denjoy, from Fréchet and, more to the point, three letters from Montel, two letters from Julia, one from Milloux and one from Ostrowski[66]. Some mathematicians received the project directly from the Academy of Sciences (this is the case of Fréchet). Others answered a request from Paul Lévy.

The file in which these letters are kept also contains a summary (one typed page) of Julia's work, which must have been added by Julia to one of his letters.

A priority quarrel

Paul Lévy was placed in the position of an arbitrator in a priority quarrel which broke out, at that time, in 1965, between two Academicians, both living, about work going back about fifty years. These are our two old acquaintances Julia and Montel. Gaston Julia was 72, Paul Montel 89—both rather old[67]. Concerning these old priority discussions—we have already seen written tracks

[64] It is amusing to follow the discovery of Lebesgue's letters to Élie Cartan in the footnotes of [Lebesgue 1991], a text which was typed without any automatic system of cross-references:

– note 900, p. 430, October 1st 1990, Henri Cartan informed the editors that he owns letters from Lebesgue to his father,

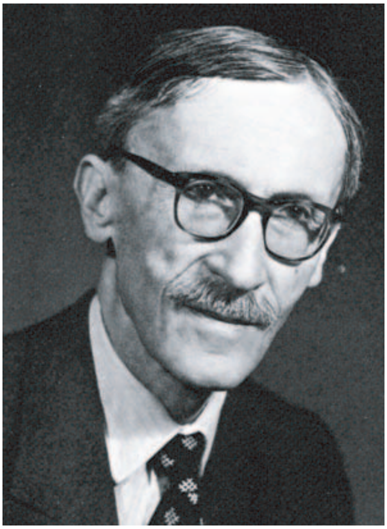
– note 977, p. 439, he communicated the only letter he owned (about de Rham's thesis, in 1930),

– note 1064, p. 453, he found another letter (dated 1925, about Dautry).

[65] At that time, there were, at the Academy of Sciences, mathematicians in the geometry, mechanics and even astronomy "sections". The mechanics section entrusted André Lichnerowicz with the writing of its text.

[66] Denjoy's and Ostrowski's letters were published in [Denjoy 1980].

[67] Paul Lévy, born in 1886, was himself seventy nine.



Paul Lévy (1886–1971)

from 1932 (in § VI.2) and we shall see that the fundamental problem is even older.

As the firm handwriting of his letters shows, Montel was, despite his age, anything except senile. Julia's handwriting was more shaky but he could have had arthritis problems.

These letters are about what were called "Julia points" or "irregular points" of a family of holomorphic functions[68]. The debate is: why give the name "Julia points" to points that Montel had, for some time, named "irregular points"? The papers Montel quoted are old: two *Comptes rendus* notes [1903 ; 1904] of 1903 and 1904 and his thesis [1907]. Those of Julia are dated 1917–18, and then 1919–20; the former are grouped in the memoir [1918f] on iteration, which was awarded the Great Prize of mathematical sciences in 1918, and the latter in his book [1924a].

It is surprising, when we know what happened at the end of 1917, to find that the priority debate involves Julia and Montel. Paul Montel invented normal families, he proved that the property of "being normal at z" is a local property, he named "irregular" the points at which a family is not normal (this is a closed subset). Then Fatou and Julia, in their work on iteration, investigated the properties of the set of irregular points in the case of the family of iterates of a function.

[68] In the case of iteration of a rational fraction, Julia points are those in the set $\mathcal{F}$ of § III.2—namely they are those that constitute what has, since the 1980's, been called the Julia set.

One could consider this polemic to be an argument between stubborn old men, who, besides, did not even speak to each other, and that it is not of much interest. In addition, the question is about rather old-fashioned mathematical topics. However, we shall find, as we examine two of these letters, some information (or at least affirmations) that was never explicitly formulated, at least not in public.

Here are (a part of) these letters. The underlining in the handwriting has been replaced by italics. We stress that all these letters were written and sent to Paul Lévy. It is not even certain that either of the protagonists knew what the other was writing. Montel was very warm, even friendly, with Paul Lévy, as the tone of his letters shows. He explains delicately: it is not that you quote yourself too much, it is only that you do not quote the others enough... He was nevertheless quite firm—and rather angry—on the priority questions he raised.

Julia's style is direct, not especially friendly[69]. He was clearly very irritated. A transcription would give only a very weak idea of the demeanour of his letters (with their underlining and double underlining, a hand-written version of the wealth of italics in his paper [1918f] that has already been mentioned in § III.1). These letters of Julia are thus not reproduced *in extenso* here—besides, they do not really give new information[70].

In this chapter, the accompanying text is smaller, like this, while the text of the letters is normal size (and not indented as in the rest of the book).

Montel-Lévy, July 9th 1965

The first letter is from Paul Montel to Paul Lévy. It is dated July 9th 1965. It clearly answers a question of Paul Lévy, who probably asked where irregular points came from (Montel himself gave the definition).

July 9th [6?] 1965
My dear Colleague and Friend,

I have done some bibliographical research.

In my thesis (1907), one reads p. 3[71]:

"A point around which the convergence is not uniform is called *irregular*; I show that the set of irregular points is perfect, continuous, non-dense and

[69] Gaston Julia and Paul Lévy were colleagues at the École polytechnique for many years. But how to know what their relationship was....

[70] We sought permission from Paul Lévy's family (his daughter Marie-Hélène Schwartz and his grand-daughter Claudine Schwartz) to publish these letters. Ostrowski's letter had already been published. Having no information on Montel's and Milloux' beneficiaries, we did not seek any other permission.

[71] In the published version [1907] of Montel's thesis in the *Annales de l'École normale supérieure*, the definition of irregular points is given on p. 320. The proof of perfection follows. The other properties are proved on p. 334.

all in one piece with the boundary of the domain, and I study how the series behaves in the neighbourhood of such a point.

The main results of this work were stated in Notes that were inserted in the *Comptes-rendus de l'Académie des sciences* (February 1903 and June 1904)[72]."

In my scientific Notice, one reads p. 23: "the problem of iteration was completely solved in two memoirs of first importance, one of them being due to P. Fatou and the other to M. Julia[73]. Both used this theory (that of normal families) as a tool. 'We have used exclusively propositions of M. Montel, P. Fatou writes[74]. We thus recognised that the limitation of the domains of convergence of the iterates of a rational fraction was related to the investigation of a perfect set that we define as the set of points at which the iterates do not constitute a normal sequence.' As for M. Julia, he writes about the set E of repelling points that he is about to study: 'This is a countable set. The work of M. Montel on normal sequences immediately gives simple properties for any point P of E.'["]

Julia introduced in the Comptes Rendus a set he wrongly pretended to be perfect. Fatou pointed it out. He corrected it, without quoting Fatou[75].

Julia established the existence of the lines I called[76] J, assuming that the function had an asymptotic value. When he came to see me, I immediately showed him that this assumption was useless[77]. He thus published the general result, without quoting me.

I shall bring you the Thesis & the Notice and perhaps the Notes of 1902 and 1903[9?] on Monday[78].

Sincerely
Paul Montel

One also reads in the Notice p. 17: "I already encountered the set of these (irregular[79]) points in my thesis in 1907 and in my 1912 memoir. But it is

[72] Montel's Notes [1903; 1904] are actually dated May 25th 1903 and February 22nd 1904, respectively.

[73] P. Fatou and M. Julia, never M. Fatou and M. Julia, and even less G. Julia and P. Fatou. Montel was a friend of Pierre Fatou, not of Gaston Julia.

[74] One recognises here a quotation from the introduction of [Fatou 1919b] already used on page 93.

[75] This is probably about the set E', and is hence an imperfect recollection of the mistake (that he corrected) we mentioned in Remark III.1.2.

[76] One probably recalls (see § IV.3) that the J-lines (Montel even thinks he himself gave them this name) have nothing to do with the J-points.

[77] His proof is given below (see page 221).

[78] Montel and Lévy saw each other at the Academy of Sciences, on Mondays.

[79] We interpose here a remark (as compelling a remark as the one included in the Note 62 of Chapter II: in his memories [1985] collected by his nephew, Szolem Mandelbrojt states: "I am the one who had the idea of irregular points, which were used by Montel" (Szolem Mandelbrojt arrived in Paris in 1920...).

P. Fatou who was the first, in his investigations on iteration, to introduce systematically the set of points at which a family is not normal and to show its importance, which would increase owing to the work of M. Julia and M. Ostrowski.[”] PM[80]

After this, probably on July 18th (see below), Paul Lévy must have sent him and Julia a preliminary version of his text.

---

[80] Mon cher Confrère & Ami,

J'ai fait quelques recherches bibliographiques.

Dans ma Thèse (1907), on lit p. 3:

“Un point autour duquel la convergence n'est pas uniforme est appelé *irrégulier*; je fais voir que l'ensemble des points irréguliers est parfait, continu, non dense et d'un seul tenant avec la frontière du domaine, et j'étudie comment se comporte la série dans le voisinage d'un de ces points.

Les principaux résultats de ce travail ont été énoncés dans des notes insérées aux *Comptes-rendus de l'Académie des sciences* (février 1903 et juin 1904).”

Dans ma Notice scientifique, on lit p. 23: “ Le problème (de l'itération) a été complètement résolu dans deux mémoires de première importance dont l'un est dû à P. Fatou et l'autre à M. Julia. Tous deux ont utilisé cette théorie (celle des familles normales) comme instrument de travail. ‘Nous avons fait exclusivement usage dans nos recherches des propositions de M. Montel, écrit P. Fatou. Nous avons ainsi reconnu que la limitation des domaines de convergence des itérées d'une fraction rationnelle est liée à l'étude d'un ensemble parfait que nous définissons comme l'ensemble des points où les itérées ne forment pas une suite normale.’ M. Julia écrit, de son côté au sujet de l'ensemble E des points fixes répulsifs qu'il se propose d'étudier: ‘C'était un ensemble dénombrable. Les travaux de M. Montel sur les suites normales donnent immédiatement des propriétés simples pour tout point P de E.’[”]

Julia avait introduit dans les Comptes Rendus un ensemble qu'il déclarait à tort parfait. Fatou le lui fit remarquer. Il rectifia sans citer Fatou.

Julia avait établi l'existence des droites que j'ai appelées J en supposant que la fonction avait une valeur asymptotique. Quand il vint me voir, je lui montrai aussitôt que cette hypothèse était inutile. Il publia donc le résultat général, sans me citer.

Je vous apporterai lundi la Thèse & la Notice et peut-être les Notes de 1902 et 1903[9?].

Cordialement

Paul Montel

On lit aussi dans la Notice p. 17: “J'avais déjà rencontré l'ensemble de ces points (irréguliers) dans ma thèse en 1907 et dans mon mémoire de 1912. Mais c'est P. Fatou qui, le premier, dans ses recherches sur l'itération, a introduit systématiquement l'ensemble des points où une famille n'est pas normale et en a fait voir l'importance qui devait s'accroître encore par les travaux de M. Julia et de M. Ostrowski.[”] PM

Montel-Lévy, July 31st 1965

Another letter of Montel was written July 29th. He must have brought it to the Academy of Sciences on August 2nd to give it to Paul Lévy and, the latter not being there, sent it by mail (the envelopes were not kept).

July 31st 1965
My dear colleague & friend

I read your work without delay. I find it excellent, very clear and informative.

You are afraid of having quoted yourself too much. You state necessary facts for the exposition. But reading your name is more striking than reading the names of others. On the other hand, the living authors might perhaps think that they were not quoted so much, less owing to pride than to the deep knowledge of the domain they explored.

[...]

As for normal families p. 22 you quote with reason Julia, A. Bloch, Valiron; there are also beautiful works by Mandelbrojt, Dieudonné, Baganas, Marty[81], Dufresnoy, Fatou, Ostrowski, etc.

The first use of irregular points is due to Fatou, in his work on iteration and, later Ostrowski, Milloux, de la Vallée Poussin, Miranda (cf. cf. Bulletin de Liège, p. 264[82]).

On the other hand, I pointed out, from the very beginning, that an irregular point has the same properties as an essential singular point[83] when the family reduces to a single function (cf. p. 23, in fine, in P. Lévy).

Forgive this pro domo plea.

[...]

Finally, I point out that there is no identity between normal families and compact familes[84].

---

[81] Frédéric Marty, who entered the ENS in 1928, wrote, between 1931 and 1937 several papers on the distribution of the values of a meromorphic function, algebraic functions, and coverings. He was one of the devotees of the Hadamard Seminar and he also wrote two talks for the Julia Seminar. He is the author of a normality criterion that is stated in [Ahlfors 1978, p. 226] and that justifies our quoting him in this book. He was assistant-professor [maître de conférences] at Marseilles. A flying officer, he was killed in 1940, when he was not yet 30, one of the normaliens "dead for France" of World War II who did not die in deportation and was not murdered as a resistant. His father, Joseph Marty, also a mathematician, normalien in class entering the school in 1905, was killed in 1914.

[82] There is an article of Carlo Miranda in the *Bulletin de la société royale des sciences de Liège* in 1962, but it has no page 264, indeed we were not able to find any paper of Miranda with a page 264.

[83] For the analogies between irregular points and essential singularities, see our § I.5. In the article [Montel 1907], this is what is proved at the bottom of page 327.

[84] What Montel points out here is that a normal family is only relatively compact.

In the latter case, the limit function is in the family, but in the former it is not.

Kind regards
Paul Montel

[added in the margin] I thought I would see you at the Academy today, August 2nd. Have a good holiday![85]

Montel-Lévy, undated

This probably accompanied the previous letter (but there is no date on this document); it contains two pages by Montel (a list of results), a re-writing by Paul Lévy on a third page, and Montel's comments on this re-writing, on the same page.

In my 1907 thesis "On infinite sequences", one reads, in the introduction, p. 3.

"a point around which the convergence is not uniform is called *irregular*"

–

A Comptes rendus note of November 25th 1907, called "On irregular points of converging series of analytic functions" contains the following lines: "These

[85] Mon cher confrère & ami,

J'ai lu sans tarder votre travail. Je le trouve excellent, très clair et fort instructif.

Vous craignez de vous être trop cité. Vous énoncez des faits nécessaires à l'exposé. Mais la lecture de votre nom nous frappe plus que celle des autres. En revanche, les auteurs vivants trouveront peut-être qu'ils n'ont pas assez été cités moins par orgueil qu'à cause de la connaissance approfondie du domaine qu'ils ont exploré.

[...]

Pour les familles normales p. 22 vous citez avec raison Julia, A. Bloch, Valiron; il y a aussi de beaux travaux de Mandelbrojt, Dieudonné, Baganas, Marty, Dufresnoy, Fatou, Ostrowski, etc.

La première utilisation des points irréguliers est due à Fatou dans son travail sur l'itération et, plus tard Ostrowski, Milloux, de la Vallée-Poussin, Miranda (cf. Bulletin de Liège, p. 264).

D'autre part, j'ai signalé dès le début qu'un point irrégulier a les mêmes propriétés qu'un point singulier essentiel dans le cas où la famille se réduit à une fonction unique (cf. p. 23, in fine, dans P. Lévy).

Excusez ce plaidoyer pro domo.

[...]

Je signale enfin qu'il n'y a pas identité entre famille normale et famille compacte.

Dans ce dernier cas, la fonction limite appartient à la famille, mais non dans le premier.

Cordiales amitiés

Je pensais vous voir à l'Académie aujourd'hui 2 août. Bonnes vacances!

irregular points have, with respect to the family of the functions $f_n(z)$, properties that are stated in the same way as some properties of isolated essential singular points of analytic functions"

–

In the 1912 memoir at the Annales scientifiques de l'École Normale Supérieure, the definition of irregular points in the general case is given and investigated.

–

Finally, in the 1933 Notice on scientific Works, see Introduction p. 17, bibliographical information is given.

–

It is through using irregular points that I gave to Julia the general proof of the existence of what I called a "Julia line".

PTO

Julia came to me one day to state his proposition on the existence of a line of accumulation of the values of an entire function endowed with an *asymptotic value.*

I immediately showed him a proof which works in every case, demonstrating that the presence of an asymptotic value is unnecessary.

Let $f(z)$ be the entire function. The sequence of functions $f_n(z) = f(nz)$, $n = 1, 2, \ldots$ takes[86], in the unit circle $|z| < 1$, the values of f in the circles $|z| < n$. This family cannot be normal in $|z| < 1$ since in this case, as $f_n(0) = a_0$, the function $f(z)$ would be bounded. There thus exists a point z_0 in the unit circle around which all the functions $f_n(z)$ take any value for n large enough. The line containing the points nz_0 answers the question[87].

I wanted to publish this result but Borel told me that we had to help the disabled Julia[88].

Julia never spoke of this proof and, as a gesture of thanks, changed the name of irregular point to introduce his name[89].

–

This is some information. With my kind regards.

Paul Montel[90]

---

[86] Montel first wrote $f(nz)$, then he crossed out the z and wrote $f(z/n)$. Similarly, the $|z| < n$ below replaced a deleted $|nz| < 1$.

[87] We also must ensure that such a z_0 exists, which is not 0. For this, it is sufficient, as Montel has in his book [1927], to consider an annulus rather than the unit circle.

[88] To my knowledge, this is the only place where a mathematician says explicitly that Julia enjoyed preferential treatment because of his being a broken face. In 1927, when he wrote his book [1927], Montel did not mention that he was himself the author of that proof.

[89] We have seen that it was Ostrowski who was responsible for the terminology.

[90] Dans ma thèse de 1907 "Sur les suites infinies", on lit dans l'Introduction, p. 3.

"un point autour duquel la convergence n'est pas uniforme est appelé *irrégulier.*"

The third page is written, first by Paul Lévy:

p. 22, 3rd paragraph. New writing.

We also owe to Montel the beautiful theory of *normal families*, which are sets of holomorphic functions [circled] in a domain D and that satisfy there a condition which is analogous to that characterising compact sets$^{\times}$. This theory is related to the above-mentioned Schottky theorem, that gives conditions for a family to be normal. It also rests on a theorem of Montel, according to which, if a family is not normal, there exists at least one point a in D which is responsible for this fact, namely, there exists no neighbourhood of a in which it is normal. The points with that property are called *irregular points*. This theorem of Montel has important applications, which were studied firstly by P. Fatou, then by Ostrowski, Milloux, de la Vallée Poussin, and Julia. One calls a Julia half-line any half-line...

The rest is unchanged.

—

Une note aux Comptes-Rendus du 25 novembre 1907, intitulée “Sur les points irréguliers des séries convergentes de fonctions analytiques” contient les lignes suivantes: “Ces points irréguliers possèdent, par rapport à la famille des fonctions $f_n(z)$, des propriétés qui s'énoncent de la même manière que certaines propriétés des points essentiels isolés des fonctions analytiques.”

Dans le Mémoire de 1912 aux Annales scientifiques de l'École Normale Supérieure, la définition des points irréguliers est donnée dans le cas général et étudiée.

Enfin, dans la Notice sur les Travaux scientifiques de 1933, voir Introduction p. 17, des indications bibliographiques sont données.

C'est par l'utilisation du point irrégulier que j'ai donné à Julia la démonstration générale de l'existence de ce que j'ai appelé “droite de Julia”.

TSVP

Julia est venu me trouver un jour pour m'énoncer sa proposition sur l'existence d'une droite d'accumulation des valeurs d'une fonction entière pourvue d'*une valeur asymptotique.*

Je lui ai indiqué aussitôt une démonstration s'appliquant à tous les cas et montrant que la présence d'une valeur asymptotique est inutile.

Soit $f(z)$ la fonction entière, la suite de fonctions $f_n(z) = f(nz)$, $n = 1, 2, \ldots$ prend dans le cercle unité $|z| < 1$ les valeurs de $f(z)$ dans les cercles $|z| < n$. Cette famille ne peut être normale dans $|z| < 1$ car dans ce cas, comme $f_n(0) = a_0$, la fonction $f(z)$ serait bornée. Il existe donc un point z_0 du cercle-unité qui est irrégulier autour duquel toute fonction $f_n(z)$ prend n'importe quelle valeur pour n assez grand. La droite portant les points nz_0 répond à la question.

Je voulais publier ce résultat mais Borel m'a dit qu'il fallait aider Julia mutilé.

Julia n'a jamais parlé de cette démonstration et, en genre de remerciements, a changé le nom du point irrégulier pour introduire le sien.

Voilà un ensemble d'indications. Avec mes cordiales amitiés.

Paul Montel

× Il don't think it is useful to make things more precise. It is only popularisation. A precise definition would take two or three lines more. Do you think this would be useful?[91].

To this Montel answers, on the same page:

My letter of July 31st was meant for M. Paul Lévy whether useful or not. He will get what he finds useful out of it.

The theory of normal families does not concern only holomorphic functions but also any family of continuous functions. It is related to work of Ascoli and Arzelà and relates to the Schottky th. only in the case of analytic functions[92].

Julia-Lévy, September 16th 1965

Julia responded to the request of Paul Lévy, on September 16th. In his long letter, he listed some affirmations, numbered from 1 to 7, which can be summarised thus: Montel invented normal families, but it was Julia who used the points at which the family is not normal—which Montel called irregular—and these are thus called J-points or Julia points. Julia concluded his letter by suggesting that Paul Lévy slightly modify his text to say this:

> "*The points with that property are called Julia points or J-points by some, irregular points by others*[93]."

Three remarks on this letter:

---

[91] p. 22, 3ième alinéa. Nouvelle rédaction.

On doit aussi à Montel la belle théorie des *familles normales*, qui sont des ensembles de fonctions holomorphes dans un domaine D et y vérifiant une condition analogue à celle qui caractérise les ensembles compacts×. Cette théorie se rattache au théorème de Schottky mentionné ci-dessus, qui donne des conditions suffisantes pour qu'une famille soit normale. Elle repose aussi sur un théorème de Montel, d'après lequel si une famille n'est pas normale, il existe au moins un point a de D qui est responsable de ce fait, c'est-à-dire qu'il n'existe aucun voisinage de a où elle soit normale. Les points ayant cette propriété sont appelés *points irréguliers*. Ce théorème de Montel a d'importantes applications, qui ont été étudiées d'abord par P. Fatou, puis par Ostrowski, Milloux, de la Vallée-Poussin, et Julia. On appelle demi–droite de Julia toute demi-droite...

suite sans changement.

× Je ne pense pas utile de préciser davantage. Il ne s'agit que de vulgarisation. Il faudrait 2 ou 3 lignes de plus pour donner une définition précise. Pensez-vous que ce soit utile?

[92] Ma lettre du 31 juillet s'adressait à M. Paul Lévy à toutes fins utiles ou inutiles. Il en tirera le parti qu'il jugera utile.

La théorie des familles normales ne concerne pas les fonctions holomorphes seules mais toute famille de fonctions continues. Elle est liée aux travaux d'Ascoli et d'Arzelà et ne se relie au th. de Schottky que dans le cas particulier des fonctions analytiques.

[93] Les points ayant cette propriété sont appelés par les uns points de Julia ou points J, par les autres points irréguliers.

(1) Fatou's name does not appear in it.
(2) The question of O/J-points that we discussed in § IV.3 is not mentioned.
(3) There is a reference to a letter by Ostrowski which "is not a spontaneous correction; it was written under pressure from Lebesgue[94]". We have seen no other mention of this.

There are probably traces of this polemic that can be found, in the form of handwritten pencil notes, in Julia's copy of the proceedings of the Zurich International Congress (now at the CIRM library): he marked all the places where his work was quoted, by Nevanlinna and especially in Valiron's talk.

Lévy-Fréchet, September 19th 1965

Let us insert here an excerpt of a letter from Paul Lévy to Maurice Fréchet, taken from [Lévy & Fréchet 2004].

[...] I am more concerned by the fact that I reawakened the old Julia-Montel quarrel regarding normal families. I cannot, in an exposition made more or less on behalf of the Academy, behave like a referee; nor can I suppress all allusions to normal families. I am afraid I will irritate both of them. [...][95]

Ostrowski-Lévy, September 24th 1965

Most probably when he got Julia's previous letter, Paul Lévy, who must have been quite embarrassed, wrote to Milloux and Ostrowski to ask their opinion.

Here is Ostrowski's answer (this letter was published in [Denjoy 1980]; we restore in the French version Ostrowski's nice French form and original grammar):

September 24th 1965
My dear colleague

Back in Montagnola[96], I found here your letter of September 19th and I hurry to answer you on the eve of my departure to the United States (I am going to spend three months at the Mathematics Research Center, University of Wisconsin, Madison, Wisconsin).

As I am the one who introduced the names of Julia points and lines, I have to explain first how I could have missed the memoir in question. M. Montel was kind enough to send me everything he wrote on the theory of functions, except one piece—and this was the memoir at the AENS[97] in which he mentions the

[94] n'est pas une rectification spontanée; elle a été écrite sous la pression de Lebesgue
[95] Je suis plus préoccupé du fait que j'ai réveillé la vieille querelle Julia Montel pour les familles normales. Je ne peux pas, dans un exposé fait un peu au nom de l'Académie, me poser en arbitre; ni supprimer toute allusion aux familles normales. Je crains de les mécontenter tous les deux.
[96] Montagnola is near Lugano, in the Swiss Tessin, on the shore of Lake Maggiore. Ostrowski, who retired from Basel University, lived (and died) in Montagnola.
[97] This is the *Annales de l'École Normale Supérieure* and the article [Montel 1916].

existence of irregular points. On the other hand, the library of the Göttingen Seminar did not receive the AENS series. Thus, this is the only memoir of M. Montel I did not study.

However, the fact under consideration is not expressed l.c. in a very neat way. When M. Montel called my attention to his memoir, I could find the place, but I knew what I was looking for. It is hard to say, with certainty, but I have the impression that, even had I known this memoir, I might have missed this place. M. Julia discovered this remark independently and he used it as a starting point for extremely brilliant results that I admired very much when I read his C.R. Notes.

There is naturally no denying that M. Montel was the first to publish the fact in question. However, I am not sure that, even knowing this, I would not give Julia's name to the points and lines in question.

Besides, this reminds me of an analogous occurrence that happened a long time ago. After a talk at the Deutsche Mathematiker Vereinigung in which I spoke with much admiration of the theorem of choice of M. Montel, the late Frédérick Riesz called my attention to the Arzelà theorem on families of uniformly bounded functions, of which Montel's choice theorem is an immediate consequence. I keenly remember that I found almost bizarre the idea of attributing part of the paternity of the beautiful theory of normal families, which M. Montel was able to develop with so much spirit and imagination, to Arzelà.

It is very painful for me to declare myself on the comparative merits of two mathematicians I admire and to the influence of whom I owe the inspiration of some of my work, which some benevolent colleagues tell me is among the most important of my contributions to our science.

In particular, as I wrote once to M. Julia, I perfectly understand that if I myself had been able to obtain some important results in this theory, this was only because I could stand on M. Julia's shoulders.

However, I must say that all that I just wrote comes from my memory. But, taking my age into account (I will be 72 tomorrow), I usually quote a fact only after having checked it. Unfortunately, I am not able to do so in this case.

I would like to hope, my dear colleague, that your subtlety and your delicacy will allow you to find a satisfying solution for two scientists who so much enriched the scientific glory of France.

If you find it desirable to show this letter to other people, you are allowed to do so.

With my very best regards

A. Ostrowski[98]

[98] Mon cher collègue,

De retour à Montagnola, j'ai trouvé ici votre lettre du 19 septembre et je me dépêche de vous répondre la veille de mon départ pour les États-Unis (je vais passer trois mois auprès du Mathematics Research Center, University of Wisconsin, Madison, Wisconsin).

Puisque c'était moi qui avais introduit les noms des points et des lignes de Julia, il me faut expliquer d'abord comment le mémoire en question a pu m'échapper. M. Montel a eu la bonté de m'envoyer tous ses écrits sur la théorie des fonctions, sauf un seul — et c'était le mémoire des AENS ou [*sic*] il mentionne l'existence des points irréguliers. D'autre part, la bibliothèque du séminaire de Göttingen ne possédait pas la série des AENS. De cette façon, c'était le seul des mémoires de M. Montel que je n'avais pas étudié.

Toutefois, le fait en question n'est pas exprimé l.c. d'une manière trop nette. Quand M. Montel avait attiré mon attention à son mémoire j'ai pu trouver l'endroit en question, mais je savais ce que je cherchais. Il est difficile de le dire avec l'assurance, mais j'ai l'impression que même si je connaissais le mémoire en question, cet endroit aurait très bien pu échapper à mon attention. M. Julia a découvert indépendamment cette remarque et en a fait le point de départ des résultats d'une extrême brillance que j'ai admiré [*sic*] beaucoup quand je lus ses notes dans les C.R.

Il est naturellement incontestable que M. Montel a été le premier à publier le fait en question. Toutefois, je ne suis pas sûr que même en connaissance de ce fait je n'attribuerais pas le nom de Julia aux points et lignes dont il s'agit.

D'ailleurs, ça me rappelle un fait analogue qui se produisit il y a longtemps. Après une conférence devant la Deutsche Mathematiker Vereinigung dans laquelle je parlais avec beaucoup d'admiration du théorème de choix de M. Montel, feu Frédérick Riesz attira mon attention au théorème d'Arzelà sur les familles des fonctions uniformément bornées dans leur ensemble, dont le théorème de choix de Montel est une conséquence immédiate. Je me rappelle vivement que j'avais trouvé presque bizarre l'idée d'attribuer une partie de la paternité de la belle théorie des familles normale [*sic*] que M. Montel a sue [*sic*] développer avec tant d'esprit et d'imagination, à d'Arzelà.

Il m'est très pénible d'avoir à me prononcer sur les mérites comparés de deux mathematiciens que j'admire tous les deux et à l'influence des quels je dois l'inspiration de quelques travaux que des collègues bienveillants m'ont indiqué [*sic*] comme étant parmi les plus importants de mes contributions à notre science.

En particulier, comme j'ai écrit une fois à M. Julia, je me rends parfaitement compte que si j'ai pu, moi, obtenir dans cette théorie quelques résultats importants, ce n'était que parce que j'ai pu monter sur les épaules de M. Julia.

Toutefois, il me faut dire que tout ce que je viens d'écrire est pris de ma mémoire. Or, étant donné mon âge (je serai demain 72) je me suis habitué de ne citer un fait qu'après l'avoir vérifié. Malheureusement, il ne m'est pas possible de le faire dans ce cas.

Je voudrais espérer, mon cher collègue, que votre finesse et votre tact vous permettront de trouver une solution satisfaisante pour les deux savants qui ont tellement enrichi la gloire scientifique de la France.

Si vous trouvez désirable de montrer cette lettre à d'autres personnes, vous y êtes authorisé [*sic*].

Croyez, mon cher collègue, à l'expression de mes sentiments très cordialement dévoués.

Milloux-Lévy, September 30th 1965

This is how Milloux[99] answered Paul Lévy:

Caudéran (Gironde) Sept. 30th 1965
10 rue Lenotre

Dear Sir

Forgive me for not having answered your September 19th letter earlier, I could only read it yesterday, when I came back from a trip.

I think the passage you allude to is the 2nd paragraph on page 288. This is a very thorny question, which makes us regret that two very prominent scientists clash passionately with each other.

The first to draw important consequences from the J-points is certainly M. Julia, in his first memoir, dated 1919 [*sic*]. It is in this memoir that he establishes the existence of what were later called Julia lines (or directions)[100].

As for the J-points themselves, we should find a reference in the previous work of M. Montel; more precisely, in that which was published between 1916 and 1919.

As soon as I am free of the task of the second session of the bachelor examination, which is rather heavy in Bordeaux, namely about the end of October, I will do some bibliographic research; either way, whether it is negative or positive, I will let you know.

M. Montel's memoir "On the families of analytic functions", dated 1916, appeared in the Annales de l'École normale supérieure (t. XXXIII, oct. 1916, p. 224–302). In the introduction, the notion of irregular points does not appear. Superficially browsing the memoir itself, I saw nothing akin to that. But I plan to read it slowly so that nothing will escape me, together with other memoirs.

On the other hand, I believe I remember that M. Valiron attributed to M. Julia the "remark" (this is his very expression) of the existence of a singular (or J) point at least in a domain where a family is not normal.

But we can only base our judgement on texts. In particular, it is impossible to take into account some tales which look rather dubious[101], such as the following, which I learned from a close colleague of M. Montel: during a meeting between the latter and M. Julia which took place before 1919, M. Montel would have discussed with Julia the "remark" I just mentioned. It is then that M. Julia thought of using this remark.

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[99] Henri Milloux (1898–1980), a specialist in holomorphic functions, professor at Bordeaux, a non-resident member of the Academy of Sciences from 1959, the thesis of whom we have already met [Milloux 1924] in § IV.1.

[100] As we know, the J-lines do not appear in the memoir on iteration.

[101] The tale is dubious, the story belongs to oral tradition, but it is nevertheless quoted, most probably because Milloux believed in it. However, it seems that Borel's comment according to which one had to help the disabled Julia has not reached Milloux.

Expecting to tell you about my investigations, I send you, dear Sir, my best and respectful regards[102].

H. Milloux

Montel-Lévy, October 5$^{\text{th}}$ 1965

October [2, crossed out] 5$^{\text{th}}$ 1965 My dear Colleague and Friend

One reads in my 1933 scientific Notice, p. 17.

"The property for a family of functions to be normal is a local property: I proved, in my 1916 memoir (p. 227), that a normal family at each point of

[102] Cher Monsieur,

Excusez-moi de ne pas avoir répondu plus tôt à votre lettre du 19 septembre, dont je n'ai pu prendre connaissance qu'hier, à mon retour de voyage.

Je pense que le passage auquel vous vous référez est le 2^{e} alinéa de la page 288. C'est une question bien épineuse, qui fait fortement regretter que deux savants très éminents se heurtent avec passion.

Le premier qui ait tiré des conséquences importantes des points J est sûrement M. Julia, dans son premier mémoire, datant de 1919. C'est dans ce mémoire qu'il établit l'existence de ce qu'on a appelé ultérieurement droites (ou directions) de Julia.

En ce qui concerne les points J eux-mêmes, il faudrait trouver une référence dans les travaux antérieurs de M. Montel; d'une façon précise, dans ceux qui ont été publiés entre 1916 et 1919.

Aussitôt que je serai libéré des tâches de la 2^{e} session d'examens de licence, assez lourds à Bordeaux, c'est-à-dire vers la fin d'octobre, je ferai des recherches bibliographiques; de toute façon, qu'elles soient négatives ou positives, je vous mettrai au courant.

Le mémoire de M. Montel "Sur les familles normales de fonctions analytiques", datant de 1916, a paru dans les Annales de l'École normale supérieure (t. XXXIII, oct. 1916, p. 224–302). Dans l'Introduction, on ne voit pas apparaître la notion de points irréguliers. En feuilletant superficiellement le mémoire lui-même, je n'ai rien vu qui y ressemble. Mais je me propose de le lire lentement pour ne rien laisser échapper, ainsi que d'autres mémoires.

D'autre part, je crois me rappeler que M. Valiron attribuait à M. Julia la "remarque" (c'est sa propre expression) de l'existence d'un point singulier (ou J) au moins dans un domaine où une famille n'est pas normale.

Mais on ne peut se baser que sur les textes. En particulier, il est impossible de tenir compte de certains récits qui paraissent bien hasardeux, tel que le suivant, que je tiens d'un collègue très lié avec M. Montel: au cours d'une entrevue entre celui-ci et M. Julia, datant d'avant 1919, M. Montel aurait fait part à M. Julia de la "remarque" qui vient d'être rappelée. C'est alors que M. Julia aurait pensé à utiliser cette remarque.

En attendant de vous mettre au courant de mes investigations, je vous prie d'agréer, Cher Monsieur, l'expression de mes sentiments dévoués et déférents.

a domain is normal in this domain. Thus there exists, when a family is not normal, an *irregular* point at which it stops being so. I had already met the set of these points in my Thesis in 1907 and in my 1912 memoir, but it was P. Fatou who was the first, in his investigations on the iteration problem, to introduce systematically the set of points where a family is not normal and who showed its importance, which increased after M. Julia and M. Ostrowski's work.

I showed that irregular points play, with respect to the collection of functions, the role played by the essential points for a single function."

One reads in my Thesis (1907) p. 3

"A point around which the convergence is not uniform is called an *irregular point*: I show that the set of irregular points is perfect, continuous, non-dense, and all in one piece with the boundary of the domain...

The main results of this work were announced in Notes included in the Comptes-rendus of the Academy of Sciences (February 1903 and June 1904)."

(Cf. the C. Rendus of Nov. 25$^{\text{th}}$ 1907 entitled:

"On the irregular points of the convergent series of an analytic function") [*sic*, this should read convergent series of analytic functions]

All the mathematicians in the Theory of functions agree with Ostrowski's opinion. Fatou, in the Notice on scientific work[103], says (p. 18) about his work on iteration:

"... one is led to consider a particular set, the set of *irregular* points at which these iterates cease to form a normal sequence in the sense of M. Montel,...".[104]

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[103] Pierre Fatou's 1921 Notice says precisely, on page 12:

> one is led to consider a particular set: the set of points at which these iterates cease to form a normal sequence in the sense of M. Montel. [on est conduit à considérer un ensemble particulier: l'ensemble des points où ces itérées cessent de former une suite normale au sens de M. Montel.]

[104] Mon cher Confrère & Ami

On lit dans ma Notice scientifique de 1933, p. 17.

" La propriété pour une famille de fonctions d'être normale est une propriété locale: j'ai démontré dès mon mémoire de 1916 (p. 227) qu'une famille normale en chaque point d'un domaine est normale dans ce domaine. Il existe donc, quand une famille n'est pas normale, un point *irrégulier* en lequel elle cesse de l'être. J'avais déjà rencontré l'ensemble de ces points dans ma Thèse en 1907, et dans mon mémoire de 1912, mais c'est P. Fatou qui le premier, dans ses recherches sur l'itération a introduit systématiquement l'ensemble des points où une famille n'est pas normale et en a fait voir l'importance qui devait s'accroître encore par les travaux de M. Julia et M. Ostrowski.

J'ai montré que les points irréguliers jouent, par rapport à la collection de fonctions, le rôle que jouent les points essentiel par rapport à une fonction unique."

On lit dans ma Thèse (1907) p. 3

Julia-Lévy, October 11$^{\text{th}}$ 1965

This letter is much shorter: Paul Lévy asked Julia to write two additional items, and so did he. Here is the first one.

> 1° "After he studied *iteration of rational fractions*, M. Julia applied it to the investigation of their *permutability* $\{R_1[R_2(z)]\} = \{R_2[R_1(z)]\}$ and their *semipermutability* $\{G[R_1(z)]\} = \{R_2[G(z)]\}$ for R_1 and R_2 rational and G meromorphic."[105]

The second one concerned later work of Julia on operators on Hilbert spaces. Julia, who had already written several pages on this three weeks before, could not refrain from adding:

> *NB* As for J-points, it is incontestable that I found the *first examples of existence* in the iteration of entire and meromorphic functions, and that I was the first to *see their interest and to study their properties* together with certain applications[106].

... Since the priority quarrel took place between Julia and Montel, there is no question: the first of them to have used irregular points in the iteration problem was certainly Julia. But... haven't we forgotten Fatou?

Finally, Paul Lévy's text

Here at last is the part of Paul Lévy's published text related to our question (in principle, the names of the Academicians were printed in small capitals while the

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" Un point autour duquel la convergence n'est pas uniforme est appelé *point irrégulier*: je fais voir que l'ensemble des points irréguliers est parfait, continu, non dense et d'un seul tenant avec la frontière du domaine...

Les principaux résultats de ce travail ont été énoncés dans des Notes insérées aux Comptes-rendus de l'Académie des sciences (février 1903 et juin 1904)."

(Cf. la note aux C. Rendus du 25 nov. 1907 intitulée:

" Sur les points irréguliers des séries convergentes d'une fonction analytique")

Tous les mathematicians de Théorie des fonctions partagent l'avis de Ostrowski. Fatou, dans la Notice de ses travaux scientifiques dit (p. 18) à propos de ses travaux sur l'itération:

" ... on est conduit à considérer un ensemble particulier: l'ensemble des points *irréguliers* où ses itérées cessent de former une suite normale au sens de M. Montel,..."

Votre bien affectueusement dévoué
Paul Montel

[105] Après avoir étudié *l'itération des fractions rationnelles*, M. Julia l'a appliquée à l'étude de leur *permutabilité* $\{R_1[R_2(z)]\} = \{R_2[R_1(z)]\}$, et de leur *semipermutabilité* $\{G[R_1(z)]\} = \{R_2[G(z)]\}$, pour R_1 et R_2 rationnelles et G méromorphe.

[106] Pour les points J: il n'est pas contestable que j'en ai trouvé les *premiers exemples d'existence* dans l'itération dans les fonctions entières et méromorphes, et que j'en ai le premier *vu l'intérêt et étudié les propriétés* et certaines applications.

names of the mathematicians who were awarded prizes by the Academy were in italics. However, there are some errors, for instance, the name of the once again unlucky Fatou should have been in italics):

We also owe to MONTEL the beautiful theory of *normal families*. These families are certain sets of analytic functions defined on a domain D such that, from any infinite subset, a sequence of functions can be extracted, which tends to a limit, in the sense of uniform convergence in the closed complex plane. MONTEL looked in particular at the case of holomorphic functions that admit two exceptional values in D and at the case of meromorphic functions that have three. He was thus led to a new proof of PICARD's and Schottky's theorems, thus happily completing BOREL's work on the first of these theorems.

MONTEL also proved, as early as 1914, that if a family is normal at every point of D (namely, if every point has a neighbourhood in which it is normal), then it is normal in D (1). Hence if it is not normal, there must be in D at least one *irregular point* at which it is not normal. This fact, and the importance of the irregular points, have been emphasised in apparently[107] independent work of Fatou and JULIA on the iteration of rational functions. The beautiful memoir of JULIA, crowned by the Academy in 1918, was the basis of the work of MILLOUX and Ostrowski, and the latter gave irregular points the name JULIA *points*. JULIA applied these results to the study of the conditions of permutability of two rational fractions[108]. He then applied irregular points to the theorem of existence of JULIA *lines*: for any entire function $f(z)$ that is not reducible to a polynomial, there exists at least one half-line such that, in

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(1) MONTEL's priority being often forgotten, let us point out that this statement is on page 227 of his 1914 memoir printed in 1916 (*Ann. Éc. Norm. Sup.* 33, p. 223–302). Furthermore, let us recall that MONTEL was also the author of beautiful work on series of polynomials.

any angle containing it, $f(z)$ takes infinitely many times any non-exceptional value. A line containing such a half-line is called a JULIA line. Naturally, all the lines parallel to it have the same property; one can therefore speak of JULIA directions.

[...]

In a class of ideas close to those of PICARD, let us mention the beautiful work of R. Nevanlinna on meromorphic functions, and subsequently by G. JULIA, *G. Pólya*, *G. Valiron* and MILLOUX, and also by L. Ahlfors, who proved the correctness of a hypothesis of A. DENJOY: an entire function of order α has at most $(2\alpha + 1)$ asymptotic values[109].

[...]

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107 What is the reason for this adverb "apparently" here?

108 And, as none of his correspondents mentioned it to him, Lévy did not mention here Fatou's work on permutability.

109 [...] On doit aussi à MONTEL la belle théorie des *familles normales*. Ces familles sont des ensembles de fonctions analytiques définies dans un domaine D tels que,

And Pierre Fatou?

Paul Montel was a friend of Pierre Fatou; he and Paul Lévy inevitably saw Fatou a lot between the years 1910 and 1920 as they all sat on the SMF committee [110]. At that time, as we have said, the SMF would organise quite a lot of "sessions" in which people would speak of mathematics. It is rather surprising that neither of them remembered Fatou's work—who was no longer

de tout sous-ensemble infini, on puisse extraire une suite de fonctions qui tendent vers une limite, au sens de la convergence uniforme dans le plan complexe fermé. MONTEL s'est en particulier intéressé aux cas des fonctions holomorphes qui admettent dans D deux valeurs exceptionnelles et à celui des fonctions méromorphes qui en admettent trois. Il a été ainsi conduit à une nouvelle démonstration des théorèmes de PICARD et de Schottky, qui complète heureusement les travaux de BOREL sur le premier de ces théorèmes.

MONTEL a aussi, dès 1914, démontré que, si une famille est normale en tout point de D (c'est-à-dire que chaque point a un voisinage où elle est normale), elle est normale dans D ([1]). Si donc elle n'est pas normale, c'est qu'il y a dans D au moins un *point irrégulier* où elle n'est pas normale. Ce fait, et l'importance des points irréguliers ont été bien mis en évidence dans des travaux apparemment indépendants de Fatou et de JULIA sur l'itération des fonctions rationnelles. Le beau mémoire de JULIA, couronné par l'Académie en 1918, a été la base des travaux de MILLOUX et d'Ostrowski, et ce dernier a donné aux points irréguliers le nom de *points de* JULIA. JULIA a appliqué ses résultats à l'étude des conditions de permutabilité de deux fractions rationnelles. Il a enfin appliqué les points irréguliers au théorème d'existence des *droites de* JULIA: pour toute fonction entière $f(z)$ qui ne se réduit pas à un polynôme, il existe au moins une demi-droite telle que, dans

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([1]) La priorité de MONTEL étant souvent oubliée, précisons que cet énoncé se trouve à la page 227 de son mémoire de 1914 imprimé [en?] 1916 (*Ann. Éc. Norm. Sup.* 33, p. 223–302). Rappelons d'autre part que MONTEL est aussi l'auteur de beaux travaux sur les séries de polynômes.

tout angle la contenant, $f(z)$ prend une infinité de fois toutes les valeurs non exceptionnelles. Une droite contenant une telle demi-droite est appelée *droite de* JULIA. Naturellement, toutes les droites qui lui sont parallèles ont la même propriété; on peut parler des *directions de* JULIA.

[...]

Dans un ordre d'idées encore voisin de celui de PICARD, mentionnons les beaux travaux de R. Nevanlinna sur les fonctions méromorphes, poursuivis par G. JULIA, *G. Pólya*, *G. Valiron* et MILLOUX, et aussi par L. Ahlfors, qui a démontré l'exactitude d'une hypothèse d'A. DENJOY: une fonction entière d'ordre α a au plus $(2\alpha + 1)$ valeurs asymptotiques.

110 On Fatou and the SMF, see § V.8. In 1909, Fatou and Montel were vice-secretaries; in 1910, Montel was secretary, Lévy and Fatou were both vice-secretaries; in 1915, Lévy and Montel were secretaries, Fatou was vice-secretary; in 1922, Fatou, Lévy and Montel were all vice-presidents.

present and could not defend himself. Already in 1921, to conclude the warm report[111] he was writing on Fatou's work, Hadamard would note:

> Such inspiring work has perhaps been too often forgotten and deserves the full attention of those who devote themselves to the mathematical science of our country.

Resentment

It seems to me that Montel was perfectly able to understand, and even to accept, the fact that he, Montel, was the one who invented the notion of an irregular point, but that Julia used it so beautifully that the "J-points" terminology was not really shocking—this was what Ostrowski kindly said in his letter. It also seems to me that what this controversy shows is rather the resentment that Montel accumulated against Julia. For some reason or some other, in 1965, he decided at last to say that the young Julia did not always behave very correctly and that this was accepted by the community of mathematicians because of his war wound.

VI.4 As a conclusion: O for a biography of Gaston Julia

Readers of this text will have understood, that Gaston Julia, a beloved son of the "parental" generation after the end of World War I, a visible and unforgettable icon—as a "broken face"—of the sacrificial generation of which he was both one of the most brilliant representatives and one of the few survivors, who worked in a long-lasting hysterical anti-German atmosphere, accumulated rewards, promotions, honours, and signs of recognition:

- five prizes of the Academy of Sciences from 1917 to 1921,
- two Peccot courses[112],
- positions at École polytechnique, at ENS, at the Sorbonne,...
- a talk (one of the *grosse Vorträge*) at the Zurich international Congress,
- an early election to the Academy of Sciences,
- ...

There is no doubt that, after having been an extremely gifted, quick and brilliant student, Julia became a very good mathematician. However, the honours may have been slightly excessive, the weight of the expectation placed on

[111] Fatou file again, in the archives of the Academy of Sciences.

[112] A Peccot course is a honorary distinction, which at that time was a salaried position. A few years before, when Lebesgue gave his, the salary was 3 000 F, about half the yearly salary of a professor in the provinces. See [Lebesgue 1991].

The first recipients of this manna were Borel (in three consecutive years), Lebesgue, Baire, Lebesgue again, Servant, Boutroux (in two consecutive years), Zoretti...

him by part of the mathematical community must have been heavy to bear at times.

It seems obvious that Julia was not always very magnanimous towards his colleagues (recall the tone of his letter [1917]). However, the extinction of the generation qualified in France as parental (Hadamard, Borel... and especially Picard), and another war in which the exaggerated patriotism of the previous one would in some cases be reincarnated in political collaborationism—all this was necessary before it became possible to point out that his honours might have been excessive. Nobody expressed himself publicly (this is the so-called esprit de corps). It was in their letters, that were not intended to be published, that some contemporaries of Julia eventually wrote that, yes, the atrocious war wounds of Julia had given him some rights that would not have been accorded to others[113].

One cannot give a better current evaluation of the competing work of Fatou and Julia on iteration than Milnor does [2006a, p. 40]:

> The most fundamental and incisive contributions were those of Fatou himself. However, Julia was a determined competitor and tended to get more credit because of his status as a wounded war hero.

Difficulties for the biographer

The biographical data we have on Gaston Julia's childhood and youth appear as a kind of heroic tale leading to the glorious 1915 wound. The son of a mechanic who was sent to the lycée in Oran, his school successes, the notable way he learned German (in what was the second year of secondary school), his excellence in mathematics, the agrégation problem he was able to solve before he even passed the baccalaureate, the typhoid which delayed his entry to the Parisian lycée Janson de Sailly (and which, as for the wound to come, which it seems to prefigure, did not hinder his work), his unique shortened preparatory year leading him to the most brilliant success so that at age eighteen he was ranked first both at the ENS and at the École polytechnique—it is quite hard, under these iconic images to imagine who was the twenty-one-year old young man who had not already received the terrible wound which would dominate the rest of his life, so much so that he described it as a "second birth[114]".

The 1914 letters from the soldier Julia to Borel[115] show, however, a young mathematician mostly concerned with writing up lectures for the *Collection de monographies sur la théorie des fonctions* he wanted to finish and publish as soon as possible.

[113] He represented "a generation based both on self-consciousness and on the regard of others, whether younger or older" [une génération fondée à la fois sur la conscience de soi et sur le regard des autres, plus jeunes ou plus âgés] [Sirinelli 1992].

[114] For instance in the article [1942–1950] he devoted to Lagrange, born on a January 25^{th}... the date of this second birth.

[115] As, always the Borel collection, archives of the Academy of Sciences.

As early as the 1930's, the story of Gaston Julia was set in stone. The speeches collected in [Julia 1970] would rehearse again and again the same heroic story, from the German lessons at the lycée and the examinations in 1911 to the war wound and the army citation that young people in the audience would hear stand up [Favard 1961, p. 239][116].

The twenty-one-year-old mathematician, sent to war and atrociously injured turned into a glorious hero for whom one is obliged to feel respect and admiration, but for whom it has become hard to feel compassion. Julia and his leather mask became an icon, an untouchable icon as we have said, but also an inaccessible one.

[116] After all this glory, it is rather unseemly that the testimony of a nephew, the description of the hero's relation with a parrot or of his apprenticeship in tango, should appear.

Appendix

In this appendix, we publish:

– two letters, from Montel and Lebesgue to Élie Cartan (these two are "new" in the sense that they did not even appear in the first (French) version of this book),

– a report written by Hadamard on Fatou

– two letters from Fatou to Fréchet

– a few letters from Fatou to Montel.

In every case, we give a translation in English, followed by the original text, in a smaller font.

Two letters to Élie Cartan

We publish here two letters sent to Élie Cartan before the 1934 election at the Academy of Sciences. The first is a long and beautiful letter of Lebesgue which explains to Cartan why he should vote for Montel and expresses some conclusive arguments in favour of Montel and against Julia. The second letter was written by Montel after a discussion he had with Cartan. We note that there was no letter of Julia to Cartan with these two letters, which is easy to understand, as we explain below.

Lebesgue's letter is not dated. The "Monday when I came back from the Academy" cannot help in determining the date: Monday was the usual day for the Academy sessions. It is about an election at the Academy for which both Julia and Montel were candidates and there was only one, since Julia was elected at his first attempt. The date is thus November or December 1933 or January 1934. It is more likely to be December or January, since Lebesgue mentions the "beginning of November" (and not "the beginning of the month").

Montel's letter is dated December 24^{th} 1933. It deals with the same period, the same discussion. Its timing with respect to Lebesgue's letter is not completely certain: either Montel's explanations are part of the "dissection"

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Lebesgue mentions and so Lebesgue's letter was written later, or the discussion with Montel and his letter were provoked by Lebesgue's letter and so it was written slightly before...

We start with a few comments, both on this issue and on the mathematics in question in these two letters.

Digression (No letter from Julia to Élie Cartan). As far as we know, there exists no letter from Julia to Cartan on this topic and it is very probable that there was no such letter: Julia was living in Versailles and Cartan in Le Chesnay[1], so they had numerous opportunities fro discussion. Let us quote an excerpt of a speech Julia gave a few years later, on May 18th 1939, for Élie Cartan's jubilee (reproduced in [Julia 1970]).

> Time passes; your former student, now your colleague, had to emigrate, as you did, to the "Water City" [Versailles]. It is now every week that the train brings us together to take us back to Versailles, lost in the crowd of suburb lovers. The coach is almost always full and noisy, but the lack of comfort does not disturb us, with your astonishing physical robustness of a man born in a country family. We would carry on casual conversations, in which academic, professional or mathematical discussions met with all kinds of controversies. [...]
> This was the time when you invite me to your Le Chesnay house and to the small austere study that was relieved only by the square of a window opening on to branches, and, in the shadows, by an hospitable Morris armchair. During frequent visits there we could, better than in the E classroom[2], better than in the suburb trains, better than in the Versailles avenues, rehearse those words, interrupted by silences, in which the inner man comes to light. [...][3].

[1] Their addreses, on February 1st 1934 can be found in the booklets "Vie de la société" in the *Bulletin* of the SMF,

– 27 avenue de Montespan, Le Chesnay, for Cartan,

– 4 bis rue Traversière, Versailles, for Julia.

Montel was living in Paris, 79 rue du Fbg St-Jacques.

[2] A classroom at the ENS, in which Julia took courses by Élie Cartan in 1914, as he mentions at the beginning of the same text.

[3] Du temps passe; votre ancien élève devenu votre collègue, a dû comme vous émigrer vers la "cité des eaux". Désormais, c'est presque chaque semaine que le train nous réunit pour nous ramener à Versailles, perdus dans la foule des amateurs de banlieue. Le wagon est presque toujours plein et bruyant, mais l'inconfort ne vous gêne pas, avec votre résistance physique étonnante d'homme issu de la terre. Nous y poursuivons des conversations à bâtons rompus, où les propos universitaires, professionnels ou mathématiques se mêlent aux controverses de tout genre. [...]

C'est vers cette époque que vous m'avez appelé dans votre maison du Chesnay et dans l'austère petit bureau qu'égayait seulement un carré de fenêtre ouvert sur des branches, et, dans l'ombre, un fauteuil Morris aux bras accueillants. Au cours de fréquents visites, on pouvait là, mieux que dans la salle E, mieux que dans les trains de banlieue, mieux que dans les avenues de Versailles, égrener ces propos, coupés de silences, où se révèle l'homme intérieur. [...]

Hence Julia spoke with Cartan in the train, in the streets in Versailles, and even in his house at Le Chesnay where Cartan invited him. The text allows us to give an approximate date for this "invitation": the musician Jean Cartan was dead[4] (this fact is mentioned in the subsequent sentences) on the one hand, and Julia was not yet academician (as the next paragraph says) on the other. Hence between 1932 and 1934, during the period we are interested in.

Élie Cartan had the reputation of being a very kind man although rather indecisive, but fair: "the conscience itself", as Lebesgue would write. He must have been in quite a delicate situation.

Digression (Montel's and Julia's mathematics in the two letters). The Notice Julia printed for the 1934 election is that in the first volume of his Works. He is obviously less grateful here to Montel's ideas than he was in 1917. We remember from page 62 that he wrote in his December 1917 Note [Julia 1917]:

> At that time, I was not aware of M. Montel's works. His Note dated June 4th 1917 drew my attention. I then studied them in a reprint M. Montel was kind enough to send me.

About the same work, Humbert in his report on the Great Prize had written:

> he [Julia] systematically introduces, not only attracting fixed points, but invariant points at which the absolute value of the multiplier is *greater* than one; their fundamental property is that they are *repelling points*. More precisely, if one of them is surrounded by an arbitrarily small domain, the successive consequents of this domain eventually contain in their interior all the points of the plane except at most one or two.
> The analogy of this statement with a theorem of M. E. Picard is not at all mysterious: M. Julia's proof relies here, as often in the rest of the memoir, on M. Montel's theory of *normal families*, a theory whose close relation with M. Picard's theorem is known[5].

We have seen what Pierre Fatou wrote (see page 93). So that, in 1917, everybody agreed that the notion of normal families had been decisive for the work on iteration. However, as we have seen on page 196, in 1934, when he wrote his Notice, Julia's point of view was quite different.

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[4] Jean Cartan, Élie Cartan's second son, was a musician. A student of Dukas and Roussel, he died from tuberculosis on March 26th 1932, aged 25.

[5] il [Julia] introduit systématiquement, non plus les points invariants attractifs, mais les points invariants où le module du multiplicateur est *supérieur* à l'unité; leur propriété fondamentale est d'être des *points de répulsion*. D'une manière plus précise, si l'on entoure l'un d'eux d'un domaine arbitrairement petit, les conséquents successifs de ce domaine *finissent* par comprendre à leur intérieur tous les points du plan, sauf un ou deux, au plus.

L'analogie de cet énoncé avec celui d'un théorème de M. E. Picard n'a rien de mystérieux: la démonstration de M. Julia repose ici, comme souvent dans le reste du Mémoire, sur la théorie des *suites normales* de M. Montel, théorie dont on sait le lien étroit avec le théorème de M. Picard.

A letter from Lebesgue

Wednesday morning

My dear Cartan

It is from my bed that I answer you; I went to bed on Monday when I came back from the Academy, feverish, with a relapse of a flu that has been plaguing me since the beginning of November, from which I have been unable to free myself. On Tuesday, my wife joined me and here we are, side by side, coughing, sneezing, spitting, which is infinitely touching.

This prevented me from summoning Julia. Your benevolence is unlimited after he made you spend 15 days dissecting Montel's work word by word to prove that his work does not owe anything, or very little, to Montel's—to dissect as you never dissected any text, to dissect in such a way that, if one were doing the same work on Poincaré, there would be nothing, absolutely nothing, proven by Poincaré—you content yourself, especially in this memoir, that the issue is to prove that it is independent of Montel, with a quotation where it is said, I think, that Montel generalised from Lindelöf and where after that the theorem quoted is in effect that of Lindelöf, and not that of Montel, which is the one that is used. After that also, Montel becomes a member of the Scandinavian school and the first method, being Scandinavian, becomes independent of Montel!!!!

And nevertheless, you granted me that Julia "*diminished*[6]" Montel's role in his famous Zurich talk[7]!

No, to tell the truth! Cartan, you exaggerate when you pretend not to see what is dazzling. That you forgive Julia for it, that is quite another thing; he is special and when we recognise a shortcoming or even a flaw we can legitimately pretend that this is a mental aberration due to the awful state in which the war left him and that you feel very sorry for him. I would find nothing to object to in that because this is the indulgence I would like to engage in. You don't help me; in questioning facts you recall the reproaches that can be made to him. And I would indeed need much help because nothing is harder than forgiving those who are so individual that they are unfair to others, since nothing is more contrary to my nature. And I never saw anybody as starkly individual as Julia.

Allow me to remind you of my way of doing things, it is so different from that of Julia that it will explain to you the difficulty I have in standing for his way. When some people tried to push me under Borel's feet, I said to myself, what would you have done if Borel had not existed? My vanity dictated to me

[6] As always, italics here replace underlining in the handwritten letter. All the notes are the author's.

[7] This is the plenary talk given by Julia at the International Congress at Zurich en 1932. See footnote 3 of Chapter VI and also page 233.

all kinds of "self-satisfying" answers, but I did not listen to it and I told myself: you cannot answer with certainty, you must let Borel pass but also express, in truth, that he is logically as well as chronologically ahead of you. And I did not feel reticent in doing it. There was perhaps some merit in that, because Borel was not always fair to me; but when I had to criticise, I didn't do so secretly, I told him straightforwardly and publicly, without any diplomacy, unceremoniously, but also very sincerely. I defy anybody to find in my bitter claim an unfair word on Borel's work, a tendencious attempt to push myself to his detriment. And my Philippic would have had enough effect to have strongly helped me in a campaign against Borel if I had wanted to undertake one; I had Goursat supporting me in the section, and Picard, and Kœnigs outside it[8], who were more than willing to fight, but I declined. And I said to myself: a seat at the Academy is not worth me demeaning myself in my own eyes, you wait[9].

And when Jordan died I made no move; I made no visit, either in the Section or outside it. It was only when the Section told me: we are putting you forward unanimously, that I started to write my Notice; so that the election had to be delayed. This election went as you know; everybody received votes except you[10]. And this did not surprise me since I observed during the visits that only two people did not ask those they visited to vote for them: I myself, who had no point in doing so, and you.

I thought that, in the present circumstances, you would condemn as I do any action which reveals an ambition that is so great that it does not stop short of injustice, since you were able to wait, even more than I was, until the others' judgement was favourable towards you, whatever personal judgement you legitimately had. And I repeat that to condemn the process does not automatically condemn the man when he is disabled[11].

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[8] This is the geometry section at the Academy of Sciences; remember that Kœnigs was a member of the mechanics section.

[9] The relation between Borel and Lebesgue became, at the end of the war, lamentable. The correspondence [Lebesgue 1991 ; 2004] ends very sadly. To the scientific rivalry, one should add the resentment of Lebesgue against the non scientific activities to which Borel devoted more and more of his time as time went by (the *Revue du mois*, politics). The elections mentioned by Lebesgue in this letter are those of

– April 11th 1921, for the replacement of Georges Humbert, the meeting in secret committee proposing Borel in first place and Lebesgue in second, and the 54 academicians electing Borel with 48 ballots (and 4 to Lebesgue),

– May 29th 1922, for the replacement of Jordan, Lebesgue obtaining 44 ballots (Vessiot 5, Drach 3, Cartan 2).

[10] Jordan died on January 21st 1922 and the election took place four months later, on May 29th. And it is not quite true that Cartan received no vote. See footnote 9.

[11] Lebesgue reproaches to Julia of being "starkly individual". It is quite interesting to note that Lebesgue considers Julia's war wound and that what he dislikes in his personality may be a consequence of this injury. Moreover, he is very careful to distinguish between the disabled person and his behaviour.

How far we are from the peacefulness and the kind of purely scientific concerns that should be the only important things in a scientific election. Whose fault was it? Montel's?

What was it about? Denjoy having withdrawn for Montel, which is a weighty tribute to the latter, we have to know whether Julia and Montel were worthy of the Academy and in which order they should be appointed. If we judge that they are both worthy, whatever the order in which we rank them scientifically—and I hasten to repeat that I rank Montel first—a 17 years' difference in age is relevant and is humanly peremptory, but what counts much more for me is that nobody can say what Julia would have done if Montel had not existed as his two most important pieces of work depend on Montel.

Scientifically I am for Montel because when everybody was walking at random and without a good understanding of what they were doing he showed everybody the fundamental fact from which everything follows and on which effort had to be concentrated. A wide range of theorems became special cases of a unique simple fact. Whereas invention, ingenuity and luck were needed the day before, the application of a method was the only thing needed the day after. This, this is a very big thing[12]. Since one of its merits is to help in understanding what preceded it, there must have been predecessors. One might pretend that the analytic geometry of Descartes and Fermat only repeated what had been in common use among mathematicians since Apollonius; that Newton's derivation was due to Barrow, Fermat, etc. All this doesn't trouble me.

This allows us to see things from above and proves the intellectual calibre of Montel. But he did many other things—we tend to forget this while examining the fine points raised by Julia—either using normal families or in another way. In particular I must say that I appreciate greatly the difficulties Montel overcame to prove, at last, that the Cauchy conditions $\dfrac{\partial P}{\partial x} = \dfrac{\partial Q}{\partial y}, \dfrac{\partial P}{\partial y} = -\dfrac{\partial Q}{\partial x}$ are sufficient, without anything further. His investigations on the existence of derivatives of functions of one or several variables are also within my area of expertise and this latter case was not well studied before him.

I don't want to make a catalogue, useless with you that are the conscience itself; it was probably not even necessary to remind you that Montel was not only responsible for normal families.

Julia has lots of beautiful work to his credit but I believe that less will be abide than of Montel's, that it will be less useful to the progress of science than Montel's has already been. Why? because, except for the Julia lines that are by far his main contribution, he was never a pioneer, because in his work the abundance, the erudition, the skilful technique, sometimes impress

[12] The big idea of Montel is the theorem on normal families. As we have seen, this is a characterisation of relatively compact subsets in spaces of holomorphic functions. The point then was to apply a theorem on families, on sequences, to the investigation of a single function, replacing the study of f at 0 by the study of the sequence of functions $z \mapsto f(z/2^n)$. See §I.5.

us superficially when all this is actually exterior to the true value. This value is nevertheless real and I don't wish at all either to dispute it or to "*diminish*" it, but I had to tell you what influences my assessment.

We still need to know whether Julia's two main works use as essential tools those invented by Montel. In iteration, Julia, Fatou and the referee Humbert categorically acknowledged this, but this confession might weigh upon Julia. I, who lack your infinite benevolence, imagine that he told you: I was misinformed, I should have quoted Landau and Carathéodory and not Montel—because, all the same, you did not find Landau and Carathéodory by yourself. It is good enough to read Montel and Julia, if we were to read all those they quote, then where would we be?

Thus, your attention drawn, you read Landau and Carathéodory and you find there what you told me. Let us imagine that Landau and Carathéodory have not only happened to construct a proof close to that of Montel's criterion or the proof itself, but also that they were conscious of this and that they stated it; what then? Would this prevent what Julia uses coming from Montel's ideas since, as you said, Landau and Carathéodory expressly refer to the ideas and results of Montel. Thus on this first point (and whatever happens with the *different* question of the priority Montel-Landau) my opinion is firm.

(As for the priority question, ask Montel. I am not very impressed by the question; a memoir published in a journal *dated* 1911 and a result announced in a 1911 note cannot have much influence on each other. The memoir refers to Montel, and that is the criterion, and Montel knew the memoir when he wrote his detailed 1912 paper; but such can be quite different types of knowledge)

As for Julia lines. Julia has a three-pronged attack:

a J-points. Settled? See the 1st paragraph of Montel's 1912 memoir, reproduced almost word for word (along with many other things from the same memoir) in Julia's book.—This, in brackets, is so outrageous that it shows the pathological character of Julia's claims.

b My proof, Julia says, is entirely different from that of Montel. Montel says $f(2^n z)$ would be normal but it is not. I proceed the other way round, which is quite different since Montel uses the exceptional values to prove that the sequence is not normal and I don't.

That there was a difference here that he wanted to emphasise is natural, since this is where the advantages he gets from his presentation come from; but this is quite another thing: we must judge that this has nothing to do with Montel. Now this depends on Montel in two essential ways: normal families and the construction of the sequence on which the function is reflected, which is the *only* construction or invention there is in all these matters.

Here again, I am fair to Julia, I don't care to know whether Montel would have introduced $f(2^n z)$ if it had not be abnormal, if he did not know that it was abnormal in every case, he proved it again in the rest of the memoir, if he did not prove it simply because he did not need it, if the proof of abnormality is significant or if it goes without saying, if this is not the kind of things we sometimes call an unimportant gap, etc. I agree that Julia is right in all this.

And then? His proof is dependent on two major things: the theory of normal functions, and the construction of the sequence $f(2^n z)$.

c "But I have another method, independent of normal families and of Montel's construction, one that uses the results of the Scandinavian school." The only Scandinavian as it happens is Montel who proved a theorem of which Lindelöf says that he could neither prove it nor to disprove it by an example (which is not a sufficient reason for why this theorem should not be counted towards Montel). And this theorem could only be proved by normal families and then J.'s two methods look as alike as two sisters.

Any long speech demands a conclusion; this one will lack it or rather, coming back to your letter, I will conclude: perhaps Julia does not denigrate Montel's work but he uses anything he can to get free from it. The result looks then so much like a denigration that you will excuse such a short-sighted man as I for being mistaken. As for me, I would prefer to magnify those I want to defeat rather than to diminish them.

So, with the intermediary of Rigollot, of cupping and gargling, I spent the day answering you. My inaction is the excuse for my length; I am a little sorry about that but I am happy to have told you my thoughts exactly. I shall end with a wish, that your indecisiveness stops so that we can say clearly to the candidates, whom each of us supports.

This said I lie down; I am certainly much better, but I feel feeble.

Yours

[signed] H. Lebesgue

Mercredi matin

Mon cher Cartan

C'est du lit que je vous réponds; je me suis couché Lundi à ma rentrée de l'Institut brûlant de fièvre, rechute de cette grippe que je traîne depuis le début de novembre sans avoir pu m'en libérer. Le mardi ma femme est venue me retrouver et nous voici cote à cote toussant, éternuant, crachant, ce qui est infiniment touchant.

Ceci m'a empêché de convoquer Julia. Votre bienveillance à vous est infinie et après qu'il vient de vous faire passer 15 jours à éplucher mot par mot les travaux de Montel pour prouver que ses travaux à lui ne doivent rien, ou si peu, à ceux de Montel — à éplucher comme jamais vous n'avez épluché aucun écrit, à éplucher d'une façon telle que si l'on faisait le même travail sur Poincaré il n'y aurait rien, absolument rien de démontré par Poincaré — vous vous satisfaites, précisément dans ce mémoire qu'il s'agit de montrer indépendant de Montel, d'une citation où il est dit, je crois, que Montel a généralisé le Lindelöf et quand après cela le théorème cité est celui de Lindelöf inopérant et non celui de Montel utilisé. Après cela aussi Montel devient de l'Ecole scandinave et la première méthode, étant scandinave, est indépendante de Montel!!!!

Et pourtant, vous m'avez concédé que Julia a "*amenuisé*" le rôle de Montel dans sa fameuse communication de Zurich!

Non, en vérité! Cartan, vous exagérez en ne voulant pas voir ce qui est éclatant. Que vous en excusiez Julia, cela c'est tout autre chose; il est à part et même si on reconnaît en lui un travers ou même une tare on peut très légitimement prétendre que c'est une déformation mentale due à l'état lamentable dans lequel la guerre l'a laissé et que vous ne l'en plaigniez que davantage je n'aurais rien à y objecter / car c'est à cette mansuétude que je voudrais arriver. Vous ne m'y aidez pas; en contestant l'évidence vous me faites [? ranimer, raviver] davantage les reproches qu'on lui peut faire. Et j'aurais pourtant bien besoin d'être aidé car rien n'est plus difficile que de pardonner à ceux qui sont tellement personnels qu'ils sont injustes envers les autres car rien n'est plus contraire à mon tempérament. Et je n'ai jamais vu personne d'aussi âprement personnel que Julia.

Permettez-moi de vous rappeler ma façon d'agir, elle est tellement différente de celle de Julia qu'elle vous expliquera la difficulté que j'ai à encaisser ses façons de faire. Quand certains ont cherché à me pousser dans les jambes de Borel, je me suis dit, qu'aurais-tu fait si Borel n'avait pas existé? Ma vanité me dictait pas mal de réponses “avantageuses” je ne l'ai pas écoutée et je me suis dit: tu ne peux répondre avec certitude, tu dois laisser passer Borel mais même exprimer, ce qui est la vérité, c'est qu'il est logiquement, tout aussi bien que chronologiquement, avant toi. Et je l'ai fait sans aucune réticence. J'y avais peut-être quelque mérite, car Borel ne m'avait pas toujours traité avec justice; mais ce que j'avais eu à lui reprocher, je n'avais pas été le dire à l'oreille, je le lui ai dit crûment et publiquement, sans aucune diplomatie, sans ménagement, mais aussi en toute sincérité. Je défie qu'on trouve dans mon âcre revendication un mot injuste pour l'œuvre de Borel, un essai tendancieux de me pousser à son détriment. Et ma philippique avait assez porté pour qu'elle m'ait puissamment aidé dans une campagne contre Borel si j'avais voulu en faire une; j'avais pour moi Goursat et dans la section, Picard Koenigs en dehors d'elle et qui ne demandaient qu'à faire campagne, j'ai dit non. Et à moi je me suis dit: une place /

(2)

à l'Académie ne vaut pas qu'on se diminue à ses propres yeux, tu attendras.

Et quand Jordan est mort je n'ai fait aucune démarche; je n'ai été voir personne, ni dans la Section, ni en dehors d'elle. C'est seulement quand la Section m'a dit: nous vous présentons à l'unanimité, que j'ai commencé à rédiger ma Notice; si bien qu'il a fallu reculer l'élection. Cette élection a été ce que vous savez; tout le monde y a eu des voix, sauf vous. Et cela ne m'a pas étonné car j'avais pu constater au cours des visites que deux personnes seulement ne demandaient pas à ceux qu'ils visitaient de leur donner leur voix: moi, qui n'y avais guère de mérite, et vous.

J'avais [aurais?] cru que, dans la circonstance actuelle, vous condamneriez comme moi tout acte révélant une ambition poussée au point de ne pas s'arrêter devant l'injustice puisque vous aviez su attendre, plus encore que moi, que le jugement des autres vous soit favorable, quelque jugement personnel que vous puissiez légitimement avoir. Et je répète que condamner les procédés, ce n'est pas nécessairement condamner l'homme quand il s'agit d'un grand mutilé.

Comme nous voici loin de la sérénité et du genre de préoccupations purement scientifiques qui seules devraient nous importer dans une élection scientifique. De qui est-ce la faute? De Montel?

De quoi s'agit-il? Denjoy s'étant retiré devant Montel, ce qui tout de même est un hommage de poids envers celui-ci, il faut savoir si Julia et Montel sont dignes de l'Académie et dans quel ordre ils doivent y être appelés. Si nous jugeons qu'ils en sont tous deux dignes, quel que soit l'ordre dans lequel nous les placions scientifiquement — et je me hâte / de répéter que je place Montel en premier — une différence de 17 ans d'âge compte et est péremptoire humainement mais ce qui pour moi compte bien autrement c'est que nul ne pourrait dire ce que Julia aurait fait si Montel n'avait pas existé car ses deux travaux les plus importants relèvent de Montel.

Je suis scientifiquement pour Montel parce que là où tout le monde marchait au hasard et sans bien comprendre ce qu'il faisait il a fait voir à tous le fait fondamental duquel tout découlait et sur lequel devait porter l'effort. Des assortiments de théorèmes sont devenus des cas particuliers d'un même fait bien simple. Là où la veille il fallait de l'invention, de l'ingéniosité et du bonheur, il n'a plus fallu le lendemain que l'application d'une méthode. Cela, c'est une très grande chose. Puisque l'un des mérites de cette chose est de faire comprendre ce qui l'a précédée, il faut qu'il y ait des prédécesseurs. On a pu prétendre que la géométrie analytique de Descartes et Fermat ne faisait que répéter ce qui était d'usage courant chez les mathématiciens depuis Apollonius; que la dérivation de Newton était de Barrow, de Fermat, etc. Tout cela ne me trouble pas.

Aussi parce qu'à côté de cette preuve d'intelligence qui permet de voir les faits de haut il a tout de même fait bien d'autres choses — on a dans l'examen des pointes d'aiguilles soulevées par Julia trop tendance à l'oublier — et par les familles normales et autrement. En particulier je dirai que j'apprécie fort les difficultés vaincues par Montel pour arriver, enfin, à montrer que les conditions de Cauchy $\frac{\partial P}{\partial x} = \frac{\partial Q}{\partial y}, \frac{\partial P}{\partial y} = -\frac{\partial Q}{\partial x}$ sont suffisantes, sans rien de plus. Ses recherches sur l'existence des dérivées des fonctions d'une ou de plusieurs variables sont aussi de ma compétence et ce dernier cas n'était pas bien abordé avant lui.

Je ne veux pas faire une énumération, inutile avec vous qui êtes la conscience même; il n'était sans doute même pas nécessaire de vous rappeler que Montel n'est pas seulement les familles normales.

Julia a à son actif quantité de beaux travaux mais je crois /

(3)

qu'il restera moins de ses travaux que de ceux de Montel, qu'ils seront moins utiles aux progrès de la science que ceux de Montel ne l'ont déjà été. Pourquoi? parce que, hors les droites de Julia qui sont de beaucoup son principal titre, il n'a jamais été un initiateur, que dans ses travaux, le foisonnement, l'érudition, la technique savante, parfois inutilement, nous impressionnent et que tout cela est en réalité extérieur à la vraie valeur. Cette valeur est réelle et je ne pense nullement ni à la contester, ni à l'"amenuiser" mais il fallait que je vous dise ce qui influe sur mon jugement.

Reste à savoir si, oui ou non, les deux principaux travaux de Julia utilisent comme outils essentiels les outils créés par Montel? Pour l'itération, Julia, Fatou et le rapporteur Humbert l'ont reconnu formellement mais cet aveu pèse peut-être à Julia. Moi, qui manque de votre bienveillance infinie, j'imagine qu'il vous a dit: j'étais mal renseigné, j'aurais pu et dû citer Landau et Carathéodory et non Montel — car, tout de même vous n'avez pas trouvé seul Landau et Carathéodory. C'est bien assez de lire Montel et Julia s'il fallait encore lire tous ceux qu'ils citent, où irions nous?

Donc, alerté, vous avez lu Landau et Carathéodory et y avez trouvé ce que vous m'avez dit. Imaginons que Landau et Carathéodory aient non seulement construit incidemment une démonstration voisine de celle du critère de Montel ou cette démonstration même mais qu'ils en aient eu conscience et qu'ils l'aient énoncé; eh bien? Ça empêchera-t-il que ce qu'utilise Julia ne provienne des idées de Montel puisque, m'avez-vous dit, Landau et Carathéodory se réfèrent expressément aux idées et résultats de Montel. Donc sur ce premier point (et quoiqu'il arrive de la question *différente* priorité Montel-Landau) j'ai une opinion ferme.

(Quant à la question de priorité, demandez à Montel. Je ne suis pas très impressionné par la question; un mémoire paru dans un périodique *daté* 1911 et un résultat annoncé dans une note de 1911 ne peuvent pas avoir beaucoup d'influence l'un sur l'autre. Le mémoire se réfère à Montel voila le critère et Montel connaissait le mémoire lors de son article développé de 1912; mais ce peuvent être deux connaissances assez différentes) /

Pour les droites de Julia. Triple offensive de Julia:

a Points J. Liquidée n'est-ce pas? voir le 1^er^ paragraphe du mémoire Montel 1912, reproduit presque textuellement (avec bien d'autres choses du même mémoire) dans le livre de Julia. — Ce qui, entre parenthèses, est tellement énorme que ça montre le caractère maladif des réclamations de Julia.

b Ma démonstration, dit Julia, est entièrement différente de celle de Montel. Montel dit $f(2^n z)$ serait normale or elle ne l'est pas. Je procède en [?] inverse et c'est tout différent car Montel utilise les valeurs exceptionnelles pour prouver que la suite n'est pas normale et moi pas.

Qu'il ait dit qu'il y avait là une différence qu'il tenait à souligner car de là venaient les avantages qu'il tire de sa présentation, rien que de naturel; mais c'est tout autre chose: nous devons juger que ça n'a aucun rapport avec le Montel. Or ça dépend de Montel de deux façons essentielles: les suites normales et puis de la construction de la suite sur laquelle la fonction se reflète, ce qui est la *seule* construction, invention qu'il y ait dans toute [*sic*] ces questions.

Là encore je fais beau jeu à Julia, je ne m'occupe pas de savoir si Montel aurait introduit $f(2^n z)$ si elle n'avait pas été anormale, s'il ne savait pas qu'elle était anormale dans tous les cas, il l'a encore démontré dans la suite du mémoire, s'il ne l'a pas démontré seulement parce qu'il n'en avait pas besoin, si la démonstration d'anormalité est de celles qui comptent ou qui vont de soi, si ce n'est pas de l'ordre de ce qu'on appelle parfois une lacune sans importance, etc. Je donne raison à Julia sur tout cela. Et puis? Sa démonstration reste tributaire des deux faits majeurs: théorie des fonctions normales, construction de la suite $f(2^n z)$.

c "Mais j'ai une autre méthode indépendante des familles normales et de la construction de Montel celle qui utilise les résultats de l'Ecole scandinave." Le seul scandinave en l'occurrence est Montel qui démontre un théorème que Lindelöf déclare n'avoir pu ni prouver ni mettre en défaut par un exemple (ce qui n'est tout de même pas une raison pour que ce théorème de Montel ne compte pas à Montel). Et ce théorème n'a pu être prouvé que par les familles normales et les deux méthodes de J. se ressemblent alors comme deux sœurs.

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(4)

Tout long discours demande une conclusion; celui-ci s'en passera ou plutôt, revenant à votre lettre, je conclurai: peut-être que Julia ne dénigre pas l'Œuvre de Montel mais il ne recule devant rien pour essayer de s'en affranchir. Le résultat ressemble alors si fort à un dénigrement que vous excuserez un myope comme moi s'il s'y est trompé. Pour moi j'aimerais mieux grandir ceux que je voudrais vaincre que de les diminuer.

Voilà, avec les intermédiaires de Rigollot, de ventouses et gargarismes, la journée s'est passée à vous répondre. Mon inoccupation est l'excuse de ma longueur; j'en suis un peu confus mais content de vous avoir dit exactement ma pensée. Je terminerai par un souhait, que votre indécision ne dure plus et que nous puissions dire nettement aux candidats pour qui chacun de nous est.

Sur quoi je m'allonge; je vais certainement bien mieux mais suis faible.

À vous

[signé] H. Lebesgue

A letter from Montel

Paris, Dec 24th 1933

Dear Monsieur Cartan,

I found your letter yesterday evening when I returned home[13].

Julia's proof[14] of Picard's theorem is indeed identical to the one on page 299 of my 1916 memoir as you pointed out to me[15].

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[13] December 24th was a Sunday, it was on Saturday that Montel found the letter he is answering. Cartan must have written it on Thursday evening or Friday, after having discussed with Montel on Thursday December 21st.

[14] This is in Julia's book [Julia 1924a], the notes of his 1920 Peccot course, written by Paul Flamant. The book appeared in 1924. As Julia says in the preface (dated August 1923):

> [...] the study of the function in a circle surrounding the essential singular point can be reduced to the study of a sequence of functions in a circular annulus surrounding this point, every function being *meromorphic* in the annulus. From this point of view, one is led, with M. Montel, to a new proof of M. Picard's theorems: Chapter III describes the main properties of normal families of functions; the modular function plays an essential role in the proof of a criterion given in 1911 by MM. Landau and Carathéodory in the form of a convergence criterion, but the transformation of this in terms of a normal families criterion allows us to re-prove simply the theorems of Chapter II [l'étude de la fonction dans un cercle entourant le point singulier essentiel peut se ramener à l'étude d'une suite de fonctions dans une couronne circulaire entourant ce point, chacune des fonctions étant *méromorphe* dans la couronne. De ce point de vue, on est conduit, avec M. Montel, à une nouvelle démonstration des théorèmes de M. Picard: le Chapitre III expose les propriétés essentielles des familles normales de fonctions; la fonction modulaire y joue un rôle essentiel pour l'établissement d'un critérium donné en 1911 par MM. Landau et Carathéodory sous forme de criterium de convergence, mais dont la transformation en criterium de familles normales permet de redémontrer simplement les théorèmes du Chapitre II.].

[15] This is the proof given on pages 79-80, in the Chapter *Les familles normales de fonctions* of [Julia 1924a], which is indeed identical to that of Montel, p. 299, except that Montel treats the more general case of a meromorphic function.

It is different from that on page 252 of the same memoir which relies on the fact that the function has no zero. But on this page 252, I referred in a Note to a first proof of Picard's theorem which is at page 514 of my 1912 memoir. The latter applies directly to the case Julia considers[16].

For me, the main issue lies in substituting for the criterion of exceptional values any normal family criterion and mentioning that Picard's theorem follows.

I claim that my theorem on page 296 goes further and contains the previous one as a special case. I prove there that any criterion gives a new theorem of Landau type, or Schottky type, or Picard type.

The reasoning I showed you on Thursday shows that $f(2^n z)$ cannot be normal for $|z| < 1$. Could it have a unique irregular point as in the case where $f(z) = z$ for instance? One sees, as in my proof on page 514 (1912), or otherwise, that this would contradict Weierstrass' theorem. *

All that precedes is about the proof of the actual theorem of Picard. Of course I never thought of claiming the notion of Julia line. The references in my later memoirs are perfectly explicit on this point and my opinion is formulated in the last paragraph of page 20 in my Notice.

Yours very sincerely

[signed] Paul Montel

*My reasoning indeed shows that no partial sequence extracted from $f(2^n z)$ can be normal for $|z| < 1$. Thus $f(2^n z)$ tends uniformly to infinity for $\frac{1}{2^2} < |z| < \frac{1}{2}$, since any partial sequence tends uniformly to infinity.

Paris, le 24 déc. 1933

Cher Monsieur Cartan,

J'ai trouvé votre lettre hier soir, en rentrant chez moi.

La démonstration de Julia du théorème de Picard est bien identique à celle de la page 299 de mon mémoire de 1916, comme vous me l'avez fait remarquer.

Elle diffère de celle de la page 252 du même mémoire qui s'appuie sur le fait que la fonction est dépourvue de zéro. Mais à cette page 252, je renvoie en Note à une première démonstration du th. de Picard qui se trouve à la page 514 de mon mémoire de 1912. Cette dernière s'applique immédiatement au cas envisagé par Julia.

Pour moi, la question principale / réside dans le fait de substituer au critère des valeurs exceptionnelles un critère quelconque de famille normale et de mentionner que le théorème de Picard s'en déduit.

Je dis que mon théorème de la page 296 va plus loin et comprend le précédent comme cas particulier. Je démontre à cet endroit que tout critère entraîne un nouveau théorème type Landau, ou type Schottky, ou type Picard.

Le raisonnement que je vous ai indiqué jeudi montre que $f(2^n z)$ ne peut être normale pour $|z| < 1$. Pourrait-elle avoir un seul point irrégulier comme dans le cas

[16] This is now about his proof, page 252. He had indeed already proved Picard's theorem (this time for an analytic function). His proof uses the fact that the f_n in the sequence he considers do not take the value 0.

où $f(z) = z$ par exemple? On voit, comme dans ma démonstration de la page 514 (1912), ou autrement, que cela est en contradiction avec le théorème de Weierstrass.*

Il s'agit dans tout ce qui précède de la démonstration du th. de Picard proprement dit. Il va de soi que je n'ai / jamais songé à revendiquer la notion de droite de Julia. Les indications de mes mémoires ultérieurs sont parfaitement explicites sur ce point et mon opinion est formulée au dernier alinéa de la page 20 de ma Notice.

Bien cordialement à vous

[signé] Paul Montel

*Mon raisonnement montre en effet qu'aucune suite partielle extraite de $f(2^n z)$ ne peut être normale pour $|z| < 1$. Donc $f(2^n z)$ tend uniformément vers l'infini pour $\frac{1}{2^2} < |z| < \frac{1}{2}$, puisque tout suite partielle tend uniformément vers l'infini.

Hadamard's report

[This is a four and a half page handwritten report that Hadamard wrote for a secret committee meeting of the Academy of Sciences, during which he probably read it, on July 4th 1921. This meeting was preparatory to the vote of July 11th 1921, after which the Academy of Sciences would propose Lebesgue in first place and Fatou in second for a position at the Collège de France.]

Secret Committee
of July 4th 1921

M. *Fatou*[17], assistant-astronomer at the Paris Observatory, worked on the most difficult and lofty areas of current mathematics and, each time, he gave the studies he approached a fruitful and important impetus.

This was firstly the case for the Thesis on *Taylor series*. In this subject where so much obscurity yet needs to be cleared up, it seems that we still constantly need researches that orientate the science and that not only bring it results but give it directions to follow. This is what Fatou has been fortunate enough to do since his Thesis. He devotes himself firstly to the examination of the limiting values to which the function can tend when approaching the circle of convergence.

Better than stating the theorems to which he was led we shall indicate their importance by saying that they have already been used in the work of Carathéodory on the conformal mapping theorem, of one F. and one M. Riesz on the definition of a function by Fourier constants.

Secondly, Fatou attacked the question of the convergence of the series, and he arrived at the following statement, which immediately became a classic due to its wonderful superiority: "The necessary and sufficient condition for the series $\sum a_n z^n$ to converge at any regular point of the circle of convergence is that the limit of a_n is zero". Here again, as for those we mentioned above, this statement made way for new research by MM. F. and M. Riesz, while another

[17] Our *italics* replace the underlining of Hadamard's manuscript.

statement obtained by the author, through combining his own viewpoints with those M. Lebesgue introduced into the science, induced work even of one Hermann Weyl, among others.

The history of Fatou's work in the theory of iteration is no less striking. This concerns, as is known, the behaviour of the quantities z_n obtained sequentially by the recurrence equation $z_n = \varphi(z_{n-1})$, the function φ being given (e.g. a rational function). It was, as is also known, our colleague M. Kœnigs who posed and solved this question in the local domain, that is, in a neighbourhood of a double point of the substitution, namely a root of the equation $\varphi(z) = z$.

Nobody had dared to tackle the question in the whole plane when, in 1906, in a short *Comptes rendus* note, M. Fatou, giving example of the extraordinary results met, showed at once the interest and the high difficulty in doing so. The chosen substitution was quite simple ($z_1 = \dfrac{z+z^2}{2}$); starting from this simple case however, the strangest singularities show up: the *domains* of the two limit points, namely the regions in which the starting point of the successive substitutions should be taken in order to reach eventually one or the other of the two double points, are separated by a non-analytic curve.

Our Academy considered that such investigations deserved to be pursued and recently put the question to Concourse. It thereby obtained the first rate works of M. Julia and also of M. Lattès, which were a great success for French mathematical science, showing, right after the war, that it kept its vitality and its power. Not only should we not forget that these conquests find their origin in M. Fatou's initiative [namely, Note [Fatou 1906d]], but that the latter, whose state of health prevented him from taking part in due time in the competition, obtained slightly later, often by simpler methods, more decisive results than the previous ones. Let us mention this one: *except for some exceptional* (and easily recognisable) *cases, the boundary of the domains under consideration has no tangent at any point.*

Once again, as one sees, following the example given by the case of Kleinian functions in Poincaré's work, the most delicate and subtle notions in the modern theory of sets and of functions of real variables necessarily appear in certain problems with a very simple statement. If our predecessors did not recognise this intervention, that is because they ran away from it without noticing it and they avoided the difficult problems where they would have been forced to come up against it.

We could multiply examples, either in the two theories we just spoke about, or in other questions by the same author, including those which combine the first two (such as the investigation of Taylor series the coefficients of which are obtained from each other by iteration). Those that precede are enough to show that M. Fatou has been essentially an innovator. As we see it, the list of geometers who were inspired by him is relatively long and includes, for a large part, the most prominent young researchers in France and abroad.

Such inspiring work has perhaps been too often forgotten and deserves the full attention of those who devote themselves to the mathematical science of our country.

[signed] J. Hadamard

Comité secret
du 4 juillet 1921

M. *Fatou*, astronome-adjoint à l'Observatoire de Paris, a travaillé sur les parties les plus élevées et les plus difficiles des Mathématiques actuelles et, chaque fois, il a donné aux études [auxquelles, biffé] qu'il abordait une impulsion importante et féconde.

Ce fut tout d'abord le cas pour la thèse de la *série de Taylor*. Dans ce sujet où il reste tant d'obscurités à dissiper, il semble que nous ayons encore constamment besoin de recherches qui orientent la science et qui non seulement lui apportent des résultats, mais lui montrent les directions utiles à suivre. C'est ce qu'il fut donné à Fatou de faire dès sa Thèse. Il s'y consacre tout d'abord à l'examen des valeurs limites vers lesquelles peut tendre la fonction lorsqu'on s'approche d'un point du cercle de convergence.

Mieux [que l'énoncé, biffé] qu'en énonçant les théorèmes auxquels il a été conduit nous en ferons prendre l'importance en disant qu'ils ont servi aux travaux d'un Carathéodory sur la représentation conforme, d'un F. et d'un M. Riesz sur la définition d'une fonction par des constantes de Fourier.

Que, d'autre part, M. Fatou s'attaque à la question de la convergence de la série, et il aboutit à l'énoncé suivant, devenu immédiatement classique par sa magnifique supériorité: "La condition nécessaire et suffisante pour que la série $\sum a_n z^n$ converge en tout point régulier du cercle de convergence est que la limite de a_n soit nulle". Ici encore, comme ceux dont nous parlions il y a un instant, cet énoncé a ouvert la voie à des recherches nouvelles de MM. F. et M. Riesz, pendant qu'un autre énoncé obtenu par l'auteur en combinant ses propres points de vue avec ceux qui ont été introduits dans la science par M. Lebesgue provoquait entre autres celles même d'un Hermann Weyl.

L'histoire de l'œuvre de Fatou dans la question de l'itération n'est pas moins démonstrative. [Les fondements, biffé] [C'est on le sait notre confrère M. Kœnigs qui a posé et résolu ce problème dans, biffé] Il s'agit on le sait de la disposition des quantités z_n déduites les unes des autres par l'équation de récurrence $z_n = \varphi(z_{n-1})$, la fonction φ étant donnée (p. ex. une fonction rationnelle). C'est on le sait également notre confrère M. Kœnigs qui a posé et résolu ce problème dans le domaine local, c'est-à-dire un voisinage d'un point double de la substitution, càd d'une racine de l'équation $\varphi(z) = z$.

Personne n'avait osé aborder la même question dans l'ensemble du plan lorsque, dès 1906, [M. Fatou, biffé] dans une courte note aux *Comptes Rendus*, M. Fatou, par l'exemple des résultats extraordinaires qu'on y rencontre, en montrait à la fois l'intérêt et la haute difficulté. La substitution choisie était bien simple ($z_1 = \dfrac{z + z^2}{2}$); [cependant, déjà, biffé] dès ces cas simples cependant, les singularités les plus bizarres apparaissent: les *domaines* des deux points limites, c'est-à-dire les régions où il faut prendre le point de départ des substitutions successives pour aboutir finalement soit à l'un soit à l'autre des deux points doubles, sont séparés par une courbe non analytique.

Notre Académie a jugé que de telles [résultats, biffé] recherches méritaient d'être poursuivies et [quelques, biffé] récemment a mis la question au Concours. Elle a ainsi obtenu des travaux de premier ordre de M. Julia et aussi de M. Lattès, qui ont constitué pour la Science mathématique française un grand succès, surtout au lendemain de la guerre, qu'elle n'avait [rien perdu de, biffé] gardait sa vitalité et sa puissance. Non seulement on ne saurait oublier que ces conquêtes [peuvent, biffé] doivent leur origine à [biffé illisible] l'initiative de M. Fatou, mais celui-ci, que son état de santé avait empêché de participer en temps utiles au Concours, a [publié, biffé] obtenu peu après par des méthodes souvent plus simples, une série de résultats plus décisifs encore que les précédents. Citons celui-ci: *des cas exceptionnels* (et aisément reconnaissables) *exceptés, la frontière des domaines en question n'a de tangente en aucun point.*

Une fois de plus, on le voit, après l'exemple [donné, biffé] fourni par le cas des fonctions Kleinéennes dans l'œuvre de Poincaré, les notions les plus délicates [biffé illisible] et les plus subtiles de la théorie moderne des ensembles et des fonctions de variables réelles apparaissent nécessairement dans des problèmes d'énoncé très simple. Si [les géomètres, biffé] nos prédécesseurs n'en avaient pas reconnu l'intervention, c'est qu'ils la fuyaient sans s'en rendre compte, en évitant les problèmes difficiles où ils auraient été exposés à [la renco, biffé] s'y heurter.

Nous pourrions multiplier les exemples, soit dans les deux théories dont nous venons de parler, soit dans d'autres questions accessoirement par le même auteur, [comme celle des séries de Taylor, biffé] y compris celles qui combinent les deux premières (comme l'étude des séries de Taylor dont les coefficients se déduisent les uns des autres par itération). Ceux qui précèdent suffisent à montrer que M. Fatou a été essentiellement un novateur. Comme on a pu le voir, la liste des géomètres qui se sont inspirés de lui est relativement [cet adverbe rajouté au dessus de la ligne] longue et comprend, pour une large part, celle des jeunes chercheurs les plus en vue tant en France qu'à l'étranger.

Une œuvre aussi suggestive a peut-être été trop souvent perdue de vue et mérite tout l'intérêt de ceux qui s'attachent à la Science mathématique dans notre pays.

[signé] J. Hadamard

Two letters from Fatou to Fréchet

[We found two letters from Fatou to Fréchet in the Fréchet collection at the archives of the Academy of Sciences, there may be some others there (this huge collection is only partly catalogued). They are not dated, but their contents clearly reveal their dates (a few days before and the day of the defence of Fatou's thesis). Fréchet and Fatou were both born in 1878 and were more or less contemporaries at the ENS, although Fréchet entered this school only in 1900, after his military service. A student of and closely allied with Hadamard, Fréchet defended his thesis in 1906. Fatou wrote to him about mathematics, at the moment when he defended his own thesis.]

Sunday evening [February 10$^{\text{th}}$ 1907]

My dear friend

We have[18]

$$\begin{aligned}S_n(x) &= \frac{\sin x}{1} + \frac{\sin 2x}{2} + \cdots + \frac{\sin nx}{n} \\ &= -\frac{x}{2} + \int_0^x \left(\frac{1}{2} + \cos t + \cdots + \cos nt\right) dt \\ &= -\frac{x}{2} + \frac{1}{2}\int_0^x \frac{\sin(2n+1)\frac{t}{2}}{\sin\frac{t}{2}}\, dt = -\frac{x}{2} + \int_0^{x/2} \frac{\sin kt}{\sin t}\, dt\end{aligned}$$

$$(k = 2n+1, \quad 0 < x < \pi, \quad \frac{x}{2} < \frac{\pi}{2})$$

$$\int_0^{x/2} \frac{\sin kt}{\sin t}\, dt = \underbrace{\int_0^{\pi/k}}_{j_0} + \underbrace{\int_{\pi/k}^{2\pi/k}}_{j_1} + \underbrace{\int_{2\pi/k}^{3\pi/k}}_{j_2} + \cdots + \int_{h\pi/k}^{x/2}$$

Making the substitution $\left(x, x + \frac{\pi}{k}\right)$, we see that

$$j_r j_{r+1} < 0, \quad |j_r| > |j_{r+1}|$$

Thus $\int_0^{x/2} \frac{\sin kt}{\sin t}\, dt$ lies between j_0 and 0. But we have

$$j_0 = \int_0^{\pi/k} \frac{\sin kt}{\sin t}\, dt < \frac{\pi}{k} \times \text{maximum} \left|\frac{\sin kt}{\sin t}\right|$$

hence $j_0 < \frac{\pi}{k} \times k = \pi$. We thus have, for x between 0 and π:

$$S_n(x) > -\frac{x}{2}$$
$$S_n(x) < \pi - \frac{x}{2}$$

It follows that in every case

$$|S_n(x)| < \pi$$

The same thing happens for the Fourier series of any bounded variation function (one considers it as the difference of two non-negative decreasing functions and applies the same reasoning).

All this follows immediately, as you see it, from the classical Dirichlet reasoning.

[18] This abrupt beginning shows that this letter answers a question asked by Fréchet, which might have been related to the publication of his Note [Fréchet 1907] on January 21st, and might have arrived together with a reprint of this Note (a Note could be printed very quickly, in the week following its presentation).

This is not without interest, as you seem to believe, and is likely to produce applications, but it is not very new.

I will add that for a Fourier series of any bounded function, we only have the bound

$$|S_n(x)| < C \log n$$

I shall send you without delay a copy of my thesis that I will pass next Thursday[19]. I must point out that it is very badly written and needs to be re-read and corrected.

I recognised recently that one can introduce as a factor of convergence[20] of a Fourier series, instead of r^n (which leads to the Poisson integral), the factor $n^{-\alpha}$ $(\alpha > 0)$.

We have indeed, if:

$$\begin{aligned} &f(x) \text{ (\textit{bounded} integrable function)} \\ &\quad \sim a_0 + (a_1 \cos x + b_1 \sin x) + \cdots + a_n \cos nx + b_n \sin nx + \cdots \end{aligned}$$

(the sign $\sim$ meaning that the right hand side is the Fourier series of $f(x)$), then the following propositions hold If $\alpha > 0$, the series

$$\sum \frac{a_n \cos nx + b_n \sin nx}{n^\alpha} = \psi(x, \alpha)$$

converges uniformly between 0 and 2π and the same is true of the conjugated series $\sum \frac{a_n \sin nx - b_n \cos nx}{n^\alpha}$.

We have, secondly, $\lim_{\alpha \to 0} \psi(x, \alpha) = f(x)$, at any point of continuity of $f(x)$, and more generally whenever

$$\lim_{r \to 1} [a_0 + r(a_1 \cos x + b_1 \sin x) + \cdots + r^n(a_n \cos nx + b_n \sin nx)] = f(x)$$

thus this second summation process is at least as general as that of Poisson.

It would be interesting to make a complete study of these various convergence factors, namely to find what are the expressions

$$\psi(x, \alpha)$$

enjoying the following properties:

(1) $\psi(x, \alpha_0) = 1$

(2) Any Fourier series $A_0 + A_1 + \cdots + A_N + \cdots$ becomes uniformly convergent if its general term is multiplied by $\psi(n, \alpha)$ (for $\alpha > \alpha_0$ for instance)

(3) $\lim_{\alpha \to \alpha_0} |A_0 + A_1 \psi(1, \alpha) + \cdots + A_n \psi(n, \alpha) + \cdots| = f(x)$ when $f(x)$ is continuous

[19] This confirms that Fatou's thesis was indeed defended on February 14$^{\text{th}}$ 1907, which was actually a Thursday... and allows us to date this letter, the previous Sunday being February 10$^{\text{th}}$.

[20] Fatou will explain what this is in the letter.

It seems to me that the choice of these multipliers must be rather arbitrary.

In addition to these small complements to the theory of Fourier series, I have undertaken more extensive research on iteration; but I lack the energy to write it all up, together with some already ancient arithmetical research.

My health has improved during the last few months; I could resume my job at the Observatory. I am happy to see that your class still allows you some time to do some maths.

Yours very sincerely
P. Fatou

Do you sometimes see M. Lebeuf, director of the Observatory? He is an excellent man who could be of some help to you[21].

Mon cher ami

On a:

$$\begin{aligned} S_n(x) &= \frac{\sin x}{1} + \frac{\sin 2x}{2} + \cdots + \frac{\sin nx}{n} \\ &= -\frac{x}{2} + \int_0^x \left(\frac{1}{2} + \cos t + \cdots + \cos nt\right) dt \\ &= -\frac{x}{2} + \frac{1}{2}\int_0^x \frac{\sin(2n+1)\frac{t}{2}}{\sin\frac{t}{2}}\, dt = -\frac{x}{2} + \int_0^{x/2} \frac{\sin kt}{\sin t}\, dt \end{aligned}$$

$$(k = 2n+1, \quad 0 < x < \pi, \quad \frac{x}{2} < \frac{\pi}{2})$$

$$\int_0^{x/2} \frac{\sin kt}{\sin t}\, dt = \underbrace{\int_0^{\pi/k}}_{j_0} + \underbrace{\int_{\pi/k}^{2\pi/k}}_{j_1} + \underbrace{\int_{2\pi/k}^{3\pi/k}}_{j_2} + \cdots + \int_{h\pi/k}^{x/2}$$

En faisant la substitution $\left(x, x + \frac{\pi}{k}\right)$, on voit que:

$$j_r j_{r+1} < 0, \quad |j_r| > |j_{r+1}|$$

Donc $\int_0^{x/2} \frac{\sin kt}{\sin t}\, dt$ est compris entre j_0 et 0. Or on a:

$$j_0 = \int_0^{\pi/k} \frac{\sin kt}{\sin t}\, dt < \frac{\pi}{k} \times \text{maximum} \left|\frac{\sin kt}{\sin t}\right|$$

donc $j_0 < \frac{\pi}{k} \times k = \pi$. On a donc, pour x compris entre 0 et π:

$$S_n(x) > -\frac{x}{2}$$
$$S_n(x) < \pi - \frac{x}{2}$$

Par suite dans tous les cas

$$|S_n(x)| < \pi$$

[21] Auguste Lebeuf was the director of the Besançon Observatory and Fréchet was teaching at the lycée of Besançon at that time.

La même chose a lieu pour la série de Fourier de toute fonction à variation bornée (on la considérera comme la différence de 2 fonctions positives décroissantes et on appliquera le même raisonnement).

Tout cela découle immédiatement comme tu le vois du raisonnement classique de Dirichlet.

Cela n'est pas sans intérêt, comme tu parais le croire, et susceptible d'applications, mais ce n'est pas très neuf.

J'ajouterai que pour une série de Fourier d'une fonction bornée quelconque, on a seulement la limitation:

$$|S_n(x)| < C \log n$$

Je t'enverrai sans tarder un exemplaire de ma thèse que je passerai jeudi prochain. Je dois te préciser qu'elle est assez mal rédigée et aurait besoin d'être revue et corrigée.

J'ai reconnu dernièrement qu'on pouvait introduire comme facteur de convergence d'une série de Fourier, au lieu de r^n (ce qui ramène à l'intégrale de Poisson), le facteur $n^{-\alpha}$ $(\alpha > 0)$.

On a en effet, si:

$$f(x) \text{ (fonction \textit{bornée} intégrable)} \sim a_0 + (a_1 \cos x + b_1 \sin x) + \cdots + a_n \cos nx + b_n \sin nx + \cdots$$

(le signe $\sim$ signifiant que le deuxième membre est la série de Fourier de $f(x)$), les propositions suivantes:

Si $\alpha > 0$, la série $\sum \dfrac{a_n \cos nx + b_n \sin nx}{n^\alpha} = \psi(x, \alpha)$ est uniformément convergente entre 0 et 2π et il en est de même de la série conjuguée $\sum \dfrac{a_n \sin nx - b_n \cos nx}{n^\alpha}$.

On a en second lieu $\lim_{\alpha \to 0} \psi(x, \alpha) = f(x)$, en tout point de continuité de $f(x)$ et plus généralement chaque fois que

$$\lim_{r \to 1} [a_0 + r(a_1 \cos x + b_1 \sin x) + \cdots + r^n (a_n \cos nx + b_n \sin nx)] = f(x)$$

c'àd que ce second procédé de sommation st au moins aussi général que celui de Poisson.

Il serait intéressant de faire une étude complète de ces divers facteurs de convergence c'àd de trouver quelles sont les expressions

$$\psi(x, \alpha)$$

jouissant des propriétés suivantes:

(1) $\psi(x, \alpha_0) = 1$
(2) Toute série de Fourier $A_0 + A_1 + \cdots + A_N + \cdots$ devient uniformément convergente si on multiplie son terme général par $\psi(n, \alpha)$ (pour $\alpha > \alpha_0$ par exemple)
(3) $\lim_{\alpha \to \alpha_0} |A_0 + A_1 \psi(1, \alpha) + \cdots + A_n \psi(n, \alpha) + \cdots| = f(x)$ lorsque $f(x)$ est continue

Il me semble que le choix de ces multiplicateurs doit comporter beaucoup d'arbitraire.

Outre ces petits compléments à la théorie des séries de Fourier j'ai fait des recherches plus étendues sur l'itération; mais il me manque le courage pour rédiger tout cela ainsi que quelques recherches arithmétiques déjà anciennes.

Ma santé s'est améliorée depuis quelques mois; j'ai pu reprendre mon service à l'Observatoire. Je suis heureux de voir que ta classe te laisse le temps de faire encore un peu de math.

Bien cordialement à toi
P. Fatou

Vois-tu quelquefois M. Lebeuf directeur de l'Observatoire? C'est un excellent homme qui pourrait être une ressource pour toi.

Thursday evening [February 14$^{\text{th}}$ 1907]

My dear friend

The difficulty with regard to the periodicity[22] of $f(x)$, to prove that

$$S_n(x) = \frac{1}{2\pi} \int_{\alpha}^{\alpha+2\pi} \frac{\sin(2n+1)\frac{x-t}{2}}{\sin\left(\frac{x-t}{2}\right)} f(t)\, dt$$

stays a bounded function when f has bounded variation, is not very considerable. There is no difficulty indeed except for the part of the integral related to the interval $(x-h, x+h)$, h having any finite value: $h = \dfrac{\pi}{10}$ for instance, since, for all t outside this interval the integrand will be $< \left|\dfrac{f(t)}{\sin \frac{\pi}{10}}\right|$.

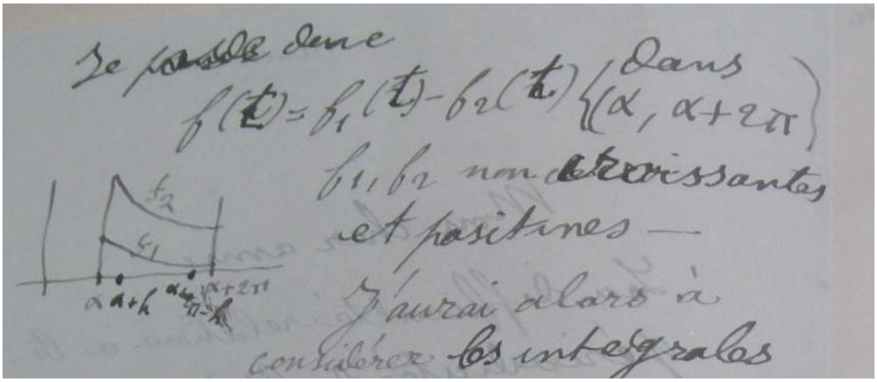

Fig. A.1. The February 14$^{\text{th}}$ letter

[22] The Sunday evening letter must have been sent on Monday, and have arrived in Besançon on Tuesday, so that Fréchet could raise another question in a letter which arrived early enough for Fatou to answer it on Thursday evening.

[figure] I thus put $f(t) = f_1(t) - f_2(t)$ (in $(\alpha, \alpha + \pi)$) f_1, f_2 non-increasing and non-negative. I will then have to consider the integrals

$$\int_{x-h}^{x+h} \frac{\sin(2n+1)\frac{x-t}{2}}{\sin\frac{x-t}{2}} f_1(t)\,dt \qquad \int_{x-h}^{x+h} \cdots f_2(t)\,dt$$

to which the Dirichlet reasoning will apply; one will have nevertheless to assume x lies between $\alpha + h = \alpha + \frac{\pi}{10}$ and $\alpha + 2\pi - h = \alpha + 2\pi - \frac{\pi}{10}$. But this is of no importance since α is arbitrary, and for the values of x equal (mod. 2π) to the values we just excluded it is enough to take another value of α, for instance: $\alpha + \pi$ (perhaps with other functions f_1', f_2').

I would be pushed to give you accurate bibliographic information: this is in some sense implicitly in Dirichlet and in all those who studied Fourier series after him, but I don't remember having seen the remark explicitly in the books I consulted. This is neither in my thesis nor in Lebesgue's lessons. But for things that are basically so classical, it is not necessary to give so much information, you can simply say that it follows from Dirichlet's classical analysis that, etc. adding, if you want, that you owe this remark to me, but do not insist, because this is easy and besides I am not sure I have the priority[23].

I greatly advise you to study the essential things in the theory of Fourier series. This will not be very difficult for you and it is always dangerous to use results without knowing where they come from, since one then risks interpreting them in a wrong way.

You will find what is most important to know in the analysis treatises of Jordan or Picard. If you want to study things more deeply, there are Lebesgue's lessons.

I defended my thesis this morning in front of a most benevolent committee[24]; happily it is finished.

I shall always be very happy to give you information in the seldom matters I am competent; you can ask me without hesitating.

Yours very sincerely
P. Fatou

Fejér's theorem is indeed very elegant, it is superior to the other summation processes and gives only finite Fourier sequences. But it does not make such considerable progress as might one think. Besides, the summability by some process is always far from ordinary convergence.

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[23] In the Note [Fréchet 1907], as in the article [Fréchet 1908] that will follow, the question is to approximate a continuous function by a (finite) trigonometric sum, this raising the question of the convergence of the sequence of functions thus defined. In the article [Fréchet 1908], which is subsequent to the letters we present here, the questions raised are not mentioned and neither Fatou, nor even Dirichlet, appear (but Fréchet quotes [Lebesgue 1906]).

[24] Let us recall that the members of the committee were Appell, Painlevé and Borel. The defences at that time seem to have been less festive than they are today... Fatou defended his thesis, went back home, and answered a mathematical letter.

Mon cher ami

La difficulté relative à la périodicité de $f(x)$, pour démontrer que

$$S_n(x) = \frac{1}{2\pi} \int_{\alpha}^{\alpha+2\pi} \frac{\sin(2n+1)\frac{x-t}{2}}{\sin\left(\frac{x-t}{2}\right)} f(t)\,dt$$

reste bornée si f est à variations bornées n'est pas bien considérable. En effet il n'y a de difficulté que pour la partie de l'intégrale relative à l'intervalle $(x-h, x+h)$, h ayant une valeur finie quelconque: $h = \dfrac{\pi}{10}$ par exemple, car pour t extérieur à cet intervalle la fonction à intégrer sera $<$ que $\left|\dfrac{f(t)}{\sin\frac{\pi}{10}}\right|$.

[figure] Je pose donc $f(t) = f_1(t) - f_2(t)$ (dans $(\alpha, \alpha+\pi)$) f_1, f_2 non croissantes et positives. J'aurai alors à considérer les intégrales

$$\int_{x-h}^{x+h} \frac{\sin(2n+1)\frac{x-t}{2}}{\sin\frac{x-t}{2}} f_1(t)\,dt \qquad \int_{x-h}^{x+h} \cdots f_2(t)\,dt$$

auxquelles s'appliqueront [*sic*] le raisonnement de Dirichlet; on devra toutefois supposer x compris entre $\alpha + h = \alpha + \frac{\pi}{10}$ et $\alpha + 2\pi - h = \alpha + 2\pi - \frac{\pi}{10}$. Mais cela n'a aucune importance puisque α est arbitraire, et pour les valeurs de x congrues (mod. 2π) aux valeurs que nous venons d'exclure il suffira de prendre une autre valeur de α, par exemple: $\alpha + \pi$ (éventuellement d'autres fonctions f_1', f_2').

Je serais embarrassé de te donner un renseignement bibliographique exact: cela se trouve en quelque sorte implicitement dans Dirichlet et dans tous ceux qui après lui ont étudié les séries de Fourier, mais je ne me rappelle pas avoir vu cette remarque exprimée explicitement dans les ouvrages que j'ai consultés. Il n'en est pas question dans ma thèse ni dans les Leçons de Lebesgue. Mais pour des choses aussi classiques dans le fond, il n'est pas nécessaire de donner tant de renseignements, tu peux dire simplement qu'il résulte de l'analyse classique de Dirichlet que etc. en ajoutant si tu y tiens, que tu me dois cette remarque, mais sans insister car cela est facile et je ne suis d'ailleurs pas certain d'avoir la priorité.

Je te conseille vivement d'étudier les choses essentielles de la théorie des séries de Fourier, cela ne te demandera pas beaucoup de peine et il est toujours dangereux d'utiliser des résultats sans savoir d'où ils viennent, car on risque de les interpréter de travers.

Tu trouveras ce qu'il y a de plus important à savoir dans les traités d'analyse de Jordan ou Picard. Si tu veux étudier les choses plus à fond, il y a les leçons de Lebesgue.

J'ai soutenu ma thèse ce matin devant un jury des plus bienveillants; heureux d'en avoir fini.

Je serai toujours très heureux de te donner des renseignements dans les rares matières où j'ai quelque compétence; tu peux m'en demander sans hésiter.

Bien cordialement à toi
P. Fatou

Le théorème de Féjer [*sic*] est en effet très élégant, il a la supériorité sur les autres procédés de sommation de ne donner lieu qu'à des suites finies de Fourier, mais cela ne constitue pas un progrès si considérable que l'on pourrait le croire. D'ailleurs il y a toujours loin de la sommabilité par un procédé quelconque à la convergence ordinaire.

Letters from Fatou to Montel

[Montel kept sixteen letters or cards, plus a fragment of a letter, all written by Fatou, and the draft of a letter he wrote him. Most of Fatou's letters are not dated. Sometimes Montel added a date. To fix the chronology of these letters, we only have their content and the nature of the paper Fatou used: one can imagine Fatou finishing his box of blue notepaper before he looked for some other supply...

These are friendly letters, in which it is quite clear that what interests Fatou is the mathematics: he suggests research topics to his friend, criticises such and such a detail in the proofs of his articles, and shows himself fanciful and full of humour.]

Paris, November 29th 1906

My dear friend,

I went this morning to the secretary's office at the Faculty of sciences, but Guillet told me that Painlevé had not brought back your manuscript; the truth is that there is quite some disorder in his office and that he had to move several theses which were piled up higgledy-piggledy on his desk before he gave me the assurance that it was still in Painlevé's hands. You had better speak to Painlevé to find out what is happening. I must tell you however that your thesis was not, on the register of the secretary, among those which have been brought back.

As for me, I am not yet sure of the date of my defence, since I have not received proofs since September. It has been going on so long that I have grown tired of thinking about it, and this wait does not perturb me at all.

I continue working from time to time on iteration. Incidentally, I obtained a result on Taylor series which I communicated to the Société mathématique the other day and which Hadamard liked[25]. Here is what it is about.

Assume a Taylor series

$$(1) \qquad u + u_1 x + \cdots + u_n x^n + \cdots$$

the coefficients of which are determined by the induction law

$$u_{n+1} = f(u_n)$$

(f an analytic function). Without being more specific we cannot say anything, since any Taylor series can be obtained this way.

But let us assume that the substitution $(u \sim f(u))$ has a regular limit point, that is, a point which is a root α of the equation

$$u - f(u) = 0 \text{ with } |f'(\alpha)| < 1$$

[25] See note 88 in chapter V.

(f holomorphic at α). If the initial value u lies in the domain of convergence relative to the point α, the series (1) represents a meromorphic function which is the *quotient of two entire functions of genus zero and order zero*, the entire function in the denominator being

$$(q = f'(\alpha)) \qquad (1-x)(1-qx)(1-q^2x)\cdots(1-q^nx)\cdots$$

a function that we meet in the theory of elliptic functions.

This result is rather curious, as you see, as it gives a precise result on the analytic extension of a large class of Taylor series given by the law of their coefficients, this law being in general not analytic (that is, not of the form $a_n = \varphi(n)$ where φ is a given analytic function.

I regularly attend Poincaré's course, which is rather interesting: he deals with the work some astronomers did to eliminate certain discrepancies between observations and tables and reviews the ways that have been tried to modify Newton's law; at the end of his course, he will speak of the application of astronomy to the mechanics of the electrons. I am very curious[26] to know what he will say on this completely new subject.

I thank you very much for the trouble you went to to give my sister the information about Nice which will be very useful for her; she asked me herself to thank you, and I would have done so earlier if my good intentions had not been so often wasted by my epistolary laziness.

If you want me to go and see Painlevé for you, send me a note.

Most sincerely yours, and see you soon

[signed] P. Fatou

Mon cher ami,

Je suis passé ce matin au secrétariat de la Faculté des sciences, mais Guillet m'a déclaré que ton manuscrit n'avait pas été rapporté par Painlevé; il est vrai qu'il y a pas mal de désordre dans son cabinet et qu'il a remué un certain nombre de thèses entassées pêle-mêle sur sa table avant de me donner l'assurance qu'il était encore entre les mains de ce dernier. Tu feras donc bien de t'adresser à Painlevé pour savoir ce qui en est. Je dois dire cependant que ta thèse n'était pas marquée sur le registre du secrétariat, parmi celles qui ont été rapportées.

Pour moi, je ne suis pas encore fixé sur la date de ma soutenance, n'ayant pas reçu d'épreuves depuis le mois de septembre. Il y a si longtemps que cela dure que j'ai fini par me lasser d'y penser et cette attente ne m'agite pas du tout.

Je continue de travailler un peu de temps en temps sur l'itération. J'ai obtenu incidemment un résultat sur les séries de Taylor que j'ai communiqué l'autre jour à la Société mathématique et qui a plu à Hadamard. Voilà de quoi il s'agit. Suppose une série de Taylor

$$(1) \qquad u + u_1x + \cdots + u_nx^n + \cdots$$

[26] The curious reader may look at [Poincaré 1905].

dont les coefficients sont déterminés par la loi de récurrence

$$u_{n+1} = f(u_n)$$

(f fonction analytique). Si l'on ne précise pas davantage, on ne peut rien dire car toute série de Taylor s'obtient de cette façon.

Mais supposons que la substitution ($u \sim f(u)$) possède un point limite régulier c'àd un point racine α de l'équation

$$u - f(u) = 0 \text{ avec } \left|f'(\alpha)\right| < 1$$

(f holomorphe en α). Si la valeur initiale u se trouve dans le domaine de convergence relatif au point α, la série (1) représente une fonction méromorphe qui est le *quotient de 2 fonctions entières de genre zéro et d'ordre zéro*, la fonction entière du dénominateur étant

$$(q = f'(\alpha)) \qquad (1-x)(1-qx)(1-q^2x)\cdots(1-q^nx)\cdots$$

fonction que l'on rencontre dans la théorie des fonctions elliptiques.

Ce résultat est assez curieux, comme tu le vois, il donne un résultat précis sur le prolongement analytique d'une classe étendue de séries de Taylor données par la loi de leurs coefficients, cette loi n'étant pas en général analytique (c'àd n'étant pas de la forme $a_n = \varphi(n)$ où φ est une fonction analytique donnée).

Je suis régulièrement le cours de Poincaré qui est assez intéressant: il traite des travaux des astronomers entrepris pour faire disparaître certaines divergences entre les observateurs et les tables, et passe en revue les modifications que l'on a essayées [*sic*] d'apporter à la loi de Newton; à la fin de son cours, il parlera de l'application à l'astronomie de la mécanique des électrons, je serai curieux de savoir ce qu'il fera sur ce sujet tout à fait nouveau.

Je te remercie beaucoup de la peine que tu t'es donnée pour fournir à ma sœur des renseignements sur Nice qui lui seront fort utiles; elle m'a priée [*sic*] elle-même de te remercier ce que j'aurais fait plutôt [*sic*] si mes bonnes intentions n'étaient si souvent gâtées par ma flemme épistolaire.

Si tu veux que j'aille trouver Painlevé pour toi, envoie-moi un mot.

Bien cordialement à toi et à bientôt

[signé] P. Fatou

Saturday, February 13th [1915]

[The date 13/2 was added, in pencil, by Montel. This letter is about a paper that was published in 1912. The years after 1912 in which February 13th was a Saturday are 1915 and 1926. The second one seems to be too late: in 1926, Fatou had certainly read this article a long time since. In this letter, we see Fatou discuss the singular lines of the Kleinian functions—between the Note [Fatou 1906d] and the memoir on iteration.]

My dear friend

In thanking you for your memoir which I read with much interest, I am compelled to point out to you some reasoning that seems faulty to me, though this has only minor importance from your point of view, since you are able to prove much more general propositions in an entirely correct way, but my attention was drawn to this for some reasons I will indicate.

You consider (p. 495)[27] the function

$$\varphi(x) = \log \frac{f(x) - a}{f(x) - b}$$

$f(x)$ not taking the values of a linear continuum with extremities a, b and you conclude that the imaginary part of $\varphi(x)$ varies between -3π and $+3\pi$. Now, your reasoning is obviously wrong if you think of *any* linear continuum. Indeed assume that the cut ab (or 0∞) has the shape of a spiral at one of its ends; to go from m to m' *without crossing the cut*, you have to turn around 0 a certain number of times which is larger and larger as m is closer and closer to 0 and the argument varies by $2k\pi$, k being able to increase indefinitely.[figure]

Now you can imagine linear continuums having a dense set of asymptotic points (singular lines of Kleinian functions).

It is thus unwise to apply this kind of reasoning if one does not assume the continuum is more or less rectilinear.

Now here is what led me to this criticism: I tried to apply your transformation to generalise the properties I obtained in my thesis for analytic functions that are bounded in a circle and in this story the modular function gives no result, while the logarithm would work very well, without the difficulty I have pointed out to you. I have not yet found a way of coping with this.

Tell me if you agree with me, and if you can see a way of determining from a function that does not take the values of a linear continuum another one which is bounded using only simple transformations.

Yours sincerely

[signed] P. Fatou

[Note added by Montel on the letter: this is a curve and not a linear continuum. On a curve, one should replace $-3\pi, 3\pi$ by $-k\pi, k\pi$; or take for the curve two sufficiently close points (a', b') in place of the points a and b.]

Mon cher ami

En te remerciant de ton mémoire que j'ai parcouru avec beaucoup d'intérêt, je suis obligé de te signaler un raisonnement qui me paraît défectueux, ce qui n'a d'ailleurs qu'une importance minime à ton point de vue, puisque tu parviens à démontrer des propositions beaucoup plus générales par une voie tout à fait correcte, mais mon attention s'est portée là-dessus pour des raisons que je t'indiquerai.

Tu considères (p. 495) la fonction

$$\varphi(x) = \log \frac{f(x) - a}{f(x) - b}$$

[27] The memoir in question is [Montel 1912].

$f(x)$ ne prenant pas les valeurs d'un continu linéaire d'extrémités a, b et tu conclus que la partie imaginaire de $\varphi(x)$ varie entre -3π et $+3\pi$. Or ton raisonnement est évidemment inexact si tu envisages un continu linéaire *quelconque*. Suppose en effet que la coupure ab (ou 0∞) ait la forme d'une spirale à l'une de ses extrémités; pour aller de m en m' *sans traverser la coupure*, tu dois tourner autour de 0 un certain nombre de fois d'autant plus grand que m est plus rapproché de 0 et l'argument varie de $2k\pi$, k pouvant croître indéfiniment.[figure]

Or tu peux imaginer des continus linéaires ayant un ensemble dense de points asymptotes (lignes singulières des fonctions kleinéennes).

Il est donc imprudent d'appliquer ce mode de raisonnement si l'on ne suppose pas que le continu est à peu près rectiligne.

Voici maintenant ce qui m'a conduit à cette critique: j'ai essayé d'appliquer ta transformation pour généraliser les propriétés que j'ai obtenues dans ma thèse pour les fonctions analytiques bornées dans un cercle et pour cette histoire là la fonction modulaire ne donne aucun résultat, tandis que le logarithme marcherait très bien sans la difficulté que je t'ai signalée. Je n'ai pas encore trouvé le moyen de m'en tirer.

Dis-moi si tu es bien de mon avis, et si tu vois le moyen de déduire d'une fonction qui ne prend pas les valeurs d'un continu linéaire une autre qui soit bornée en n'employant que des transformations simples.

Bien cordialement à toi

[signé] P. Fatou

[Note ajoutée par Montel sur la lettre: il s'agit d'une courbe & non d'un continu linéaire. Il faut, même pour une courbe, remplacer $-3\pi, 3\pi$ par $-k\pi, k\pi$; ou bien prendre pour la courbe deux points assez rapprochés (a', b') remplaçant les points a et b.]

Probably the end of 1919

My dear Montel,

I willingly accept the solution you suggest, because I also believe that it is better to start this publication straight away. However I will try to give you Ch. VI and VII shortly, even if I must then give you some additional notes later on.

I noticed that Julia has started the publication of his memoir on entire functions[28] and that he has announced the one on iteration; the latter has been with the printer for almost a year; I think he had to revise it a lot, since in the past I mentioned to him some mistakes in his C.R. Notes. It is not very easy to finalise all this and he must have understood that, despite his haste to publish.

[28] Julia's memoir on entire functions consisted of three articles [Julia 1919c ; 1920b ; 1921a] that appeared in the Annales de l'École Normale Supérieure. According to what is written on the second of these three papers, the first one (the one Fatou is speaking of here) is dated April 1919.

I send you a letter of Lémeray which I am very pushed to answer, despite my title of archivist[29].

Yours sincerely

[signed] P. Fatou

Mon cher Montel,

J'accepte volontiers la solution que tu me proposes, car je crois aussi qu'il vaut mieux commencer dès maintenant cette publication. Je tâcherai cependant de te remettre les ch. VI et VII dans un délai assez rapproché, quitte à te donner plus tard quelques notes supplémentaires.

J'ai vu que Julia a commencé la publication de son mémoire sur les fonctions entières et qu'il annonce celui relatif à l'itération; voilà près d'un an que ce dernier est à l'impression; je pense qu'il a eu à le remanier pas mal, car je lui ai autrefois signalé des fautes qui apparaissaient dans ses notes des C.R. Tout cela n'est pas très facile à mettre au point et il dû s'en apercevoir malgré sa hâte de publier.

Je te communique une lettre de Lémeray à laquelle je suis fort embarrassé de répondre, malgré mon titre d'archiviste.

Cordialement à toi

[signé] P. Fatou

Tuesday [Jan. 6th 1920]

[The date is in Montel's handwriting. In writing this letter, Fatou uses a blue notepaper for the first time (a Christmas present?).]

My dear Montel

If instead of making the substitutions of the group $(z|k^{\pm n}z)$ act on z, you use the substitutions $(z|z+p\omega+q\omega')$ where p and q denote all the integers and (ω,ω') any two numbers, and you consider the functions

$$F_{pq}(z) = F(z+p\omega+q\omega')$$

where F is an entire or a meromorphic function, you must obtain new and interesting results. It seems that Julia did not think of that, otherwise he would have published it urbi et orbi[30]. As for me, I thought of this for one or two days, but I do not want, after the effort I just made, to become absorbed in work that demands much thought. You will do what you like but, being familiar with that kind of subject, you could, if you had some time left to conduct this kind of research, obtain some interesting things.

29 We have seen that Fatou was the archivist of the SMF between 1918 and 1921. The fact that Julia's paper on iteration had been with the printer for early a year dates this letter, as no earlier than the end of 1919.

30 Fatou suggested this question, on which Julia did not publish, to Montel, but it would seemingly appear in the course on uniform functions [Julia 1924a].

1) There exist functions for which the $F_{pq}(z)$ constitute a normal family in any bounded domain, for all ω and ω' (there is no loss of generality if we assume ω and ω' to be non-zero and ω/ω' imaginary).

Examples: among the meromorphic functions the elliptic functions and constant coefficient linear combinations of elliptic functions with distinct periods or not.

Among the entire functions, e^z and the periodic or quasi-periodic functions represented by a *finite* expansion $\sum Ae^{\alpha z}$ where the α are real.

One can easily find necessary and sufficient conditions for a function to be in this category: for instance, if $z, z', z'' \ldots$ tend to infinity and if $F(z), F(z'), F(z'') \ldots$ constitute a bounded sequence, this should also hold in circles of constant radii with centres at $z, z' \ldots$ both for the function itself and for its derivatives; if, on the contrary, the numbers $F(z^\nu)$ tend to infinity and $\left(\frac{1}{F(z)}\right)^{(q)}$ to zero in the same sequence of circles; etc.

It seems more difficult to find sufficient conditions. I do not know any other example apart from the ones mentioned above (probably the Painlevé Boutroux functions: $y'' = 6y^2 + x$ —one should look closer at this).

The limit functions Φ of the F_{pq} are themselves meromorphic or entire functions that can be reduced to constants, among which are always the asymptotic values, especially infinity if $F(z)$ is entire; in the last case there are always infinitely many distinct limit functions. I think that in all cases the limit functions cannot all be constant but I don't know how to prove it.

2) If a function does not fall into the previous category, there exists at least one pair of numbers (ω, ω') and at most two [?] and a domain D where the functions F_{pq} do not constitute a normal sequence, hence a point z_0 where the sequence is not normal; the function F thus takes all values except at most two in circles of constant radius and as small as we want having for centres the points equal to z_0 (mod ω, ω').

It seems that almost all functions fall into this category; examples e^{z^2}, $z + e^z$, $\sin(z^2)/z$ (one sees indeed that either $F'(z)$ is not bounded on the set of roots of $F(z) = a$, or that $F'(z)$ does not tend to zero on an asymptotic path of finite determination, etc.).

Another example: the entire periodic functions represented by an *infinite* expansion

$$\sum_{-\infty}^{+\infty} A_n e^{2ni\pi z/\omega}$$

for, putting $e^{2i\pi z/\omega} = y$, we obtain a Laurent series $\Phi(y) = \sum A_n y^n$ which represents a function with the two essential singular points 0 and ∞. Taking for ω the previous period and a number $\omega' = i\omega$ for instance, one is left with proving that the functions $\Phi(k^q y)$ do not constitute a normal sequence in a suitable annulus, which is precisely your theorem. If one takes the meromorphic or entire functions that satisfy the functional equation

$$\varphi(u+\omega) = R(\varphi(u)), \quad (R \text{ rational})$$

I proved in my memoir that they are continuous at infinity when one excludes a certain infinitely long half-strip from the plane; inside this half-strip, there exist others, as thin as we wish where $\varphi(u)$ takes all values except perhaps one. [The letter contains a picture of the strip.] The sequence $\varphi(u+p\omega+q\omega')$ is thus normal at some points and not at some others.

If you want a function such that no subsequence of the functions F_{pq} is normal at any point in the plane, this is even easier. Ex: I take the function that has zeros

$$z_{kn} = \sqrt{n}\, e^{2ik\pi/n}$$

and poles

$$\varpi_{kn} = \sqrt{n}\, e^{\frac{2ik\pi}{n}+\frac{\pi}{n}}$$

$(k = 0, 1, 2 \ldots n-1,\ n = 2, 3, \ldots \infty)$. Since $\sqrt{n+1} - \sqrt{n} \to 0$ along with $|z_{kn} - \varpi_{kn}|$ you see that any domain that is congruent to a given domain and that goes to infinity tends to contain more and more zeros and poles which have all points of the domain as limit points.

All this is easy. But it is the functions in the 1st category that we ought to characterise better to obtain a general theory of quasi-biperiodic functions.

I gave my memoir (Ch VI and VII) to your janitor on Friday[31]. I hope it is not lost.

Yours sincerely

[signed] P. Fatou

One could perhaps try a classification of some functions with a limited domain of existence and defined, e.g. in a circle, by means of the functions $F(S(z))$, the $S(z)$ being the substitutions of a Fuchsian group.

Your idea of replacing the study of a function by that of a sequence of functions obtained by making the substitutions of a linear group act on the variable is perhaps one of the happiest you had. You should take advantage of it.

Mon cher Montel

Si au lieu d'effectuer sur z les substitutions du groupe $(z|k^{\pm n}z)$ tu effectues les substitutions $(z|z+p\omega+q\omega')$ où p et q désignent tous les entiers et (ω, ω') deux nombres quelconques, et que tu considères les fonctions

$$F_{pq}(z) = F(z+p\omega+q\omega')$$

[31] This letter was thus written just after Fatou had sent in his third memoir on iteration. This justifies the allusion to the effort he had just made, at the beginning of the letter.

où F est une fonction entière ou méromorphe, tu dois parvenir à des résultats nouveaux et intéressants. Il semble que Julia n'y ait pas songé, car il n'aurait pas manqué de le publier urbi et orbi. Quant à moi, j'ai pensé à cela depuis un ou deux jours, mais je ne désire pas après l'effort que je viens de faire m'absorber de nouveau dans un travail qui demanderait beaucoup de réflexion. Je te transmets donc les quelques remarques que j'ai faites à ce sujet. Tu en feras ce que tu voudras, mais étant familier avec ces sortes de sujets, tu pourrais si tu avais quelque loisir pour te livrer à des recherches de ce genre, en tirer des choses intéressantes.

1) Il existe des fonctions pour lesquelles les $F_{pq}(z)$ forment une famille normale dans tout domaine borné quels que soient les ω et ω' (on ne diminue pas la généralité en supposant ω et ω' non nuls et ω/ω' imaginaire).

Exemples: parmi les fonctions méromorphes les fonctions elliptiques et les combinaisons linéaires à coefficients constants de fonctions elliptiques à périodes distinctes ou non.

Parmi les fonctions entières e^z et les fonctions périodiques ou quasi périodiques représentées par le développement *limité* $\sum Ae^{\alpha z}$ où les α sont réels.

On trouve facilement des conditions nécessaires pour qu'une fonction rentre dans cette catégorie: par exemple, si $z, z', z'' \ldots$ tendent vers l'infini et si $F(z), F(z'), F(z'') \ldots$ forme une suite bornée, il doit en être de même dans des cercles de rayon constant ayant pour centres $z, z' \ldots$ tant pour la fonction elle-même que pour ses dérivées. Si au contraire les nombres $F(z^\nu)$ tendent vers l'infini et $\left(\frac{1}{F(z)}\right)^{(q)}$ vers zéro dans la même suite de cercles. Etc.

Il paraît plus difficile de trouver des conditions suffisantes. Je ne connais pas d'autres exemples que ceux mentionnés plus haut (probablement les fonctions de Painlevé Boutroux: $y'' = 6y^2 + x$ — il faudrait voir la chose de près).

Les fonctions limites Φ des F_{pq} sont elles-mêmes des fonctions méromorphes ou entières pouvant se réduire à des constantes, parmi lesquelles il y a toujours les valeurs asymptotiques, notamment l'infini si $F(z)$ est entière; dans ce dernier cas il y a toujours une infinité de fonctions limites distinctes. Je pense que dans tous les cas les fonctions limites ne peuvent pas être toutes des constantes mais je ne sais pas le démontrer.

2) Si une fonction ne rentre pas dans la catégorie précédente, il existe au moins un couple de nombres (ω, ω') et au plus deux nuls [?] et un domaine D où les fonctions F_{pq} ne forment pas une suite normale, donc un point z_0 où la suite n'est pas normale; la fonction F prend ainsi toutes les valeurs sauf deux au plus dans des cercles de rayon constant et aussi petit qu'on le veut ayant pour centres les points congrus à z_0 (mod ω, ω').

Il semble que presque toutes les fonctions entrent dans cette catégorie; exemples e^{z^2}, $z + e^z$, $\sin(z^2)/z$ (on voit en effet ou bien que $F'(z)$ ne reste pas bornée dans l'ensemble des points racines de $F(z) = a$, ou que $F'(z)$ ne tend pas vers zéro sur un chemin asymptotique de détermination finie etc)

Autre exemple: les fonctions périodiques entières représentées par un développement *illimité*

$$\sum_{-\infty}^{+\infty} A_n e^{2ni\pi z/\omega}$$

car en posant $e^{2i\pi z/\omega} = y$, on obtient une série de Laurent $\Phi(y) = \sum A_n y^n$ qui représente une fonction avec les deux points singuliers essentiels 0 et ∞. En prenant pour ω la période précédente et un nombre $\omega' = i\omega$ p. ex on est ramené à prouver que les fonctions $\Phi(k^q y)$ ne forment pas une suite normale dans une couronne convenable, ce qui est précisément ton théorème. Si l'on prend les fonctions méromorphes ou entières qui vérifient l'équation fonctionnelle

$$\varphi(u+\omega) = R(\varphi(u)), \quad (R \text{ rationnelle})$$

j'ai démontré dans mon mémoire qu'elles sont continues à l'infini lorsque l'on supprime du plan une certaine demi-bande de longueur infinie; à l'intérieur de cette demi-bande il en existe d'autres aussi minces que l'on veut où $\varphi(u)$ prend toutes les valeurs sauf une au plus. [la lettre contient un dessin de la bande]. La suite $\varphi(u + p\omega + q\omega')$ est donc normale en certains points et pas en d'autres.

Si tu veux une fonction telle qu'aucune suite extraite des fonctions F_{pq} ne soit normale en aucun point du plan, c'est encore plus facile. Ex: je prends la fonction qui a pour zéros

$$z_{kn} = \sqrt{n}\, e^{2ik\pi/n}$$

et pour pôles

$$\varpi_{kn} = \sqrt{n}\, e^{\frac{2ik\pi}{n} + \frac{\pi}{n}}$$

$(k = 0, 1, 2 \ldots n-1,\ n = 2, 3, \ldots \infty)$. Comme $\sqrt{n+1} - \sqrt{n} \to 0$ ainsi que $|z_{kn} - \varpi_{kn}|$ tu vois que tout domaine congruent à un domaine donné et qui s'éloigne à l'infini finit par comprendre des zéros et des pôles de plus en plus nombreux ayant pour points limites tous les points du domaine.

Tout cela est facile. Mais ce sont les fonctions de la 1re catégorie qu'il faudrait arriver à caractériser un peu mieux de manière à obtenir ainsi une théorie générale des fonctions quasi doublement périodiques.

J'ai déposé mon mémoire (ch VI et VII) chez ta concierge vendredi dernier. J'espère qu'il n'est pas perdu.

Cordialement à toi

[signé] P. Fatou

On pourrait peut-être aussi essayer une classification de certaines fonctions à domaine d'existence limité et définies par ex. dans un cercle au moyen des fonctions $F(S(z))$, les $S(z)$ étant les substitutions d'un groupe fuchsien.

Ton idée de remplacer l'étude d'une fonction par celle d'une suite de fonctions obtenues par les substitutions d'un groupe linéaire effectuées sur la variable est peut-être l'une des plus heureuses que tu aies eue [*sic*]. Tu devrais en profiter.

Sunday [21st?] [1920?]

My dear Montel

I will answer your various questions. Concerning my note on invariant functions, it seems to me better that it appears in the Bulletin[32] for 1921.

[32] This was the short article [Fatou 1922f], which would indeed appear in the *Bulletin*, but in 1922.

I do not know the admiral Thomine and never heard my brother speak of him; I thus assume that they don't know each other very well. As however my brother is in Berlin and communication with him is not very fast, I would rather try to have my cousin the captain whose son entered the Navy school this year, take care of you[33].

I don't know Hamy much, I have exchanged about ten words with him in the nineteen years I have been at the Observatory[34]. As he is rather a "recluse", I don't really see what could influence him. Simonin knows him better than I do, but Simonin...

I thought of the question of the Collège de France, which will probably be raised soon because, according to what Julia told me, Humbert who takes up his course again this year remains very tired; he will try to give his course three times and to rest one month after every month of the course. Here is the hypothesis I contemplate; Humbert being no more able, after a time, to give his course, he will be replaced by Julia and will wait, before retiring, until the latter is old enough to succeed him. Julia's candidacy will be supported by Humbert, by Picard, by all representatives at the academy of the "right-thinking" and by some others. In these conditions, only the candidacy of Lebesgue could prevent that of Julia, and even this is not certain, because Julia's quoted value will only increase and become more attractive over the next few years. My own candidacy, even with Hadamard's support, of which I am not certain, would only have an infinitesimal chance, especially with the other rather scheming candidates whom you know and who will certainly apply.

Since, on the other hand, apart from these considerations, Lebesgue in this chair would be the *right man in the right place*[35], there is no sufficient reason for dissuading him from being candidate[36].

As for me, having resigned myself never to get anywhere, I shall continue quietly in my job as a subordinate of Simonin, waiting to become that of Baillaud's son; this is not glorious, but my philosophy is satisfied with it and it will not prevent me from continuing to do maths as much as I can. I am nonetheless grateful for the efforts you make to find me a better position.

A few words now about mathematical questions that, I think, interest you; I discovered in a set of booklets received this year from Scandinavia a paper by Lindelöf "On the conformal mapping of a simply connected area to a circle" (excerpt of the proceedings of the 1916 Stockholm congress) which is much clearer than the memoir of the same author in the Acta Societatis

[33] It would be pointless to pretend to be able to find one's way among the captains of vessels and other Navy officers on the Fatou family tree. Like Julia, Montel was examiner at the Navy school.

[34] Fatou being at the Observatory since 1901, this gives the letter the date 1920, beginning of 1921.

[35] In English in the original.

[36] As we know, after Humbert's death in 1921, this was indeed Lebesgue who would be elected to replace him.

Fennica. The interior problem is treated in quite a simple and elegant way, by simplifying a method of Carathéodory. As you have an expository talk to give on this question, you would probably be well-advised to look at this booklet that I am making available to you in case you don't have it.

Another thing in a similar vein. You might remember a Note of Denjoy [Montel added in pencil in the margin (1918 CR)] in which he states, generalising a little and without quoting me, one of my theorems on the limit values of analytic functions on a contour. Well, this generalisation itself can be found in a communication of M. Riesz at the same Congress (1916) "Über die Randeverte einer analytischer Funktion" that also contains other interesting things and that I also recommend to you[37].

Yours sincerely

[signed] P. Fatou

I see nothing against your telling my thoughts to Lebesgue

Mon cher Montel

Je réponds à tes différentes questions. Pour ma note sur les fonctions invariantes, il me paraît préférable qu'elle paraisse dans le pour 1921.

Je ne connais pas l'amiral Thomine et n'ai jamais entendu parler de lui par mon frère; je suppose donc qu'ils ne se connaissent pas beaucoup. Comme d'ailleurs mon frère est à Berlin et que les communications avec lui ne sont pas rapides, je verrai plutôt si je peux te faire recommander par mon cousin le capitaine de vaisseau dont tu as reçu le fils à l'école navale cette année.

Je ne connais guère Hamy avec qui j'ai échangé une dizaine de mots en tout depuis 19 ans que je suis à l'Observatoire. Comme c'est plutôt un "sauvage", je ne vois pas trop qui pourrait avoir de l'influence sur lui. Simonin le connaît mieux que moi, mais Simonin...

J'ai réfléchi de mon côté à la question du Collège de France, qui se posera probablement dans peu de temps car, d'après ce que m'a dit Julia, Humbert qui reprend son cours cette année reste toujours très fatigué; il va essayer de faire son cours en trois fois en se reposant un mois après un mois de cours. Voici l'hypothèse très vraisemblable que j'envisage; Humbert ne pouvant plus, d'ici quelque temps, continuer son cours se fera remplacer par Julia et attendra pour prendre sa retraite que ce dernier ait l'âge canonique pour lui succéder. La candidature de Julia sera appuyée par Humbert, par Picard, par tous les représentants à l'Institut de la "bienpensance" et quelques autres encore. Dans ces conditions, seule la candidature de Lebesgue pourrait faire échec à celle de Julia, et ce n'est même pas certain, car la cote de Julia ne fera que croître et embellir pendant quelques années. Ma candidature à moi, même avec l'appui de Hadamard dont je ne suis pas sûr, n'aurait que

[37] In other words, Fatou realises in 1920 that a result of Denjoy published in the *Comptes rendus* in 1918 (this is [Denjoy 1918]) was already in an article by Riesz in 1916 (this is [Riesz & Riesz 1916]): we mentioned in Chapter II, the speed of communication via the *Comptes rendus*; other foreign publications may have taken more time to reach French mathematicians.

des chances infimes, surtout avec les autres candidats assez intrigants que tu connais qui se présenteront certainement.

Comme d'autre part, ces considérations mises à part, Lebesgue serait à cette chaire le right man dans le right place, il n'y a aucune raison suffisante pour le dissuader d'être candidat.

Quant à moi qui suis résigné d'avance à ne jamais arriver, je continuerai tranquillement mon métier de sous-ordre de Simonin en attendant que ce soit du fils Baillaud; c'est peu glorieux, mais ma philosophie s'en accomode [*sic*], et cela ne m'empêchera pas de continuer à faire des math. dans la mesure de mes moyens. Je ne t'en suis pas moins reconnaissant de l'effort que tu fais pour me trouver une situation meilleure.

Quelques mots maintenant sur des questions mathématiques qui, je pense, t'intéressent; j'ai découvert dans un lot de brochures reçues cette année de Scandinavie un article de Lindelöf "Sur la représentation conforme d'une aire simplement connexe sur l'aire d'un cercle" (Extrait des C.R. du congrès de Stockholm 1916) qui est beaucoup plus clair que le mémoire du même auteur des Acta Societatis Fennica. Le problème intérieur y est traité d'une manière fort élégante et simple, en simplifiant une méthode de Carathéodory. Comme tu dois faire un exposé didactique sur cette question, tu aurais intérêt à consulter cette brochure que je tiens à ta disposition si tu ne l'as pas.

Autre chose dans des ordres d'idées voisins. Tu te souviens peut-être d'une note de Denjoy [Montel a ajouté en marge au crayon (1918 CR)] où il énonce en généralisant un peu et sans me citer un théorème de moi sur les valeurs limites des fonctions analytiques sur un contour. Eh bien, cette généralisation même se trouve dans une communication de M. Riesz au même congrès (1916) "Über die Randeverte einer analytischer Funktion" qui contient d'ailleurs d'autres choses intéressantes et que je te recommande également.

Cordialement à toi

[signé] P. Fatou

Je ne vois aucun inconvénient à ce que tu fasses part de mes réflexions à Lebesgue.

December 6th 1920

My dear Montel

In the last few days, I have done a little work on certain uniform functions and I have written a note on it, which I first thought of giving to the C.R. But, since this exceeded noticeably the authorised limits, in order not to have to write another brief and then a detailed article, I have found it convenient to give you this note for the BSM, adding to it a few applications, details of computations and of proofs. Besides, this mode of exposition has in itself some advantages and one might have more chance of being read this way. Please be so kind as to give all this to Galbrun[38].

[38] See the notes page 145. If the Galbrun clearly shows that the "BSM" is the *Bulletin* of the SMF, it is in the *Bulletin... des sciences mathématiques*, as we have said,

If you go to the Société mathématique next Wednesday, which I cannot do myself, my evening being busy, I would be very grateful if you could read out this note, of course without the final explanatory notes and even skipping some passages in § II if necessary. If this bothers you, or if you think that it will bother everybody, you can also avoid it.

Yours sincerely

[signed] P. Fatou

Note for the history of mathematics: § III was thought up at Bullier.

Supplementary note: it is not useful to read the previous note to the SM.

Mon cher Montel

J'ai fait ces jours derniers un petit travail sur certaines fonctions uniformes et ai rédigé là dessus une note que je comptais primitivement donner aux C.R. Mais comme cela dépassait notablement les limites permises, pour ne pas avoir à refaire une note succincte et faire ensuite un article détaillé, j'ai trouvé expédient de te donner cette note pour le BSM, en la faisant suivre de quelques applications, détails de calcul et de démonstration. Ce mode d'exposition a d'ailleurs en soi quelques avantages et peut-être a-t-on plus de chances d'être lu de cette manière. Tu voudras bien remettre tout cela à Galbrun.

Si tu vas à la Société mathématique mercredi prochain, ce que je ne puis faire moi-même, ayant ma soirée prise, je te serais reconnaissant de donner lecture de cette note, bien entendu sans les notes explicatives de la fin et même au besoin en sautant quelques passages du § II. Si cela t'embête et si tu penses que cela embêtera tout le monde, tu peux aussi t'en dispenser.

Cordialement à toi

[signé] P. Fatou

Note pour l'histoire des mathématiques: le § III a été trouvé à Bullier.

Note complémentaire: il est inutile de donner lecture de la note précédente à la SM.

Saturday evening [1921]

My dear friend

I have been advised by the Crédit Lyonnais about 400^{f} you paid into my bank account; I thank you but at the same time I protest, this amount is 20^{f} more than what you owe to me; we shall settle this some time. On the other hand, how did you manage to get reimbursed for the 20^{f} that disappeared? If this were not the case, you would have been paid a negative amount for

that the paper corresponding to the communication was published (this is [Fatou 1921c]). The description of an article comprising a note and computational detail corresponds rather to [Fatou 1923b], which indeed appeared in the *Bulletin* of the SMF, and of which Fatou speaks in a later letter (see the note 45).

your February talks, which seems to me unacceptable, and I think I should reimburse you.

I received Lebesgue's letter to which you contributed. I went to see Picard this morning at the end of his lecture, as I had decided. He gave me the best of welcomes and he seems to be determined to support me; I explained to him that I had some hesitation in applying for this position for health reasons; he told me that this had indeed to be considered, but that, apart from this, my place was at the Sorbonne, not at the Observatory[39]. He told me on the other hand that Denjoy's application had some supporters, but that he did not know the opinion of some of his colleagues, in particular that of Vessiot, and he advised me to see them. I am thus going to pay a visit to test the waters; I shall apply only if there is a comfortable majority in my favour and some goodwill from those who would not vote for me, so that I would be able to ask for not too hard a duty, otherwise I give up. If you were able to learn the opinion of people like Drach, Cartan, Vessiot and Kœnigs, I would be grateful if you would let me know.

Here is another property of your univalent functions in the circle $\mathcal{C}$ and equal to $a, b, c\ldots$ at $a, b, c\ldots$; one can find one of them that represents $\mathcal{C}$ on a domain the contour of which consists of 1° any arc $mpn < 2\pi$ of $\mathcal{C}$; 2° an arc of curve $mrsn$ passing as close as we want to a point q of the plane outside $\mathcal{C}$. I draw an arc of a simple curve mqn containing q and exterior to $\mathcal{C}$ and I perform an auxiliary conformal mapping of the domain $mqnp$ ($> \mathcal{C}$) on the upper Z-half-plane, so that the image of q is the point at infinity; the circle $\mathcal{C}$ will have image the bounded domain MKN.

A_0, B_0, C_0 are the symmetric counterparts of A, B, C with respect to the real axis[40]. I put

$$Z = R(Z) = Z + \varepsilon P(Z)$$
$$P(Z) = (Z - M)(Z - N)(Z - A)(Z - A_0)(Z - B) \cdots (Z - C_0)$$

I take ε real and small and I chose its sign in such a way that

$$|R'(A)| = |2 + \varepsilon P'(A)| > 1$$

which is possible if $P(A) \neq 0$. The transformation $Z' = R(Z)$ transforms the domain MKN or Γ into another one which is also simple if ε is very small and the boundary of which contains the segment MN. Moreover the transform Γ_1 will be above the real axis since

$$Y' = Y + \varepsilon Y H(X, Y) \quad \begin{cases} Z = X + iY \\ Z' = X' + iY' \\ H = \text{polynomial.} \end{cases}$$

[39] Hence, it is indeed a candidacy at the Sorbonne this letter is dealing with.

[40] See Figure A.2.

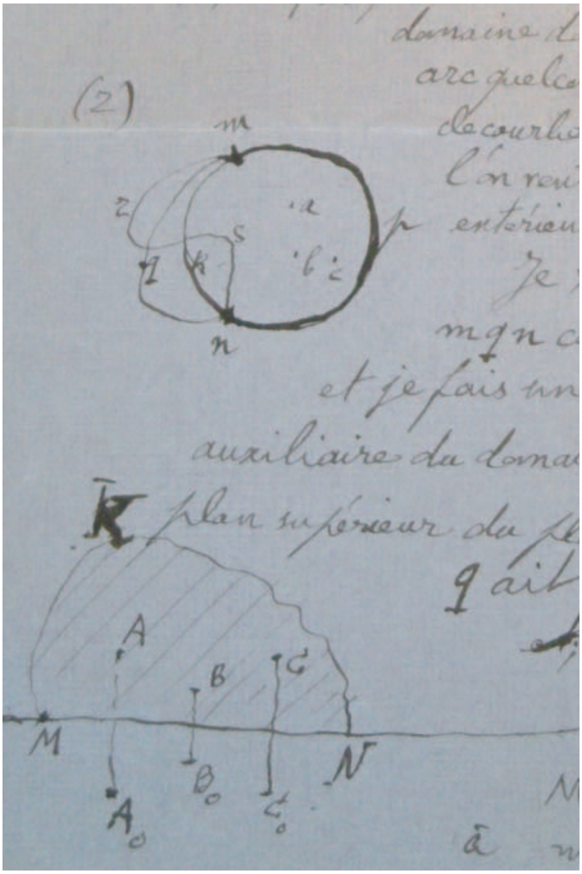

Fig. A.2. The figures in the Saturday evening letter

We don't have $Y' = 0$ for $Y \neq 0$ since this would give $1 + \varepsilon H = 0$; impossible for small ε.

One then uses my first proof procedure: one iterates the polynomial $R(Z)$ and one considers the last consequent Γ_n of Γ that is both simple and above the real axis. The domain $\Gamma_n^{(\varepsilon)}$ does not remain bounded when $\varepsilon \to 0$ (see my 1st proof). Coming back to the z-plane, we have the announced result.

I did not find anything else.

Yours sincerely

[signed] P. Fatou

The proof also shows that there are functions $f(z)$ in your family that keep the real axis invariant or even, if $\mathcal{C}$ is not circular but has an axis of symmetry, this axis.

Mon cher ami

J'ai été avisé par le Crédit Lyonnais d'un versement de 400^{f} fait par toi à mon compte; je te remercie et je proteste en même temps, cette somme excédant de 20^{f} celle que tu me dois; nous réglerons cela quelque jour. D'autre part, as-tu pu te faire rembourser les 20^{f} qui ont été escamotés? S'il en était autrement, tu aurais touché une somme négative pour tes conférences de février, ce qui me paraît inadmissible, et je suis d'avis que je devrais te rembourser.

J'ai reçu la lettre de Lebesgue à laquelle tu as collaboré. J'ai été voir Picard ce matin à la sortie de son cours, comme j'y étais d'ailleurs décidé. Il m'a fait le meilleur accueil et paraît décidé à me soutenir; je lui ai exposé que j'avais quelques hésitations à demander cette place pour des raisons de santé; il m'a dit que c'était en effet à considérer, mais qu'à part cela ma place était à la Sorbonne et non à l'Observatoire. Il m'a dit d'autre part que la candidature de Denjoy avait des partisans, mais qu'il ne connaissait pas l'opinion de certains de ses collègues, notamment de Vessiot et m'a engagé à les voir. Je vais donc faire une tournée de visites pour tâter le terrain; je ne serai candidat que s'il y a une majorité très nette en ma faveur, et une certaine bienveillance même de la part de ceux qui ne voteraient pas pour moi, de façon à pouvoir réclamer un service qui ne soit pas trop pénible, sinon j'abandonne la partie. Si tu peux savoir l'opinion de gens comme Drach, Cartan, Vessiot, Kœnigs, je te serais reconnaissant de me la faire connaître.

Voici encore une propriété de tes fonctions $f(z)$ univalentes dans le cercle $\mathcal{C}$ et égales à $a, b, c\dots$ en $a, b, c\dots$; on peut en trouver une qui représente $\mathcal{C}$ sur un domaine dont le contour se compose 1° d'un arc quelconque $mpn < 2\pi$ de $\mathcal{C}$; 2° d'un arc de courbe $mrsn$ passant aussi près que l'on veut d'un point q du plan extérieur à $\mathcal{C}$. Je trace un arc de courbe simple mqn contenant q et extérieur à $\mathcal{C}$ et je fais une représentation conforme auxiliaire du domaine $mqnp$ ($> \mathcal{C}$) sur le demi-plan supérieur du plan des Z, de manière que q ait pour image le point à l'infini; le cercle $\mathcal{C}$ aura pour image le domaine borné MKN.

A_0, B_0, C_0 sont les symétriques de A, B, C par rapport à l'axe réel. Je pose

$$Z = R(Z) = Z + \varepsilon P(Z)$$
$$P(Z) = (Z - M)(Z - N)(Z - A)(Z - A_0)(Z - B)\cdots(Z - C_0)$$

Je prends ε réel et petit et je choisis son signe de manière que

$$\left|R'(A)\right| = \left|2 + \varepsilon P'(A)\right| > 1$$

ce qui est possible si $P(A) \neq 0$. La transformation $Z' = R(Z)$ transforme le domaine MKN ou Γ en un autre également simple si ε est très petit et dont la frontière contiendra le segment MN. De plus le transformé Γ_1 sera au dessus de l'axe réel, car

$$Y' = Y + \varepsilon Y H(X, Y) \qquad \begin{cases} Z = X + iY \\ Z' = X' + iY' \\ H = \text{polynome.} \end{cases}$$

On n'a pas $Y' = 0$ pour $Y \neq 0$ car cela donnerait $1 + \varepsilon H = 0$; impossible pour ε petit.

On emploie ensuite mon premier procédé de démonstration: on fait l'itération du polynome $R(Z)$ et on considère le dernier conséquent Γ_n de Γ qui soit à la fois simple et au-dessus de l'axe réel. Le domaine $\Gamma_n^{(\varepsilon)}$ ne reste pas borné quand $\varepsilon \to 0$ (voir ma 1^{re} démonstration). En revenant au plan des z on a le résultat annoncé.

Je n'ai pas trouvé autre chose.
Cordialement à toi

[signé] P. Fatou

La démonstration prouve aussi qu'il y a des fonctions $f(z)$ de ta famille qui laissent invariantes l'axe réel ou encore, si $\mathcal{C}$ est non circulaire, mais pourvu d'un axe de symétrie, cet axe de symétrie.

Saturday [1921?]

My dear Montel

Here is some additional information[41] about the functions $f(z)$ that are holomorphic and simple (schlicht) in a domain C and such that $z = f(z)$ leaves points $a_1, a_2 \dots a_p$ invariant. I especially consider the functions

$$f(z) = z + \varepsilon P(z) = z + \varepsilon(z - a_1) \cdots (z - a_p).$$

I pointed out to you that they are simple in C for all sufficiently small ε. This can be seen as follows

$$f(z) = f(z') \quad (z \neq z')$$
$$z - z' + \varepsilon P(z) - P(z') = 0$$
$$1 + \varepsilon \frac{P(z) - P(z')}{z - z'} = 1 + \varepsilon Q(z, z') = 0$$

Q a symmetric polynomial independent of ε; $|z| < A$, $|z'| < A$ imply that $|Q(z, z')| < B$. The previous equality is thus impossible for $|\varepsilon| < 1/B$: $f(z)$ is thus simple for $|\varepsilon| < 1/B$. We thus have substitutions that are close to the identical substitution and that answer the question; but one can also obtain others that take values that are as large as one wants in C, in other words the $f(z)$ are not uniformly bounded. I perform iteration of the substitution

$$z' = R(z) = z + \varepsilon P(z).$$

At least one of the multipliers $(R'(a_i))$ of the double points a_i has absolute value > 1 or is equal to $+1$ (if P has multiple roots) (see e.g. the beginning of my memoir on iteration). Thus the iterated functions do not constitute a normal sequence at a_i (indeed one has $R_n(z) = a_i + s^n(z - a_i) + \cdots$ $(|s| > 1)$, and for $s = 1$: $R_n(z) = a_i + (z - a_i) + cn(z - a_i)^q + \cdots$); from this one concludes (see e.g. my memoir) that the $R_n(z)$ take all finite values around a_i namely in C; the consequent domains $C_1, C_2, \dots C_n \dots$ will eventually contain any point of the plane, especially a root of $1 + \varepsilon P'(z) = R'(z)$ and starting from

[41] Similar to the previous one, this letter mentions holomorphic functions with some invariant points; this is why we placed it here.

here cease to be simple. Let C^{ε}_{n+1} be the first domain that covers itself, C^{ε}_{n} is thus the first domain for which $R(z) = R(z')$ at two of its points z and z'; now I make ε tend to 0; the domains C^{ε}_{n} don't remain bounded, according to my remark at the beginning, because, if we had $|z| < A$ in all these domains, we would have $R(z) \neq R(z')$ in each of them (for $|\varepsilon| < 1/B$).

The functions $f(z)$ are thus not uniformly bounded. Can you prove to me that they take all values? I think this is the case.

Here are, on the other hand, some immediate properties: the $f(z)$ constitute a normal family (I believe this is what you told me the other day, but I don't remember it very well). The transformed domains D of C have a domain Δ in common; what is this domain Δ? The domains D are never interior to C, nor do they include C in their interior (at least for $p > 1$).

Let $M(\theta)$ be the maximum maximorum of $|f(z)|$ for $|z| \leq \theta < 1$ (taking the unit circle for C). $M(\theta)$ is finite (but not bounded $\theta \to 1$). One has $M(\theta) > \theta$. I ask for an upper bound of $M(\theta)$.

One could also accept a pole for $f(z)$ ($f(z)$ still being simple). In this case one can always make the contour of D (transform of C) as close as one wants from a point of the Riemann sphere exterior to C.

You see that the study of this family of functions raises interesting questions. You must be able to answer some of them.

Can you find also some qualitative properties of the contours of the Ds? One could investigate where the Riemann method for the conformal mapping leads.

Yours sincerely

[signed] P. Fatou

Mon cher Montel

Voici quelques renseignements complémentaires au sujet des fonctions $f(z)$ qui sont holomorphes et simples (schlicht) dans un domaine C et telles que $z = f(z)$ laisse invariants $a_1, a_2 \dots a_p$. Je considère spécialement les fonctions

$$f(z) = z + \varepsilon P(z) = z + \varepsilon(z - a_1) \cdots (z - a_p).$$

Je t'ai fait remarquer qu'elles sont simples dans C pour tout ε suffisamment petit. On peut le voir comme il suit

$$f(z) = f(z') \quad (z \neq z')$$
$$z - z' + \varepsilon P(z) - P(z') = 0$$
$$1 + \varepsilon \frac{P(z) - P(z')}{z - z'} = 1 + \varepsilon Q(z, z') = 0$$

Q polynome symétrique indépendant de ε; $|z| < A$, $|z'| < A$ entraînent $|Q(z, z')| < B$. L'égalité précédente est donc impossible pour $|\varepsilon| < 1/B$: $f(z)$ est donc simple pour $|\varepsilon| < 1/B$. Nous avons ainsi des substitutions voisines de la substitution identique qui répondent à la question; mais on peut en obtenir qui prennent des valeurs aussi grandes que l'on veut dans C, autrement dit les $f(z)$ ne sont pas bornées dans leur ensemble. Je fais l'itération de la substitution

$$z' = R(z) = z + \varepsilon P(z).$$

L'un au moins des multiplicateurs $(R'(a_i))$ des points doubles a_i est > 1 en module ou égal à $+1$ (si P a des racines multiples) (voir p. ex le début de mon mémoire sur l'itération). Donc les fonctions itérées ne forment pas une suite normale en a_i (on a en effet $R_n(z) = a_i + s^n(z - a_i) + \cdots$ $(|s| > 1)$, et pour $s = 1$: $R_n(z) = a_i + (z - a_i) + cn(z - a_i)^q + \cdots$); on en conclut (voir p. ex mon mémoire) que les $R_n(z)$ prennent toutes les valeurs finies autour de a_i c'ad dans C; les domaines conséquents $C_1, C_2, \ldots C_n \ldots$ finiront par comprendre un point quelconque du plan, notamment un point racine de $1 + \varepsilon P'(z) = R'(z)$ et à partir de là cessent d'être simples. Soit C_{n+1}^{ε} le premier domaine qui se recouvre lui-même, C_n^{ε} est donc le premier domaine pour lequel $R(z) = R(z')$ en deux de ses points z et z'; je fais tendre maintenant ε vers 0; les domaines C_n^{ε} ne restent pas bornés d'après ma remarque du début, car si on avait $|z| < A$ dans tous ces domaines, on aurait $R(z) \neq R(z')$ dans chacun d'eux (pour $|\varepsilon| < 1/B$).

Les fonctions $f(z)$ ne sont donc pas bornées dans leur ensemble. Peux-tu me démontrer qu'elles prennent toutes les valeurs? Je pense qu'il en est ainsi.

Voici d'autre part quelques propriétés immédiates: les $f(z)$ forment une famille normale (je crois que c'est cela que tu me disais l'autre jour, mais je ne me le rappelle plus très bien. Les domaines D transformés de C ont en commun un domaine Δ; quel est ce domaine Δ? Les domaines D ne sont jamais intérieurs à C, ni ne comprennent C à leur intérieur (du moins pour $p > 1$).

Soit $M(\theta)$ le maximum maximorum de $|f(z)|$ pour $|z| \leq \theta < 1$ (en prenant pour C le cercle unité). $M(\theta)$ est fini (mais non borné $\theta \to 1$). On a $M(\theta) > \theta$. Je demande une limite supérieure de $M(\theta)$.

On pourrait aussi admettre un pôle pour $f(z)$ ($f(z)$ étant toujours simple). Dans ce cas on peut toujours faire passer le contour de D (transformé de C) aussi près que l'on veut d'un point de la sphère de Riemann, extérieur à C.

Tu vois que l'étude de cette famille de fonctions soulève des questions intéressantes. Tu dois pouvoir répondre à certaines d'entre elles.

Peux-tu aussi trouver des propriétés quantitatives des contours des D? On pourrait rechercher à quoi conduit la méthode de Riemann pour la représentation conforme.

Cordialement à toi

[signé] P. Fatou

[1921?]

My dear Montel[42]

The expression in C $(|z| > 1)$ for the uniform substitutions that render the points $a_1 a_2 \ldots a_p$ invariant is:

$$z' = z + P(z)\varphi(z) \quad (P(z) = (z - a_1)\cdots(z - a_p))$$

[42] This card could date from just before the previous letter (about schlicht functions).

One immediately reaches the goal you propose by taking a substitution infinitely close to the unit substitution, for instance

$$z' = z + \varepsilon P(z)$$

ε being a very small constant; indeed when $\varepsilon \to 0$, there is continuity of various orders; let $z' = \rho e^{i\omega}$ when $z = e^{i\varphi}$, ω varies in the same way as φ since $\dfrac{d(\omega - \varphi)}{d\varphi}$ is very small; ρ and ω are thus periodic functions of φ, and Γ is a curve with no double point. Thus we have a conformal mapping to the interior of C on the interior of Γ (Γ must cross C).

I hope this is enough for you

Sincerely

[signed] P. Fatou

[Some computation has been added, in red ink, by Montel.]

Mon cher Montel

L'expression des substitutions uniformes dans C ($|z| > 1$) qui laissent invariants les points $a_1 a_2 \ldots a_p$ est:

$$z' = z + P(z)\varphi(z) \quad (P(z) = (z - a_1) \cdots (z - a_p))$$

On arrive de suite au but que tu te proposes en prenant une substitution inf[t] voisine de la substitution unité, par exemple

$$z' = z + \varepsilon P(z)$$

ε étant une constante très petite; en effet quand $\varepsilon \to 0$, il y a continuité des divers ordres; soit $z' = \rho e^{i\omega}$ pour $z = e^{i\varphi}$, ω varie dans le même sens que φ puisque $\dfrac{d(\omega - \varphi)}{d\varphi}$ est très petit; ρ et ω sont donc des fonctions univoques et périodiques de φ et Γ est une courbe sans point double. Donc on a une représentation conforme de l'intérieur de C sur l'intérieur de Γ (Γ traverse nécessairement C).

J'espère que cela te suffit

Cordialement

[signé] P. Fatou

Fragment, 1921

[...] the situation of a deputy, who can one day be sent back to his mathematical studies[43]. This being given, you can take it that I am not applying, but don't say so yet; I will take a few more days to think and I could change my

[43] Most probably, what is discussed here is the replacement of Painlevé: Painlevé once elected to Parliament, would leave his teaching duties (but not his position)..

mind if I was to obtain more favourable information on the particulars of the duty, but this is rather unlikely since Vessiot, who is the greatest authority in these mathers and who seems kindly disposed, has given me a glimpse of the duties which do not really suit me.

On the other hand, Vessiot gave me advice that I decided to follow although this has no longer, in accordance to what precedes, any positive relevance for me, that is to print a notice[44]. Speaking of which, I found in my notes a certain number of results, some of which might not be without value, but some of them might not be new. As you read more than I do and as you are best equipped to make a bibliography, I would be grateful if you could tell me if you know the following propositions.

I. Let $F(z)$ be a holomorphic function inside the unit circle, and V the *logarithm of the maximum modulus* of $F(z)$ for $|z| = r, r_1 \ldots r_n \ldots$ being the absolute values of the zeros of $F(z)$:

1) If $\int_0^1 V\,dr$ is finite, the series $\sum(1 - r_n)^2$ is not convergent. 2) If $V \leq \frac{1}{1-r}^{\alpha}$, the series $\sum(1 - r_n)^{1+\alpha+\varepsilon}$ is convergent for $\varepsilon > 0$.

(Analogous uniform convergence theorems hold when $F(z)$ depends on parameters.)

II. Let $f(z)$ and $g(z)$ be two entire (transcendent) functions and p an arbitrary non-negative number; and let M and M_1 be the maximum absolute values of $f(z)$ and $f[g(z)]$ for $|z| = r$. One has for some unbounedly large values of r

$$M_1 > r^p M$$

Yours sincerely

[signed] P. Fatou

[...] la situation d'un député que ses électeurs peuvent renvoyer un beau jour à ses études mathématiques. Ceci posé, tu peux considérer que je ne suis pas candidat, mais ne le dis pas encore; je me donne encore quelques jours pour réfléchir et je pourrais changer d'avis si j'obtenais des renseignements plus favorables en ce qui concerne l'organisation du service, mais c'est peu probable puisque Vessiot qui est la plus grande autorité en la matière et qui paraît animé de dispositions bienveillantes me laisse entrevoir la perspective du service auquel je suis le moins adapté.

D'autre part, Vessiot m'a donné un conseil que je suis décidé à suivre bien que cela n'ait plus pour moi, en vertu de ce qui précède, d'utilité positive, à savoir de faire imprimer une notice. À ce propos, je retrouve dans mes brouillons un certain nombre de résultats dont quelques-uns ne sont peut-être pas sans valeur, mais il peut y en avoir qui ne sont pas nouveaux. Comme tu lis plus que moi et que tu es sans doute mieux outillé pour faire de la bibliographie, je te serais reconnaissant de me dire si tu as connaissance des propositions suivantes.

---

[44] It could also be about a candidacy at the ENS (Vessiot was the scientific director). Fatou printed a notice in 1921 (it is in his file at the Academy of Sciences). It is likely that this fragment was written after the Saturday evening letter: Fatou went to see Vessiot.

I. Soit $F(z)$ une fonction holomorphe à l'intérieur du cercle unité, V le *logarithme du module maximum de* $F(z)$ pour $|z| = r, r_1 \dots r_n \dots$ les modules des zéros de $F(z)$:

1) Si $\int_0^1 V\,dr$ est finie, la série $\sum(1 - r_n)^2$ est convergente. 2) Si $V \leq \frac{1}{1-r}^{\alpha}$, la série $\sum(1 - r_n)^{1+\alpha+\varepsilon}$ est convergente pour $\varepsilon > 0$.

(Théorèmes analogues de convergence uniforme quand $F(z)$ dépend de paramètres.)

II. Soient $f(z)$ et $g(z)$ deux fonctions entières (transcendantes) et p un nombre positif arbitraire; M et M_1 les modules maxima de $f(z)$ et de $f[g(z)]$ pour $|z| = r$. On a pour certaines valeurs infiniment grandes de r

$$M_1 > r^p M$$

Cordialement à toi

[signé] P. Fatou

Undated letter, around 1922

Let, my dear Montel,

$$F(z) = \int_0^1 \frac{f(u)\,du}{z - u}$$

$f(u)$ being an absolutely integrable real function. Assume first $f(u) > 0$, one has

$$\Im(F(z)) = -y \int_0^1 \frac{f(u)\,du}{(x-u)^2 + y^2} \qquad z = x + iy$$

Thus

$$\Im(F(z)) < 0$$

(staying in the upper half-plane). My theorem on the limiting values of the bounded functions can thus be applied to the function $\dfrac{1}{F(z) - i}$ and then to $F(z)$. Moreover, the limiting value is almost nowhere infinite; this amounts to saying that for a bounded function the limiting value is almost nowhere zero; this is what F and M Riesz proved (Uber die Randeverte einer analytischen Funktion, Stockholm 1916) and myself in a much simpler way at the end of the paper I gave you (On holomorphic and bounded functions)[45].

Now let $f(u) > 0$ or < 0; $f(u)$ being absolutely integrable is the difference of two non-negative functions enjoying the same property

$$f = f_1 - f_2$$

The operation we consider is distributive so that

$$F(z) = F_1(z) - F_2(z)$$

[45] These are the articles [Riesz & Riesz 1916] and [Fatou 1923b]. We have seen that Fatou gave the latter to Montel before 1923. This letter is still written on the blue notepaper so that we can approximately date it.

and the theorem still holds true for F (since we have nowhere $\infty - \infty$).

Similarly if $f = f_1 + if_2$, we can do the same decomposition.

The proposition you stated to me yesterday would thus say that functions with more than 2 exceptional values in the neighbourhood of a cut behave, from the point of view of indetermination, in the same way as bounded functions. I have some doubts on the correctness of this proposition which does not seem to me to be compatible with the existence of some Fuchsian functions that may have any finite number of exceptional values, as far as I can remember, and that, with respect to the lattice of Fuchsian polygons, is certainly not the indetermination mode that holds for bounded functions.

It is true that on the one hand I have not looked at Fuchsian functions for a long time and that I might stammer on this subject; and that on the other hand I could have badly understood your statement. I nevertheless advise you to pay attention to my objection.

Sincerely

[signed] P. Fatou

Soit, mon cher Montel,

$$F(z) = \int_0^1 \frac{f(u)\,du}{z-u}$$

$f(u)$ étant une fonction réelle, absolument intégrable. Soit d'abord $f(u) > 0$, on a

$$\Im(F(z)) = -y \int_0^1 \frac{f(u)\,du}{(x-u)^2 + y^2} \qquad z = x + iy$$

Donc

$$\Im(F(z)) < 0$$

(en restant dans le demi-plan supérieur). Mon théorème sur les valeurs limites des fonctions bornées est donc applicable à la fonction $\dfrac{1}{F(z) - i}$ et par suite à $F(z)$. De plus la valeur limite n'est infinie presque nulle part; cela revient à dire que pour une fonction bornée la valeur limite n'est zéro presque nulle part; c'est ce qu'ont démontré F et M Riesz (Uber die Randeverte einer analytischen Funktion, Stockholm 1916) et moi-même d'une manière beaucoup plus simple à la fin de l'article que je t'ai remis (Sur les fonctions holomorphes et bornées).

Soit maintenant $f(u) > 0$ ou < 0; $f(u)$ étant absolument intégrable est la différence de deux fonctions positives jouissant de la même propriété

$$f = f_1 - f_2$$

L'opération considérée étant distributive on a

$$F(z) = F_1(z) - F_2(z)$$

est le théorème est encore vrai pour F (puisqu'on n'a $\infty - \infty$ nulle part).

De même si $f = f_1 + if_2$, on opère la même décomposition.

La proposition que tu m'as énoncée hier voudrait donc dire que les fonctions qui ont plus de 2 valeurs exceptionnelles au voisinage d'une coupure s'y comportent au point de vue de l'indétermination de la même manière que les fonctions bornées. J'ai quelques doutes sur l'exactitude de cette proposition qui ne me paraît pas compatible avec l'existence de certaines fonctions fuchsiennes qui peuvent avoir un nombre fini quelconque de valeurs exceptionnelles, autant qu'il me souvient et qui, eu égard au réseau des polygones fuchsiens, n'est certainement pas le mode d'indétermination qui vaut pour les fonctions bornées.

Il est vrai que d'une part je n'ai pas regardé depuis longtemps les fonctions fuchsiennes et que je bafouille peut-être à ce sujet; que d'autre part j'ai peut-être mal compris ton énoncé. Je t'engage cependant à faire attention à mon objection.

Cordialement

[signé] P. Fatou

Tuesday [?]

[This letter was written on white paper]

My dear Montel

I received another missive from the good Fréchet who has definitely no sense of the ridiculous. I am sending you this letter along with my answer if you would be kind to post it after you have read it; if you see anything you want to change in it, just send it back to me with the corrections you require.

I received a letter from Lebesgue the contents of which you probably know. On the other hand, I went to see Goursat, who advised me to wait and not say yet that I am not applying.

I return to my letter which was interrupted, in fact, by the arrival of Lebesgue, who had come to tell me on behalf Goursat that Picard agreed with Julia that people should give me a teachnig duty that would suit me and that, in those circumstances, he advised me to apply. I thus believe that this time I will not be able to shy away; on reflection, it seems to me that I can try this experiment especially if, being simply delegated at the Sorbonne to replace Painlevé, I am on leave from the Observatory, this would allow me, I think, to return there if, for instance, after a year I see that I am not in a fit state to continue at the Sorbonne[46]. I must clarify this point, which is very important to me.

I shall go to the SM tomorrow evening[47]. Lebesgue too. Try to come if you have nothing better to do.

Yours sincerely

[46] It is more or less clear that this letter dates from the same period as the other letters in which the replacement of Painlevé is mentioned. This is a period in which Julia was already at the Sorbonne (and he taught there from 1920) but Denjoy was not (he started teaching there in 1922). There is nevertheless a small doubt on its position among the previous letters, as this was not written on the blue notepaper.

[47] The sessions of the SMF taking place on Wednesdays, and Fatou attending them regularly, this does not help much in dating the letter!

[signed] P. Fatou

Mon cher Montel

Je reçois une nouvelle missive de ce brave Fréchet qui n'a vraiment pas le sens du ridicule. Je te communique cette lettre et ma réponse que tu voudras bien mettre à la poste après en avoir pris connaissance; si tu voyais quelque chose à y changer, en ce qui te concerne, tu n'aurais qu'à me la renvoyer avec les corrections que tu demandes.

J'ai reçu une lettre de Lebesgue dont tu connais probablement le contenu. Je suis allé d'autre part voir Goursat qui m'a conseillé d'attendre et de ne pas dire maintenant que je ne suis pas candidat.

Je reprends ma lettre interrompue précisément par l'arrivée de Lebesgue qui vient de me dire de la part de Goursat que Picard s'entend avec Julia pour qu'on me donne un service qui puisse me convenir et que dans ces conditions il me conseille d'être candidat ferme. Je crois donc cette fois ne pas pouvoir me dérober; à la réflexion il me semble que je peux tenter cette expérience surtout si étant simplement délégué à la Sorbonne en remplacement de Painlevé, je suis mis en congé à l'Observatoire ce qui me permettrait, je pense, d'y reprendre du service si par exemple au bout d'un an je constatais n'être pas en état de continuer à la Sorbonne. Il faudra que j'éclaircisse ce point qui est très important pour moi.

J'irai demain soir à la SM. Lebesgue aussi. Tâche d'y venir si tu n'as rien de mieux à faire.

Cordialement à toi

[signé] P. Fatou

Friday [1922?]

My dear Montel

I accept without any reservation the 2[nd] paragraph[48] p. 41 but I have some concern about what immediately precedes it (p. 41, lines 6 to 12): "Since there is a curve ending at z_0 on which $f(z)$ has limit z_0 we deduce that $f(z)$ has limit z_0 on any curve interior to (d) ending at z_0 and fulfilling the conditions of § 23...

I return to the conclusions of § 23 and I read: Let L be a curve, interior to the angle AOB, tangent at O to OA and admitting at this point a non-zero radius of curvature...

But here, we have no reason to assume that the curve ℓ has a radius of curvature, or even a tangent distinct or not from the tangent to the circle. Besides, § 23 envisages curves that are closer and closer to the boundary and what we are interested in here, are rather the chords of the circumference.

It is to the preceding § 22 that we should go back and, actually, this § 22 confirms *more or less* your claim; but not quite since, I repeat, the argument

[48] The paper in question is [Montel 1917b], but the letter was written long after the publication of the article. It probably followed a discussion with Montel.

of the limit of $z - z_0$ may take all the values between $\frac{\pi}{2}$ and $\frac{3\pi}{2}$ (fig) so that the curve L (p. 34) would be tangent neither to OA nor to OB, and nor would it be interior to an angle $< AOB$.

However, I notice, going back to the proof of § 22 that there would probably be few things to change to make this fit. This works if the [illegible] λ_n uniformly converge to the segment A_2A_3; this also works a fortiori (§ 11) if the limit points of the λ_n are all interior to Δ'_1. I thus think that this works in the intermediate cases. Hence there is no error in the principle, as I thought at first, but only carelessness in the detail, which we mention especially because your proofs are, in general, perfectly precise and clear.

Your § 22 thus misses the proof of the following theorem which, at first sight, seems to need only a few lines (however, it would take some thought): "If $f(x)$ tends to α on any curve ending at 0 and interior to AOB, $f(x)$ uniformly tends to α in any sector $A'OB'$ which is completely interior to AOB". Until this is done, some doubt may remain about the conclusions of p. 41. [figure]

Since I began the criticism of your work, I note what follows on the subject of your memoir on normal families (1916)[49].

The proof of the theorem on pages 240 to 242, on boundaries such that the areas of the domains they cover [surround?] are bounded is skilful but it seems to me that this is redundant, the theorem reducing very easily to that on functions with two exceptional values. For, if one takes in the plane of the $X = f(x)$ two squares C and C' with side $\sqrt{M+1}$ completely exterior to each other, $f_n(x)$ cannot take some value a_n of C nor b_n of C'. From any sequence of the f_n one can extract another one for which a_n and b_n tend respectively to a and b ($a \neq b$). The $\frac{f_n(z) - a_n}{b_n - a_n}$ never taking values 0 or 1 constitute a normal family etc. You see the conclusion.

I note finally that you could have given as an example of a normal family the functions $f(x, y)$ that are holomorphic in x and continuous with respect to both x and y where y varies in the closed domain Δ and x in D' interior to D. As banal as this remark is, I think it is the starting point of interesting research on analytic functions of two variables. I give this over to you to think about.

I now jump to other subjects. Enjalbran tells me to warn you that to regularise the papers for the pension we must apply to the Recette centrale de la Seine, place Vendôme. The accounts of the tax inspectors from 1902 were all transferred there.

Lastly, I invite you to have dinner with me next Tuesday (28)[50] at the restaurant S$^{\text{t}}$ Michel 7$^{\text{h}}\frac{1}{4}$.

Sincerely

[signed] P. Fatou

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49 This is [Montel 1916].

50 There were Tuesdays the 28$^{\text{th}}$ (between 1921 and 1924): in June 1921, February 1922, March 1922, November 1922, August 1923 and October 1924. The chances are thus in favour of 1922.

Mon cher Montel

J'accepte sans restriction le 2[e] alinéa p. 41 mais je fais quelques réserves sur ce qui précède immédiatement (p. 41, lignes 6 à 12): "Puisqu'il existe une courbe aboutissant en z_0 sur laquelle $f(z)$ a pour limite z_0 on en déduit que $f(z)$ a pour limite z_0 sur toute courbe intérieure à (d) aboutissant en z_0 et remplissant les conditions du § 23.........

Je me reporte aux conclusions du § 23 et je lis: Soit L une courbe intérieure à l'angle AOB, tangente en O à OA et admettant en ce point un rayon de courbure non nul.........

Or ici nous n'avons aucune raison de supposer que la courbe ℓ a un rayon de courbure, ni même une tangente distincte ou non de la tangente au cercle. D'ailleurs le § 23 envisage des courbes de plus en plus rapprochées de la frontière et ce qui nous intéresse ici surtout ce sont au contraire les cordes de la circonférence.

Il faut donc se référer au § 22 qui précède et, effectivement ce § 22 donne *à peu près* confirmation de ton assertion; mais pas tout à fait car, je le répète, l'argument limite de $z - z_0$ peut avoir toutes les valeurs comprises entre $\frac{\pi}{2}$ et $\frac{3\pi}{2}$ (fig) [Il y a une figure dans la lettre] en sorte que la courbe L (p. 34) ne soit ni tangente à OA ou à OB, ni intérieure à un angle $< AOB$.

Je constate cependant, en me reportant à la démonstration du § 22 qu'il n'y a aurait probablement pas grand chose à changer pour que ça colle. Cela marche si les [illisible] λ_n convergent uniformément vers le segment A_2A_3; cela marche aussi à fortiori (§ 11) si les points limites des λ_n sont tous intérieurs à Δ_1'. Je pense donc que cela marche dans les cas intermédiaires. Il n'y a donc pas d'erreur de principe, comme je l'avais cru d'abord, mais seulement négligence de détail, dont on s'aperçoit d'autant plus que tes démonstrations sont, en général, parfaitement précises et claires.

Il manque donc à la suite du § 22 la démonstration du théorème suivant qui, à première vue semble devoir tenir en quelques lignes (mais encore faut-il y réfléchir un peu): "Si $f(x)$ tend vers α sur une courbe quelconque aboutissant en 0 et intérieure à AOB, $f(x)$ tend uniformément vers α dans tout secteur $A'OB'$ complètement intérieur à AOB" Tant que cela n'est pas fait, il peut subsister quelque doute sur les conclusions de la p. 41. [une figure]

Puisque j'ai entamé la critique de tes travaux, je note ce qui suit au sujet de ton mémoire sur les familles normales (1916).

La démonstration du théorème pages 240 à 242, sur les frontières telles que les aires des domaines qu'elles recouvrent restent bornés [*sic*] est habile mais me paraît inutile le théorème se ramenant bien facilement à celui qui concerne les fonctions ayant 2 valeurs exceptionnelles. Car si on prend dans le plan des $X = f(x)$ deux carrés C et C' complètement extérieurs l'un à l'autre et de cote $\sqrt{M+1}$, $f_n(x)$ ne peut pas prendre une certaine valeur a_n de C ni b_n de C'. De toute suite des f_n on peut en extraire une autre pour laquelle a_n et b_n tendent respectivement vers a et b $(a \neq b)$. Les $\frac{f_n(z) - a_n}{b_n - a_n}$ ne prenant jamais les valeurs 0 et 1 forment une famille normale etc. Tu vois la conclusion.

Je note enfin que tu aurais peut-être pu citer comme exemple de famille normale les fonctions $f(x,y)$ holomorphes en x et continues par rapport à l'ensemble des deux variables x et y quand y varie dans le domaine fermé Δ et x dans D' intérieur à D. Pour banale que soit cette remarque je crois qu'elle peut être le point de départ de recherches intéressantes sur les fonctions analytiques de deux variables. Je livre cela à tes réflexions.

Je passe maintenant à d'autres sujets. Enjalbran me dit de t'avertir qu'il faut s'adresser pour régulariser les pièces pour la retraite à la Recette centrale de la Seine, place Vendôme. Les comptes des percepteurs à partir de 1902 et suiv. sont tous transférés là.

Enfin je te convie à dîner avec moi mardi prochain (28) au restaurant S^{t} Michel $7^{\text{h}}\frac{1}{4}$.

Cordialement

[signé] P. Fatou

January 30th [1922?]

[Card]

My dear Montel

I read in your memoir on normal families of analytic functions[51], n° 38, p. 301:

"Let us consider the relation $X^m + Y^n = 1$ in which $\frac{1}{m} + \frac{1}{n} < 1$. If we solve this relation with the help of 2 uniform functions of x, it follows from what precedes that these functions have singularities other than essential isolated points".

This is wrong, since for

$$m = 2, \quad n = 4$$

$$m = 2, \quad n = 3$$

$$m = 3, \quad n = 3$$

the curve has genus 1, X and Y being elliptic functions. The th. of n° 38 is thus incorrect.

The theorem of n° 37 is correct, but is contained in that of Picard: if 2 meromorphic functions are subject to an algebraic relation, this relation has genus zero or one.— The fact is thus new only for the finite number of values of m and n for which $X^m + Y^n = 1$ has genus 1.

For this case, I proved the following th. (which is probably known, but I am not sure): 2 entire functions, or an entire function and a meromorphic function, cannot be subject to an algebraic relation of genus 1.

Yours sincerely

[signed] P. Fatou

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[51] This is [Montel 1916].

⋆

[Added by Montel: there is a mistake only in the statement—add [illegible] poles.]

Mon cher Montel

Je lis dans ton mémoire sur les familles normales de fonctions analytiques, n° 38, p. 301:

"Considérons la relation $X^m + Y^n = 1$ dans laquelle $\frac{1}{m} + \frac{1}{n} < 1$. Si l'on résout cette relation à l'aide de 2 fonctions uniformes de x, il résulte de ce qui précède que ces fonctions ont d'autres singularités que des points essentiels isolés".

Ceci est faux, car pour

$$m = 2, \quad n = 4$$
$$m = 2, \quad n = 3$$
$$m = 3, \quad n = 3$$

la courbe est de genre 1, X et Y sont des fonctions elliptiques. Le th. du n° 38 est donc incorrect.

Le théorème du n° 37 est exact, mais est contenu dans celui de Picard: si 2 fonctions méromorphes sont reliées par une relation algébrique, cette relation est de genre zéro ou un.— Le fait n'est donc nouveau que pour les valeurs de m et n en nombre fini pour lesquelles $X^m + Y^n = 1$ est de genre 1.

Pour ce cas j'ai démontré le th. suivant (qui est probablement connu, mais je n'en suis pas sûr): 2 fonctions entières, ou une fonction entière et une fonction méromorphe, ne peuvent être liées par une relation algébrique de genre 1.

Cordialement à toi

[signé] P. Fatou

⋆

[Ajouté par Montel: il y a erreur seulement dans l'énoncé — ajouter [illisible] pôles.]

February 1st 1923

[Card. The date was added by Montel.]

My dear Montel

You are right, the case of an isolated double point for the cubic must be examined, and this is quite easy. Its equation can be put in the form:

$$x(ax^2 + bxy + cy^2) = x^2 + y^2$$

by a real Möbius transformation, thus:

$$x = \frac{1+t^2}{a+bt+ct^2} \quad y = \frac{t(1+t^2)}{a+bt+ct^2}$$

One finds immediately for the parameters of the three aligned points the relation

$$\frac{t_1t_2 + t_2t_3 + t_1t_3 - 1}{t_1t_2t_3 - (t_1 + t_2 + t_3)} = \frac{a - c}{b} = k$$

Putting $t_1 = t_2 = \theta$ and $t_3 = t$, one gets a quadratic equation in θ which determines the tangents through (t):

$$\theta^2(1 - kt) + 2\theta(t + k) - (1 - kt) = 0 \qquad (\theta'\theta'' = -1)$$

hence always two real roots; the lines $O(\theta')$, $O(\theta'')$ are rectangular, and hence in general harmonic conjugates with respect to the tangents at the double point; a probably well known property, an implication of which is the reality of θ', θ'' when the tangents at the double point are imaginary and conjugated.

The proof still works for quartics when A or B are isolated.

Your remark about (hyperelliptic) curves having a double point of order $n - 2$ is interesting; in this case I do not see a proof by the procedures of classical analysis.

Yours sincerely

[signed] P. Fatou

Mon cher Montel

Tu as raison, il faut examiner le cas du point double isolé pour la cubique, ce qui est bien facile. On peut mettre son équation sous la forme:

$$x(ax^2 + bxy + cy^2) = x^2 + y^2$$

par une homographie réelle, d'où:

$$x = \frac{1 + t^2}{a + bt + ct^2} \qquad y = \frac{t(1 + t^2)}{a + bt + ct^2}$$

On trouve de suite pour les paramètres des 3 points en ligne droite la relation

$$\frac{t_1t_2 + t_2t_3 + t_1t_3 - 1}{t_1t_2t_3 - (t_1 + t_2 + t_3)} = \frac{a - c}{b} = k$$

En faisant $t_1 = t_2 = \theta$ et $t_3 = t$ on a l'équation du second degré en θ qui détermine les tangentes issues de (t):

$$\theta^2(1 - kt) + 2\theta(t + k) - (1 - kt) = 0 \qquad (\theta'\theta'' = -1)$$

donc toujours deux racines réelles; les droites $O(\theta')$, $O(\theta'')$ sont rectangulaires, donc en général conjuguées harmoniques par rapport aux tangentes au point double; propriété certainement connue d'où découle la réalité de θ', θ'' quand les tangentes au point double sont imaginaires conjuguées.

La démonstration pour les quartiques marche encore si A ou B sont isolés.

Ta remarque pour les courbes (hyperelliptiques) ayant un point double d'ordre $n - 2$ est intéressante; dans ce cas je ne vois pas la démonstration par les procédés de l'analyse classique.

Cordialement à toi

[signé] P. Fatou

Saturday evening [1923]

[Card]

I am adding a word to my letter of this morning[52] to tell you that there is no need to look for a proof that a (space) curve of order 3 admits a chord through any point of the space, since the thing is not true for space cubics if one does not admit imaginary points; the chord is always real, but the two points can be imaginary and conjugated.

Example $x_1 : x_2 : x_3 : x_4 = 1 + t^2 : t(1 + t^2) : 1 : 1 + t^3$ is projected from the point $(x_1 = x_2 = x_4 = 0)$ onto the plane $x_3 = 0$ according to the plane cubic

$$x_1^3 + x_2^3 = x_4(x_1^2 + x_2^2)$$

the double point of which $(x_1 = x_2 = 0)$ is isolated.

The statement must thus be completed by a condition which I don't see at all.

[signed] P. Fatou

J'ajoute un mot à ma lettre de ce matin pour te dire qu'il n'y a pas lieu de chercher à démontrer qu'une courbe d'ordre 3 (gauche) admet une corde passant par tout point de l'espace, puisque la chose n'est pas vraie pour les cubiques gauches quand on n'admet pas les points imaginaires; la corde est toujours réelle, mais les deux points peuvent être imaginaires conjugués.

Exemple $x_1 : x_2 : x_3 : x_4 = 1 + t^2 : t(1 + t^2) : 1 : 1 + t^3$ est projetée du point $(x_1 = x_2 = x_4 = 0)$ sur le plan $x_3 = 0$ suivant la cubique plane

$$x_1^3 + x_2^3 = x_4(x_1^2 + x_2^2)$$

dont le point double $(x_1 = x_2 = 0)$ est isolé.

Il faut donc compléter l'énoncé par une condition que je n'aperçois pas du tout.

[signé] P. Fatou

Paris, January 27th 1929. Draft of a letter from Paul Montel to Pierre Fatou

[Letterhead of the chair of rational mechanics at the faculty of sciences. This is indeed a draft, the pieces of which have probably been reordered for the version Montel sent to Fatou.]

⋆

Communicated to M. E. Meyer
for whatever purpose it may serve
(return to me)

[52] Which is not the previous card: February 1st 1923 was a Thursday. It is certain that some of Fatou's letters to Montel were not kept.

My dear friend

I was just informed that there is circulating in the scientific circles a strange version of my attitude with repsect to you during the last election at the Faculty, according to which I did not vote for you.

I would not even have thought of noticing it if this version had not been presented to me as coming from you[53], of which I remain deeply surprised & saddened.

You cannot have forgotten that I have not stopped supporting and helping you with all my strength, throughout your career.

When the Academy put the problem of iteration out to competition, I constantly urged to you to write a memoir, and I did my utmost for it to be published in the Bulletin of the Math. Soc. in due time, filling several consecutive issues, contrary to custom.

When Humbert died in 22, I was the one who had the idea of your candidacy at the Collège de France and spoke of this to the mathematicians.

[Added in the margin] When it was possible to give you a lecture at the École [normale supérieure], I asked to be replaced by you so that no-one could accuse you of having never taught. [The part from "You cannot" to here was marked II in the margin by Montel, who decided to order the letter he sent differently.]

I always supported your candidacies and, for the last election, I sat on the right of Mauguin and did not conceal from him the vote with your name which I put in the ballot box.

Besides, it has never happened that I have promised somebody I would vote for him & not fulfilled my promise. And I believe I can claim this will never happen. [from "You understand" to here, Montel put a I in the margin.]

[added in the margin] You can understand how painful it is in these circumstances to see my loyalty suspected.

I don't know if your words have been distorted, it would then be essential to correct them. In any case, I refute in the most formal way any interpretation of my attitude which would not match what I have just written to you.

Very amicably yours
[signed] PM

Revised and condensed text.

Communiqué à M. E. Meyer
à toutes fins utiles
(à me rendre)

[53] Thus the mathematical scene already loved gossip! And especially rumours on resolutions that should remain confidential...

Mon cher ami

Je viens d'être avisé que, dans les milieux scientifiques, [s'est propagée, biffé] circule une version [mensongère, biffé] étrange relative à mon attitude à ton égard à la dernière élection de la Faculté d'après laquelle je n'aurais pas voté pour toi.

Je n'aurais point songé à la relever si cette version ne m'était présentée comme venant de toi, ce dont je demeure profondément [surpris, biffé] étonné & attristé.

Tu ne peux pas avoir oublié que je n'ai cessé de te soutenir et de t'aider dans la mesure de mes forces, au cours de ta carrière.

Lorsque l'Académie a mis au concours le problème de l'itération, je t'ai continuellement poussé à rédiger un mémoire, et j'ai fait l'impossible pour qu'il parût au Bulletin de la Soc. Math. en temps utile, remplissant contre l'usage plusieurs fascicules consécutifs.

À la mort de Humbert en 22, c'est moi qui ai eu l'idée de ta candidature au Collège de France et qui en ai parlé aux mathématiciens.

[Paragraphe en marge, ajouté] Lorsqu'il a été possible de te donner une conférence à l'École, j'ai demandé à me faire suppléer par toi afin qu'on ne t'accuse pas de n'avoir jamais enseigné.

J'ai toujours soutenu tes candidatures ultérieures et, en ce qui concerne la dernière élection, j'étais placé à droite de Mauguin et je n'ai pas caché à ses yeux le bulletin portant ton nom et que j'ai déposé dans l'urne.

Au reste, il ne m'est jamais arriver [d'affirmer, biffé] de promettre à quelqu'un que je voterai pour lui & de ne pas tenir ma promesse. Et je crois pouvoir affirmer que cela ne m'arrivera jamais.

[ajouté, dans la marge] Tu comprends combien il est pénible dans ces conditions de voir ma loyauté suspectée.

Je ne sais si tes paroles ont été travesties, [en ce cas, biffé] il serait alors indispensable de les rectifier. Dans tous les cas, je démens de la manière la plus formelle toute interprétation de mon attitude non conforme à ce que je viens de t'écrire.

Bien amical. à toi
[signé] PM

Texte remanié et condensé.

Monday, 28th [January 1929]

My dear friend

I am myself very surprised by your letter, no doubt having ever existed, either in my thoughts or in my words, about the way you voted with regard to me at the Faculty of sciences. I absolutely ignore the origin of this rumour, having not spoken of this[54] for a very long time. It has stopped interesting me, as I have no longer the least intention of looking for a teaching position; at the time I spoke of this, I mentioned Picard and Andoyer and nobody else. Your justification is thus entirely superfluous and I thank you once again for having supported me in various circumstances, a fact I have not forgotten.

Could somebody therefore imagine having an interest or taking some pleasure in arousing some disagreement between us? I do not see for the moment

[54] this... is probably the affair of Fatou's candidacy at the Sorbonne—when Garnier was preferred to him (see page 179).

who this could be, but I am naturally inclined to accuse the feminine element, since this seems to me worthy of the sex[55].

Yours sincerely

[signed] P. Fatou

Mon cher ami

Je suis moi-même fort surpris de ta lettre aucune hésitation n'ayant jamais existé dans mon esprit ni dans mes paroles sur les votes que tu as émis à mon sujet à la Faculté des Sciences. J'ignore absolument l'origine de ce bruit, n'ayant pas parlé depuis longtemps de cette affaire qui a cessé de m'intéresser, n'ayant plus la moindre intention de chercher une situation quelconque dans l'enseignement; à l'époque où j'en ai parlé, j'ai mis en cause Picard et Andoyer et personne d'autre. Ta justification est donc entièrement superflue et je te remercie à nouveau de m'avoir soutenu en diverses circonstances, ce dont je n'ai aucunement perdu le souvenir.

Il y aurait donc quelqu'un qui s'imagine avoir intérêt ou qui prend plaisir à susciter quelque dissentiment entre nous? Je ne vois pas pour l'instant de qui il s'agit, mais suis naturellement porté à accuser l'élément féminin, car cela me paraît digne de ce sexe.

Cordialement à toi

[signé] P. Fatou

[55] A very banal expression of common misogyny... nevertheless unexpected in this context: no woman was mentioned in any of the previous letters and it is hard to imagine the "feminine element" influence an activity as strictly masculine as a meeting of Professors.

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1. Lecture Notes aim to report new developments in all areas of mathematics and their applications - quickly, informally and at a high level. Mathematical texts analysing new developments in modelling and numerical simulation are welcome.

   Monograph manuscripts should be reasonably self-contained and rounded off. Thus they may, and often will, present not only results of the author but also related work by other people. They may be based on specialised lecture courses. Furthermore, the manuscripts should provide sufficient motivation, examples and applications. This clearly distinguishes Lecture Notes from journal articles or technical reports which normally are very concise. Articles intended for a journal but too long to be accepted by most journals, usually do not have this "lecture notes" character. For similar reasons it is unusual for doctoral theses to be accepted for the Lecture Notes series, though habilitation theses may be appropriate.

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   – a table of contents;
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   – a subject index: as a rule this is genuinely helpful for the reader.

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