Chapter 2
The Moscow Period with I. A. Kibel

On Christmas Eve 1957, I find myself in Moscow, walking the famous Gorki Avenue, with Jean-Pascal Sislian,[1] my very close friend from Yerevan University during the student years 1949 to 1954. The capital of the Soviet Union was full of people. It was -25 °C in Moscow and many *babushkas* are calling upon me for entering a store for warming up—walking bare-headed! The next day I buy a *chapka* of rabbit fur, now being a *true* Moscovite!

The Soviet Union, as elaborated by *Pravda*, would before long, be more prosperous than the United States. This period was one of hope especially for young people because research institutes needed to recruit scientists working for a *Kandidat* thesis. Mkhitarian wanted me to work on dynamical meteorology under the direction of I. A. Kibel in the Institute of Atmospheric Physics (IFA), soon to be converted into the Institute Obukhov, the name of its Director in 1957. A. M. Obukhov, well known for the famous *Kolmogorov-Obukhov law* in turbulence, was a top scientist and in 1957, a corresponding member of the USSR Academy of Sciences.

Obukhov was—from 1961 to 1966, in Moscow, and then up to the beginning of 1990, when I returned to Paris—always interested in my results in theoretical

[1] *Jean-Pascal Sislian* emigrated to Canada in 1972 and was engaged in various unsteady gas dynamic problems in shock tubes with Prof. I. I. Glass at the Institute for *Aerospace Studies of the University of Toronto (UTIAS)*. In 1976, he worked at the NASA Langley Research Center's Hypersonic Research Branch on scramjets as an NRC Washington, Research Associate. From 1978 to 1985 he was engaged in experimental investigations of high-speed turbulent combustion phenomena by optical laser diagnostic techniques. After his Master of Sciences in Mechanics at the State University of Yerevan (Armenia) he was awarded the degree of *Kandidat* of Physical and Mathematical Sciences from the Moscow Lomonosov State University (MGU) in 1967. As a professor at UTIAS since 1985 he was engaged in numerical simulation techniques in hypersonic detonation and shock-induced ramjets. Sislian, published with I. I. Glass, the book: "*Non-stationary Flows and Shock Waves*", in 1994. In the year 2000, by invitation of the editorial board of the AIAA, Sislian wrote the chapter on: "*Detonation Wave Ramjets*" in *Progress in Astronautics & Aeronautics* 189. Since 2005 he has been Professor Emeritus at UTIAS.

R.Kh. Zeytounian, *Five Decades of Tackling Models for Stiff Fluid Dynamics Problems*,
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hydrodynamics and thanks to him some of my results were published, in the Izvestiya of the Academy of Sciences of the USSR. My Moscow publication of 1966 [Z14]—where two stream functions ψ and χ are introduced and "two first integrals" are derived for the 3D steady Euler nonviscous equations—is what I consider to be *my first valuable* research paper in theoretical fluid dynamics.

These two first integrals have been applied in different contexts, for instance: flow modelling within a *row of an axial turbomachine* [Z5] and [Z15]; *lee waves above and downstream of a mountain* [Z16], and *shear flows*, with J. J. Lautard [Z17].

Concerning my *Kandidat* thesis, I had a second chance, namely working at MGU (Moscow Lomonosov State University), with L. N. Sretenski, the great specialist of the classical "theory of waves on the surface of the water", in the years 1950–1960 in Moscow.

But very soon this second choice, at MGU, fell through, although Sretenski himself seemed much interested to accept me as an *aspirant*; in the beginning of 1957 there was no place for housing for me at the MGU campus on "Mount Lenin", and I would have to wait until July 1957 to get a room there. In fact, from the beginning of 1957, I already lived, but more as a "parasite", in Sislian's room in Area B of the MGU campus, on the 14th floor. Thanks to Sislian, again, I had a temporary pass for 2 months, issued from the secretariat of Mechanics and Mathematics of MGU.

This pass gave me access to a central building of 31 floors of the MGU where the main scientific library was situated. My knowledge of the MGU gave me the possibility to have access also to different areas of the campus via passages and rooms in the basement! Very soon I decided to make a visit to the IFA—there I was fortunate to meet A. S. Sarkisyan, who in 1957 was the Scientific Secretary of the IFA and also Senior Research Associate in Kibel's Dynamic Meteorology Department.

Thanks to the assistance of Sarkisyan, I had my first opportunity, in February 1957, to meet I. A. Kibel in the House of Scientists of the USSR Academy of Sciences. But, before this encounter with Kibel, I would need to be admitted to Kibel's Department of the IFA as an *aspirant in thesis* of the Armenian Academy of Sciences after an oral examination in aero-hydrodynamics, which was organized at the end of January 1957 by Artiom Sarkisyan (actually member of the Russian Academy of Sciences) with Lev Gutman and A. M. Yaglom (who coauthored with Andrei Monin the famous book on "*Statistical Fluid Mechanics: Mechanics of Turbulence*" [1]) as examiners. Gutman, being a very good mathematician and familiar with the boundary layer theory, asked me about the *Prandtl concept*, while Yaglom asked me some questions about *turbulence*. Thanks to an ultimate revision during 10 days, of Sects. 28 and 32, in Part II of [2], I was able to pass successfully this oral examination.

During my encounter at the House of Scientists, Kibel (in February 1957) seemed to be interested in my personality and my scientific courses at Yerevan University. He asked me some questions about my wanderings from France to

Moscow and was puzzled by the fact that I had to leave high school in France at fifteenth, without “Baccalauréat”. But he fully appreciated the good evaluation (thanks to Sarkisyan’s information) expressed by Gutman and Yaglom, after my oral examination in aerohydrodynamics, and accepted to be my tutor for a *Kandidat* thesis in the Dynamic Meteorology Department at the IFA.

To the present date, I am surely proud to have been a student of Ilia Afanasevich Kibel, and my approach to theoretical fluid dynamics—with a white sheet, a pencil, and an eraser—has been mainly inspired by the 9 years (1957–1966) during which I worked under the supervision of Kibel in Moscow.

2.1 My First 3 Months in Moscow

Despite the fact that I had passed my oral exam in aero-hydrodynamics in Moscow, my situation in February 1957, was in reality uncertain—I was not an official *aspirant* of the Armenian Academy of Sciences yet … and I did not have my “propiska” (a stamp on the Soviet identification card allowing me to live in Moscow), not even a temporary one! I had no accommodation on the campus of the Academy of Sciences in Moscow! Thanks to Artiom Sarkisyan my life in Moscow became easier. Sarkisyan was a member of the Russian Academy of Sciences, specializing in ocean dynamics, working with Martchouk, and the author of several monographs on the numerical prediction of ocean currents and ocean circulation, and he was Vice-President of the International Association of the Physics of the Ocean for several years.

At that time (late 1950s), there were many young Armenian *aspirants* in mathematics and mechanics living in Moscow. In January 1957, I stayed with my friend Jean Sislian in his little room on the MGU campus; there was Henri Damatian—a Ph.D. student of Mergelyan, who graduated from Yerevan State University in 1952, 2 years before me—at whose room I stayed the largest part of February (I met Damatian again at the French University of Lille-I in 1972, but he, for various reasons, had not been able to pursue the type of career he had hoped for); Andreï Gontchar, whose mother was Armenian (!), and who was actually more ‘*Armenian*’ than myself, later became Vice President of the Russian Academy of Sciences for some years—he was also a brilliant doctoral student with Mergelyan in whose room I slept during his absence on the MGU campus for several days in March 1957. There was Samuel Grigorian, a brilliant doctoral student of Leonid Sedov, and who early in 2000 became a full member of the Russian Academy of Sciences in Moscow and successor to G. G. Tcherniy (one of the best-known pioneers of hypersonics, *high Mach numbers*, flow theory) as Director of the MGU Institute of Mechanics for some time, and who I met again in 1998 in Moscow. There was also Alik Bagdoev, who had been Senior Researcher at the Yerevan Institute of Mechanics of the Armenian Academy of Sciences; I again had the opportunity to meet Bagdoev in 1978 and 1989, during my two brief visits to

Yerevan. I particularly remember Guy Der Mgerditchian, a graduate of Yerevan State University in 1956, and an *aspirant* of the Academy of Sciences of Armenia, who defended his *Kandidat* thesis at the end of the 1960s in statistics (applied to weather forecasting, objective data analysis) at the new Guidro-Meteo Center of Moscow (opened in June 1961). Guy had married a young Moscovite woman, Lydia, in 1959, then returned to France, and became a successful research engineer at the French Météorologie Nationale in Paris from 1970 to the 1980s, but by fate, Guy died before his time in 1989. A conference room in the French Center of Weather Prediction in Toulouse (France) is named in his honor.

On 29 January 1957, Jean Sislian left for 2 weeks to Yerevan while I was staying in Henri Damatian's room—Area B of the MGU campus to the 14th floor. On the evening of Thursday, January 31, 1957, at 6 p.m., I am at the outdoor ice skating rink of the MGU, it is already night in Moscow, the temperature is −15 °C, and I have a severe fall when addressing a turn too fast, resulting in a big bump on my left temple. Back in the changing room, I faint and fall! The next morning I woke up at the hospital, with five others in a room, under the watchful eyes of the young and charming nurse Valya. At the hospital I spend 6 days of care and was visited by Sarkisyan. Now I had to find a place to live in Moscow until the beginning of April *1957*. Luckily I received, from Yerevan, my final admission in *aspirantura* of the Armenian Academy of Sciences and was able to get housing on the campus of the Academy of Sciences in Moscow, sharing a room with two other students. One of them being Gaguig (who also went by Guy) who had graduated from Yerevan State University in 1956,… strangely, I never met the third guy?

During February and March, I regularly attended the Kibel Seminar each Friday at the Central Forecasting Institute (TsIP). The institute was located near the new Guidro-Meteo Center. The TsIP was mainly concerned with so-called "ad hoc synoptical weather predictions" based on the most precise data from scrutinizing weather charts of the preceding days. In fact, from such (initial/starting) data, synoptic ad hoc meteorology, taking into account very simple relations (e.g., those called "geostrophic"), a relation between the gradient of the atmospheric pressure and wind speed (winds being parallel to the isobaric lines and determined by Coriolis force) can give a truly '*ad hoc*' weather forecast without involving a thorough theoretical basis!

These Kibel Seminars attracted scientists from various fields: meteorologists, hydrodynamicists, as well as numerically oriented and applied mathematicians, namingly: Martchouk, Semendiaev, Monin, Obukhov, Dorodnitsyn, Tcherniy, Tikhonov, Yaglom, Yanenko, and others.

In Moscow, scientific research at the end of the 1950s was at a high level and conducted by world-renown scientists and there were plenty of seminars and specialized courses attended by students from all over the Soviet Union. Among the many scientists that I was in contact with there, I especially remember N. N. Yanenko, and I would like to elaborate a bit more on some aspects of his personality here.

During the 1960s, *Yanenko* came up with what today is referred to as the "*Method of Fractional Steps*", a technique introduced and further developed in

France by Roger Temam (see, e.g., his book of 1984 [3]), and which, it seems me, is very similar to Gourii Martchouk's "*Desintegration/Decomposition Method*", used by the latter in Oblinsk during his research on computing nuclear reactors. Yanenko became Academician and, in the beginning of the 1960s, Director of the Institute of Mechanics in Akademgorodok (in Siberia, near to Novosibirsk). In 1968 (after returning to Paris in September 1966, under trying circumstances; see Sect. 2.6 below) I had a chance to discuss with Yanenko during an International Symposium held in Monterey, California, where I had presented a lecture on a "*Criterion for filtering parasite solutions in numerical computations*" which was noticed, especially by *Yih* who asked me to write a paper for *Physics of Fluids* (published in 1969 [Z18]). I had also the possibility to discuss with Nikolaï Nikolaevich in Tbilisi (Soviet Georgia), in 1978, during the VI International Conference on Numerical Methods In Fluid Dynamics, where I had presented a paper concerning the "*Application of Lagrangian invariants to the calculation of 3D rotational flow of a perfect fluid*" (published in LNP 90, 594–599). Again in 1980, at the BAIL I Conference in Dublin, I met Yanenko. Yanenko invited me to his institute in November 1982: it was −25 to 35 °C at Novosibirsk airport (!) when I am come off the plane from Moscow. During this visit of 1 month, I had the opportunity to deliver a series of lectures on "*Asymptotic Modelling of Fluid Flows*". Yanenko was warm, upright, and with a high ethical standard and spoke French well, as he had made frequent visits to France and had a warm friendship with Jean Leray; there, Yanenko had received a medal for his lectures at the College de France in Paris. I met him, unfortunately for the last time, in January 1984 in Paris. He came with me to our apartment which we were just moving into, at 12, Street Saint-Fiacre near the Grands Boulevards in Paris. To my dismal, I very soon should loose a good friend as a result of an injection "mistake"!

On March 22, 1957, Kibel, after his seminar, met me to give me a copy of a manuscript to be published in September of 1957 (later translated into English by Pergamon Press, New York, in 1963 [4] by the title: "*Introduction to the Hydrodynamical Methods of Short Period Weather Forecasting*") and he asked me to read the Chaps. 1 through 6.

During my first 3 months of 1957 in Moscow, I also had the opportunity to assist in various lectures (mainly at MGU) given by Sobolev, Sedov, Petrovski, Thikhonov, and Kibel—and, unfortunately, I am instructed to attend an (obligatory!) course on Marxism-Leninism—which really came at the wrong time, as I had the obligation to give my *aspirant* first "*minimum*" before the end of June 1957!

During the first three semesters (up to July 1958), I needed to get two *minima* in my a*spirentura*—one in Marxism-Leninism, considered *essential* for a future Soviet scientist (!), and the other concerning my scientific speciality as an *aspirant*, namely "General Dynamical Meteorology". In reality, I was not interested in communist philosophy . . . what a euphemism! Every week I had to write an abstract concerning parts extracted from various books of Marx and Lenin, of course mainly by Lenin (Stalin had died in 1953 and very soon was denounced by Nikita Khruschev, who played a prominent part in the "de-Stalinization" of the philosophic program) and also by Marx and Engels—an absolute idiocy and today I remember only that our professor of philosophy was a pretty nice young lady who was very "tolerant" with me during the exam! Regarding the Dynamic Meteorology part, which in fact is the application of classical (Newtonian) fluid dynamics to atmospheric motions—mainly a "*bastardized*" form of classical fluid mechanics as taught by fluid dynamicians (see, e.g., our books on theories and applications of nonviscous and viscous fluids of 2002 and 2004 [Z19, Z20])—this *minimum*, I successfully completed only in June of 1958.

Kibel established a high-level 'Dynamic Meteorology School' during the 1940s to 1960s. The Academician A. A. Dorodnitsyn was a postgraduate of the Head Geodesic Observatory doing his doctoral research under the supervision of Kibel; A. S. Monin, who joined the Moscow Academy of Sciences rather late and who worked with Kolmogorov and Obukhov on turbulence, in fact began his first research with Kibel, and in 1947 and 1948 published two papers, one on *wind slopes* and the other, with Gutman on *local winds over a mountainous country* (see these references in my Kandidat thesis [Z22], in Russian). The Academician Gourii I. Martchouk, got known at the beginning of the 1950s by coworking with Kibel's other postgraduate student, N. I. Buleyev, together obtaining an analytical solution of the system of equations for the baroclinic atmosphere: the famous "Buleyev-Martchouk" solution used as the basis for the first *numerical method of short-range weather forecasting* via the quasi-geostrophic model equation. Having become independent scientists, Martchouk and Buleyev decided to be transferred to the Institute of Physics and Energetics at Obninsk, a new powerful and fast developing institution situated not far from Moscow where there were good prospects for their future scientific careers. Kibel hardly wanted to part with such good collaborators; however, he understood that he could not hold them back. Moreover, they both gave a promise not to lose touch with Kibel's department and, in fact, retained close ties: they participated in the seminars held at the department and kept in touch with its collaborators. In 1956, Martchouk defended his doctoral thesis which was published as a book in 1958, the first (on the "*Theory of Nuclear Reactors*"; Russian edition by Atom Izdat in 1961) among his numerous scientific and popular books.

Despite this sudden departure of Martchouk, it was the good advise by Kibel that led academician Lavrentiev to invite Martchouk to become Director of the new Computing Center in Akademgorodok, some years later becoming President of the USSR Academy of Sciences in Moscow. Academician Artiom Sarkisyan was also a student of Kibel pursuing a *Kandidat* thesis.

An important event, in November 1957, was the publication in the scientific journal *Tellus* [5] of a paper by E. N. Blinova and I. A. Kibel: "*Hydrodynamical Methods for the Short—and Long-range Weather Forecasting in the USSR*", there they wrote: "*The development of the hydrodynamical methods for short-range weather forecasting was initiated more that 15 years ago, when the possibility was demonstrated of resolving the problem by means of the expansion in powers of the small parameter that multiplies time derivatives in the system of equations of motion*"—this parameter, let us denote it by ε, is defined as:

$$\varepsilon = 1/l_c t_0, \qquad (2.1)$$

where l_c is the Coriolis parameter and t_0 is the *characteristic time* of the problem (of the order of 24 h). Indeed, Kibel, in 1940, had succeeded in removing the principal defects of Richardson: Kibel's approach did not provide a means to separate the atmospheric weather-producing mechanisms from processes of all other scales, which are described by the equations—through exclusion from the equations the terms which are related to the temporal and spatial scales which are much smaller than those of the atmospheric synoptical-scale processes—about 1,000 km and 25 h, respectively. Reducing the dynamical prediction problem to modelling adiabatic processes in the inviscid fluid, Kibel obtained simple formulas suitable for 24-h predictions of the surface pressure and temperature with reasonable accuracy in operational environment through performing *computation by hand*. The developed technique was used for daily routine weather forecasting until the early 1950s.

Coming back to my personal life, the good news, on March 16, 1957, from Yerevan, is my final admission in *aspirantura* to the Academy of Sciences of Armenia and at the end of March I received from Yerevan my first scholarship with an extra bonus for purchasing books. But in March 1957, I finally would need to find housing on the MGU campus after squatting on the 14th floor, every night in a different vacant room. I worked in the math library and often I spent 1 or 2 h at the MGU sports club playing table tennis. Of course, I had a few not so serious "friendly adventures", such as with the nurse Valya and fellow students of MGU: Natacha (very capricious), Galia (sportive), and Nina (a young very sympathetic Armenian girl). I remember especially, Nina, who was a student in her first year of literature at MGU, and 10 years younger than me (she observed maliciously that this was precisely the right match for her parents!) and during the first 3 months of 1957 at MGU, I had many discussions and pleasant encounters with Nina, which significantly improved my spoken Russian.

2.2 Love Story, Philo, and Meteo

On April 6, 1957, ending my rather unstable housing situation, I had the possibility to share a room (with two others) on the Moscow Academy of Sciences campus, and was furnished with a 3 years temporary *propiska* for Moscow. I received a scholarship, and by means of doing some Russian-French translations, I was able to live more or less satisfactorily, by Moscovian standards, and many friends coming up to visit me. But mostly, I concentrated my efforts on my ultimate goal. Ten years had elapsed since my first arrival in Yerevan, the balance sheet was rather satisfactory and I was on the right track. Obviously for me the main thing was to succeed during my 3 years in *aspirentura* with Kibel! But, just at that moment, by destiny I was turned away for 4 months!

Indeed, the period from 13 April 1957 to August 1957 would be the most "animed" and change completely may future life in Moscow. It is precisely a Saturday that this love story began, during a dance party, on the Moscow campus of the Academy of Sciences, with a young student of the Fine Arts Institute, Natalia—love at first sight? Very soon, with Natalia, it became a friendly and good understanding—Natalia spoke, . . .bewitched me, and I listen enthralled. Together we visited almost all the museums of Moscow, we often went out to theaters and cinemas and saw various American films of 1930s to 1940s (brought back from Berlin by the Red Army). During May (Moscovian summer having started) and June (despite my examination in philosophy!) we found time for getaways around Moscow—Barvikha, Odintsovo, Peredelkina, Abramtsovo, . . . and next, to establish between ourselves a love relationship by early August 1957. After the summer holidays—Natalia, from the Smolensk region, where she had lived with an aunt who was a fellow of the Fine Arts Institute, and myself from Yerevan—this love story led on September 27, 1957 to our wedding in Moscow, with Lyda and Guy as witnesses!

My private life having caused a slight backdrop on my studies the philosophy exam was now scheduled 18 June 1957 at 10 a.m. With Natalia being away from Moscow for 2 weeks at the beginning of June, for a landscape painter stage in Istria (a typical provincial town, known for its famous monastery of "New Jerusalem", partially destroyed by the German Army during the years 1941) I got the opportunity to catch up and obtained a satisfactory mark in the philosophy exam, and was able to retain my scholarship.

The result of my experience with Soviet philosophy is a rather strong reticence firmly anchored into my mind and is persistent even when I am writing now. Philosophical discussion is quite popular in France, but I was not really interested in philosophy, except for a little dialectics and some aspects of logic, but I was certainly not into Marx and Engels, even less Lenin. . . not to mention Stalin!

In Moscow, despite the strenuous living and housing conditions especially from 1958 to 1966, I was happy with Natalia. Then our daughter Christine was born in June 1960! .. what an eventful Soviet period! Only, at the end of 1959 did we obtain from the city council an order for one single room in a four-room apartment with Soviet (!) comfort. Mainly thanks to Natalia's mother, Raissia Lvovna, a truly efficient aid, did I obtain my permanent *propiska* for Moscow in 1958.

Concerning the second *minima in 1958*, I mainly had to read the book by L. C. Gandin, D. L. Laikhtman, L. T. Matveev, and M. I. Yudin, 1955, in Russian [6] (647 pages!), and I passed the examination with ease in June 1958; but my answers to questions linked with the physics of the atmosphere were not always satisfactory and so I only received 4 out of 5 points which was somewhat disappointing for Kibel; and Kibel seemed to be not quite happy with the fact that I had married. I referred back to the rather uninteresting course in physics at Yerevan State University as an exculpation for the unsatisfactory grade concerning my examination of the physics of the atmosphere. To start myself on the good way I intended to use Chap. 1 within his manuscript (in fact, his book [4]), in order to deepen the problem of ground conditions regarding the problem of *local* *atmospheric convection/circulation*—the subject of my thesis. He seemed to be interested and encouraged me to work in that direction. This led (in December 1959) to my first publication (at the age of 31!), involving some slight modifications by Kibel, entitled: "*On Theory of Convection at Small Scale*", published in Doklady of the Armenian Academy of Sciences [Z21]. For ground condition $z = 0$ in a boundary layer formulation, (as in my thesis, published in 1964 [Z22]), I write:

$$\lambda^* \partial\theta^*/\partial z - \lambda[\partial\theta/\partial z + (L/Cp)\partial\theta/\partial z] + a\theta^* = (1 - \Gamma)\, r(t), \qquad (2.2)$$

where $\theta^* \equiv \theta$ and $q \equiv bs\theta$ (the perturbation of the *humidity*), at ground condition $z = 0$, where θ is the perturbation of the air temperature, Γ is the albedo and r(t) an external radiation, λ^*, λ, L, Cp, a, and b being various physical constants. In particular the function $s = 1$ in a water surface.

Coming back to my work for the second *minima*, I read, especially, Chap. 15, §4 of Gandin et al. [6] on: "*Local Atmospheric Circulation*", a material which was very useful when working on my thesis from 1958 to 1960. Concerning that book, I understand that the basic fluid dynamics is simply adapted, with ad hoc arguments rather than as a result of logical discussion from the classical one. Such logic was provided by the asymptotic approach, initiated by Kaplun, Lagerstrom, and Cole at the beginning of the 1950s, and the book by Van Dyke of 1964 ([6] of Introduction) that was mainly ignored by meteorologists during the 1950s to 1970s. A *main key step*, in asymptotic modelling theory, is the formulation, from the very start, of a physically realistic mathematical consistent problem written in a *dimensionless form*—this first step being not a trivial one, if we want to take into account the main physical atmospheric effects. For this, in a spherical coordinate frame rotating with the Earth, it is necessary to consider the full Navier-Stokes-Fourier (NSF) atmospheric equations, for a thermally perfect gas, which is a viscous, compressible, and heat-conducting dry atmospheric air—taking into account the gravity and Coriolis forces—considered as a Newtonian fluid (in Sect. 4.3.3, the reader can find these NSF equations). Such an asymptotic modelling—the '*RAM Approach*'—is performed in our 2004 paper [Z23], in honor of the 100th birthday of I. A. Kibel, and the NSF equations are derived in detailed form in Chap. II of our 1991 LNP m5 [Z24] devoted to "*Meteorological Fluid Dynamics*".

Concerning the Kibel manuscript, I remember to the present day, how it was difficult for me to understand within his Chap. 4 (see [4]) the introduction of two times, simultaneously: a first short time concerning the problem of unsteady adaptation/adjustment to geostrophism from an initial given meteo situation, while the second, a long time was the one actually appropriate to weather prediction/forecasting via the quasi-geostrophic model equation.

It was, in fact, my first contact with *asymptotic modelling* or rather one aspect—the so-called *Matched Asymptotic Expansions* (MAE)—of that way of thinking. I venture to call it an *internal logic* when trying to translate physical phenomena by some mathematical construction. The *quasi-geostrophic equation* consists in a partial differential equation which is devoted to describe general trends of motions in the atmosphere, neglecting terms which are related to special events which are considered as not significant (at least for short-range weather forecasting). Such a mathematical construction, let us call it a model, has an internal consistency and describes future evolution of a phenomenon, here weather prediction, conditioned by what we know of its state at an initial time via the given data. What was understood by Kibel is that: *Acoustic propagation is not included (is filtered!) into the quasi-geostrophic basic equation*. But Kibel knew very well that the initial data are obtained by readings at a given time of the meteo situation and that those contain what will condition acoustic propagation which is not included into geostrophism. The idea of Kibel was to solve such a contradiction by introducing a *small time* in order to evacuate or filtering out—via *unsteady adaptation to geostrophism*—such parts of the initial data.

If, weather prediction concerns several days, on the contrary acoustic propagation goes through the whole thickness of the atmosphere in less than 1 min—it seems clear that the weather prediction is a *Cauchy initial value problem related with a "double time phenomenon"*. It took me quite a long time before I had clearly understood that point about which I spoke within my 1975 General Lecture "*La Mé téorologie du point de vue du mécanicien des fluides*" (and published in 1976 [Z2]). It seems that, in Moscow at that time, only *Andrei Monin*, with his remarkable small book published in Russian in 1959 and translated and published as "*Weather Forecasting as a Problem in Physics*" by MIT Press, Cambridge in 1972 [7], was the first to draw attention on an asymptotic approach to the problem in the Soviet Union! We shall come back to this later (see, for instance, the *Retrospective Summary*, but also the various sections of Chap. 4) on this point as one aspect of what I call *Rational Asymptotic Modeling*, in short the '*RAM Approach*'.

2.3 My Kandidat: Physics-Mathematics Science Thesis

The beginning of my work on the thesis is somewhat disturbed not only as a consequence of my new life with Natalia, but mainly owing to the fact that the Kibel Department (in 1958) is transferred to the Institute of Applied Geophysics, subject to defense secret! A pass which would provide me access to this facility was flatly denied—born in France and being from a capitalist country... all arguments that had allowed me to escape military instruction at Yerevan State University! As a result, up to June 1961, I seldom met Kibel within the entrance hall on ground floor and there was no real chance to have longer conversations him regarding my research. The transfer of the Kibel Department, mainly seemed to be due to the tense situation between Blinova and Obukhov, Director of the IFA. My unsubdued character stirred me to write a letter to the academician Victor Ambartsoumian, President of the Armenian Academy of Sciences in Yerevan. My main argument was that without a pass, it would almost be impossible to consult Kibel about my difficult work regarding my *Kandidat* thesis. Academician I. K. Fedorov, Director of the Institute of Applied Geophysics, was very upset about my inquiry and threatened to instruct my return back to Yerevan. Arto Mkhitarian asked me to recover my equanimity, to compose myself, and wait for the installation of the new "Meteorological Computing Center" in Moscow (today the "Hydrometeorological Research Center of Russia"). This little scandal ended rather leniently, because I got a 6 month extension of my *aspirentura* and scholarship! During this time I worked very diligently on my thesis and this was positively perceived by Kibel, who thought I showed aptness for a promising scientific career. At the end of April 1960, after 3 years as an *aspirant* I had sufficient results to submit to Kibel, and he was favorable for starting to prepare my *Kandidat* (a Russian Ph.D.) thesis, and he wrote *on a sheet of paper, by hand, with a pencil and an eraser*, a short positive critical appraisal of my work (I still keep, after 53 years, this handwritten hardly readable sheet of paper!).

On September 19, 1960, I presented the main results of my thesis at the Kibel Seminar at the Dynamical Meteorology Department of the Institute of Applied Geophysics; in the protocol of this seminar (Sept. 21, 1960) it is mentioned that the two "*Contradictors*" during my *Kandidat* defense would be A. Kh. Khrguiyan (who was well known by his famous book: *Physics of Atmosphere* [24]) and E. M. Dobrichman. After 4 months of intense working I had, with the aid of Natalia concerning the Russian, a handwritten Memoir (as a Proceedings edited by GuidrometeoIzdat, Leningrad, in 1964 [Z22]) in an "almost" perfect Russian language, which is ready at the end of February 1961. Our daughter, Christine, born on 9 June 1960, was often on my knees looking at my hand flying over the sheets of paper during the handwriting of my thesis—maybe these early visual 'sessions' were a motivation for her later choice for becoming a literary scholar! The title of my thesis was: "*On Nonlinear Theory of Unsteady Local Atmospheric Circulations*" the public defense taking place on 29 March 1961, at 3 p.m., at the Faculty of Physics of Lomonosov Moscow University (MGU)—the newspaper "*Moscow Evening*" of 20 March 1961, in the column '*Defense of Thesis*' on the last page published a short note of 13 lines concerning my public defense. In the first paper, resulting from my *Kandidat* thesis [Z21], the temporal evolution, from rest, of a local wind (*mountain breeze*) along a mountain slope is elaborated—the influence of the slope and the thermal convection generated by the thermal nonhomogeneities on the slope ground being coupled. A second paper [Z25], relative to my *Kandidat* thesis (with some numerical results), was published in the USSR Doklady of 1960 by recommendation of Leonid Sedov.

2.3.1 A Short Account of My Kandidat Thesis

First the slope ground temperature was assumed to be known as a power series expansion of the square root of time t, $\tau = \sqrt{t}$, and in that case the free air being at rest, the unsteady motion in the atmospheric boundary layer starts from the state of rest.

The analytical solutions can be obtained, valid at any rate, for small time intervals—an excellent idea derived from Dorodnitsyn in the 1950s (see, e.g., pp. 530–534 in the book of *Gandin* et al. [6]—which is also determined in a series of τ. As regards the choice of the method to be applied, it should be pointed out that Oleinik [8] succeeded in substantiating its merit by furnishing conclusive proof that the series so constructed are *true* representations of the solution and the incident *error* is of the order of magnitude of the *neglected* term. Obviously enough, it would be futile to look to the method for what lies beyond its scope. For instance, for the perturbation of the temperature θ, horizontal component ϕ, and vertical component σ of the velocity vector, we derive in the particular simple case, from the starting boundary layer problem, the following set of coupled equations for the terms in the power series of τ: θ_0, θ_6, ϕ_0, ϕ_1, ϕ_6, and σ_0:

$$\begin{gathered}
\partial^2\theta_0/\partial s^2 + 2s\partial\theta_0/\partial s - 4\theta_0 = 0; \theta_0\big|_{s=0} = f_{0\,0}(\xi),\\
\partial^2\theta_6/\partial s^2 + 2s\partial\theta_6/\partial s - 16\theta_6 = 4[\phi_0\partial\theta_0/\partial\xi + \sigma_0\partial\theta_0/\partial s],\\
\partial^2\phi_0/\partial s^2 + 2s\partial\phi_0/\partial s - 8\,\phi_0 = -4X\,\theta_6; \phi_0\big|_{s=0} = 0,\\
\partial^2\phi_1/\partial s^2 + 2s\partial\phi_1/\partial s - 10\phi_1 = 8A_0\partial/\partial\xi\left[Z\int_{\infty}^{s}\theta_0 ds\right] \qquad (2.3)\\
\partial^2\phi_6/\partial s^2 + 2s\partial\phi_6/\partial s - 20\,\phi_6 = -4X\,\theta_6 + 4[\phi_0\;\;\partial\phi_0/\partial\xi + \sigma_0\partial\phi_0/\partial s],\\
\partial\sigma_n/\partial s = -\partial\phi_n/\partial\xi, n = 0, 1, 6, s = \infty : \theta_0 = \phi_0 = 0.
\end{gathered}$$

In the above system of Eqs. (2.3) the vertical coordinate is: $s = \zeta/2\sqrt{\tau}$, where ζ is the vertical coordinate directed along the outer normal to the slope and the curvilinear coordinate ξ is the abscissa along the slope surface ($\zeta = s = 0$). The coefficients A_0, X, and Z are dimensionless scalars that are linked with the geometry of the mountain slope, and the given function $f_{00}(\xi)$ simulates the temperature distribution (for θ) along the slope.

In our book of 2009 [Z26], pp. 320 and 321, devoted to "*Convection in Fluids*", various figures show the qualitative evolution of a local wind along the mountain slope, under the double action of the slope and the thermal field at the slope (see Fig. 2.1, taken from page 321 of [Z26]). A detailed discussion of this qualitative evolution with various comments was given in Sect. 2.2, pages 39–44, of my Proceedings N°3 [Z22].

This first experience in research during the years 1958–1960, and especially my very favorable relation with Kibel, strengthened my determination to further work in theoretical research applied to fluid dynamics and confirmed my interest in physical problems putting aside abstract mathematics such as nonlinear functional analysis.

During my *Kandidat* thesis defense, Khrguiyan, as a compatriot (!), was favorably disposed to me … and Dobrichman gives a positive judgment on the results obtained in my thesis (Dobrichman, with Chakina, had been working on a paper concerning the *local winds in a turbulent atmosphere* and published in 1962 [9]). Despite these above two facts, it is difficult for me today to objectively estimate my work for the *Kandidat* thesis!

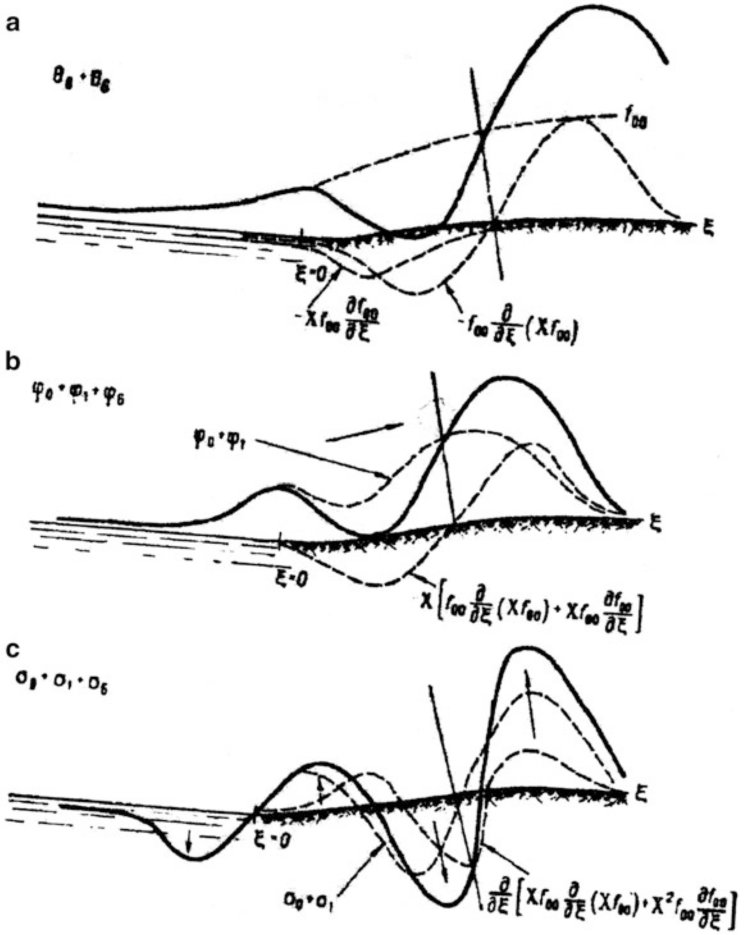

Fig. 2.1 Global effect of the mountain slope wind (reprinted with kind permission from [Z26])

What is sure now, is that at the time I had worked hard (and practically without any help from, Kibel!) in order to obtain a rather general formulation of the problem of local winds, within the boundary layer approximation, above a site subjected to heat fluxes coming both from the wind as well as from the ground. From the mathematical point of view, according to the above Eq. (2.3) system of differential equations, we see that it is necessary to resolve, at least for the terms in Eq. (2.3), the following *nonhomogeneous* differential equation:

$$\partial^2 \mathrm{Y}/\partial \mathrm{s}^2 + 2\mathrm{s}\partial \mathrm{Y}/\partial \mathrm{s} - 2\mathrm{m}\mathrm{Y} = \Sigma_{\mathrm{i,j}}\, \mathrm{B}_{\mathrm{i,j}}\, L_i L_j \qquad (2.4\mathrm{a})$$

with the two conditions

$$s = 0: \quad Y = 1 \text{ and } s \to \infty: Y = 0. \tag{2.4b}$$

In Eq. (2.4a) the functions L_i *and* L_j are dependent of the vertical coordinate s, and both are solutions of the associated *homogeneous* equation:

$$\partial^2 Y/\partial s^2 + 2s\partial Y/\partial s - 2kY = 0, k = i \text{ and } j \tag{2.5a}$$

such that,

$$s = 0: \quad Y = 1 \text{ and } s \to \infty: Y = 0. \tag{2.5b}$$

The coefficients $B_{i,j}$ in (2.4a) are independent of s and the following relation

$$m - (i + j) = 2r, \tag{2.6}$$

is always satisfied. In particular, if $r = 1$, it is not *very* difficult to show that the solution of Eq. (2.4a), with the two conditions (2.4b), is:

$$Y = L_m + \Sigma_{i,j} \; \left[B_{i,j}/2\right] \; \Lambda_{i,j}\left\{L_{i+1}L_{j+1} - L_m\right\}, \tag{2.7a}$$

where

$$\Lambda_{i,j} = A_i \; A_j/A_{i+1}A_{j+1}. \tag{2.7b}$$

In Eq. (2.7b) the coefficient A_k is linked with the function L_k (s) such that:

$$L_k \; (s) = (A_k/k!) \; \int_{\infty}^{s} (s - s') \; \exp[-s'2]ds', \tag{2.7c}$$

$$\text{with } A_0 = -2/\sqrt{\pi}, A_1 = 1, A_m = 2m \; A_{m-2}. \tag{2.7d}$$

The case $r > 1$ is considered on pp. 69–70 in Proceedings N°3, 1964 [Z22].

In LNP 672 of 2006 [Z8], section 6.3, the reader can find a RAM Approach for the derivation of a consistent mathematical model for a wind breeze (a free-circulation problem on a flat ground surface)—starting from the full unsteady, non-adiabatic and viscous, compressible equations, taking into account the Coriolis force, the gravitation acceleration (modified by the centrifugal force), and the effect of thermal radiation.

2.4 My Approach to Lee Wave Motion Above and Downstream of a Mountain

For an Eulerian (non-viscous) steady compressible fluid flow as an exact system of three equations for **u** (the velocity vector), ρ (the density), and S (the specific entropy) we have [Z19]:

$$\begin{aligned} &\nabla.(\rho\mathbf{u}) = 0, \\ &\mathbf{u} \wedge (\nabla \wedge \mathbf{u}) = \nabla \mathrm{H} - T\nabla \mathrm{S}, \\ &\mathbf{u}.\nabla \mathrm{S} = 0, \end{aligned} \tag{2.8a-c}$$

where

$$\mathrm{H} = h + (1/2)\mathbf{u}^2 + U \text{ and } \mathbf{u}.\nabla \mathrm{H} = 0, \tag{2.9a,b}$$

where H is the *total enthalpy* when we assume that the gravity force is conservative, and h is a known function of the density ρ, and the temperature T is a known function of ρ and S. As a consequence of Eqs. (2.8c) and (2.9b), both surfaces, S = const. (*for entropic* surface) and H = *const* (*Lamb* surface) are stream surfaces for the Eulerian 3D steady compressible fluid flow, such that, in a general case, we write, for two stream surfaces ψ and χ of our Eulerian fluid flows:

$$\mathbf{u}.\nabla\chi = 0 \text{ and } \mathbf{u}.\nabla\psi = 0. \tag{2.10}$$

2.4.1 2D Steady Nonlinear Case

Already in September 1960, before the defense of my *Kandidat* thesis, Kibel incited me to study the 1953 and 1955 papers of R. R. Long concerning "*Some aspects of the flow of stratified fluids*" [10, 11], published in *Tellus*. Without doubt it was Long who was the first to make a serious study of nonlinear effects associated to the exact slip boundary condition on mountains in 2D steady problem and undertook the investigation, by an analytical method, of the effects of lee waves with a finite amplitude downstream of the mountain. Unfortunately, Long's investigations could not solve the fundamental direct nonlinear problem which arose for a given a priori mountain for which the flow corresponds to a given value of the Scorer-Dorodnitsyn parameter:

$$\mathrm{K}^2{}_0 = (S_0/\gamma)[\mathrm{Bo}/\mathrm{M}]^2, \tag{2.11}$$

where S_0 is the "hydrostatic stability parameter" (defined in [Z8], p. 260), M the *Mach* number—the ratio of a constant, far upstream, wind speed U_∞, to a constant *sound speed* corresponding to a constant standard *hydrostatic* temperature $T_s(0)$ *for a flat* ($z = 0$) *ground.*

In Eq. (2.11), the parameter Bo (used first in [Z1]) is the Boussinesq number—the ratio of the characteristic (constant) vertical length scale H_c of the lee wave motion to the standard height,

$$H_s = RT_s(0)/g \tag{2.12a}$$

of an *homogeneous hydrostatic* atmosphere. Bo is explicitly defined by:

$$\mathrm{Bo} = g\, H_c/RT_s(0). \tag{2.12b}$$

Namely, the method of Long does not resolve the direct lee waves problem—for an a priori given form of the mountain, for which the lee wave motion is calculated!

With some approximations (see [Z8]), when it is assumed that:

$$\mathrm{K}^2{}_0 = \mathrm{O}(1), \text{such that } \mathrm{Bo}/\mathrm{M} = B* = \mathrm{O}(1), \tag{2.13}$$

both M and Bo *tend to zero,* then for B^* and S_0 fixed, the 2D steady wave motion, with dimensionless streamlines in the x, z plane, described by a single stream function ψ(x, z), is the solution of a classical (linear!) Helmholtz equation:

$$\partial^2\psi/\partial x^2 + \partial^2\psi/\partial z^2 + \mathrm{K}^2{}_0[\psi - z] = 0, \tag{2.14a}$$

with the following, *exact*, slip condition:

$$\psi(x, \mu_0 h(x)) = 0, \quad \mu_0 = h_0/H_s. \tag{2.14b}$$

with $h_0 = h(0)$ and $z = \mu_0 h(x)$ being the dimensionless equation of the mountain.

We observe that, according to the above Scorer–Dorodnitsyn parameter (2.11), the Helmholtz equation (2.14a), and the slip boundary condition on the wall of the mountain (2.14b), are both written in dimensionless form. Indeed, Long's imaginative approach was to solve *indirectly* the above problem (2.14a, 2.14b), corresponding to a wave motion in a plane channel,

$$0 \le z \le H_s \ \text{ with a constant velocity } \ U_\infty, \tag{2.14c}$$

far upstream, $x \to -\infty$, using Fourier series for ψ as function of z after a linearization of slip condition (2.14b) assuming that $\mu_0 << 1$. In such a case, the *dimensionless problem* (2.14a, 2.14c) with, as an upper boundary condition:

$$\psi(x, z = 1) = 0, \tag{2.14d}$$

is a linear problem. The resulting analytical solution, bounded for $x \to +\infty$, of this linear problem shows that we have: "on the one hand, far upstream $x \to -\infty$, a constant flow parallel to the x axis, and, on the other hand, exhibits the so-called steady lee waves far downstream of the mountain up to $x \to +\infty$."

Unfortunately, in Long's paper [10], for various (a priori unknown!) values of $K^2{}_0$, the lee wave configurations are derived for a "*fictitious mountain*"—appreciably *different from* the shape of the *given mountain*, described, a priori, by $z = h_0 h(x/H_s)$—namely, the one such that for

$$z = g(x) \text{ we have the condition : } \quad \psi(x, g(x)) = 0, \tag{2.15a}$$

and also

$$g(x) \equiv 0, \text{respectively, for } x \leq L_1 \text{ and } x \geq L_2; L_1 < L_2. \tag{2.15b}$$

During the years 1961–1965, in the new Moscow Meteorological Computing Center, I worked on the steady 2D lee waves problem and approached the full nonlinear problem directly, with the exact slip condition (2.14b), where $\mu_0 = O(1)$. In such a case the problem is *considerably complicated* because the necessity to solve, in place of a simple linear Helmholtz equation (2.14a), the following very stiff partial derivatives equation:

$$\begin{aligned}
&\partial/\partial\eta\Big\{\Big[\Big[1 + (1-\eta)^2(d\Delta/dx)^2\Big]/(1-\Delta)\Big]\partial\Psi/\partial\eta + (\eta - 1)\big(d\Delta/dx\big)\partial\Psi/\partial x\Big\} \\
&\quad + \partial/\partial x\big\{(1-\Delta)\partial\Psi/\partial x + (\eta-1)\big(d\Delta/dx\big)\partial\Psi/\partial\eta\big\} \\
&\quad = (1-\Delta)\, K^2{}_0\big[\Psi + \eta + (1-\eta)\Delta\big],
\end{aligned} \tag{2.16a}$$

for $\Psi(x, \eta)$, when in place of z we have introduced a new vertical coordinate,

$$\eta = [1/(1-\Delta)](z - \Delta), \text{with } \ \Delta(x) \equiv \mu_0 h(x). \tag{2.16b}$$

As an appreciable advantage of the above change (2.16b), in the resolution of the above, strongly nonlinear, Eq. (2.16a), as conditions relative to the new vertical coordinate η, is the possibility two assume, for Eq.(2.16a), the following two homogeneous conditions:

$$\Psi(x, 0) = 0 \text{ and } \ \Psi(x, 1) = 1. \tag{2.16c}$$

To resolve Eqs. (2.16a–2.16c), a nonlinear problem, I used the method of "*integral relations*" of Dorodnitsyn [12], within the interval $\eta \in [0, 1]$. As a result, we derive a single equation for the functions: $\Psi(x, \eta_k) = \Psi_k(x)$, where $\eta_k = k/n$, with $k = 0, 1, 2,\ldots n$. A long... but rather easy calculation gives for the functions $\Psi_k(x)$, with $k = 1, 2,\ldots n - 1$, the following system of ordinary differential equations [Z27]:

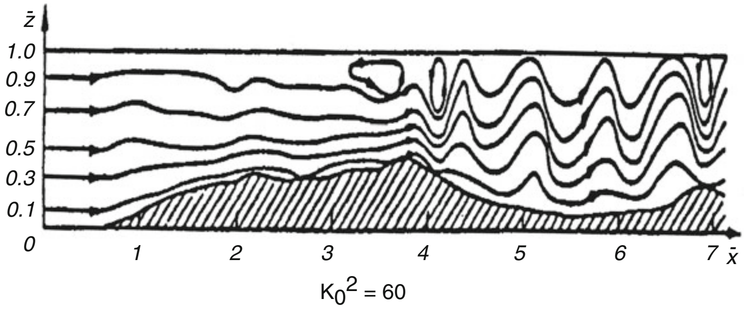

Fig. 2.2 Calculation of lee waves above the Sierra Nevada (reprinted with kind permission from [Z19])

$$
\begin{aligned}
&d^2\Psi_k/dx^2 - \{2[1/(1-\Delta)]d\Delta/dx\}d\Psi_k/dx \\
&- \left\{[1/(1-\Delta)]d^2\Delta/dx^2\right\}\Psi_k - K^2{}_0[-\Psi_k + k/n + (1-(k/n))\Delta] \\
&+ \Sigma_{s=1\,to\,n-1}\left\{\alpha_{s,n,k}\, d\Psi_s/dx + \beta_{s,n,k}\Psi_s\right\} = \Lambda_{n,k},
\end{aligned} \tag{2.17a}
$$

where the coefficients: $\alpha_{s,n,k}$, $\beta_{s,n,k}$, and $\Lambda_{n,k}$ are known when the shape h(x) of the mountain and parameter μ_0 are both given. Figure 2.2 gives a calculation of lee waves above the Sierra Nevada, according to our above-described model system of Eqs. (2.17a) [Z28].

The above ordinary differential equation (2.17a) must be solved numerically, the most serious difficulty being that in the numerical calculation it is "*practically impossible*" to introduce the boundary conditions, associated with Eq. (2.17a), at upstream and downstream infinity relative to x:

$$
\Psi_k = k/n \ \text{ at } \ x \to -\infty, \left|\Psi_k\right| \ = O(1) \ \text{ when } \ x \to +\infty! \tag{2.17b}
$$

In my first paper [Z27], devoted to this lee wave problem, the reader can find a (rather naïve!) method for filtering the *parasite solutions* which are introduced into numerical solutions when, in place of the above asymptotic conditions (2.17b) we consider either the Cauchy conditions far upstream ($x \to -\infty$) or two conditions in two points situated—sufficiently far at the mountain—respectively, on the right and on the left of the given a priori mountain.

In our paper of 1969 [Z18] the reader can find some valuable information concerning this filtering process. In fact, it seems that: "*When the lee waves motion is established downstream of a mountain, then in a such case the motion must be disturbed very little at a sufficient distance at upstream of this mountain*"!

Associated with this difficulty mentioned just before in Eq. (2.17b) one observes that: "a little perturbation of the flow far upstream from the mountain may perturb significantly the lee waves motion downstream."

Concerning our publications for the 2D steady nonlinear lee waves problem, see for instance: [Z28] and [Z29]. In our LNP m5 [Z24] the reader can find (there in Chap. VI) some information concerning the method of "integral relations" of

Dorodnitsyn [12] and, in particular, some formula for the calculation of various coefficients in this Dorodnitsyn method. Curiously, even a simple model with *only one intermediary level* gives very satisfactory results (for instance, relative to exact "fictitious" Long solutions)! Our computation shows the importance *of the curvature of the mountain upstream*, on the formation of the regime of rotors downstream of the mountain. As a conclusion we notice that: The method of Long is not, in fact, capable to obtain the lee waves motion in the downstream of an a priori given mountain in the nonlinear case when the exact slip condition is imposed on the wall of this mountain. On the contrary, our approach allows to obtain the waves in the lee of the mountain for a given mountain by taking into account the associated slip condition.

During 1965, in Moscow, our approach with *Pekelis* was to predict the regime of lee waves downstream the Crimean mountains, which are formed on the coast of the Black Sea and which would provide precious information for helicopters flying between Simféropol and the famous Yalta seaside resort!

2.4.2 3D Steady Linear Case

Concerning the steady 3D lee waves, in 1963–1965 we considered in Moscow a linear theory and in 1965 [Z30] I derived a *single equation* (rather *awkwardly*) for *vertical perturbation of the wind* W(x, y, z), taking into account a basic wind $\mathbf{V}_\infty(z)$, at infinity upstream with a 2D speed function of the vertical coordinate (altitude) z:

$$\mathbf{V}_\infty(z) = U_\infty(z)\,\mathbf{i} + V_\infty(z)\mathbf{j} \ \text{ and } \ \mathbf{V}_\infty(z).\mathbf{k} = 0. \tag{2.18}$$

This equation is written in the following dimensionless form:

$$\begin{aligned}&(U_\infty\partial/\partial x + V_\infty\partial/\partial y)^2\left[\partial^2 W/\partial x^2 + \partial^2 W/\partial y^2 + \partial^2 W/\partial z^2\right]\\&+D^2\left[\partial^2 W/\partial x^2 + \partial^2 W/\partial y^2\right]\\&-\ (U_\infty\partial/\partial x + V_\infty\partial/\partial y)\left[\left(d^2U_\infty/dz^2\right)\partial W/\partial x + \left(d^2V_\infty/dz^2\right)\partial W/\partial y\right] = O(\lambda),\end{aligned} \tag{2.19}$$

when we assume that the 3D steady wave motion is a divergenceless velocity vector ($div\mathbf{V}_\infty = 0$), but density, ρ, is not constant such that, the condition: $\mathbf{V}_\infty$. grad ρ = 0 is satisfied. In the particular case when: $U_\infty(z)$ is constant ($\equiv U_\infty(0)$) and $V_\infty(z) \equiv 0$ we recover the case considered by Kibel in 1955 [13], and also the cases of Wurtele [14] and Crapper [15]. The case considered by Sawyer [16], and also, by a different method, by Veltichev [17], when it is assumed that: $W = W_0(z)\exp[i\,(kx + ly)]$, is also a particular case of our above considered case.

In the above Eq. (2.19), for the right-hand-side term, we have the following expression:

$$O(\lambda) = 2\lambda(U_\infty \partial/\partial x + V_\infty \partial/\partial y)\ d/dz[U_\infty \partial W/\partial x + V_\infty \partial W/\partial y], \qquad (2.20a)$$

where $\lambda = (1/2)\,\beta\, H_c$.

Assuming that far upstream, for: $x \to -\infty$, we have:

$$\rho_\infty(z) = \rho_\infty(0)\exp(-\beta z), \qquad (2.20b)$$

then, in a such case, in above Eq. (2.19):

$$D^2 = \beta(H_c)^2\left[g/(G_\infty(0))^2\right], \qquad (2.20c)$$

with

$$G_\propto(z) = U_\infty(0)\left[(U_\infty(z))^2 + (V_\infty(z))^2\right]^{1/2}. \qquad (2.20d)$$

In the case of the Boussinesq approximation (see, for instance, Sect. 3.2.2 in Chap. 3), when we observe that (for usual meteo values):

$$2\lambda/D^2 = Fr^2 \equiv G_\infty(0)^2/g(H_c)^2 \ll 1, \qquad (2.21a)$$

and with the *similarity relation*:

$$D^2 = 2\lambda/Fr^2 = O(1), \qquad (2.21b)$$

we have *necessarily*:

$$\lambda \ll 1. \qquad (2.21c)$$

As a consequence, the $O(\lambda)$ term in the main Eq. (2.19) can be neglected in the *first, leading-order, approximation*—which is, in fact, the famous Boussinesq assertion. But with such a conclusion concerning the Boussinesq assertion, the only thing I needed to do in 1968, after returning to Paris, was to fully elucidate the asymptotic nature of Boussinesq's assertion of 1903. Finally, in place of the above Eq. (2.19) we can write, after some obvious changes, the following working equation:

$$(\cos\alpha_\infty \partial/\partial x + \sin\alpha_\infty \partial/\partial y)^2 \left\{ A(z)W + \partial^2 W/\partial x^2 + \partial^2 W/\partial y^2 + \partial^2 W/\partial z^2 \right\}$$
$$+ B(z) \left(\cos\alpha_\infty \partial/\partial x + \sin\alpha_\infty \partial/\partial y\right) \left(\sin\alpha_\infty \partial W/\partial x - \cos\alpha_\infty \partial W/\partial y\right)$$
$$+ D^2 \left[\partial^2 W/\partial x^2 + \partial^2 W/\partial y^2\right] = 0, \tag{2.22}$$

with

$$A(z) = (d\alpha_\infty/dz)^2 - (1/G_\infty)d^2G_\infty/dz^2, \tag{2.23a}$$
$$B(z) = d^2\alpha_\infty/dz^2 + 2(1/G_\infty)(dG_\infty/dz)d\alpha_\infty/dz, \tag{2.23b}$$
$$\mathrm{tg}\,\alpha_\infty(z) = U_\infty(z)/V_\infty(z). \tag{2.23c}$$

Here, we envisage only a simple working model in which the vertical variation of W, in the layer z ∈ [0, 1], can be expressed by means of a quadratic polynomial in z. In such a case it will be possible to study the W field in the plane $z = C^{te}$ parallel to the plane z = 0. In particular, if this plane $z = C^{te}$ is found to be at the level z = 1/2 we can write:

$$\left(\partial^2 W/\partial z^2\right)\Big|_{z=1/2} = 4\left(\Gamma - 2\Omega\right), \quad \Omega = W\big|_{z=1/2},$$
$$\Gamma = G_\infty(0)[\cos\alpha_\infty(0)\partial\Delta/\partial x + \sin\alpha_\infty(0)\partial\Delta/\partial y], \tag{2.23d}$$

where the function Δ(x, y) simulates the considered obstacle in the 3D problem. Finally when, in the plane z = 1/2, we envisage a rotation of the axes x and y by an angle equal to $\alpha_\infty(1/2) = \alpha^*_\infty$ and introduce X and Y as new nondimensional coordinates in plane z = 1/2, we obtain our *working model problem* for the steady 3D lee waves at the level z = 1/2, in the form of Eqs. (2.24a–2.24c):

$$\partial^4\Omega/\partial X^4 - \partial^4\Omega/\partial X^2\partial Y^2 + \left[D^2 - 8 + A(1/2)\right]\partial^2\Omega/\partial X^2$$
$$-B(1/2)\,\partial^2\Omega/\partial X\partial Y + D^2\partial^2\Omega/\partial Y^2 = -4\partial^2\Gamma/\partial X^2, \tag{2.24a}$$

with

$$\left[\Omega, \partial\Omega/\partial X, \partial^2\Omega/\partial X^2, \partial^3\Omega/\partial X^3\right] \to O \text{ for } X \to -\infty \tag{2.24b}$$
$$\Omega \to O, \text{for } Y \to +\infty \tag{2.24c}$$

The references [Z24, Z31, Z32, Z33] provide various numerical results, as an application of Eq. (2.24a), with the conditions (2.24b) and (2.24c).

Figure 2.3a illustrates the flow configuration above a typical obstacle of the form of a paraboloid of revolution, range of vertical speeds in the plane z = 4 km; Fig. 2.3b provides a calculation, with $D^2 = 50$, regarding precipitation zones (very well correlated with the positive zones, *hatched line*, of vertical perturbation speeds of the wind) above the Basin of Arcachon (France) for stable stratification

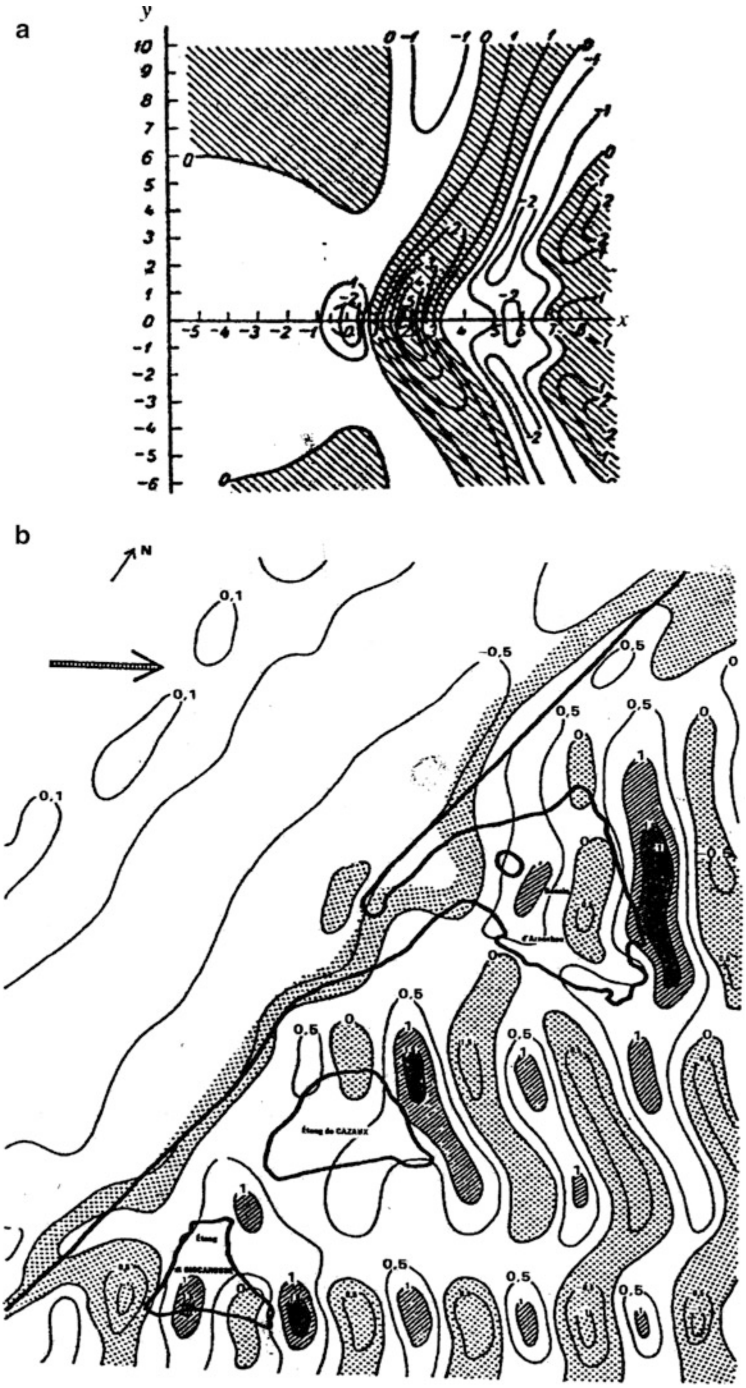

Fig. 2.3 (**a**) Typical 3D result (reprinted with kind permission from [Z19]). (**b**) Wet zones above the Bassin d'Arcachon for stable stratification $D^2 = 50$. Negative: *broken line* and positive: *hatched line* (reprinted with kind permission from [Z19])

according to the computations by Trochu [18] of Méteo France (Paris). In particular, for the calculation of rain zones above the Arcachon Basin, we have represented the coast as an algebraic sum of typical simple obstacles of paraboloids of revolution; we observe that the coast, which separates the sea from the land, plays the role of an obstacle (mountain!) due to the difference in the roughness of the land relative to that of the sea. With the coast represented by such a sum, we have calculated the influence of each paraboloid at each point of the region investigated and have obtained the sum (as we consider a linearized lee waves problem!) of these influences.

A detailed solution of Eq. (2.24a) is provided on pp. 71–78 of [Z24] along with conditions (2.24b) and (2.24c), with Ω in plane z = ½, for a model problem based on a *single intermediate level* in altitude.

For further details concerning 3D-linearized lee waves, see our book LNP m5 of 1991 [Z24], pp. 66–84, and also [Z19], pp. 168–169.

Our *first publication* [Z31] (1963, in Moscow), presents our results concerning the steady 3D case of the lee waves linear problem, and [Z30] the *analytical* derivation of Eq. (2.19) from the linearized 3D steady Euler system of five linear equations for the small perturbations of pressure, three components of the velocity vector, and density.

The paper cited as [Z32], published with N. A. Fedotova in 1965, gives the various results linked with the calculation of rain zones above Moscow and Sofia (Bulgaria) and [Z33] of 1963 provides a tentative comparison regarding orographic cloudiness in a stable and unstable atmosphere. In the well-documented review paper by Miles of 1969 [19], the Addentum (pp. 75–76) cites some references to work relative to “relief waves” in the former Soviet Union, during the period 1938–1966.

2.5 At the Moscow Hydro-Meteo Computing Center with I. A. Kibel

The new research unit—whose name would change several times over the years, and dubbed the “Hydromet Centre of Russia”—was opened in 1961 and thanks to V. P. Sadokov I became one of the first researchers officially enlisted in March 1961. Sadokov was brilliant and Kibel’s favorite scholar; at that time he studied the: “*Allowances for friction and orography effects in numerical forecasting models*”, for short-range weather prediction. This new institution (“Hydromet Centre of Russia”) together with the Main Computer Centre of Roshydromet, the Main Radio-Meteorological Centre, and All-Russian Research Institute of Hydrometeorological Information together constituted the “World Meteorological Centre (WMC) of Moscow”.

In 1961, my situation in Moscow was stable thanks to my permanent *propiska* as a Moscow resident. I had worked at Kibel’s Department up to the end of August

1966—at the beginning as a research associate (with a *Kandidat of Sciences* degree) and then, from 1965, as a '*Chief Scientist*'—a position attributed by the Moscow Academy of Sciences (Meteo-Dynamics speciality); this resulting from my various results related with the calculation of lee waves occurring downstream of a mountain in a baroclinic and stratified atmosphere during the years 1960–1965. This was for me a new big opportunity—now the *fourth*—after three important career stages:

1. at the Yerevan evening school with the mathematics teacher,
2. with Mergelyan at Yerevan State University,
3. with Atho Mkhitarian at the Yerevan "Institute for Water and Energy", and now:
4. working at Kibel's Department in Moscow.

Till the present day I am proud to have been a student of Kibel and my approach to Theoretical Fluid Dynamics—with a white sheet of paper, a black pencil, and a big gum eraser—has mainly been inspired by the 9 years during which I worked under his supervision in Moscow. Ilya Afanasevich Kibel was a very noble man and distinguished himself by his interests, not only for hydrodynamics but also, for literature, poetry, theatre, and ballet. Kibel was exacting with his students but equitable and with him it was relatively easy to defend a *Kandidat* thesis (Ph.D. à la Russe) for a highly motivated student. Unfortunately, this was not the case with the defense for a doctoral degree—in this respect he wanted only the best and highest motivated students to graduate from his department. So he brought up all kinds of arguments for improvement in order to postpone the defense of a doctoral dissertation! In spite of this, I think that Kibel was proud for the success of his former students in their new jobs.

As I shall explain below, I did not inform Kibel about my intention of returning to Paris, as he could have prevent this (see Sect. 2.6, my "*Return to Paris Adventure*"). I unfortunately did not have the possibility to meet and discuss with Kibel concerning this return! Two years after this leaving Kibel's Department, I received an invitation from Nikolaï Yanenko to participate in a special symposium on "Applications of numerical methods in modern gas dynamics" [Z34] in Akademgorodok, near Novosibirsk. Being in Moscow for 3 days, on way to Akademgorodok, in the Hydro-Meteorological Center, despite favorable intervention of E. N. Blinova, Kibel refused to receive me and give me the opportunity of explaining to him the reasons of my return to Paris and the major problems I needed to face for this. I had hoped to be able to tell him directly, and to show my appreciation of having been one of his students. This unrealized encounter with Kibel, 1 year before his passing away, is still for me a severe reminiscence.

Kibel's book [4] was a decisive step in applying the theories developed in dynamic meteorology to weather forecasting in the Soviet Union. But, besides investigations on synoptic-scale processes, Kibel directed his attention (as a hydrodynamicist working for some 40 years at TSAGUI, the Russian NASA equivalent) also on meso-scale atmospheric phenomena such as flow over mountains and local winds. In the 1960s he launched upon an ambitious program (see [20], which includes a section concerning the problems of mesometeorology) of research in mesometeorological processes and numerical *meso-meteo forecasting*. He himself, with the help of his students, took the first steps toward their implementation. Mainly, on the basis of my publications in mesometeorology in Moscow, during the years 1961–1966, in Kibel's department at the Hydro-Meteorological Center, I published my Course of Mesometeorology [Z35] in 1968—and in 1969, obtained the title of 'Doctor d'Etat ès Sciences' from the University of Paris (Sorbonne) for my thesis [Z16]. More precisely, concerning the above course [Z35], it is one of the topics in the written lectures delivered during the years 1967–1970 to students of the Meteorological School of Paris, already graduated from the famous "*École Polytechnique*"—a similar course was also taught by C. S. Yih from Michigan State University in the early 1970s (as an invited professor) at the University of Paris VI.

Everyone working with, or having the privilege of having known Kibel, remembers him as a selfless, devoted scientist. Selected works of his most important contributions to dynamic meteorology are compiled in a volume of the USSR Center for Hydrometeorology, published in Leningrad by Hydrometeo-Izdat in 1984 [21].

Quite interesting though, during the 1960s, despite the high level of theoretical dynamic meteorology in the Soviet Union, hydrodynamical short-range numerical weather prediction was not reliable over periods of several days and some members of the Central Committee of the Communist Party in Moscow were very critical towards Kibel's scientific activities at the Hydro-Meteo Computing Center! Izvestya, (Atmospheric and Oceanic Physics 6(11), 720–721, 1970, English ed.) published a *Personalia* in memory of Ilya Afanasevich Kibel.

I was much upset about Kibel's blunt refusal to meet me in August 1969 in Moscow. Then, in the Proceedings of the IUTAM Congress at Munich 1964 (published in 1966) Kibel had simply omitted my results in mesometeorology that I had obtained in Moscow at his department from 1960 to 1965! I was in possession of the typewritten text of Kibel's oral presentation on mesometeorology in Moscow of 1964 (before Munich) which I had attended and where my name was quoted several times and several figures of atmospheric flow were presented from my research. I stated this case of omission and suppression during the EuroMech 1972 Meeting on Relief Waves in Austria, where I presented various results on models of relief waves (see my two Notes in CRAS [Z36]). German meteorologists who attended both this 1972 meeting in Austria and the 1964 IUTAM Congress in Munich, later told me that my name was actually mentioned more than once by Kibel in his oral presentation. My "*Course of Mesometeorology*" of 1968 [Z35] and my 1969 "*Doctoral thesis*" [Z16], are both still being ignored in Moscow and those publications from my time in Moscow during the years 1960–1965 are not mentioned in the references of various papers related to mesometeorology and more particularly those related to lee waves published in Moscow after 1966!

But curiously, in the years 1969–1989 and after the *perestroika* in new Russia and in the Free State of Armenia, during my numerous scientific visits and several participations in symposia and congresses, I always had very kind relations with Babenko, Bagdoev, Barenblatt, Belotserkovski, Dikii, Dobrichman, Dorodnitsyn, Grigorian, Gutman, Kadechnikov, Kojevnikov, Kurbatkin, Martchouk, Monin, Obukhov, Ovsiannikov, Pekelis, Pukhnachev, Ruban, Sarkisyan, Sedov, Shkadov, Sychev, Yanenko, . . .

However, all these deplorable events linked with putting my scientific work on the index in the Soviet Union, did not in any way prevent me from retaining a very positive remembrance of all the years spent with Kibel at the Hydro-Meteo Computing Center.

It is my deep conviction that working intensely within Kibel's team for 6 years was the most fruitful period for my scientific development—a period during which my personality was strengthened and my scientific curiosity expanded and stimulated—to a large extent due to my positive relationship with Kibel.

Kibel began working at TSAGUI (equivalent to the U.S. NASA in the 1930s to 1940s) where he studied mainly the compressible boundary layer during supersonic flight, in order to estimate heating on the wall of a body in movement. He was probably one of the first, if not *the* first to obtain the temperature on the wall as a function of the Mach number theoretically. His result was questioned by research engineers in charge of building rockets, because their estimate was much less pessimistic than Kibel's. But, in the end, Kibel's results were recognized by the engineer-constructors and Kibel was awarded a significant amount of roubles.

E. M. Dobrichman, who had been the second "*contradictor*" in the defense of my *kandidat* thesis, and another student of Kibel who I knew pretty well during my time in Moscow, published in a paper in 2004 in Atmospheric and Oceanic Physics 40(5), dedicated to the Centennial Birthday of I. A. Kibel [25], where he relates very interesting memories. It seems to me that Kibel's idea of using primitive adiabatic meteo equations, quoted in the conclusion of his 1957 book (see [4]), was inspired by Kibel's earlier work on long-wave approximations for nonviscous but compressible fluid flows.

I mention here, also, a special point concerning the question why Kibel, who joined the USSR Academy of Sciences as a corresponding member in 1943 at an age of less than 40, remained a corresponding and not a full member of this Academy over a period of 27 years! This is hard to understand accordingly to his undeniable leadership in Dynamic Meteorology in the USSR during these 30 years! In fact, in spite of friendly support by friend academicians like Lavrentiev, Keldich, Sedov, and Ambartsoumian, Kibel never directly applied to become a full member of the Moscow Academy of Sciences! Such a position would obviously have given him the possibility to have his own a research institute inside the Academy of Sciences of Moscow. Strangely, on the contrary, he had been actively involved in favor of E. N. Blinova—also a corresponding member of the USSR Academy of Sciences since 1966, like him—of becoming elected as a full member without himself being a full member! An explanation for this may well be in the special debt he had towards Ekaterina Nikitichna Blinova during the Leningrad Siege of 1941, surrounded by the German army—when Blinova was a student *aspirant* with Kibel, had much worked to put his life on a less difficult level, and eventually married him.

During the years 1961–1966, at the Moscow Meteo-Computing Center, as a research scientist, my desk (sharing with V. Kadechnikov) was adjacent to Kibel's and I knew how frequently he walked from his own desk over to Ekaterina

Nikitichna. For me, obviously Kibel contributed *significantly* to the major publications of Blinova on *hydrodynamic long-time weather forecasting*. The publication of the Blinova and Kibel paper of 1957 in Tellus [5], Kibel's book of 1957 [4], and the book of Monin of 1969 [7], drew the attention of western scientists, especially from the USA, to Soviet scientists' achievements concerning meteorological dynamics—what is now referred to as "Geophysical Fluid Dynamics"—as applied to weather forecasting. Such a success is, it seems to me, the result of two complementary factors: first, the very high level of the Soviet mathematical school which did not feel *repugnant* in devoting itself to practical problems arising from physics; second, the Soviets were *very much behind,* at that time, regarding high-speed computing, even if, from the point of view of numerical algorithms and analysis, they had excellent researchers with superb analytical calculating abilities.

Kibel was spirited with a very broad education, he spoke French rather well, liked and had pleasure to recite masterly poems of Pouchkine and Lermontov, and he knew a vast lot of poems. It was said that he had had a love affair with the well-known ballerina Oulianova in Leningrad! Kibel and Obukhov in 1945, after the German-Soviet war, together worked at the Moscow Geophysical Institute of the Academy of Sciences—with as the famous Assistant Director I.K. Fedorov—who had got upset about me in 1958! By the beginning of the 1950s, Martchouk was also an *aspirant* of Kibel (in 1950–1953; at the same time with Artho Mkhitarian) and defended his *Kandidat* thesis at the TsIP in Moscow.

Very soon Kibel was sympathetic with me, and I joined in June 1961 his Dynamic Meteorology Department in the new Moscow Hydro-Meteo Computing Center, as a *Kandidat* in Science. Kibel often came over to my desk urging me to work on various problems that he himself were currently working on. A fairly typical case of this scientific relationship between Kibel and me, is my result related with two first integrals for the 3D steady Euler non-viscous, adiabatic compressible Eq. (2.8a–c) written relative to velocity vector **u**, density ρ, and specific entropy S.

Namely, if we take into account the two Eqs. (2.10) for ψ and χ, then, thanks to the continuity Eq. (2.8a) we write:

$$\mathbf{u} = (1/\rho)[\nabla\boldsymbol{\psi} \wedge \nabla\chi], \tag{2.25}$$

and as a consequence:

$$\mathrm{H} = H(\psi, \chi) \ \text{ and } \ \mathrm{S} = S(\psi, \chi). \tag{2.26}$$

In such a case from the Euler equation (2.8b), and the above Eqs. (2.25) and (2.26), we obtain first the following equation (Z14):

$$\begin{aligned}&[(\nabla \wedge \mathbf{u}).\nabla\psi]\nabla\chi - [(\nabla \wedge \mathbf{u}).\nabla\chi]\nabla\psi = \\ &\rho\{\partial H/\partial\chi - T\partial S/\partial\chi\}\nabla\chi + \rho\{\partial H/\partial\psi - T\partial S/\partial\psi\}\nabla\psi,\end{aligned} \tag{2.27}$$

and then using a vectorial formula, for the left-hand side of Eq. (2.27), we derive:

$$(\nabla \wedge \mathbf{u}).\nabla\psi = \rho\{\partial H/\partial\chi - T\partial S/\partial\chi\}, \tag{2.28a}$$

$$(\nabla \wedge \mathbf{u}).\nabla\chi = -\rho\{\partial H/\partial\psi - T\partial S/\partial\psi\}, \tag{2.28b}$$

with **u** given by Eq. (2.25). These two *first integrals* (2.28a, 2.28b), discovered in Moscow in 1965, are very useful for steady cases to analyze 3D compressible nonviscous adiabatic Eulerian fluid flow phenomena. For instance, *for the 3D steady lee waves problem above and downstream of a mountain under the action of gravity*, in our paper of 1966 [Z14], p. 111 of the Russian original; the introduction of the two 3D perturbed stream functions ψ' and χ' relative to the *2D problem*, as given by the above Eq. (2.14a) for 2D steady stream function ψ(x, z) and give then *two linear equations for* ψ' and χ', but with variable coefficients dependent on this 2D stream function, ψ(x, z) *and its partial derivatives in x and z*. This opened the way for taking into account some complementary (3D) effects in the 2D steady lee waves problem in a consistent manner.

The rapid (!) publication of [Z14], thanks to Obukhov, as a first article in February 1966, vol. II, pp. 105–112 in Izvzestya of the SSSR Academy of Sciences (Fizika Atmosphere i Okean)—which was a 'reward' according to Mocovian standards, was perceived by Kibel as an attempt on my side to leave his department to work with Obukhov at the Institute of Atmosphere Physics! Actually, since the beginning of 1966, I had made up my mind to attempt to emigrate back to Paris with Natalia and our daughter Christine! But, as related in Sect. 2.6, this return to Paris was a very risky decision in 1966 under Brezhnev and his Leninist ideology!

The discovery of the above two first integrals (2.28a, 2.28b), was my first original analytical result and elicited some very positive reactions on the side of Kibel. This opened new options for me regarding theoretical research on fluid dynamics in France during the years 1967–1996 at ONERA and at the University of Lille-I (in Chap. 3 I give a short account of this research mainly from various book reviews of my 13 books published during the years 1974–2012.

2.6 The Years 1964–1966 in Moscow and My Return to Paris

During these eventful 3 years my situation in Moscow changed. First, as a consequence of my scientific results the High Committee for granting of titles in the High Education Ministry of the USSR in Moscow bestowed onto me (at the end of 1964) a Diploma (MCH N° 017303 MockBa 31/12:1964) associated with the position of Chief Scientist (speciality in Dynamical Meteorology) and concomitantly *doubled* my wage. Since 1965, I had considered pursuing a doctoral thesis, in Russian and based mainly on my theoretical and numerical results concerning *relief waves*, and then to commit myself to theoretical research concerning fluid dynamics problems. But this idea, with a reticent Kibel, could not be easily realized!

At the end of 1965, one particular event significantly determined my perspective on life in Moscow and in especially regarding my adventurous return to Paris in September 1966—this was the "WMO Regional Training Seminar" (see [22]) taking place from November 17 to December 14, 1965, at the WMC of Moscow. I participated in this seminar, but only as an interpreter from Russian to French and vice versa. Among the attendants were: Jean Bessemoulin, Director of the French Météorologie Nationale at Quai Branly in Paris and Guy Dady, Chief Engineer at EERM (Établissement d'Études et de Recherches Météorologiques) of the Météorologie Nationale, mainly involved with short-range numerical weather forecasting in France. Bessemoulin and Dady, but also L. Facy (Head of EERM) played a decisive role in the first days of September 1966 of giving me the opportunity to begin another scientific research career in France.

During the entire duration of this WMO Regional Training Seminar I maintained very positive relations with Dady and I let him know that I had thought of returning to Paris one day—I learned only recently from Dady that he had, after returning from Moscow to France, sent a letter to the Quai d'Orsay in Paris concerning me and had specified my desire of a return to Paris. This seems to have played a decisive role for my return to Paris (see Sect. 2.6.2). I worked only for 1 year, up to September 12, 1967, at the *Meteo* at Quai Branly (as briefly related in Sect. 3.1), but that year gave me the opportunity to get to the heart of things in Paris again! During this WMO Regional Training Seminar in Moscow I had the occasion to meet a number of specialists from Western countries, namely: P. D. Thomson, F. G. Shuman, B. R. Döös, K. H. Hinkelmann, and E. K. Knighting… and had some opinions (?) regarding their personality!

Despite the time I had to spend to organize my departure from Moscow—in the first semester of 1966, in my paper [Z37], on "*hydrostatic approximation*", I tried to understand the process of *filtering short acoustic waves* and also the role played by a *vanishing viscosity*. This was for me the occasion of a major leap in my understanding of *quasi-incompressible fluid flow phenomena* (which I have named "*hyposonic* fluid flows" [Z8]) and also on the understanding of the *Prandtl concept* of the *boundary layer* for an *unsteady* and *compressible* fluid flow! More precisely, the *filtering* short acoustic waves problem in the case of *large Reynolds* numbers (*vanishing viscosity in the vicinity of a wall*) is the subject of my CRAS article of 1980 [Z38], which gives a new perspective on the role of the *Prandtl boundary layer concept* in the case of an *unsteady compressible* fluid flow (please refer to concerning this singular nature in Sect. 4.2.3). Here, I observe only that: "*Large Reynolds number flow, near the initial time and in the vicinity of a wall, during an unsteady external compressible fluid flow around a body, leads to a consideration of a very singular situation mainly because the strong degeneracy of Prandtl boundary layer equations, which do not work near the initial time where the initial data are given*!"

2.6.1 The Brezhnev Years and the Troubled Era of Stagnation

After the fall of Nikita Khrushchev, during Leonid Brezhnev's years, life in the Soviet Union very soon became bleak and the country moved into disillusionment! Khrushchev's promises regarding communism had faded—one of the main causes for Khrushchev's dismissal from power was the relatively poor economic growth during the early 1960s. The Era of Brezhnev meant stagnation in the Soviet Union: economic, political, and social. The lack of reforms and the high expenditures on defense caused an economic backlash—a process that had started as early as 1960. This social stagnation can be traced to the Sinyavsky-Daniel trial in 1965. Politically, stagnation began with the establishment of a gerontocracy(!), which came into being as part of the policy of stability. The Soviets, fully aware of their standard of living relative to that of Western countries, were fascinated by the "consumer society"—believing by then (maybe naively!) that 'true' (!?) communism was not to be found in the East, but rather in the West. The competition between the USA and USSR opened Pandora's box—writers, painters, intelligentsia rebelled against severe censorship from the Communist Party! Finally, the Soviets improved their living standards by robbing the State! The atmosphere was unhealthy and it was time (for me!) to leave this country and go back to Paris!

For my part, I had already realized that I did not have much of a chance to pursue a "real scientific career" in Moscow—because of my bad capitalist forebears. In any case it seemed impossible for me to travel abroad, not to a congress, and not to a conference linked with my research even with a personal invitation!

During these years 1964–1966 in Moscow our family life changed. Our housing problem being partly resolved, Christine moved back with us after spending the years 1962–1964 with her mother's great-aunt, who had been an assistant manager of a House for Pioneers—future Komsomols. In these years 1962–1964 our young daughter often spent whole days within that House where she was one of the young stealing the show! Quite young she already knew how to recite poems and act in a play. Knowing her aunt, Natalia feared the typically kind of Soviet education that she would impose on Christine—and thus on Saturdays and Sundays, when Christine was with us, we tried somewhat subtly to repair the damage of the week.

Christine was a studious child, reading a great deal even quite early, with an outstanding memory. Very soon she was able to speak Russian as well as French, a language which was well-known to the great-aunt. Once back in Paris she was a brilliant student up to graduating from the University *École Normale Supérieure* in Fontenay-aux-Roses—the French prestigious school for young talented women. Quite young she liked drawing, being very hand clever, and up to now she goes on with the arts! In fact, in Moscow, problems with Christine's education at the Soviet school were starting to become evident in September 1966. For Natalia and me this was very disturbing! This got us to seriously considering to emigrate to Paris!

I have to admit that continuing to live in Moscow and trying to be a "good Soviet citizen" seemed completely disconnected to my French past. Language, symbols, emotions, memories, all the ties that connect an individual to a community and to a country and that I had acquired during the first 19 years of my life in France, all seemed to be lost forever!

2.6.2 My Return Adventure to Paris

Indeed my considerations to want to return to Paris were being reinforced upon refusal of the Soviet authorities concerning a request for travel upon an invitation of my father's youngest sister to come to Paris as a tourist *alone*—even under the condition of leaving Natalia and Christine behind as "hostages"! My father had died tragically by, accidental drowning in Yerevan in 1963, and my mother's died in 1958 in Yerevan from cancer.

The narration of this highly risky rupture (in the Brezhnev era of Soviet stagnation and dissidence period), with my fruitful beginnings as a (Soviet!) researcher in Moscow, and my return to France—is a 7-month-long distressing journey ... but with a happy end, thanks to the French Consul in Moscow, De Finance, and especially to the State visit of the French President, General de Gaulle, to Moscow in June of 1966. This return to Paris being also linked with my very painful rupture with Kibel—my entire future life carrying a regret for having deceived him... by necessity! Only Vladimir Kadechnikov, with whom I had shared a desk, knew all about the matter. But concerning this "return to Paris",[2] even if it was easy to decide, in reality this adventure was associated with many extreme difficulties!

Our return to Paris was difficult and risky, but it was also decisive for my scientific career, for Natalia as an artist, and also for the education of our child. The main difficulties from March to August 1966 involved to obtain the necessary documents from the WMC required by the OVIR (Foreign Ministry) in order to obtain the emigration visas from the Soviet Union. The first step was relatively easy thanks to V. A. Bugaev, Director of the WMC who provided the documents for the OVIR to me by the end of February ... but in March 1966 I received the first denial! Then, from April to June 1966, I received again three denials from the OVIR. Meanwhile during these 4 months, from March to June, I knocked at the door of several official Soviet governmental institutions in Moscow—the KGB, Central Committee of the Communist Party, Supreme Soviet, a.o. ...but all in vain, everywhere the same objections: "*You are a top-level research scientist and soon will be a Doctor of Sciences... the Soviet Union has invested lot of money on you... so you must... etc., etc.*"

[2] Concerning "*voyages*" to foreign countries for Soviet citizens in the 1960s, it is worth reading the "*Remembrance*", difficulties and hardship encountered with Soviet bureaucracy as elaborated in the book by the I. M. Khalatnikov [23, pp 110–176], the first Director of the Landau Institute in Moscow.

In particular, my '*visit*' to the Russian Home Secretary—a so-called *Tikhounov*, an officer with a four-star uniform and acting only according to official policies—after having been on a waiting list for more than 1 month, just turned a deaf ear on me! He was outraged by my insistence and finally said that he would expel such an impertinent person like me from the Soviet Union... with a kick in the bottom.

My reaction to such vulgarity was simply to say: "*This is just my strong desire, but I'm afraid that you do not have enough power to do that*!" and to add: "*Whatever comes of it, I am determined to leave the Soviet Union—and I am not your slave!*" ... turning on my heel, and followed by the contemptuous looks of the two officers—unusually physically dominant persons... who certainly would have been happy to throw me in jail.

The lesson learned from all these steps was, ultimately, that I became convinced that in the Soviet system of oppression and dictatorship, any serious decision would need to be taken at the very highest level!

Over all these years I had still kept my old French Identity Card, and having been brought by my parents, without passport and entry visa to the Soviet Union, I was still a French citizen...in spite of all, I figured! This reasoning would be shared by the French Consulate and Embassy in Moscow ... so that, with my return to France I would retrieve all my rights and duties—this was to become my strategy.

When I met the French Consul in Moscow, Monsieur De Finance, he said that such a return with Natalia and Christine would be very difficult to realize. A French student of the École Normale Supérieure (ENS) in Paris, who was at the time studying in Moscow at MGU, helped me to get an appointment at the French Consulate in Moscow and meet Consul De Finance. The Consul took record of my name and my inquiry to return, as a French citizen, to France with my wife and daughter. He asserted that the most difficult step would be to obtain at the highest political level the visas from OVIR for Natalia and me (our daughter being appended to my passport). Yes, sure, that was the key to our problem. I mentioned the four previous denials by OVIR!

After such a long time spent in fruitless endeavors something new happens: The French Consul tells me that an official state visit to USSR by General De Gaulle, President of French Republic, was scheduled for June 1966 and that during this time De Gaulle would spend a few hours at Moscow University (MGU). The French Consul recommended to try to meet (I had to imagine how!) somebody belonging to De Gaulle's entourage in order to deliver a paper briefly explaining my request—namely: "*I am a French Citizen and I want to return to France with my wife and our six-year-old daughter, and we want the child to start her primary education in a French school.*"

I was to add, as by the consuls proposal, that he, the French Consul Monsieur De Finance knew about and was in possession of copies of the documents relating to my case. All this sounded encouraging, but how would I be able to meet somebody belonging to the Presidents entourage? I had to come up with this, the Consul made no suggestions in this regard. However, he did reinforce his proposal by saying that a state visit usually allows for addressing such cases and requests. He said it would be sufficient to have my case on the list of French requests at the end of the State Visit.

The day of De Gaulle's visit at MGU, I am there sufficiently early and am able to oversee the who is who regarding the official French entourage. The reception was to begin in the Great Act's Hall. I positioned myself in a good place to attempt to break through the vigilance cordon. Win or lose ... everything!

On arrival of De Gaulle, with about ten personnel following him, there is a slight jostling and I take advantage of it in order to be in a favorable position. I intend to slip in, at the right moment, just behind the last person belonging to De Gaulle. He enters the Great Act's Hall. My advantage is that I can profit once again from the fact that I am a Frenchman in contrast to most of the other Soviet students.

At one go, I find myself a few meters away from De Gaulle people. He turns over and I have the fugitive feeling that De Gaulle perceives that I am there... even he seems to give a faint sort of smile. At that moment, the last of De Gaulle's people in the Hall and in front of the Act's Room, stops aside me. I turn towards him and give him my paper and add: "*The French Consul Monsieur De Finance knows about me*". The vigilance cordon seemed to be taken aback by the spontaneous interaction and luckily did not reflex to interfere ... I left the Hall without interception.

For me this instant was a moment of intense happiness and pride—even today, here at 12, rue Saint-Fiacre in Paris, when I recall this incident, it is as if it were taking place now! The wait seemed to me eternally long, but... 2 weeks having passed, I received a phone call from OVIR—the officer asks a surprising question: ... whether I don't want to make up my mind, and withdraw my application for emigration visas from the Soviet Union that he has issued and are ready for pickup—the height of impudence! ... after all these months of denial! The game was over, I had won my case, ... but I am still skeptical!

The same day I phoned Natalia, who had spent the summer quietly with Christine in Djoubga—a small resort on the north shore of the Black Sea near Novorossisk, south of the western end of the Greater Caucasus—and announced the good news. After a quick round-trip to Djoubga and return to Moscow with Natalia and Christine—at the end of August, we were ready to leave!

Though our departure is taken for granted, we are still waiting! Our few personal belongings were already sold to friends, even Christine's bed and our bed also. We had to live by fortune, eventually with Raissa Lvovna, Natalia's mother. What we wanted to ship to France was ready to go, by train, in large wooden crates. We would take only what is strictly necessary onto the Air France plane. But, it was also a big problem to get permission to take out from the USSR certain pictures of Natalia, the others are still with Lena (Natalia's sister) in Moscow. With my hand-written papers ... I surreptitiously just slipped them between my books, being afraid that the customs would find them and take them away from me!

A few days before our departure on September 2, 1966, Natalia and I went out to the Armenian restaurant *Ararat* and unexpectedly met Gaguig (see Sect. 2.1) with whom I had shared a room at the campus of the Academy of Sciences in Moscow in April 1957... and who had given me the opportunity to meet Natalia at a dance party on 13 April 1957—what a coincidence!

By September 2, 1966, while looking for seats in the Air France Caravelle taking off towards Paris, I had a last flashback of 19 years of definitively confined, never to be forgotten, rather difficult tribulations and uncertainty within the Soviet Union. On the plane, I am surprised about one of the passengers, I. K. Fedorov, Director of the Institute of Applied Geophysics (who had got upset about me in 1958), who now asked me to consider returning to Yerevan, looking at me with a stealthy grin! But I acted firmly and ignored him, I didn't have the slightest thing in common with this guy—I am now a Frenchman, free in time and speech… and no longer a Soviet slave!

At 38 years of age I returned to my native country from the unpleasant Soviet Union and I definitely appreciated being back in France. My first objective was to obtain a French "Doctorat d'État ès Sciences", then a university career! This required to become acquainted with Paul Queney, Professor at the University of Paris, and a well-known specialist on "*mountain waves*", with an international reputation through his collaboration with various institutions in the United States: the Chicago School (C. G. Rossby), Princeton Institute for Advanced Studies, and UCLA with J. Bjerknes. I hoped that my Moscow work on "*Lee waves downstream of a mountain*" were valuable for Paul Queney and that he would agree to be on the jury of my thesis defense!

After 19 years of absence, I would probably not even recognize the Courbevoie of my youth—but strangely my school street, rue Ficatier, was still there in the shadow of the new district of modern Paris, "La Défense"!

In Paris, in 1966, my future, at 38 years of age, was quite uncertain and I needed to find my place in the French fluid dynamicians community? The only sure job I had in September 1966 was with Guy Dady at the Météorologie Nationale, at Quai Branly in Paris!

In the *Foreword* to this book, Jean-Pierre Guiraud briefly referred to how we both met. There he makes reference to the first events at the Aerodynamics Department of ONERA and also to my relationship with Paul Germain linked with my acquisition of the French "Doctorat d'État ès Sciences".

Chapter 3, on "My French Scientific Career (1966–1996)", describes my contributions during the years 1966–1996; Chapter 4 and the *Retrospective Summary* elaborate on the NSF equations and on some results concerning the *RAM approach* based on the critical reviews of my books (published in English) of the years 2002, 2006, 2009, and 2012.

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