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A Few Words about my Trajectory

To begin with I wish to thank all those who made my birthday so memorable for me: Franco Gianessi and Michel Théra, the directors of the school "Guido Stampacchia", and Hedy Attouch, Guiseppe Buttazzo, Patricia Daniele and Antonino Maugeri, the organizers of the workshop on Variational Analysis in Erice in August 2018, Hoang Xuan Phu, Huynh Van Ngai, Terry Rockafellar and (again!) Michel Théra, the editors of the special issue of the Vietnam Journal of Mathematics, Lionel Thibault, fully responsible for this issue, and all participants in the events. I must confess I tried (unsuccessfully) to dissuade Lionel as I was already aware about the future issue of VJM. So when in March I got a letter from him where he wrote: "*... could you prepare some pages on your mathematical trajectory. This could include your young mathematician period, your difficult period in Russia, your period in Israel and also any other points which seem to you suitable*", I did not have any choice but to follow the instructions.

When asked about most important events in my life, I usually refer to three fundamental decisions that determined most of what happened in my conscious life, namely the decision to marry my wife 58 years ago, the decision to study mathematics and eventually to become a mathematician and the decision to leave the USSR for Israel. Speaking about the last two I shall be compelled to tell quite a bit about the Soviet Union for otherwise much of what I am telling can be hardly understood: even young people in Russia of the age of my grandchildren (that is up to early 30s) have vague ideas of what kind a country it was[1].

1. **How I became a mathematician**. My first degree was purely engineering. I graduated (got an analogue of MSc) from the Moscow Institute of Aviation Engineering, MAI, in 1961 with a degree in electrical engineering and was sent to a military research institute. I use the word "sent" because usually after graduation you did not look for a job but a special committee decided where you will work based on the list of job offers it had.

The group in which I was working was busy with technical implementation of economic (in terms of the amount of fuel spent) trajectories for some specific fighter planes. The very choice of the trajectories was beyond the objectives of the group but I got interested in the reasons behind the choice and came across

[1]For a Western reader the best way to glimpse into the life in the USSR in the Stalin and post Stalin era is probably the novel "The Noise of Time" by Julian Barnes, the 2011 Booker prize winner. Formally, this is a novel about the life of D. D. Schostakovich, a remarkable Russian composer, but actually it is a tough and pitiless essay of a person knowing and loving the Russian culture.

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some people (also engineers) involved in search of optimal trajectories. Around that time V. G. Boltyanski announced a course on optimal control in the Steklov Mathematical Institute. So I decided to attend the lectures. Needless to say that I could not understand a word. Literally. The language he used with the expressions like "Banach space" or "homotopic transformation" etc. was absolutely foreign for me. In the mathematics I studied in MAI even the concepts of space and transformation were totally absent.

I knew that in 1961 a special evening course for engineers was opened at the Mathematics Department of the Moscow State University. So I decided to enroll and started to attend it in September 1963. It was a 4 years long course that basically offered the same program as the regular course in the department. In the first two years of general studies there were 4 hours of lectures (6pm - 9.30pm) four days a week. There were about 200 students, most engineers in mid-20s, some like me already married and having children. I doubt anything like that would be possible nowadays. But it was a time of enormous enthusiasm about science.

The mathematics we started to learn was very different from the dogmatic and dull subject we were taught earlier. There were a number of outstanding mathematicians among our lecturers: Bachvalov (analytic geometry), Kurosh (algebra), Rashevskii (differential geometry). All courses were mandatory during the first two years but in the second couple of years you could choose most of the courses and seminars. The number of them offered by the department was enormous. Never again I saw anything like this. One of the courses that made a strong impression on me was the course of G. E. Shilov on the theory of integrals. The book "Integral Measure and Derivative" written by him and B. L. Gurevich following the course remains one of the best, I believe. Shilov was a fantastic lecturer also known for his love and knowledge of music. (His and P. S. Alexandrov's lectures about music remain a legend.) Unfortunately his fate was tragic. After several of his former students emigrated to Israel or USA in the early 70s, a slanderous campaign against him was initiated in the university and he died shortly afterwards in 1975.

There were also a few mandatory courses during the last two years. One of them, called Analysis 3, was actually a course on functional analysis that included elements of calculus of variations. The course was created by A. N. Kolmogorov in the 50s and the famous book by A. N. Kolmogorov and S. V. Fomin more or less corresponds to this course. The lecturer was a young professor V. M. Tikhomirov, a former student of Kolmogorov. His manner was unusual and attractive, with many references to other areas of mathematics, historic comments etc.. By that time I had already read quite a bit and was specifically intrigued by works of R. V. Gamkrelidze and V. F. Krotov on sliding regimes in optimal control and calculus of variations (basically written in 1961-63). I had some questions and one day came up to Tikhomirov and asked him. The questions were apparently new for him and he suggested that I should come and talk in more details. It turned out that we lived very close to each other. This allowed us to meet regularly and these meetings, I believe, were the decisive factor in my conversion to math-

ematics (in addition to the life long friendship). By the end of the third year, in summer of 1965, my first paper was finished and sent to Doklady of the Academy of Sciences.

2. **Mathematical youth 1966–1972**. I still like my first paper, although shortly after it was published in 1966, I learned that some of my discoveries were already contained in earlier publications. Among them I would mention analogues of Young measures, Warga's relaxation theorem and some duality formulas for convex functions. But the ways how all these were combined in one theory, in particular the possibility to use convex duality to get relaxation theorems were apparently new. Soon afterwards we came across N. N. Bogolyubov's 1930 paper where he actually built a relaxation theory for calculus of variations, including convexification of the integrand with respect to the derivative variable. I remember that I was greatly surprised when I realized (to some relief, to say the truth) that Bogolubov's paper, although published in French in the Annales Mathématiques Pures et Appliquées, remained unnoticed and the creators of the relaxation theory, L. C. Young and J. Warga aparently did not know Bogolyubov's work, as well, by the way, as R. V. Gamkrelidze and V. F. Krotov who developed an equivalent theory of sliding regimes.

Another subsequent event that substantially affected my future work was acquaintance with papers by R. T. Rockafellar (and J. J. Moreau a bit later). Rockafellar's name was mentioned by B. T. Polyak when I presented my thesis at his seminar in October 1966. I was impressed not only by the results (some of them I already knew) but mainly by the style of the papers and the ways the results were obtained and presented. The works of Moreau and Rockafellar were basically unknown in Russia, and the 1968 survey paper with Tikhomirov "Duality of convex functions and extremal problems" in Uspekhi Mat. Nauk (translated into English as Russian Mathematical Surveys) that contained also a number of new results, mainly connected with calculus of variations and approximation theory, was met with considerable interest. Further developments along these lines culminated in the 1972 paper with V. L. Levin published in the Transactions of the Moscow Mathematical Society in which we calculated subdifferentials of some functions important in applications, like integral functionals, maxima over compact sets and some others.

But most of 1971 Volodya Tikhomirov and I were busy with translation into Russian of Rockafellar's "Convex Analysis". Terry Rockafellar and I met at a conference in Tbilisi in August 1969 organized by R. V. Gamkrelidze. For me it was the first international conference[2]. In addition to Terry, I met there Alfred Auslender, Ivar Ekeland and many others. It was also my first trip to Georgia which is one of the most beautiful corners in the Soviet Union. We could fully

[2]Actually it was called "All-Union Conference on Optimal Control". So when I applied in my institute for permission to participate at the conference, I did not mention that foreigners will also be there. Otherwise I did not have any chance to get permission as meeting with foreigners were formally forbidden for those working in military RD institutions.

appreciate the beauty of the country and the legendary Georgian hospitality during the trip organized in the middle of the conference (which was very unusual for USSR conferences).

I also continued to work with V. Tikhomirov and our interests were gradually shifting towards general optimization theory. This shift was substantially stimulated by the new theory of necessary optimality conditions just developed by A. Ya. Dubovitskii and A. A. Milyutin. The theory was actively discussed in late 60s and early 70s. As a result, in 1970 we started to work over a book that was finished early in 1973. These two years were among the happiest in my mathematical life. For my future work probably the most important was the understanding of the fundamental role of the Lyusternik theorem and the discovery of the associated metric inequality, a precursor of the metric regularity property. The book was published in 1974 under the title "Theory of Extremal Problems". (English and German translations of the book appeared in 1979, Japanese in 2011.)

3. **Decision to leave, 1972–1976**. Meanwhile, the situation in the USSR after the six day Arab-Israeli war in 1967 and events in Czechoslovakia in 1968 started to deteriorate. For people identified as Jewish (either by passport or by name) it became more and more difficult to find good jobs, the number of Jewish students admitted in prestigious universities was rapidly decreasing[3] etc..

This along with strengthening feeling of association with Israel against the background of a fierce anti-Israel campaign after the six day war, forced a steadily growing number of people to think about leaving the USSR and moving to Israel. I cannot go into details here[4]. I just mention that in the beginning the idea looked rather inconceivable. No one could leave the Soviet Union without "exit visa" - a special permission by the authorities, and the first attempts to leave with or without the persmission were met with persecution (up to a death penalty in one case which was not implemented, though, thanks to a wave of international protests.)

We began to think about leaving for Israel some time in 1971 and I started to look for a new job not connected with any security problems. I estimated that I may get permission after 4-5 years of such work (too optimistic as it turned out later). Fortunately, L. V. Kantorovich decided to move from Novosibirsk to Moscow and offered me to join his laboratory in the so called Institute of Control of People's Economy, a sort of reeducation center for middle and upper level leaders of various organizations associated with economics. But the security department of the institute insisted that I should renew my security clearance. So I decided to leave after a few months. Of course, I explained to L. V. Kantorovich my decision. I

[3]In the Mathematics Department of the Moscow University a special system of "Jewish problems" was developed. The problems were offered at the oral entrance exams to Jewish or other unwanted students. They looked simple but their solution needed knowledge far beyond the school program (see e.g. Mathematical Intelligencer **16**:4, pp. 4–7 (1994) or Mathematics Enthusiast, **11**:3, article 11 (2014)). To make the system work, a very limited group of "trusted" staff members was allowed to prepare problems and take entrance exams.

[4]Anybody interested may consult the book "We Are Jews Again", Syracuse University Press 2017 by Yuli Kosharovsky which is the best and really interesting account of the movement.

think he was the first to whom I told about our intentions. We had a long and difficult conversation: L. V. was very pessimistic about perspectives of the emigration movement. I was lucky again concerning a job: the department of applied mathematics of one of the engineering schools, abbreviated MADI in Russian, was urgently looking for a person to fulfill a vacancy of associate professor. I was acquainted with the head of the department, B. S. Razumikhin. So I applied and got the job in August 1972.

During the first two years of work in MADI I did not have much time to do mathematics. The teaching load was heavy: 16 hours of lectures a week, plus several students reception hours, plus exercises left little time for anything else, although I continued to regularly attend the seminar of V. M. Alekseev and V. M. Tikhomirov in the university and tried to keep professional contacts, especially with A. A. Milyutin and M. A. Krasnosel'skii, V. L Levin and V. I. Arkin in Moscow and occasionally with V. F. Demyanov, S. S. Kutateladze, B. N. Pshenichnyi and A. M. Rubinov, when they visited Moscow.

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I was able to resume to work only by the end of the second year. Two of the three papers written that time deserve mention, I believe, both published in the SIAM Journal on Control and Optimization, the first in 1977 and the second in 1979. The first contained necessary and sufficient conditions for sequential lower semicontinuity of integral functionals of calculus of variations with respect to a wide range of topologies, and basically closed the subject. An account of the history of the developments can be found in G. Buttazzo's 1989 book (Pitman Research Notes in Mathematics, volume 207).

The second (three part) paper was much more important for my future work: it was my first experiment with the metric estimate, associated with the Lyusternik theorem, which we proved in our 1974 book, later becoming known as the "metric regularity" property. In the West this estimate became known after Robinson's 1976 famous paper where he proved the estimate for a bit more general class of mappings. The main idea stressed in the paper was that any (smooth) problem with equality and inequality constraints is equivalent to unconstrained minimization of a composition of a certain sublinear function and a smooth mapping (both fully defined by the problem), provided the equality constraint mapping is regular at the solution.

These were also years of slow reorientation to what is now called variational analysis. In 1974 Terry Rockafellar visited Moscow and brought F. Clarke's thesis. I was really impressed. My last talk in the USSR was a lecture at the seminar of B. S. Mityagin in CEMI with a survey of Clarke's results. Shortly before that Frank Clarke also visited Moscow as a tourist and brought me several of his new papers. Through Clarke I got acquainted with J.-P. Aubin a few months later. The meeting with Jean-Pierre was really fateful, not just a beginning of a lasting friendship, – his help and support, both professional and human, during the 11 difficult years of "refusal" cannot be overestimated.

4. **Difficult time – the years of refusenik life, 1976–1987**. We applied for emigration to Israel in September 1976 and were refused permission to leave. My wife who was teaching electronics in a college lost her job immediately after we applied. This was rather a typical case. But I was not fired, just demoted to a "researcher" with loss of about 40% of my salary and transferred first to the (non-teaching) computer department and then to some reeducation courses for engineers associated with the institute. (Many years later, in 2009, I was invited to a conference devoted to scientific communication during the iron curtain era to tell about the situation with "refusenik" scientists. And I discovered a number of cases when administrations of science institutions, especially involved in international cooperation, successfully resisted the pressure from the KGB and the communist party officials to fire those who applied for emigration.) It was only in January 1988 that we got permission and left the USSR.

In many respects, our life was not easy all these years. Officially we all were somewhere between "victims of imperialist and zionist propaganda" and traitors. Not surprisingly, many of our acquaintances, friends and even relatives, fortunately not all, were afraid to communicate with us and preferred to cut all contacts. It was painful but more or less understandable, and after things changed in Russia, some of our lost contacts and even friendships were restored.

We knew that our telephone was tapped (once even cut for a year) and much of the mail did not reach us. (For instance, starting with early 1977 none of the issues of the SIAM Journal of Control and Optimization sent to me as a member of the editorial board was delivered.) There was no question about the possibility to participate in seminars or conferences. And of course, no chance to publish a paper in a Soviet journal. I even knew a number of cases when a reference to a person who emigrated or applied for emmigration was excluded from papers by editors of journals. Of course, I was aware of that even before we applied and the three papers I wrote during 1972-1976 were all published in Western journals. Again, I must add that there was no possibility to formally submit a paper to a foreign journal. Even to submit a paper to a Soviet journal one had to receive a permission (with all necessary signatures and stamps) of the security department of the organization one was working. Sending a paper by mail would also be hardly useful.

So the only possibility was to smuggle a manuscript out of the country using occasional foreign visitors. There were however difficulties, again connected with communication when e.g. proofs were sent to me by mail. As a rule I did not receive them which could be (and was several times) a cause of substantial delay of publication. Not always. For example, in 1979 the Transactions of the American Mathematical Society published my paper with the following comment: *The author's original manuscript contained two more sections which the referee felt were not ready for publication. We asked the author's approval for the deletion but have received no reply. We are therefore publishing this part of the work: it should not be suppressed merely because of difficulty in communication.* I was extremely touched when I saw the paper published and read this comment.

With all the hardships, we still keep a lot of positive memories of the years of refusal. Our social life during these years was extremely rich. The refusenik community in Moscow was sufficiently well organized, with scientific seminars, Hebrew studies, women's groups, private kindergardens etc., although entering this life took some time. We also felt a lot of support coming from various circles ouside of the USSR, mainly from Western Jewish and scientific communities. The Moscow Sunday refusenik seminar attracted many visitors. We were able to listen to and communicate with a number of outstanding scholars. Let me just mention Arno Pensias, David Gross (both Nobel prize winners), Martin Kruskal, Simon Levin. Some of our friendly contacts established at the seminars, e.g. with Joe and Joan Birman, remained alive for many years.

I still keep four volumes of the Annals of the New York Academy of Sciences, edited by Joel Leibowitz, for a long time president of the academy, published after unofficial conferences organized by the seminar and its supporters. (It should be mentioned, though, that two of the conferences did not take place physically because foreign participants were refused visas to the Soviet Union). The professional level of the seminar was actually very high. There were a number of first class people among refusenik scientists regularly attending the seminar, like Solomon Alber, Mark Freidlin, Gregory Freiman, Yuri Golfand, Naum Meiman, A. D. Sakharov was also an often attendee of the seminar. As Joel Leibowitz writes in the introduction of one of the volumes, A. Pensias told him that it was one of his best audiences. The atmosphere at the seminar was always very relaxed and friendly. And for us it was the only place where you could tell what you were doing. This created a definite incentive to work and a sort of discipline as you had to tell what you were doing to people who were not specialists in your field. Of course, this could not replace normal professional life with the possibility of free and regular communication with close colleagues. Not surprisingly, quite a few could not return to science after years of refusal.

The authorities tried to cancel the seminar but probably did not have legal ways to do so. In 1980 Victor Brailovski, the host of the appartment in which the seminar met, was arrested on the pretext of his involvement in some unofficial publications and sent to exile for 5 years. We continued the seminar and KGB started simply to block the entrance to his appartment. Eventually it became a sort of cat and mouse game. They did not allow us into the appartement where we planned to meet, so we went to some other appartment and started the seminar a couple of hours later. As a rule they did not manage to reorganize and block the second appartment and the seminar took place anyway.

My work intensified substantially during the first years of my refusenik life. The attention and support I got from my friends and colleagues was extremely helpful. Jean-Pierre Aubin, Francis Clarke, Terry Rockafellar, Richard Vinter, Jack Warga sent me a lot of papers, preprints and books. I had probably the richest collection of optimization literature in the USSR that time! They also took care of many of my publications, including sending papers to journals, occasional proof reading etc. Not surprisingly, in some of my publications of that time you

will find something like "c/o ..." with one of these names where my address was supposed to be. In 1980 J.-P. Aubin and R. Vinter organized a two days meeting "Convex Analysis and Optimization" in Imperial College to support my cause (see Pitman Research Notes in Mathematics, volume 57, 1982). I got acquainted with Roger Wets, Philip Loewen and Szymon Dolecki who visited me when they were in Moscow and spent a lot of time talking with John Jayne and Loren Graham who were on sabbatical in Moscow. (Loren Graham, a professor of history of science in MIT and Harvard, was mainly interested in developments of physics and mathematics; his book "Moscow stories" gives an interesting picture of Moscow life that time; another his book "Naming infinity" could be even more interesting for mathematicians.) Although my mail was basically blocked, my "pocket" correspondence was very active. Through it I got acquainted with lots of people. In several cases the correspondence grew into future cooperation and friendship, like with Marian Fabian, Roberto Lucchetti and Jean-Paul Penot.

No less important for us was that quite a few of our friends inside Russia continued to keep normal relations with us. In my recent book I express my gratitude to V. M. Alekseev, S. S. Kutateladze, B. T. and G. M. Polyak, V. M. Tikhomirov and N. D. Vvedenskaya whose friendship and encouragement were absolutely decisive in our emotional survival, especially during the first couple of years. I would also mention A. M. Vershik and M. A. Krasnoselskii among those whom I continued to meet from time to time. We occasionally spoke about mathematics although there was of course no chance to work together. Still, the very possibility to be distracted from hardships that dominated our thinking had noticeable warming and soothing effect. Social life in Moscow of that time was concentrated mostly in private apartments and the tiny flat of Nikita Vvedenskaya was one of such centers where you could meet many interesting people. There was a circle of mainly Nikita's university friends that included V. Alekseev, B. Polyak and V. Tikhomirov but also R. Dobrushin, E. Paducheva, V. Uspenskii, A. Zaliznyak among others (all oustanding scientists and also lively companions and interesting conversationalists). It was there that I first met Louis Nirenberg in 1983. That year was one of the most difficult. Could I have imagined at that time that in less than 9 years my son will be postdoc in Courant Institute and one day we shall be sitting with Louis in a Russian restaurant in Brighton Beach!

My son, although he finished one of the best Moscow schools with mathematical orientation, did not have any chance to be accepted to Moscow University or any other prestigious institution of higher education. He was studying in an engineering school similar in the status of the one I was working in. Nonetheless he has managed to acquire some experience in mathematics. In particular, Mark Freidlin once gave a short home course on probability theory for several refusenik boys, and our son had a series of conversations with Mark afterwards. As a result, he had a draft of a paper by the time he was able to leave Russia that allowed him to be accepted for graduate studies at the Technion.

The story of his emigration deserves to be told. It was still unclear in 1986 when and whether we shall get permisson to leave. So he applied for exit visa with his

wife and one year old daughter and after two or three months was refused on the ground that he and his wife did not have relatives in Israel. It was a cynical and dishonest answer, I wrote to Gorbachev a letter of protest but received no answer (which was expected of course). So early in January 1987 I started a hunger strike. The support I got was really unprecedented. (I was told that the teletype of the President of the Academy of Science was basically delivering indignation and protests letters.) On the 19th day of the hunger strike I was invited to the Academy and informed that my son and his family would get permission to leave shortly. In February 1987 they left Russia and we followed them after 11 months.

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As I have mentioned, the first years of the refusenik life my work substantially intensified. Then after 4-5 years the work started to slow down and after 1983 and up to coming to Israel in 1988 I could not work at all. All my papers published till 1990 (at least their drafts) were written not later than in 1983.

My research that time was mainly concentrated on three areas: representation of measurable set-valued mappings, subdifferentials of nonsmooth nonconvex functions and (metric) regularity theory.

The main results relating to single-valued representation of measurable set-valued mappings published in 1978–1980 say that every measurable closed valued mapping $F(t)$ from a measure space T into a Polish space X can be represented by a certain Carathéodory mapping $f(t,u) : T \times U \to X$ (where U is some other Polish space) in the sense that $F(t) = f(t,U)$ for almost every t. Moreover, it was also shown that U and f can inherit some properties of F. For instance, if X is separable Banach and F is convex valued, then U can be a closed convex subset of a separable Banach space Y and $f(t,\cdot)$ can be linear bounded mappings $Y \to X$.

Clearly, these studies are connected with the problem of measurable selections of set-valued mappings. We dedicate a lot of attention to the subject in our book with V. Tikhomirov and I returned to it after acquaintance with a draft of "Survey of measurable selection theorems" by D. H. Wagner (SIAM Journal on Control and Optimization 1977). I wrote a supplement to the survey specifically devoted to what was done by Russian mathematicians in the 30s and 40s. (Actually, quite a bit. There were predecessors of the Kuratowski and Ryll-Nardzewski theorem and Castaing's representation theorem, as well as the famous Lusin-Yankov theorem about uniformization of Suslin sets.) The mentioned results in my papers do have some aesthetic attraction. They also imply many known facts about measurable set-valued mappings (e.g. Castaing's theorem). But I am unaware of any new applications. In any case, I have never returned to this subject. On the contrary, the other two areas occupy fairly central positions in today's variational analysis and I continued to work over various associated problems for many years.

A search for approximating mechanisms that could lead to necessary optimality conditions more selective than those involving Clarke's generalized gradients started right after the appearance Clarke's first publications. In my 1981 pa-

per in the Transactions I mention works of Halkin, Mordukhovich and Warga of 1976–1977. The basic idea behind my work in 1978–1979 was that for a non-smooth mapping between Banach space homogeneous set-valued mappings may provide a better quality of approximation than sets of linear operators. This led to a concept of prederivative introduced in the above mentioned 1981 paper. A similar construction was used a bit earlier by L. Thibault in his definition of a compactly Lipschitz mapping. But further use of it in our works was different. Lionel worked with sets of linear operators as a local approximating tool for such mappings that allowed him to extend Clarke's theory of generalized Jacobians to compactly Lipschitz mappings into Banach lattices. I on the other hand, used certain prederivatives as an approximating tool. The class of prederivatives I considered consisted of certain convex valued mappings I called fans. The structure of fans was sufficiently simple which allowed to develop a sufficiently rich theory that included inverse function and some extensions of Lyusternik-Graves theorems to Lipschitz mappings between Banach spaces.

There were also some rather unexpected applications of the theory of fans such as a totally new and fairly simple proof of the Hahn-Banach-Kantorovich theorem, or actually of a more general fact that the Hahn-Banach extension property for convex mappings from a Banach space into an ordered Banach space is valid if and only if any bounded subset of the range space has a least upper bound.

But very soon it became clear that the theory of limiting subdifferentials initiated by A. Kruger and B. Mordukhovich in 1976 offered a more precise instrument of optimization theory oriented analysis (although as it was understood at the very beginning of developments of the calculus of limiting subdifferentials, the existence of a compact-valued strict prederivative is needed to prove some workable calculus rules for Lipschitz continuous mappings). I learned about limiting Fréchet subdifferentials from B. Mordukhovich some time in 1977 or 1978 but was able to read his 1976 article only in 1979. It didn't take much time to understand that the limiting subdifferential does have a nice "one sided" calculus comparable with the calculus of Clarke's generalized gradient (and in some respects even better as it became clear later), so I put aside all other work and for a year or more fully concentrated on these studies. In a couple of years, when I first met Alex Kruger, I learned that a parallel work had been done by him. There were differences of course: he worked with Fréchet and limiting Fréchet subdifferentials in Fréchet smooth spaces while I used the Dini-Hadamard subdifferential introduced by J.-P. Penot in 1978 as the basic object and worked in arbitrary Banach spaces. By now the nonconvex subdifferential calculus is basically finished. I should mention also A. Jourani, M. Lassonde, B. Mordukhovich, J.-P. Penot and L. Thibault as main contributors to the subject.

When I look back, I see that the most important for my future work was not the calculus as such but its application to metric regularity theory. The 1979 paper in the Transactions of the American Mathematical Society (which I have already mentioned) was a principal step. It dealt with restrictions of locally Lipschitz mappings between Banach spaces to closed subsets in the domain space.

The metric estimates in our 1974 book and Robinson's 1976 paper are direct consequences of the main result of the paper but the latter was substantially more precise as it did not just declared that the mapping is metrically regular but offered an upper estimate for the rate of metric regularity in terms of generalized gradients of certain functions naturally associated with the mapping. A similar estimate (in a slightly less general situation) was obtained by Dmitruk, Milyutin and Osmolowskii in their famous 1980 paper in Uspehi (Russian Mathematical Surveys) which can be viewed in a sense as the beginning of "real" metric regularity theory: for the first time the metric nature of the theory and the equivalence of the surjection (linear openness) and metric regularities property were emphasized there.

More precise upper estimates involving nonconvex subdifferentials followed in the papers published in 1984 (Transactions AMS) and 1987 (Nonlinear Analysis TMA) and subsequent publications. In a short 1984 note B. Mordukhovich observed that in the finite dimensional case the estimates are precise. As was shown by A. Kruger, this is true also for mappings between Fréchet smooth spaces. Another development was a series of publications by A. Jourani and L. Thibault in the 90s with the introduction of the concept of graphic metric regularity, a property equivalent to metric regularity in the local theory and convenient in many situations as it allows to work exclusively with Lipschitz continuous functions. To complete the story, I would mention that, in turn, metric estimates close to (and in some important cases coinciding with) metric subregularity play an important role in the subdifferential calculus as they give the best qualification conditions for various calculus rules. This was first observed in my 1989 paper and then developed in 1996 by A. Jourani and L. Thibault, on the one hand, and J.-P. Penot and myself on the other (see also my 2017 book for more details).

5. **Back to normal life. The beginning**. We left Moscow for Vienna on January 22, 1988 and after a two days stop at Vienna arrived to Israel. It is difficult to explain today the kind of shock experienced by every person who never left the Soviet Union and suddenly was transferred to a place like Vienna. We were walking around with open mouths. All was different: the way people behaved and even were dressed, streets full of colors, so different from Moscow's uniformly gray residential areas, shops full of attractively looking goods (maybe the most shocking after the empty Moscow shops where for a pack of milk one had to spend half an hour in queue) etc..

Fortunately we were able to meet two good friends. John Jayne was there visiting IIASA (or maybe the university – I do not remember) and another of our dear friends, Alan Howard from London, came to Vienna with his daughter to meet us. I should have mentioned his name earlier. Alan was a successful lawyer and one of leaders of a group of British intellectuals called "Conscience" that was involved in various human right issues, the problem of refuseniks in particular. We met by chance late in 1976. He was visiting Moscow and somebody asked him to pass medicine to one of our friends who spent several years in prison for dissident activities. As he did not speak Russian, Alan was given my telephone number.

Somehow we almost immediately felt mutual sympathy and understanding – it was a unique experience in my life. Alan used to call me almost every month during all the years of refusal. Even when our phone was disconnected we managed to speak regularly. As I understand, Conscience supported the 1980 meeting in Imperial College organized by Jean-Pierre Aubin and Richard Vinter. Alan tried to come again to the USSR but was not given a visa. Only after 10 years in October 1986 he was able to come. And again it was fateful: I told him about my intention to begin a hunger strike and he was definitely a driving force behind the support I received during the strike. Curiously, when in a couple of years the Soviet Embassy in London had some legal problems with London municipality, it (the Embassy) turned to Alan's firm for consultation! We were able to meet only a few times after that: he died of cancer in 1992. I keep and occasionally open a book with Tennyson's poems he brought to me with a moving inscription ended with the famous quotation from Tennyson's Ulisse: "... strong in will to strive, to seek, to find and not to yield".

When we came to Israel, we were met by Zvi Zigler, a former dean of the Technion Mathematics Department. I actually applied to Technion in 1979 and got a position of Associate Professor shortly afterwards. I learned about that in a short while but was able to see the formal letter only already in the Technion. Zvi asked me when do I plan to start to work and I said – tomorrow.

I was indeed eager to resume to work but it did not take much time to realize that I had made a big mistake since the department was ready to free me from any obligations for the first semester. As a particular consequence, I did not have enough time for Hebrew lessons and my Hebrew remained not perfect to put it mildly, still noticeably worse than my English. There was also an additional thing I did not take into account. It was still a year and a half before the beginning of mass emigration from the USSR - nobody had actually an idea that something like that might happen, and rather few were able to leave Russia. So veteran refuseniks were in great demand and there was an avalanche of invitations, personal and official. Enough to mention that for a relatively short period (one year or so) I met, either personally or in a small group of former refuseniks, with Itzhak Shamir, the Israeli Prime Minister, Bob Hawke, Australian Prime Minister, the State Secretary George Schultz and Lionel Jospin, one of the leaders of the French Socialist party. I believe we have never traveled as much as during the first years after leaving Russia. I have very vague memories of this time, I must confess, and all events mix up in my memory. The feeling of constant tiredness was absolutely prevailing. But certain events I do remember up to small details.

My first morning in Paris in March 1988 is one of them. It was my first trip abroad (from Israel). I arrived late in the evening and was brought to the hotel by the person who met me at the airport. I did not have any idea about the location of the hotel. It was rue Monge as I discovered later. I got up very early, the weather was excellent and I decided to make a short run as I used to do. So I left the hotel, turned left and ran down the street. In a short while I reached boulevard Saint Germain and noticed a sign "rue Lagrange" at the opposite side of the boulevard.

I certainly continued there, ran up to the end of the street, turned right and in a few seconds stopped almost choked with happy surprise as I saw Notre Dame in front of me, just accross the river. This memory came very vividly back to me recently when I learned about the terrible fire in the cathedral.

Well, needless to say that I was permanently short of time. And although returning to mathematical work remained my first priority, things were moving ahead very slowly, much slower than I expected. I think that only by the end of 1993 I got a feeling that I have reached a reasonable degree of professional recovery. The possibility to attend conferences (of which the 1988 meeting in Erice organized by Franco Giannessi was probably the most memorable) and to speak to many colleagues was very helpful. Joint work with Frédéric Bonnans and Alex Shapiro in 1991-1993 strongly contributed to the recovery. We considered some very concrete problems relating to sensitivity and stability in mathematical programming when the nominal problem has a set of solutions containing non-isolated points. My contribution was largely technical, but probably this was precisely the kind of work I needed to do after years of detachment from mathematical research.

6. **Back to mathematics**. Starting from 1993 and on I was basically working with the following classes of problems:

(1) Metric regularity and transversality (certain aspects of the theory jointly: with A. Lewis 2008 (critical points of simple functions), J. Outrata 2008 (metric qualification conditions for subdifferential calculus), Y. Sekiguchi 2009 (regularity rates for convex multifunctions), D. Drusvyatskiy and A. Lewis 2015 (transversality and approximating projections), R. Cibulka and M. Fabian 2015 (primal estimates for regularity rates)), M. Fabian and J. Revalski 2018 (separable reduction of local metric regularity).

(2) Metric critical point theory (with E. Schwartzman and closely connected with simultaneous studies by J. N. Corvellec, M. Degiovanni and M. Marzocchi).

(3) Calculus of variational and optimal control (one of the key results obtained in a joint paper with R. T. Rockafellar).

(4) Variational analysis of special classes of objects (e.g. semi-algebraic, mainly with D. Drusvyatskiy and A. Lewis, polyhedral etc.).

(5) Subdifferential calculus (partly with J. Borwein and J.-P. Penot).

(6) Generic well-posedness in optimization (with R. Lucchetti and J. Revalski).

(7) Separable reduction of Fréchet subdifferentials (with M. Fabian).

Metric regularity theory (I prefer to interpret this as "metric theory of regularity" rather than "theory of metric regularity": regularity is a more traditional term referring to rich and beautiful classical theory, and "metric regularity" proper is just one of the three faces of the regularity property along with linear openness and Aubin's pseudo-Lipschitz condition) is definitely one of the most central chapters in the modern variational analysis. It is difficult to find a class of problems to which regularity theory has not been applied one way or another. Not surprisingly, practically every one working in variational analysis has been working with the theory or applied it.

For me it was a favorite subject since a very early stage of my mathematical life. My last return to the theory was stimulated by the conference dedicated to Lyusternik's centennial organized by A. Granas in 1999 in Bedlewo, small but filled with interesting discussions. Another and more concrete stimulus came around the same time from a preprint by D. Azé, J.-N. Corvellec and R. Lucchetti (the paper was published in 2002) who observed that the concept of slope introduced by E. De Giorgi, A. Marino and M. Tosques is a suitable instrument for estimating quantities relating to the rates of linear openness. Since then and up to completion of the book on regularity theory published by Springer late in 2017 regularity theory remained in the focus of my interests.

Among the publications that most affected my work on regularity theory, I would mention papers by Dontchev-Lewis-Rockafellar on the radius of metric regularity (2003), Robinson (1992) and Dontchev-Rockafellar (1996) on variational inequalities over polyhedral sets, Frankowska and Quincampoix on Hölder metric regularity (2012), Kruger on regularity of collection of sets (2006), Ngai-Tron-Théra on regularity of implicit functions (2013) and books by Clarke (2005) and Vinter (2000). Occasional conversations and communication with A. Dontchev, M. Théra and R. Vinter were very helpful.

My "trajectory" seems to be approaching the end. It is natural at the age of 80 and I take it as a natural inevitability, although still hope to be able to work for a few more years. My current interests and plans for the future are mainly connected with optimal control. I do not think I shall be able to return to any of the four last subjects, although there are still some questions there (especially in the fourth) that remained unanswered. On the contrary, I shall definitely appeal to the first two because they play fundamental role in the approach to the optimal control theory I try to develop.

The key element of the approach is a reduction of the original optimal control problem to one or a sequence of unconstrained minimization problems for Bolza-type functionals. It has already demonstrated its efficiency: I used it in 1997 to get the first proof of the maximum principle in optimal control of differential inclusions with nonconvex velocity sets. And in the very recent paper in the Journal of Optimization Theory and its Applications this result was extended to problems with state constraints and more general dynamics, perhaps so far the most general. There is also a yet non-published new second order condition for a strong minimum in the standard Pontryagin-type optimal control problem with state constraints. I also hope that it will be possible to apply the approach to study problems associated with value functions (e.g. Hamilton-Jacobi theory) and to build eventually a sufficiently rich optimal control theory on this new basis.

Alex Ioffe

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