N.G. KHIMCHENKO

From the "Last Interview" with A.N. Kolmogorov

I, for one, have followed all my life the precept that truth is sacred, that it is our duty to seek it out and to defend it, regardless of whether it is pleasant or not.

A.N.K., 1984

While I pursued a fairly wide range of practical mathematical applications, and at times obtained useful results, I remain predominantly a pure mathematician. I admire those mathematicians who became significant in technology; I fully recognize the importance of computers and cybernetics for the future of mankind; nonetheless I feel that pure mathematics in its traditional form has not yet ceded its deserved place of honor among the sciences. The only thing that could kill it would be too sharp a division of mathematicians into two tendencies: those who cultivate the newest abstract facets without account for their ties to the real world which bred them, and those who busy themselves with "applications," neglecting the need for in-depth analysis.

A.N.K., 1963

I consider my scientific career, in the sense of getting new results, to be completed. This saddens me, but I yield to the inevitable. In recent years I have been directing my energies elsewhere, on textbooks for secondary schools and books for the mathematically talented. I feel the desire to participate in this project with the vigor of youth.

A.N.K., 25 April 1986 (His next-to-last birthday.)

The great Russian mathematician Andreĭ Nikolaevich Kolmogorov was open and outgoing with friends, but rarely granted interviews; few direct records of conversations with him survive. Fortunately, the film-maker Aleksandr Nikolaevich Marutyan, in planning his successful 1983 film "Stories on Kolmogorov," tape-recorded long, wide-ranging conversations in which he explored areas of potential use for the film. After Kolmogorov's death in 1987, the unique interest of these tapes was recognized by V.M. Tikhomirov, with whom they had been left.

The enormous task of transcription of the fragmentary (sometimes incomprehensible) materials was undertaken by Natal'ya Grigor'evna Khimchenko. The full printed text she prepared circulated privately, and recently became available in the book *Yavlenie Chrezvychaĭnoe* (Extraordinary phenomenon) devoted to Kolmogorov.[1]

For presentation to the general reader, it seemed appropriate to sift the materials and put them in some kind of order. Fortunately for the mathematical public, N.G. Khimchenko returned to her labors, editing and organizing the text and translating it into English. Thanks also to V.M. Tikhomirov and Ya. G. Sinaĭ for advice, to Smilka Zdravkovska for great help in the final editing—and of course to A.N. Marutyan for conducting the interview in the first place.

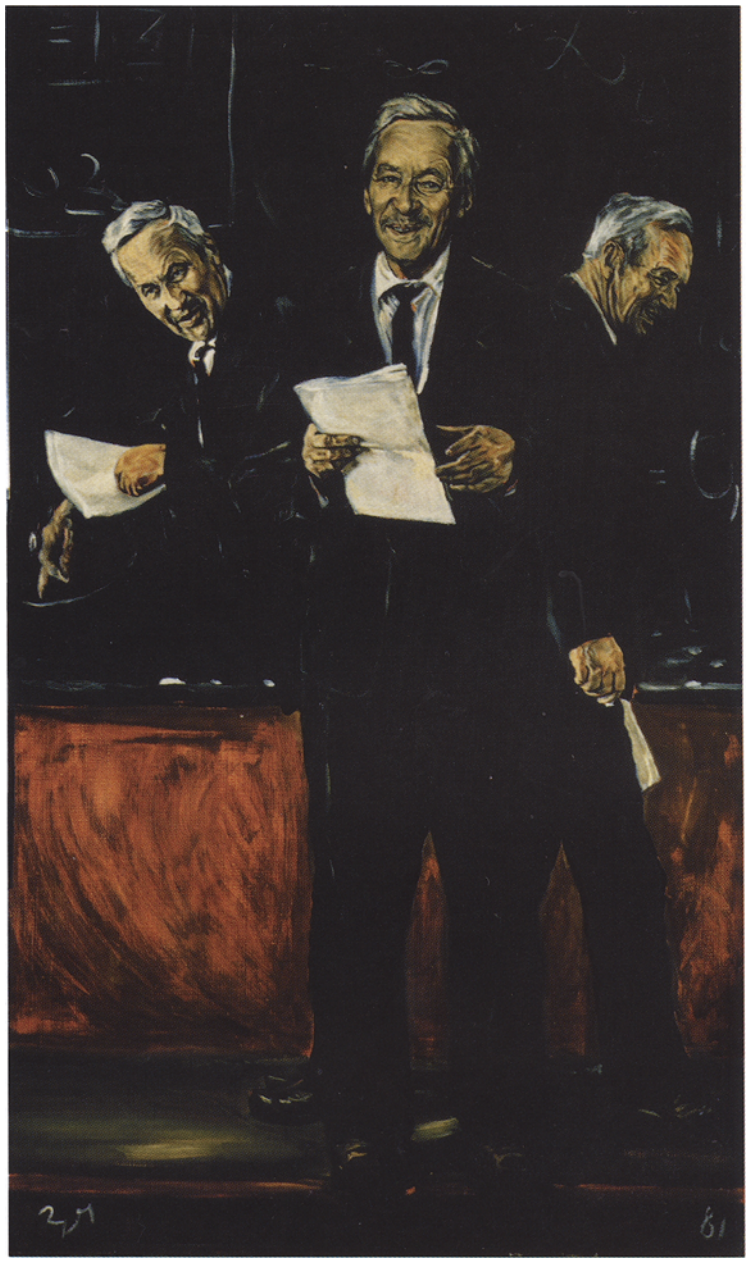
In the original interview, Marutyan often doubled back on a topic several times. The editing process, aimed at unifying and at reducing repetition, sometimes juxtaposed passages from different points in the tapes.—Editor's Note

A.N. Marutyan: Andreĭ Nikolaevich, I wanted to ask how you began your journey into mathematics, what influenced your choice of direction, your choice of specialty in mathematics.

A.N. Kolmogorov: It seems to me that for young mathematicians the most common scenario does not involve a *free choice* of direction, but rather an attempt to solve concrete problems presented by the older generation. This is the norm. My first works in trigonometric series were all of this nature.

Ideas of undertaking, let us say, a reconstruction of an entire branch of science arise at a later time. In my case, it was not that much later; namely, when I was investigating the basic tenets of probability theory, I aspired from

[1]Edited by V.M. Tikhomirov. FAZIS, MIROS, Moscow, 1999. See pp. 183–214.



View(s) of Andreĭ N. Kolmogorov by the famous portraitist Dmitriĭ I. Gordeev. (Used by permission of A.N. Shiryaev.)

the start to build a more logical *system* of the concepts of this whole science. This was in the early 1930s, when I was around 30 years old.

Generally mathematicians start from a certain catalogue of existing problems of interest to a given mathematical circle—the Luzin school in Moscow, for instance. And the young people struggle over the solutions. When they cannot solve one problem, they take up another. There are always plenty of those closest unsolved problems to choose from. At times an excessive insistence on solving precisely *one stated* problem is quite detrimental to a mathematician's career. The majority of mathematicians begin their work under someone's tutelage; the supervisor has many such unsolved problems, and his task becomes one of matching the young students with the most suitable problems. If success is long in coming, then he may suggest a switch to some other similar problem.

M.: How do you face the fact that you have worked all your life in a field which most people do not understand?

K.: Calmly . . . I think that the achievements of mathematics prove useful to mankind, while to us, mathematicians, they bring such inner satisfaction! It is a perfect solution, to have such a peaceful coexistence.

A friend of mine, a pure humanist, used to say that to him mathematicians were like useful domesticated animals [Marutyan laughs], that they had to be treated in a utilitarian way. To him, all true cultural values were humanistic; but, technology being necessary, mathematics is necessary, so we have to give mathematicians what they need to stay alive and keep going.

M.: Have you ever felt jealous of someone accomplishing the same task you were working on, or doing it more elegantly?

K.: No.

M.: Why? Because you didn't see them as rivals, or are you simply free of such a complex?

K.: Probably the latter. If a problem is solved—this is good and I am simply glad to know it. I understand that what I am saying sounds like copy-book morality. But looking back, I really do not recall such a situation of rivalry. Upon discovering that something I had been working on had already been done. I would feel a relief of sorts: Thank God, it's done! I am not boasting.

M.: You have probably felt that some of your abilities surpass those of your colleagues?

K.: Well, in some cases yes, in others no. Of course, when it does happen, the feeling is a pleasant one, I guess.

M.: Have you ever encountered ill-will because you were generally quicker, deeper, more talented?

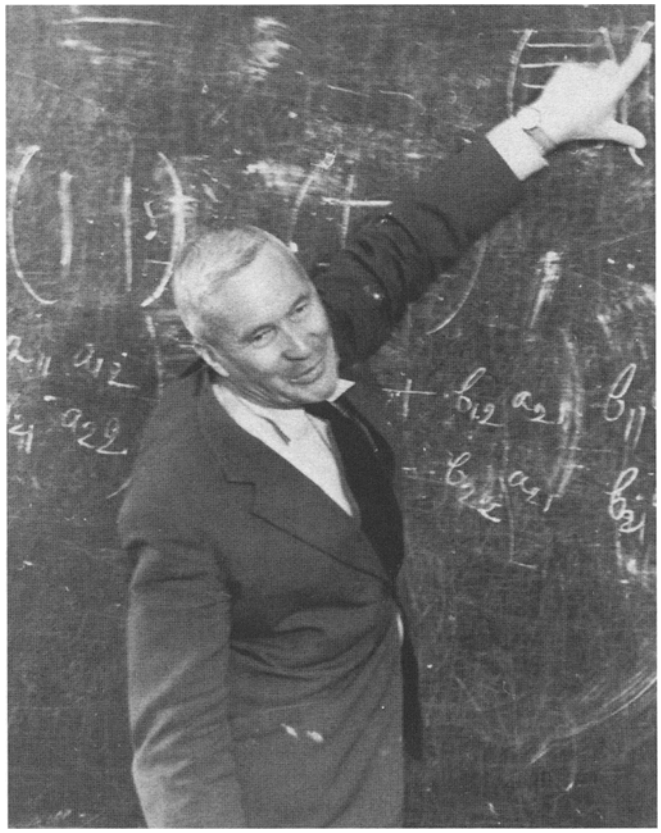
K.: No, I must say, I don't think I have.

M.: Would you say this is because of the fairness and objectivity of mathematicians?

K.: Mathematics is generally a reasonably objective science. The potential of a new idea to go far and to solve existing problems becomes evident fairly quickly. Mathematics is a tremendously pleasant field of work precisely because real progress is not lost, and is in the vast majority of cases acknowledged in time—to a greater degree than in any other sphere of human activity.

M.: But was this on your mind when you were choosing mathematics?

K.: That it would be a calmer life? [Laughs.] No, I was not thinking that. My final decision to opt for mathematics as my main field came fairly late, at a time when I had already produced works of my own. At that point it had become the clearly logical path. [Pause.] Until then I had been turning over several possibilities for myself. My first serious adult idea of a future career was forestry. I was also seriously interested in history.



A.N. Kolmogorov at the blackboard.

Especially, I was always extremely interested in working in general education.

I studied in an altogether extraordinary school, founded by two dedicated women, Repman and Fëdorova. And one wish that I have always felt is to concentrate on realizing a somehow *ideal* school. There was a rather long period when my greatest ambition was to be director of a school—

A.N. Kolmogorov with a hiking party. V.N. Tikhomirov is the man on the right.

I don't mean specially a mathematical school. This was the influence of my own experience as a student. The Repman high school was set up by a group of Moscow intellectuals specially for their own children. [Pause.]

I finally decided to become a mathematician only when I became convinced that that would *suit me*, whereas I had no idea whether anything else would work or not.

M.: You didn't want to risk trying out as a school director, eh?

K.: Right. But I was secretary of a school soviet for two and a half years. At Potylikh. Did you know about that episode in my life, the Potylikhin school?

M.: But Andreĭ Nikolaevich, that Potylikhin school was so experimental.... Were you also involved in the experiments?

K.: I wasn't *involved*, I was *carrying out* the experiments: the so-called Dalton plan. To this day I believe there was a great deal of good in them.

The scheme was, the teacher of each school subject, say mathematics, if there were 5 hours for it [per week], would tell about the subject, in an entertaining way, with demonstrations—but only for one of those hours. The remaining time the students would follow a monthly schedule of tasks: look at such-and-such a book, read such-and-such, solve such-and-such problems, find such-and-such a relationship and represent it graphically.

M.: What are your interests outside of mathematics?

K.: If I were to rank them, then after mathematics comes my interest in educating the young, in all fields.

As to prosody [the study of verse forms], which even among humanistic fields is a very special nook—that I regard as another branch of my scientific work. I am taken seriously there. I even served as *opponent* [external examiner] of the doctoral dissertation of the philologian Gasparov. My works are published; Zhirmunskiĭ valued them, and among foreign experts, Jacobson.

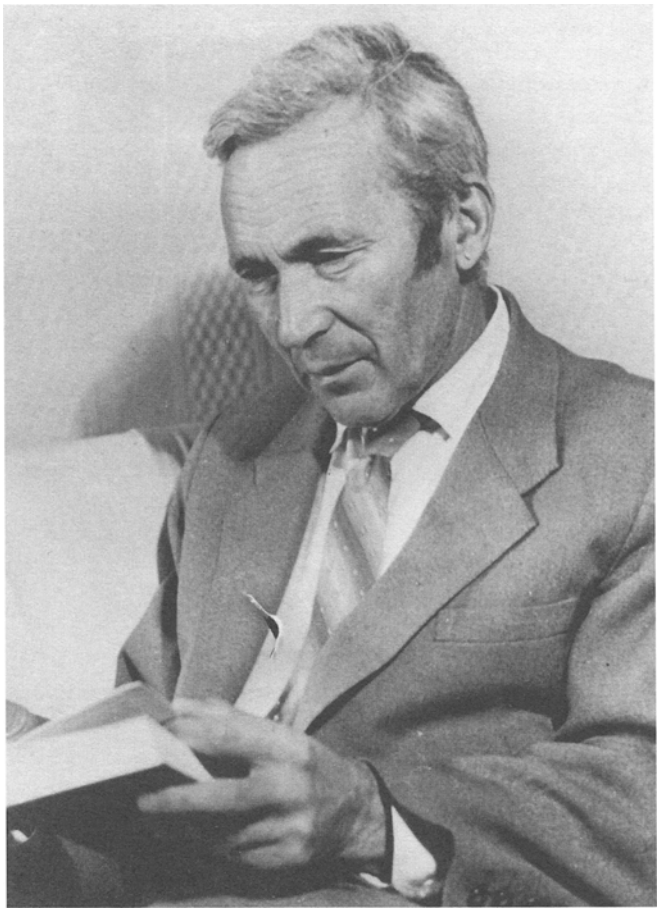
Among other interests, let me call them *consumer* interests (where I do not produce anything myself), I would name music and also early pictorial arts, especially Russian.

An indelible impression was made on me by my adventures in Russia's North. I would set off on such voyages, having found the location of old wooden churches in Grabar's *History of Russian Art*. From one church to the next I would travel alone, sometimes on foot, sometimes by rowboat, sometimes on board ship. The project would lead me to priests, who would often put me up for the night.

Traveling with a companion was also a favorite pursuit. In these activities people trust each other and fully open up. I traveled with Dima Arnol'd[2] for 20 days, with Igor' Zhurbenko. Earlier, it would be with my own age group, often with Gleb Selivërstov. He was one of my closest friends. One of my first works on trigonometric series was done jointly with him; he was something of a mathematician.

A few people were really close to me: Volodya Tikhomirov, Igor' Zhurbenko; and Il'dar Ibragimov, my friend in Leningrad.

M.: You were probably also attracted by the art world?



Kolmogorov in his study.

K.: No, I was attracted by art *itself*, but I rarely got in step, so to say, with the latest trends in our intelligentsia.

Once I was returning from Rome to Moscow, and found myself in the same car with Cardinal Wyszyński, who was returning from Rome to Częstochowa. And in the same two-person compartment with me was Archbishop Poznański. We talked for hours in German about Christianity and about non-religious humanism—of Thomas Mann, say.... But the Archbishop, naturally, took the position that true humanism not based on religion is impossible; I, naturally, set about trying to prove to him that on the contrary, faith in the eternal is not necessary for a positive human philosophy.... This went on pretty much all the way from Rome to Częstochowa, where though he hadn't reached Warsaw he had to get off together with Wyszyński. Right at the end of the conversation, my Archbishop took a tape recorder out from under the seat [Marutyan laughs] and said, "Herr Professor, I hope you have no objection to my having recorded our instructive exchange." [Both laugh.]

M.: So, a tricky agent, I'd say!

K.: I told him I had no objection; I don't think I had been saying anything terrible.... And we parted friends.

M.: Whereas I am openly taking a recording, Andreĭ Nikolaevich.

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[2]V.I. Arnol'd.

What attracted you in people—common interests, or were you intrigued by people from spheres distant from yours?
K.: No, in most cases I was drawn to people with similar interests to my own.
M.: How important in your life was your friendship with Pavel Sergeevich Aleksandrov?
K.: Very important. Although the difference in age between us was only seven years, in 1929 when we grew close this was still noticeable. I was the junior in our relationship, a sort of protégé or ward. And this tinge of patronage persisted all our life, completely accepted by me. Have you seen the little note Pavel Sergeevich wrote for my jubilee?
M.: Yes, certainly, and also the memoir he wrote.
K.: That little note about me he wrote less than a year before his death. You remember how he speaks there of this friendship which in the whole 53 years was always unclouded.
M.: People say of him, and he has said himself, that at the beginning of his mathematical career there was a time when things didn't go well, he got no results, and he was of a mind to give up mathematics.
K.: Yes, that's true. Nikolaĭ Nikolaevich Luzin, the teacher of both of us, took great pleasure in constructing hypotheses, which sometimes worked out and sometimes didn't, about who in his immediate circle of students should work on which problem and would get somewhere. In the case of Pavel Sergeevich, Nikolaĭ Nikolaevich got the notion—how, I don't know—that a famous problem for which there was then no known avenue of approach, the Problem of the Continuum, was going to be solved by Pavel Sergeevich Aleksandrov. With great insistence, and with the great persuasiveness he possessed, he planted this notion in Pavel Sergeevich's soul, where it led to such a crisis as to make him decide to leave mathematics.
M.: So you and Pavel Sergeevich had difficult relations with Luzin. . . .
K.: Yes.
M.: But just when you were beginning your independent work, you left the Luzin entourage and the two of you founded a new circle.

To what extent was your work with graduate and undergraduate students a creative process?
K.: Well, even when a seminar covers elementary material, and the teacher is already familiar with the solutions to all the problems, one has the challenge of putting oneself on the same level as the student.
M.: What role have your students played in your life?
K.: A very significant one, certainly. Emotionally, of course some were more important than others. Some are very close to me personally.

All my years of active work at the university would include two hours a week of some required course. I conducted many of these core classes: Theory of functions of a real variable, Functional analysis, Differential equations, Theory of probability. This was a common distribution of work for all our professors: one such required course, and a two-hour special course in lecture format addressing recent work, including one's own. Then normally there would be one or two seminars a week, in which about ten students would take turns presenting papers, but the instructor would speak more than the other participants. The students with whom one eventually undertakes individual work tend to come from these seminars.

A portrait that hangs in Komarovka. (Artist unknown.)

M.: Do you take naturally to collective work, or does it take away the joy of your own creativity?
K.: It need not. Maybe, with the critical problem of overspecialization, we must find new forms of collective work, finding ways somehow to divide up a problem and solve it *piecemeal.* Among my students, by the way, are *masters* of organizing collective work, with the ability to divide an area of research into pieces, overcoming the difficulties that this entails by constant contact, distributing the work among close colleagues: Izrail' Moiseevich Gel'fand, and now Vladimir Igorevich Arnol'd.
M.: Have you been successful in such collective work, or do you have more of a "loner style"?
K.: No, compared with them I did not find it easy to organize *large* groups. When I try to think of my work I carried out. . . . I must say that when I wasn't working entirely on my own I did best in a collaboration of *two.* This worked with Pavel Sergeevich Aleksandrov, with Boris Vladimirovich Gnedenko, with various collaborators.

Pavel Sergeevich and I collaborated intensively only once, on a fairly narrow question of topology; after that, only occasionally. I was interested in his works and he in mine, but a really shared project occurred only that one time.

The first people with whom I worked *closely and successfully* were Dmitriĭ Evgen'evich Menshov—on trigono-

Here is the list of Kolmogorov's students,* as found in V.M. Tikhomirov's article in *Golden Years of Moscow Mathematics* (ed. S. Zdravkovska and P.L. Duren), American Mathematical Society, 1993, pp. 125–127.

A.M. Abramov (education)
V.M. Alekseev (classical mechanics)
A.M. Arato (probability)
V.I. Arnol'd (superpositions, classical mechanics)
E.A. Asarin (complexity)
G.M. Bavli (probability)
G.I. Barenblatt (hydrodynamics)
L.A. Bassalygo (information theory)
Yu.K. Belyaev (stochastic processes)
V.I. Bityuskov (probability)
E.S. Bozhich (mathematical logic)
L.N. Bol'shev (mathematical statistics)
A.A. Borovkov (probability)
A.V. Bulinskiĭ (stochastic processes)
N.A. Dmitriev (stochastic processes)
R.L. Dobrushin (probability)
A.N. Dvoĭchenkov (theory of functions)
E.B. Dynkin (stochastic processes)
V.D. Erokhin (approximation theory)
M.K. Fage (functional analysis)
S.V. Fomin (ergodic theory)
G.A. Gal'perin (dynamical systems)
I.M. Gel'fand (functional analysis)
B.V. Gnedenko (probability)
O.S. Ivashev-Musatov (theory of functions)
A.T. Kondurar' (theory of functions)
M.V. Kozlov (stochastic processes)
V.V. Kozlov (probability)
V.P. Leonov (probability)
L.A. Levin (complexity)
A.I. Mal'tsev (mathematical logic)
P. Martin-Lef (complexity)
A.V. Martynov (probability)
R.F. Matveev (stochastic processes)
Yu.T. Medvedev (mathematical logic)
L.D. Meshalkin (probability, ergodic theory)
V.S. Mikhalevich (probability)
M.D. Millionshchikov (turbulence)
A.S. Monin (oceanology, turbulence)
S.M. Nikol'skiĭ (approximation theory)
A.M. Obukhov (atmospheric physics, turbulence)
Yu.S. Ochan (set theory)
Yu.P. Ofman (complexity)
B. Penkov (probability)
A.A. Petrov (probability)
M.S. Pinsker (information theory)
A.V. Prokhorov (study of prosody)
Yu.V. Prokhorov (probability)
Yu.A. Rozanov (stochastic processes)
M. Rozenblat-Rot (stochastic processes)
B.A. Sevast'yanov (stochastic processes)
A.N. Shiryaev (stochastic processes)
F.I. Shmidov (theory of functions)
Ya.G. Sinaĭ (ergodic theory)
S.Kh. Sirazhdinov (probability)
V.M. Tikhomirov (approximation theory)
A.N. Tulaĭkov (theory of functions)
V.A. Uspenskiĭ (mathematical logic)
I.Ya. Verchenko (theory of functions)
V.G. Vinokurov (probability)
V.G. Vovk (complexity)
A.M. Yaglom (turbulence)
B.M. Yunovich (theory of functions)
V.N. Zasukhin (stochastic processes)
I.G. Zhurbenko (probability)
V.M. Zolotarëv (probability)

*Someone whose name does not appear on this list of formal advisees, but who in effect was Kolmogorov's last student and is often listed as such, is the frequent Mathematical Intelligencer contributor, Alexander Shen.

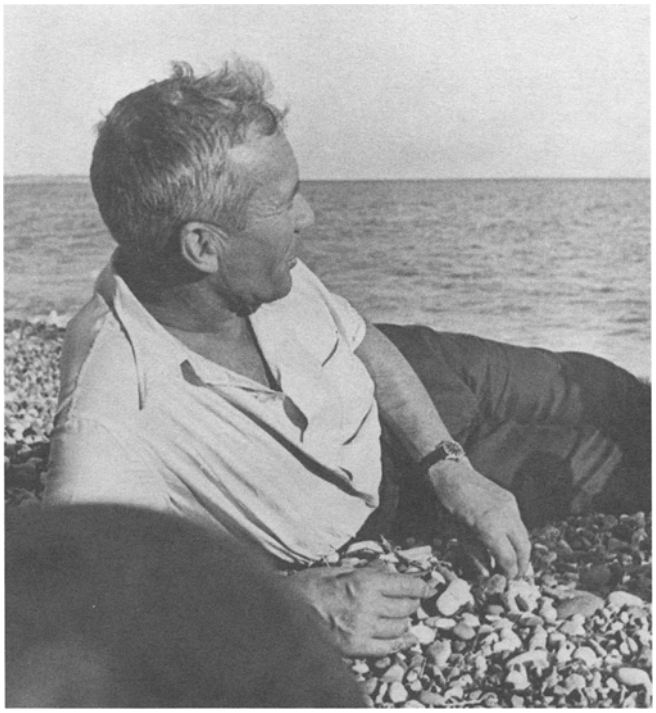
metric and orthogonal series—and Aleksandr Yakovlevich Khinchin—on application of function-theoretic methods, especially trigonometric series and orthogonal functions, to important aspects of probability theory. This led to joint publications with both of them. The collaboration with Khinchin was the most productive of all, because we got *really* important results: we found the criteria for convergence of random series, and conditions for applicability of the law of large numbers, and more.

Then I should mention the relatively brief period of working with Arnol'd. The so-called Hilbert 13th problem should really be counted as a joint result of the two of us. The decisive step was taken by Arnol'd, although all the foundation for this and related problems was laid by me. The particular problem stated by Hilbert was settled by Arnol'd; soon after, I was able to find a much simpler alternative solution, but it was published later. This happens often in science: a problem is solved in a roundabout way, and only later is a more direct approach discovered.

M.: Among twentieth-century mathematicians, whom would you name as the greatest contributors to the progress of science?

In the country house where P.S. Alexandrov and A.N. Kolmogorov held their famous gatherings for many years, there was a seminar room with a blackboard. Several years after Kolmogorov's death it had not been erased, and still had in his hand, in English, the motto

MEN ARE CRUEL, BUT MAN IS KIND.



A.N. Kolmogorov at the seashore.

K.: [Pause.] Hilbert, of course. . . . Hadamard. . . . After that it gets more difficult. . . .

M.: Did you pursue administrative jobs, or did they overtake you?

K.: I never pursued them. In some cases there was a feeling of duty, a belief that if I took on a task it would be better done. In the case of my deanship, for example.

M.: Do you think you were successful in this?

K.: To a degree. For our Department, at least, this was one of the better periods.

I never approached administrative duties with revulsion. When Otto Yulievich Schmidt asked me to join the Presidium of the Academy of Sciences, as the academic secretary of the physics and mathematics section, in 1939, I was straightaway interested. The underlying reason for Schmidt's bringing me in was this. The physics and mathematics section didn't give much administrative role to mathematics (there are many more physics institutions, and their material resources are much greater). At that time there were two physicists with every claim to leadership of all of physics: Kapitsa and Ioffe. So Schmidt took it into his head to set between them a very young mathematician! And it really didn't work out too badly. [Marutyan laughs.] When the astronomers had to select a location for a large geophysical observatory somewhere in the South, I visited the proposed sites—in private capacity, just as a tourist—to get a better perspective on them. My Academy work was only three years, 1939–42.

M.: Andreĭ Nikolaevich, you have said several times, if I understand correctly, that the distinction between pure and applied mathematics is not at all sharp. But in the popular image, applied mathematics is especially computers and computation, whereas pure mathematics is more abstract flights of fancy not always directly related to anything in the real world. Can you explain your point of view on this?

K.: [Hesitates.] The essential thing is, what is an *applied mathematician*? There is really no separate science of *applied mathematics*. An applied mathematician is a mathematician who knows how to apply ordinary mathematics to real problems. Thus a real applied mathematician is interested in the real problems of some related field. [Pause.] He effectively ceases to be a pure mathematician.

M.: So it all depends on the nature of the problem he is solving?

K.: Yes, and an applied mathematician working on something like ocean hydrodynamics is treating the study of *the ocean* with mathematical *tools*.

M.: This leads into another question: theoretical physics. I know that often mathematicians are very skeptical about theoretical physicists, because they apply mathematics in a "dirty" way. You personally, when you worked on turbulence—how far were you from the real problems of physics?

K.: I would first like to reply to your reference to "dirty" mathematics. You see, mathematicians always want mathematics to be as pure as possible, in the sense of being rigorous, proof-oriented. But generally the most interesting problems brought before us are not tractable in this manner. Then it is very important for the mathematician himself to be able to find, not a rigorous, but an effective treatment of the problem. Anyway for me this was always the way: if I am studying turbulence, then I am studying *turbulence*. If purely mathematical methods do not work, then I look into experimental materials, seek to discover in them a thread of coherence, and then proceed to make rigorous mathematical deductions, but starting from such entirely *speculative assumptions*. I for one value most highly this type of applied mathe-

The bust at Kolmogorov's grave in Novodevichi Cemetary.

matician, who essentially ceases to be a mathematician and simply solves problems of physics—if possible by rigorous "pure" deduction, but if that doesn't work, then by introducing assumptions.

M.: So you favor flexibility of thought?

K.: And where possible, participating in *experiment* with the physicists.

M.: In the course of solving a problem, have you ever thought about whether it was important? Did you have global goals?

K.: No. To be sure, in the *overall* planning of one's work, in choosing which new books to read, in combing the scientific journals to decide which works need closer study—there this rational element is present, must be present. But on the other hand, spontaneous sparking of interest in a hypothesis which just leaps into one's head can often be crucial.

M.: Has intuition played a role in your thought process, and to what degree?

K.: Of course, a very important role. This is very common for a mathematician.

M.: How do you work, Andreĭ Nikolaevich?

K.: Real scientific work? Usually it goes as follows: You read books, you prepare your own lectures with some new variations, and suddenly, an unexpected idea springs up out of the soil of this everyday work: what if the problem at hand can be solved entirely differently from known approaches? Another way becomes vaguely visible. . . . Then for a mathematician, and probably for any other scientist, it is overwhelmingly important to set aside everything else and simply think, think unrestrictedly on this new way which has just appeared. Fortunately I usually had the opportunity to do this.

There's a mathematician named Boris Nikolaevich Delone. You must have heard a lot about him.

M.: Yes, sure.

K.: When he would speak to students and they would ask him what is the essence of a scientist's creative work, he would answer like this: "Suppose you are in a mathematical Olympiad. They give you four hours to solve five problems (that's how it goes in the Olympiad). So you devote around an hour to a problem. Now imagine a problem which would take you not one hour to solve, but (say) 5000 hours of constant thought! Then you'll get some idea of what a real scientist does." Now no doubt Boris Nikolaevich was greatly exaggerating. For me, anyway, in the making of all of my scientific discoveries, such utter concentration, excluding all else, would last sometimes a week, maybe sometimes two weeks, but no more.

M.: What considerations led you sometimes to divert your attention sharply into new areas?

K.: I do not believe you put the question correctly, because the various fields of mathematics in which I have worked have usually led directly into one another, so that passing from one to another was *natural.*

In principle, you see—not with a conviction that I will undoubtedly achieve something, but out of general curiosity—*all* mathematics more or less interests me. When listening to some scientific lecture or reading something new, you inevitably try to figure: perhaps I could do something here?

M.: Andreĭ Nikolaevich, in the days of Archimedes or even Newton, the study of the surrounding world was accessible to any educated person.

K.: That is true.

M.: But now it is not sufficient simply to be educated. One may have understanding, and not that full, in a single field of theory. Do you feel that this condition is a natural result of progress, or is it a stage through which we will pass and perhaps once more return to some kind of common understanding?

K.: Are you familiar with Shklovskiĭ's *The Universe, Life, and the Mind*?

M.: Yes.

K.: He maintains that the development of every culture, if it is not aborted by some catastrophic events—and we all know what might befall humankind now—culminates in a stage of *loss of interest* in technology. Perhaps he really is right.

M.: What does "loss of interest in technology" mean? You mean that people occupy themselves more with humanistic problems?

K.: Not really humanistic *problems.* But it must be possible to return to a more basic and child-like *joy* in living.

Do you know the German writer Hesse?

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Natal'ya Grigor'evna Rychkova (married name Khimchenko) has been an associate professor in the Chair of Probability Theory at the Moscow State University Mathematics Department since 1964. She worked under the supervision of A.N. Kologorov on mathematical problems of linguistics and poetry. Currently she is much occupied with recovering and editing works left by Kolmogorov. Both interests are exemplified in the article "Analysis of the rhythm of Russian verse and probability theory" prepared from an unpublished manuscript of his, published in *Teoriya Veroyatnostei i eë Primeneniya* 44 (1999), 419–431.

M.: Yes.

K.: In *Das Glasperlenspiel,* Hesse depicts such a society, and quite brilliantly, I would say. A society which has lost interest in technological progress.

M.: What role has chance played in your life? [Both laugh.] After all, you worked on stochastic processes.

K.: I would be hard pressed to say. On the whole I believe that in a slightly different time, with a different form, still essentially what I was able to contribute to science would have been done if the distribution of roles had been different.

M.: In other words, if you had been surrounded by other people, worked in different circumstances . . . ?

K.: It is likely that the objective outcome would have been more or less the same.

M.: Andreĭ Nikolaevich, you know there will have to be music in this film I'm making. . . .

K.: Yes, certainly.

M.: And I'd like the music heard in the film to be something close to you. Do you have some favorite pieces?

K.: I hope there will be a place in the film where you tell about the musical evenings at Komarovka for our friends. Pavel Sergeevich and I would regularly have a good many guests for those occasions. At that point in the film I would like you to play Bach's Concerto for Two Violins.

M.: That was the favorite piece of you and Pavel Sergeevich?

K.: I think we had that in common. We would listen often to Mozart's G minor Symphony.