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Excitons: How we worked on the Volume

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1. Prologue: Instead of Introduction

In the former Soviet Union (USSR), we recognized long ago the existence of species that we used to call *Homo Sovietiko*. Actually, all people who were living in what was termed in the East as "the Socialist Camp", and in the West as "behind the Iron Curtain", were *Homo Sovietiko*, only to a somewhat different degree. This species was unique and developed because of the long isolation from the external world, just like the fauna of Australia. People of the USSR were the most isolated and therefore became the most typical of that species. We were neither better nor worse than other people, but the lifestyle and mentality of *Homo Sovietiko* really was somewhat unique. We had our specific problems, and everybody who had courage to come into close professional contact with us had to share these problems. In spite of all our problems, many physicists in the USSR did their best to retain their personal dignity and high professional level. As a result, we had rather good science. Many of our foreign colleagues wished to collaborate with us professionally, and we were eager to collaborate with them. It was difficult for us to participate in such collaborations and our foreign friends also had to pay for their participation. They turned into co-victims of our past.

This Volume is a *Festschrift* in Honor of Michael Sturge, a prominent scientist and a good friend of mine. We cemented our friendship as co-Editors of the Volume of review papers on excitons published in 1982 [1]. All Soviet contributors to the Volume, myself included, did our best to minimize troubles for the Western co-Editor. Nevertheless, we produced many problems for him. This paper is my *Repentance*, and I ask Michael for *Forgiveness*. I will tell here only about a small grain of the problems we brought upon him. To tell the story, I will need to speak not only about Michael, what seems to be most appropriate for the *Festschrift* honoring him, but also about us, the Soviet contributors to the Volume. To make the essence of those problems clear to a Western reader, I will need to speak about us even more than about him.

In the late 1970s, the North-Holland Publishing Company stepped forth with an attractive Project of a multivolume Series of monographs on Condensed Matter Physics. Volodya Agranovich and Alex Maradudin were appointed as General Editors of the Series. The Series aimed at bringing together condensed matter physicists of the East and the West. Each Volume had one Soviet and one Western co-Editor and was supposed to include contributions both from the USSR (and Eastern Europe) and from the West. In those specific

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geographic notions, Japan with its huge scientific potential was considered as "the Far West" and, therefore, Japanese contributors cooperated most closely with Western co-Editors. Agranovich was active in finding topics for new Volumes. He appointed Soviet Editors, and was very instrumental in settling numerous bureaucratic problems. Maradudin was a wonderful choice as Western Editor since, in addition to all his scientific merits, he had Russian roots, spoke Russian fluently, knew the Russian scientific community, and had many friends in the USSR.

In the fall of 1978, Michael and I were appointed co-Editors of the Volume "Excitons". I was excited since I read and appreciated some of Michael's papers and knew his excellent scientific reputation. I was eager to collaborate with him. However, I was also distressed because the invitation came just after I came back from the Edinburgh International Conference on Physics of Semiconductors. Although we had both attended the Conference and I was interested in Michael's paper, we failed to meet in person! (It could be irretrievable and we could never meet again). A *Homo Sovietiko* was not supposed to travel abroad. The normal state of a *Homo Sovietiko* was to be confined in the USSR and not to speak to foreigners too much. It was a huge privilege to go abroad for any scientific Conference even if expenses were covered by the organizers of the Conference. I had been very lucky in 1978! After nine years of confinement, I was allowed to go abroad twice, first to Copenhagen for a workshop in NORDITA and then to the Edinburgh Conference. This success gave me some deceptive hope, but in fact I was to be confined for eight more years. After that, I was also unable to invite Michael to Moscow. Because of our failure to meet in Edinburgh, Michael and I had to start working without any preceding personal contact and acquaintance. Also, we had no real hope of meeting during our collaboration. We had to work out some different ways of coordinating our efforts.

2. Merits of the Project

The Project proposed by North-Holland was outlined as a bridge between active researchers in the West and the East. At that time the gap between them was very wide. The American Institute of Physics was doing terrific job by publishing English translations of leading Soviet journals. However, everybody knew that circulation of Sov. Phys.-JETP could not be compared with that of Phys. Rev. An even larger disproportion existed between the circulation of Sov. Phys. - Uspekhi and the circulation of the best European and American review journals. It was believed that publishing the contributions of scientists from different countries *under the same cover* would promote mutual contacts and advance knowledge about the best research done in the USSR, Europe, and the US.

I need to explain here the origin of that gap and why we in the USSR anticipated that this joint Project could partially bridge it.

There existed different breeds of *Homo Sovietiko*. One of them, small in number, was allowed to go abroad anywhere and anytime. These guys were termed as "ezduny" in our slang and as "travel cadre" in the East Germany bureaucratic language. The second breed was allowed to go abroad only sometimes, mostly in groups, by mere chance or strong support. E.g., I was on the very bottom of the second group. At that time most of our scientists were not allowed to go abroad and, therefore, have never been abroad. They were "non-going-abroad-able", an English translation of our bureaucratic term "nevyezdosposobnyi". It was most difficult "to lose one's virginity", i.e., to go abroad for the first time. Believe me, I was very close to attending the International Semiconductor Conference in Prague, 1960, but … When foreign scientists visited our institutions, they had to follow programs prepared for them; so, only few people were able to speak to them. Special permission was needed to send a paper abroad, and in some institutions people were denied such permissions; the farther from Moscow the harder the problems. Publishing a review paper abroad was even more difficult. Because of increasing subscription prices, shortage of hard currency, and participation in the Copyright Convention by the USSR, in the 1970s most of the journals became available only in a few central institutions. Isolation resulted in a language barrier, and the barrier increased the isolation. It was a self-consistent process. In addition, our

manuscripts looked prehistoric: we had no computers, still used carbon paper, equations were inserted and figures were drawn by hand, etc.

The Project proposed by North-Holland solved some of these problems. The Soviet co-Editor extended formal invitations for writing papers and helped with formalities in Moscow. North-Holland provided special funds to pay for translating papers of Soviet contributors. It was a heavy burden of the Western co-Editor not only to express his opinion about the scientific merits of Soviet papers but also to check and improve the English. It was also the Western co-Editor who had to bring the whole Volume together and to deal with North-Holland. To keep a balance, the English edition of each Volume was supposed to be followed by the Russian edition published by "Nauka" (Science), the Publishing House of the Academy of Sciences. Editing of the Russian edition was a time consuming task left for the Soviet co-Editor, but cheap Russian edition was accessible to scientific libraries of the USSR and promoted knowledge about the advanced research performed in the West.

And the very last but not the least important point. Soviet contributors were paid by checks of the "Beriozka" shops, where they could buy goods not available in regular shops.

That is my outlook on the Project from the East. I cannot evaluate it from the Western point of view, but about 30 Volumes were published and, to the best of my knowledge, most of people who were invited to edit Volumes and contribute to them accepted these invitations.

3. We are co-Editors: joint approach to the Volume

I felt a responsibility to make a good Volume, and from my correspondence with Michael I had confidence that his intentions were also very serious. However, it was far from obvious what the notion of a good Volume would mean. An excellent review paper by Knox (1963) covered most of the real knowledge on physics of excitons available then. However, in the following 15 years knowledge increased drastically in this area. Excitons were discovered in new classes of materials, and exciton physics became a very diversified branch of solid state physics and chemistry. A multitude of theoretical approaches and experimental techniques were developed, a number of new exciton-related phenomena were discovered, and practical applications were predicted. The field could not be covered by a Volume of any reasonable size, and the size of our Volume was predetermined. We mentioned this problem in the Preface to the Volume by referring to the aphorism of a Russian literary personage, Koz'ma Prutkov: "Nobody can embrace the unembraceable". Fortunately, we both had similar ideas about the structure of the Volume.

We believed that exciton physics constituted a single field despite the fact that excitons are "three-faced like Hecate – there being three basic types of excitons (Frenkel, charge transfer and Wannier excitons)". Contrary to that diversity, different types of excitons show the same basic properties, and we felt that they should be treated from a unified standpoint. I cannot express this attitude better than Michael did it in his Introduction to the Volume: "Work on Wannier excitons has in the past tended to proceed almost in ignorance of that of Frenkel excitons, and vice versa. This is in part an unfortunate consequence of the artificial educational and semantic barriers which often separate chemists and physicists … One of the aims of this book is to emphasize this underlying unity of the subject". Most of the reviewers of the Volume appreciated our approach, but there was also some criticism. One of the reviewers considered such approach as old-fashioned and expressed a hope that the Volume was the very last attempt of that kind. Should the narrowing of the scientific scope be our task?

We also felt that the Volume should concentrate on modern achievements in active branches of exciton physics, where the most fundamental experimental and theoretical advances were being made. We did our best to avoid repeating material already elucidated in numerous review papers published previously and in textbooks. We also excluded a number of exciting subjects like surface excitons, the bulk of the research on molecular excitons, magneto-spectroscopy of excitons, exciton condensation into electron–hole droplets, etc., which were covered by different Volumes of the same Series

of monographs. Most of them appeared after our Volume.

The principles outlined in the two above paragraphs were hardly compatible. To combine them we needed an extensive introductory chapter covering the whole field of exciton physics in a concise manner, establishing connection between the different papers included in the Volume, and providing references to other sources. I was most happy when Michael proposed that he write the Introduction. Michael did a great job. That chapter became a concise, well balanced and contemporary review of different types of excitons and exciton related phenomena. We also provided contributors with information needed for making appropriate cross-references within the Volume.

4. Our Volume

A comparable, about 50–50, participation of Soviet and Western authors in different Volumes could result in severe unbalance since Soviet contribution to the solid state research was less than that from the West and was gradually decreasing. The enthusiasm of the short period of democratic changes and illusory hopes known as the "Khrushchev thaw" culminated in 1956. It was well behind us then, and we were sinking into the Brezhnev swamp. But for excitons, the problem was not critical because of a strong tradition of the theoretical and experimental research in this field in the USSR. Experimental research on semiconductors was concentrated mainly in Leningrad, on molecular crystals in Kiev, on rare gas solids and magnetic insulators in Kharkov, on alkali halides in Estonia, etc. However, not all areas of active research were represented in the USSR. Therefore, I felt that I needed to tell Michael what we potentially could contribute. My archive does not exist any more, but I remember that I did it. Michael worked in the Bell Labs. For me it was the Mecca of semiconductor physics since the invention of transistors. The reputation of Bell Labs in exciton physics had been established through papers by J.J. Hopfield and D.G. Thomas published around 1960. Michael had at his disposal a large part of the scientific contributors around the world, but the wealth of choice implied challenge. He needed to evaluate a number of different papers, talks at the March 1979 Meeting of the American Physical Society, etc., and to choose topics which were most appropriate for our Volume. Finally, we arrived at the structure of the Volume in which we had three areas of research covered by groups of papers, and a number of papers on specific topics.

The first group of papers was related to exciton polaritons, a very active field of research initiated by the seminal paper by Pekar (1957). The main issue were additional light waves and additional boundary conditions (ABC) which produced heated scientific discussions. A theoretical survey was given by Joe Birman who made a decisive contribution to the theory of the resonance Brillouin scattering by polaritons. Light scattering experiments allowed to prove in the most direct manner the existence of two simultaneously propagating modes and to find their parameters, while the transmission and reflection experiments allowed the investigation of the ABC. These two approaches were reviewed by Emil Koteles and Eugene Ivchenko, respectively.

Properties of exciton complexes, free and bound, were elucidated in the second group of papers. Free two-exciton complexes known as biexcitons show very different properties in direct gap semiconductors, like CuCl, and indirect gap semiconductors, like Si. Their spectroscopy is rather different. Narrow band spectra of direct gap semiconductors are an excellent candidate for non-linear spectroscopy and can be treated in terms of polaritons. In many-valley indirect gap semiconductors the competition between excitons, biexcitons and the electron–hole liquid was then the central issue. These two fields were reviewed by J.B. Grun et al. and V.B. Timofeev. Narrow emission spectra of multiexciton complexes bound to impurities were a matter of controversy for a long time. A detailed analysis of these spectra in the framework of the Kirczenov's shell model with a proper accounting of the degeneracy of the conduction and valence bands was presented by M.L.W. Thewalt.

Interaction of excitons with phonons, the subject of the third group of papers, manifests itself in a very different way in the weak and strong exciton–phonon coupling limits. Challenging

phenomenon of narrow-band multiphonon emission in the spectral region of fundamental absorption, typical for weak coupling to optical phonons, was reviewed by Serguei Permogorov. Experimental discovery of the coexistence of free and self-trapped excitons in several types of crystals was then the main issue in the exciton spectroscopy in the strong coupling limit. Ch.B. Lushchik surveyed exciton spectra of alkali halides. I accepted the invitation extended by Michael to review the theory of the self-trapping of excitons. It provided me with an opportunity to discuss different aspects of the self-trapping problem and also some of my former results (1957) which were then available only in Russian as well as our most recent results on the tunnel self-trapping of excitons.

The Volume also included three papers discussing "hot" topics in the spectroscopy of Wannier–Mott excitons in semiconductors. G.E. Pikus and E.L. Ivchenko outlined a theory of the optical orientation and alignment of free and bound excitons and discussed some experimental data. This method of investigating fast exciton dynamics was first used in the early 1970s and was actively applied at that time by different experimental groups, especially in France and the USSR. A.G. Aronov and A.S. Ioselevich presented a paper on the intricate subject of electro-optics of excitons, in which both the Coulomb attraction of the electron and hole and the Franz–Keldysh effect were consistently taken into account. The paper by R.J. Nelson reviewed basic developments in the spectroscopy of free and bound excitons in semiconductor alloys having important implications both in the physics of localization and in semiconductor technology.

Some new topics in the spectroscopy of Frenkel excitons were also included in the Volume. Y. Tanabe and K. Aoyagi reviewed spectroscopy of magnetic materials and showed how the existence of an additional degree of freedom, a generalized spin, affects exciton spectra. Piezo-spectroscopy became a powerful tool in the physics of semiconductors. V.I. Sugakov reported successful application of the uniaxial strain technique for finding energy spectra of Frenkel excitons. M.V. Belousov described application of a technique originally developed for electronic excitons to the reconstruction of the energy spectra of vibrational molecular excitons from experimental data. Dynamics of molecular excitons in systems of different effective dimensionality were reviewed by A.H. Zewail et al.

P.M. Pearlstein discussed the basic ideas and data related to the role of Frenkel excitons in biological processes. His "biological" definition of excitons was applicable to the systems where an exciton has no momentum and the excitation transfer is mostly due to hopping.

We felt that the Volume gave a balanced outlook on the physics of excitons because such basic issues as different types of excitons, their energy spectra and dynamics, coupling to the light and phonons, interaction of excitons, role of the defects and disorder, effect of the external fields and deformations, etc., were covered. Contributions came from the US, USSR, France, and Japan. Amongst the Soviet contributors there were senior scientists, Arkady Aronov and Gregory Pikus, who had been cut off from international contacts then, and a number of young scientists who still had none of their own international connections.

5. We speak by phone!

Michael and I easily came to mutual understanding when dealing with basic science. Curiously, dealing with simple technical problems was very difficult. I think, this was because we came from different social experiences and backgrounds. Michael was an Englishman while I was a *Homo Sovietiko*. I suppose that the change of British Prime Ministers influenced him much less than the change of Soviet Chiefs influenced us. Science is really great. It smoothes differences related to the social experience. They are more visible in small practical problems of everyday life, especially if people cannot meet in person. Apparently, Kipling's verses:

Oh, East is East, and West is West, and never the twain shall meet...

written well before we both were born, had some relation to us.

Michael's web page says that he graduated from the Cambridge University, received his Ph.D. there, and worked for Phillips Research Labs and Royal

Radar Establishment in UK before joining the Bell Labs in 1961. This is an excellent record for a bright young scientist from a democratic European country. All institutions are very famous and well known to everybody. I cannot add anything and I doubt whether any comments are needed.

We had something in common. In fact, we received our Ph.D. degrees at nearly the same time (1956/57). However, the beginning of my career was more turbulent than his, just as was our life.

I graduated from the University of Kiev in 1949. That time solid state theory flourished there. Solomon Pekar (1946) created a theory of adiabatic polarons; the term "polaron"came from him. Alexander Davydov (1948) proposed his theory of molecular excitons (Davydov splitting), which explained sharply polarized absorption bands discovered by Antonina Prikhot'ko (1944). Kirill Tolpygo (1950) developed a theory of lattice polaritons (the term came later) in ionic crystals. As a student, I benefited from that atmosphere. I worked on bipolarons under Davydov and on the scattering of polarons under Pekar. It was during the defense of my Diploma that Nikolai Bogolyubov learned about Pekar quantum theory of polarons. Their subsequent discussion resulted in a new version of Pekar theory and in the Bogolyubov–Tyablikov theory. However, my fate depended on the University authorities rather than on scientists. I was sent to a radar factory near the city of Taganrog where nobody needed me. Pekar and Bogolyubov failed in their attempts to change that decision and to enroll me as a graduate student.

After returning to Kiev, I got temporary jobs in two technical Institutes of the Ukrainian Academy of Sciences. The Institutes were involved in computing strains in dams. It was believed then that the more dams built on the rivers of the USSR, the closer we would come to Communism. In the evening time I was supposed to write conspectuses of Stalin revelations in linguistics; he discovered that languages are based on grammar and vocabulary. These conspectuses were prototypes of the notorious Chinese "red books" with quotations from Mao. It was my heresy that I preferred studying Landau and Lifshits books. Working on dams, I proved that the standard recipe for calculating gravitational strains fails catastrophically for *growing* elastic bodies and proposed a new procedure. It was applied immediately. However, in the fall of 1952 the atmosphere in the country grew even more gloomy, and the strongman of the Academy, the chief of the Personnel Department, ordered the Institutes to fire me.

For the next two years I worked as a teacher of physics in evening schools for young workers. In the day time I did research. I worked in isolation and had no access to the current scientific literature. Therefore, I needed to choose a subject where the scientific background acquired during my undergraduate student years was enough to make some independent research. I chose the self-trapping of excitons. Three most likely candidates as exciton substances were at that time Scheibe polymers (J-aggregates), benzene, and Cu_2O. I investigated the effect of the specific mechanism of the particle–phonon coupling (polar, non-polar) and dimensionality on the self-trapping pattern and proved that free and self-trapped excitons can coexist in 3D. The concept of coexistence contradicted the paradigms of the self-trapping theory of the time. Afterwards, I received my Ph.D. degree for these results. They were published in 1957.

In the meantime, Stalin died (1953) and the "Khrushchev thaw" had began. In 1954 Vadim Lashkarev got permission from the vice-president of the Ukrainian Academy of Sciences to appoint me as an engineer in the Institute of Physics. He argued that needed me for working on a Government Project in transistor electronics which he led. We were far behind Americans in this field, and the authorities were forced to hire at least a few able young people. Unfortunately, the general policy of hiring scientists mainly according political, social, national, etc., criteria rather than their professional abilities was not changed.

I worked on the theory of devices in the semiconductor department headed by Lashkarev. Supervision and advice of K.B. Tolpygo were extremely instructive for me. Lashkarev was a remarkable scientist and personality. Shortly before WWII he discovered the change in the sign of the thermo-e.m.f. across the rectifying layer in Cu_2O devices, i.e., a p–n–junction in them. Just after WWII, he investigated the bipolar diffusion and drift of photo-injected carriers and the mechanism

of the photo-e.m.f. in Cu_2O. He was allowed to go abroad only once in his life, to Bulgaria. Unfortunately, his best papers were published only in Russian and remained unknown abroad.

All the above implies that Michael and I had different social experience. This difference resulted in different pattern of behavior. I will tell only one story about a phone dialogue we had. It seems funny now, but was annoying then. To settle our problems, we needed to communicate. It took from one to two months for letters to cross the Atlantic. Sometimes they were lost totally. For short communications we used cables. In one of his cablegrams Michael proposed we speak by phone. Fast and convenient? Apparently, it seemed quite *natural for him*, but it was *inconceivable for me*.

At that time I worked in the L.D. Landau Institute for Theoretical Physics and lived in the Chernogolovka campus of the USSR Academy of Sciences, about 30 miles from Moscow. The campus was closed to foreigners. Even in 1988, at the peak of Perestroika and Glasnost, when the very first group of Americans came to Chernogolovka for an one-day seminar, their bus made a huge detour, apparently, to deceive "the enemy". The trick was so obvious, that Horst Stormer came to the blackboard and drew a scheme of the detour. I was in doubt whether my request for a phone conversation with a scientist in the US would be accepted at all, and there was no doubt that huge turmoil could arise. To speak to the US from my home phone? From Chernogolovka? In 1978? Crazy!

There was also a different problem. At that time we had no automatic phone connection – not even with Moscow. I had to request a connection and wait for hours, sometimes until the next day. When the connection was finally provided, audibility was usually bad.

So, I decided: Michael had invented this unlikely adventure, let him go ahead. I cabled him my phone number and expected (even hoped!) that his attempt would fail. I was astonished when the local phone company notified me about the scheduled time and connected me exactly on time. My family was awestruck: I spoke to America! The audibility was better than I expected. There was a different problem: Michael's fast English and my broken English were incompatible. We spoke for one hour. The most critical word in that conversation was "Timofeev" – the last name of our contributor from Chernogolovka. I understood it only after Michael repeated it five times. We both realized that phone communication was not for us. I was upset. This was a visible manifestation of the results of our isolation in the USSR. Later on I relaxed. The happy final came at the Party in Honor of Michael in Boston (November 2, 1998) when he told me that Bell Labs (not he) had paid that huge bill.

6. Our correspondence: "Damaged, but usable"

When the work on the Russian manuscripts was finished, we sent them to Michael in the safest way possible. For a couple of months we waited tensely. All but one manuscripts, one by one, reached Michael. The unlucky contributor had to prepare a new manuscript: to retype it, to redraw figures, etc. Finally, Michael received the manuscript. "Damaged, but usable", I read in his triumphant cable.

Our conclusion supplied a lot of work for Michael. Even with all the financial support from North-Holland, the English in our manuscripts was far from perfect. In the whole USSR, I knew only about three translators who were good enough both in English and physics to translate our papers by themselves. They were usually busy. To get satisfactory results we worked with the translators. Michael helped with the final editing of our manuscripts, and the contributors were highly grateful to him.

During the course of our joint work, I received from Michael a lot of lessons, including some in Western professional ethics. I wrote the first draft of the Preface to the Volume. Michael changed it considerably and sent it to me with his comments. Even in those places where only language changes were suggested, Michael avoided saying directly that my English was *wrong* or his English was *better*. He used to say only that the same concepts could be expressed in a *different* way.

Because Michael accepted the invitation to serve as the Western co-Editor of the Volume, I felt that he was working on excitons at that time. All his following activity confirmed this expectation.

However, I do not remember discussing any specific current research in our regular correspondence. Following Michael's papers, I concluded that his interests gradually shifted from spectra of magnetic ions, Jahn–Teller effect, bound excitons in nitrogen doped GaP, etc., to $Al_xGa_{1-x}As$ alloys and, especially, to physics of 2D electrons in Si and GaAs. I realized Michael's interest in the fast dynamics of excitons from his appreciation of the dynamic diffraction lattices techniques expressed in one of the letters. His papers on exciton dynamics started to appear soon after our work on the Volume and our regular correspondence ended. These papers, written with numerous collaborators, cover the extensive field of alloys, quantum wells, superlattices, quantum wires, and quantum dots, and include a lot of exciting physics: band structure, localization, excitons, radiative and nonradiative transitions, etc. They answer questions and pose new questions. Three questions on trions, which Michael posed in his short presentation at the Session of the EXCON'98 Conference honoring him [2], were based on recent research of his group.

7. The Index

I did not think then about the simple fact that the Volume needed the Index. I was surprised when Michael wrote that he received some support from North-Holland for a student who would help him in that work. Michael initiated a grand project. He prepared the *Author, Subject, and Substance Indices*. He wrote me that it was the Index that took most time. I could understand this only later when I worked on the Index for the Russian edition. One of the top managers of North-Holland told me that Michael's Index became a model for the whole Series.

The Russian edition was published in 1985. Russian contributors translated the papers of their foreign colleagues. Since the excellent figures in the Western papers made by computers did not fit our State standards, all figures were redrawn by "Nauka" Publishers. A manager from North-Holland was shaken. He could only murmur: "It is so expensive!" He did not understand *our* economics. Labor was cheap. The paper for printing the Volume was the issue. "Nauka" decided to print as *few* copies of the Volume as possible. I did my best to inform people who were interested in the Russian edition of the Volume that it would be available only by subscription.

North-Holland was gratified by the Volume. In 1987 they published a paper back edition of it in the North-Holland Personal Library [3]. This edition included 9 of the 18 papers published in the original (1982) edition.

8. We meet: Prayer Book of Ivan the Terrible

Finally we met. It was in Leningrad in 1987 when Michael came for a Binational USSR–US Symposium. After the Symposium he came to Moscow and gave a seminar at the P.L. Kapitsa Institute of Physical Problems. People from the Institute for Solid State Physics came for that seminar from Chernogolovka. After the seminar, Vladislav Timofeev and I had the pleasure of bringing Michael to Triniti-Sergii monastery, a historical place in Zagorsk near Moscow. When walking through the museum of the monastery, we came to an exhibit designated as a prayer book of Czar Ivan the Terrible. It appeared, there was nothing very special in that exhibit at the first glance. Ivan used to abdicate his throne and go to different monasteries to pray and meditate. All noblemen had to come and implore him to return. Those who did not come were usually executed. However, Michael noticed a fine detail. The date on the prayer book was about half a century after Ivan's death (1584). I translated the question to the guide who was looking at us with condemnation since our wispers annoyed her. She was astonished: "The original prayer book was lost. This is the copy. Your foreign guest is the very first person to ask me this question!" Michael's attention to fine details and anomalies revealed itself again!

9. Epilogue

I first came to the US in December 1989 for a US–USSR Conference in New York. During the week following the Conference, I visited several

laboratories on the East coast: Bell Labs at Murray Hill, IBM T.J. Watson Research Center, Brown University, and the Physical and Chemical Departments of the MIT. On a Friday, Michael came to the MIT to pick me up and bring me to Dartmouth College (Hanover) for weekend. My trip to the US, with all the troubles with formalities and the excitement of the scientific program, was coming to an end. In Michael's car I relaxed. We started to speak freely and discuss everything. Amazingly, English was no longer an insurmountable problem, and something surprising, really striking, happened to us. Differences coming from our previous experiences were gradually disappearing. We had much in common! I discovered Michael not only as my respected colleague and kind host, but also as my dear friend:

But there is neither East nor West...
When ... men stand face to face, though they come from the ends of the earth!

In Hanover I was Michael's guest. On a cold snowy December evening he brought me to his home and introduced to his wife Mary. There were excellent Russian books in their library. Mary asked me to read Russian poetry for them. It was surrealistic, actually inconceivable, to read Anna Akhmatova's and Boris Pasternak's poems in Russian in an American cottage in New England for an American couple of English descent:

A candle was burning on the table.
A candle was burning.

Magic poems crossed the Atlantic. They have outlived their authors and the persecutors of the authors.

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