Russian Journal of General Chemistry, Vol. 72, No. 4, 2002, pp. 493–494. Translated from Zhurnal Obshchei Khimii, Vol. 72, No. 4, 2002, pp. 530–531.

On My 70th Birthday

It is known that Buckminster Fuller never allowed anybody to write about him: He wrote himself. Why wouldn't I do the same but this once? Naturally, a person whose jubilee is being celebrated may not write his/her own personalia with enumerating awards. However, some kind of report on his/her side is quite appropriate, and the attitude of an editor prompts me to such an accost to readers. As known, each report is mainly concentrated on achievements. I hope mentioning them will not be taken for ego trip from which, believe me, I am very far.

Recent ten years have been challenging. Engagements widened after my election to the Russian Academy of Sciences. First, I was elected a member of the Division Bureau and the Chairman of the Scientific Council on Colloid Chemistry and Physicochemical mechanics of RAS, and, then, the Editor-in-Chief of the Journal of General Chemistry. In 1995 was elected (for three years) President of the Mendeleev Russian Chemical Society (MRCS) (I remind readers that our journal is a continuation of the Journal of Russian Physicochemical Society). Looking at the position of MRCS in the world, there was a wide field of action. In the end of 1995, I undertook the first official visit of the MRCS President to the headquarter of the American Chemical Society in Washington. In 1996 visited Japan to get acquaintance with the Chemical Society of Japan. In the same year, participated in the Assembly of the Federation of European Chemical Societies in Athens and, in 1997, in the world meeting of presidents of all chemical societies in Geneva.

As a result of activity, I can say that not only strengthened links of MRCS with the West, but also opened "east policy" of MRCS. When in Geneva, I was acquainted with the President of the Federation of Asian Chemical Societies (FACS) H. Ohtaki and drew his attention to the fact that Russia is also an Asian country. Japanese Ohtaki, however, considered Russia as a European country and reasoned this with the argument that our capital was located in Europe. Eventually, we agreed that an Asian MRCS office would be founded in Novosibirsk to maintain links with FACS. The way to the East was open. A concluding gala chord of my presidency in MRCS was the organization, in St. Petersburg in May 1998, of the 15th Mendeleev Congress on Pure and Applied Chemistry devoted to the 250th anniversary of national chemical science.

However, science and education have been and remain my main playground. First about science. For recent ten years, published three monographs: "Micellization in Surfactant Solutions" (Harwood Academic, Cambridge, 1997); "Interfacial Tensiometry" (co-authored with V.A. Prokhorov; Elsevier, Amsterdam, 1996); and "Physicochemical Hydrodynamics of Capillary Systems" (co-authored with V.V. Krotov; Imperial College Press, London, 1999). The first two issued both at home and abroad, the third only in English. Nothing to do if domestic publishers, because of their difficulties, are practically not in a state to invite scientists to the authorship. As for Imperial College Press, it suggested us to participate in their ambitious project of issuing textbooks and monographs of high standards.

When speaking about a scientist, they usually ask: "What has he discovered"? The question is reasonable. As is known, a scientist discovers and an engineer invents. However, we have a lot of engineers who invented nothing and a lot of scientists who discovered nothing. I hope I don't belong to that large group. According to patent experts, there are two kinds of discoveries: the discovery of new laws and the discovery of new phenomena. As a specialist in thermodynamics, I am just engaged in laws only. Almost every my work is establishing a new regularity and, therefore, a certain discovery. I even have never attempted to count as how many such discoveries have been done. Probably, the most important of them is the effect of charge sign in the phenomenon of nucleation, explaining the origin of atmospheric electricity.

Another matter is the discovery of new phenomena. I can count them on my fingers, and they all have been made collectively, since new phenomena are only discovered by experimentalists. A theoretician can predict a new phenomenon (still not discovered yet then, phase transitions in soluble monolayers of surfactants were characterized in detail in my monograph "Phase Equilibria and Surface Phenomena" in 1967). However, to predict does not mean to discover. On the other side, a theoretician can explain inscrutable facts, not understandable for an experimentalist, and create a theory of a phenomenon to become, in this way, a competent co-author of the discovery. I had to act in this role several times. As for my lab at the University, new methods of investigation were elaborated to discover a dramatically

1070-3632/02/7204-0493 $27.00 © 2002 MAIK "Nauka/Interperiodica"

increasing thickness of the surface layer of a liquid solution near the critical solution point and the temperature anomaly of the surface density of water.

An actual discovery implies a breakthrough in mind: an experimentalist observes something that seemed to be impossible. I illustrate this with two close-to-me examples. In 1968, G.M. Bartenev and L.A. Akopyan first observed the anisotropy of wetting (the dependence of the contact angle of a sessile drop on direction on a wetted surface) of strained elastomers. This is not permitted by the classical Young equation, and all become clear only as I managed to formulate the theory of wetting of deformable solids in the late 70's. In another work in 1988, A.M. Dyachenko, G.V. Berenshtein, and the author discovered the mechanochemical effect of the sign of elastic strain (for example, the rate of dissolution of a bent crystalline plate of KCl turns to be different at the convex and concave sides). The classical theory of elasticity rejects this possibility since, according to Hook's law, all strain effects are square-law and cannot depend on the strain sign. The contradiction was eliminated after the reformulation of the theory of elasticity and of mechanochemistry with accounting surface phenomena (see details in this journal, 2000, vol. 70, no. 3, p. 329).

Scientific discoveries were subjected to registration in USSR. In contrast with registering inventions, an author first published his discovery in open press (already informing the whole world about his discovery in this way) and then applied to the State Committee on Inventions and Discoveries for obtaining a document confirming that the author has indeed made the discovery. This meaningless bureaucratic procedure (as I remember, Mongolia followed it among other countries) was an element of that absurd system in which we lived and which, naturally, obeyed. After being elected People's Deputy of the USSR in 1989, I applied to the USSR Council of Ministers with a proposal to cease the registration of discoveries, and the corresponding decision was eventually adopted. Amusingly, that happened just at the moment when our discovery of wetting anisotropy was under registration. Similarly to the case with GKCP-premier V.S. Pavlov, it would be appropriate to publish a commentary "Has not timed" about me (V.S. Pavlov ordered to reduce the allowance of food for prisoners just before he was arrested himself). The Russian Academy of Natural Sciences now tries to reanimate the unnecessary registration of discoveries and even gives makeshift diplomas (naturally, of no official validity). Such acts seem to be consequences of troublous imposture period which, I hope, is coming to an end.

The above discoveries are in the past. Being asked whether or not it is possible to discover new phenomena now, I would reply "possible, but with very low probability." The potential ability of making discoveries is mainly determined by the laboratory equipment. Because of currency deficiency, the backlog of our instrumental base began since the middle of the past century and now takes a catastrophic character. Another factor is the outgo of clever people from national science (my co-authors on discoveries are not an exception). Still Albert Einstein said: "Science is a wonderful thing if one does not have to earn one's living at it." The situation with the educational activity is scarcely better: it also requires equipment (and reagents). It is hard to mention a Russian institution able to prepare a chemist satisfying all necessary requirements (our students have never seen many modern apparatus). In addition, the salary of academicals is so low that young people have no desire of working at the high school (according to the opinion poll, only 5% of students agree to stay for further work even at the Moscow University). Grants do not save: they are small and granted to a chosen few.

Since 1987, I have headed the Chair of Colloid Chemistry at the St. Petersburg University. Thankfully, the small collective, including three labs and nine professors, still works efficiently. What is it all about? I can say that eight professors of nine are of pensionable age, and we have no other place to go to. But I add that it is of interest for us to be engaged in science and to stimulate young people (in particular, with our own patterns) to be interested in science. As for money, we have always worked not for it, certainly, having a necessary minimum. I guess the example of our chair explains the paradoxical fecundity of the whole Russian chemical science. If a chemist has a head, the chemist will find what to be engaged in, at any circumstances. Evidence for this is given by the high scientific activity of the authors of this journal and many manuscripts in editor's hand. I send cordial greetings to all the authors and readers of Journal of General Chemistry!

A.I. Rusanov