Chapter 16
Publishing in a Divided World

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16.1 Some Personal Recollections About Publishing During the Soviet Time

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What you will read here is not a historical review, but I like to share some personal reflections which I have written down for this book on request of the editor. Now one remembers the Soviet time already as something unreal, and the only idea which comes up is the wish that this will never happen again.

I remember my first attempt, still being a student, to see a Western chemistry journal, and my surprise when I was told in the library that I need a special permission for this. This was at the beginning of the 1950s. For many years, practically until the *Perestroyka* (middle of the 1980s), science in the West and in the USSR was developing on parallel roads, without much interaction. Just as an example, the ideas about the noosphere came up independently on both sides, sometimes with time gaps of several years. Privileged were those people who had access to information from the West.

Western scientists usually did not know much about what was done in the USSR: only a few could read Russian, and for us it was very difficult to publish abroad. Further, in USSR, the scientists had a rather limited knowledge of foreign languages, and it was necessary to get special permissions for the legal publication of papers in

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foreign journals, which normally took on average half a year. I will give here only one example from my own life: In 1990 I met in Australia Dr. Florence who is famous for having introduced what is now known as the "Florence electrode." This electrode (and methodology) played a revolutionary role in stripping voltammetry. We had a long discussion. Meeting him was very rewarding and pleasant. I still remember it as very enjoyable. When we departed, I gave him a reprint of a paper where we have given a very detailed account of the mechanisms and the methodology of working with such electrode, published in the journal "Elektrokhimiya" in 1969 [1]. The paper has been submitted on October 26th 1966, i.e., 3 years before printing! The paper of Florence [2] appeared in 1970. Dr. Florence was very surprised, expressed his thanks and we departed as friends. This example shows how Soviet scientists suffered from extraordinary long review and publication times.

Despite the fact that translations of the main Soviet journals started to be published, on both sides of the divide did not develop a habit to receive and read each others work. By the way, this tendency has not yet been overcome on the Western side, which can be easily seen by comparing the quoting of the same scientists in Western and Russian journals and also from the impact factors of the journals. This is very unfortunate for both, the Soviet and Western scientists. Probably, science would have developed much faster and perhaps more fruitful if it were not for these limitations. I remember a conference on polarography (First All Union Conference on Polarographic Analysis, Kishinev, 1959) where the first talks were given on stripping voltammetry. The authors of the talks were A. G. Stromberg, Ye. N. Vinogradova, and Yu. I. Vaynshteyn. They have shown a peak of lead after its accumulation on a mercury hanging electrode, and the term "nakoplenie" (Russian for accumulation) was introduced as a new term (in Russian, stripping voltammetry is frequently called "voltamperometriya s nakopleniem," i.e., voltammetry with accumulation). So it was, although the papers of Barker and Jenkins [3], Kemula and Kublik [4], Rogers et al. [5], and Delahay et al. [6] have been published before, these were not known to our scientists at that time.

16.2 Recollections of a Bulgarian Scientist

Alexander Milchev

I graduated in Chemistry at Sofia University "St. Kliment Ohridski" in June 1968 and immediately joined the Institute of Physical Chemistry at the Bulgarian Academy of Sciences (BAS). At that time, the Director was Rostislaw Kaischew, professor and regular member of BAS, the first student and coworker of the famous physical chemist Iwan Stranski. Both Stranski and Kaischew laid the foundations of the Bulgarian School in the field of crystal growth phenomena.

My first scientific paper was based on the results of my diploma work and appeared in English, in *Comptes Rendus de l'Académie Bulgare des Sciences* in 1969. How did we publish our scientific studies at that time is certainly an interesting question and in what follows I will provide some information on this subject.

The first, most important and, sometimes, dangerous step was the presentation of the scientific results at the Institute's Seminar. We called it *Colloquium* but if one thinks that it was just a simple *preliminary oral examination* as written in the dictionaries for this word it would be a serious mistake. The audience consisted of the scientific staff of the Institute of Physical Chemistry with Rostislaw Kaischew at the first row and behind him his first students and coworkers: Alexey Scheludko, Jordan Malinovski, Evgeni Budevski, and Georgi Bliznakov, professors and later also regular members of BAS. They were all extremely clever, intelligent, and competent not only in electrocrystallization but also in other fields of the physical chemistry science. What followed was the rest of the Institute's staff consisting of younger chemists and physicists with different scientific degrees. Often we had also guests from other Institutions of BAS and different Universities, and I must say that this was the most competent and, also, the most merciless audience that I have ever met!

The speaker was interrupted with questions, comments, and critical remarks and the procedure was the same for everybody, does not matter whether male or female and communist party member or not! I remember how once a colleague of mine getting nervous of this critical atmosphere exclaimed: "Would you let me finish my presentation and then I will answer all your questions?!" "No"—Kaischew said—"impossible, and if you don't like this, it is entirely your problem."

After about 1 hour, it was clear to everybody whether the results must be published or not and, also, whether all or part of them had to be patented beforehand. We did not vote. Kaischew just summarized the Seminar decision, and the speaker was allowed to go and lick his/her wounds.

The next step was sending the manuscript in the language it was written together with a Bulgarian translation to the Security Department of BAS. After about a week or so the corresponding author was informed about the final decision and if it was positive he/she was provided with an official permission to send it abroad. Without such a stamped certificate, no post office would accept it. Of course, all this refers to the publication procedure accepted at the Institute of Physical Chemistry of BAS, and I am not sure whether the situation was exactly the same in other Institutes of BAS, as well as in Sofia University.

Writing all this, I got interested in how many scientific papers I, myself, published and submitted for publication within the period 1969–1989. The result was 1 patent and 41 papers—3 of them in Bulgarian, 3 in Russian (2 with English translation), and 35 in English (1 in a Bulgarian and 34 in western journals, among them Electrochimica Acta, Journal of Electroanalytical Chemistry, Journal of Crystal Growth, Thin Solid Films, etc.). I suppose that readers might also be interested in how many times those papers were cited in the international scientific literature because the number of citations is, in my opinion, more indicative than the number of papers published. The answer is 1263 till now and I think that for my field of competence this is not a bad result.

Concerning publishing original scientific papers in Bulgarian language, I should say that nowadays it is, perhaps, not worth doing that, unfortunately. The main reason is that it makes sense presenting our scientific results in a readable, understandable, and, also, usable way, and it seems that English language is more suitable for the purpose. Just a simple example in support of this statement: In my case, the average number of citations per article is approximately 31, and my first scientific paper, which is published in English, in a Bulgarian journal, is cited 29 times, which is close to this value. At the same time, the three papers, which I published only in Bulgarian language, were cited 23 times altogether, although their scientific level is, in my opinion, even higher. I wonder if scientists from the English speaking countries realize what a great advantage they have nowadays.

Of course, if one day I decide to make a contribution to the *belles-lettres*, it will be in my own Bulgarian language!

References

1. Royzenblat EM, Brainina KhZ (1969) Elektrokhimiya 5:396
2. Florence TM (1970) J Electroanal Chem 27:273
3. Barker GC, Jenkins IL (1952) Analyst 77:685
4. Kemula W, Kublik Z (1956) Chem Anal 30:1005
5. Lord SS Jr, O'Neill RC, Rogers LB (1952) Anal Chem 24:209
6. Mamantov G, Papoff P, Delahay P (1957) J Am Chem Soc 79:4034