The 'First All-Union Conference on Automatic Control', Moscow, December 1940

Journal Item

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By Christopher C. Bissell

It is often claimed that classical control was a product almost entirely of the wartime laboratories of the United States and, to a lesser extent, the United Kingdom. World War II, however, was not quite such a watershed as is often claimed. Even before the war, in a number of countries, process control was moving toward an understanding of three-term (proportional-integral-derivative) control. And there were various pockets of expertise where the "communications approach," deriving from Nyquist and others, was being applied to control systems. For example, by the end of 1940 many of the fundamental building blocks of what came to be known as classical control theory were fairly widely known in the USSR. In particular, a conference held in Moscow in late 1940, but little known outside Russia, included papers on and discussion of: the Nyquist criterion; operator methods; transient response; the use of the operator $\exp(-Tp)$ to model a time delay of T seconds; and the $|z| < 0$ criterion for the stability of a sampled-data system.

Conferences held during the early days of the development of a discipline are an important source for historians of technology. In the case of control engineering, the early conferences held in Western Europe and the United States are comparatively well documented. In most cases the published proceedings are still fairly easy to locate in libraries [1], and they often include detailed discussion of the papers presented (which in some cases is of greater historical interest than the papers themselves). The situation regarding the former Soviet Union is rather different, however. Published proceedings rarely included discussion, and the background of the conferences themselves was often intimately bound to the contemporary demands of the Communist Party, and the general scientific-political environment, in a way very different from the political context in the West [2]. The latter points were certainly an important aspect of the Moscow 1940 meeting.

Background

In later Soviet literature, the conference held in Moscow in December 1940, often referred to as the "First All-Union Conference on Automatic Control," was portrayed as fundamental for the subsequent development of the discipline. It was, we are invited to infer, a stimulating forum for the exchange of views on the emerging discipline and for the forging of fruitful professional relationships. For example, a historical paper at the "Second All-Union Conference," held in Moscow 15 years later, suggested a well-focused meeting with clear significance for the future:

> At this conference [delegates] took stock of the whole range of activities in the field of automatic control during the pre-war period, and planned new directions for further theoretical development. [3]

The distinguished Russian control theorist M.A. Aizermann, writing in 1975, was considerably more enthusiastic:

> The second factor which played an important role in the establishment of control theory in the pre-war period was the First All-Union Conference on Automatic Control in 1940. It was a comparatively modest affair—fewer than a hundred participants—but it allowed, for the first time, the meeting of those working in various sectors. In particular, the strength of two existing schools was demonstrated: the recently established Institute of Automation and Remote Control [in Moscow], and the Leningrad school centred around I.N. Voznesenskii at the Central Boiler and Turbine Institute. On the whole, the conference attracted a young audience—postgraduate students, many of whom were destined subsequently to lay the foundations of Russian control theory. Here first contacts were made and relationships forged (albeit interrupted by the war), and without this conference the whole subsequent development of control theory would undoubtedly have taken a very different path. What is more, it was at precisely this meeting that we got to know Aleksandr Aleksandrovich Andronov [4], who had come from Gorkii [Nizhnii Novgorod] in search of new applications for the theory of oscillations; the unusual power of his fiery talent immediately became apparent. [5]

Now, while there is certainly truth in such accounts, they completely fail to reflect the atmosphere and background of the 1940 conference, which needs to be viewed in its sociopolitical context—in particular, in light of an acrimonious dispute surrounding the work of the Institute of Automation and Remote Control. This article, based primarily on documents held in the Archives of the Russian Academy of Sciences in Moscow [6], will attempt to do this.

The Attack on the Work of the Institute of Automation and Remote Control

The conference was held at a time when the activities of the Institute of Automation and Remote Control (IAT) [7], and

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particularly the work of G.V. Shchipanov, N.N. Luzin [8], and V.S. Kulebakin (then the Institute's Director), were under investigation by a special commission of the Academy of Sciences. Briefly, these figures and some of their colleagues had been accused of "pseudo-scientific" research (the pejorative term often used in such Soviet disputes at the time); the investigation lasted from March 1940 to May 1941, at which time both the Institute and the individuals concerned were heavily censured [9]. (Kulebakin lost his position as Director, and Shchipanov was transferred to another institute.) In 1959, however, this work was officially "rehabilitated" and spawned a whole series of conferences on "invariance" theory [10].

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According to one of the documents in the file of the investigating commission (a lengthy, highly critical report by M. Granovskaya written in summer 1940 [11]), a conference on the theoretical aspects of control had been proposed at a meeting in Kharkov as early as March 1939. Initially the Institute of Automation and Remote Control had planned to organize this for autumn 1939, but for one reason or another —perhaps the worsening political atmosphere—this was postponed for over a year. Indeed, Granovskaya seized on this delay to make a further criticism of the Institute: "It seems that the Institute is deliberately dragging its feet, fearing legitimate criticism of its 'theoretical' work..."

One particular problem was the relationship between the IAT in Moscow and the Central Boiler and Turbine Institute in Leningrad (TsKTI). Relations at the time of the conference appear to have been tense, at least as far as those IAT researchers who had been singled out for most criticism were concerned. One of the IAT's major critics—and perhaps even the originator of the criticism—was I.N. Voznesenskii, the leading light of control theory in Leningrad referred to by Aizerman. Voznesenskii appears to have been a formidable character of the Soviet old school, although it is never easy to know quite how to interpret statements like the following:

> Ivan Nikolaevich took an active part in public life ... . In 1936 he was elected by workers at the Polytechnic Institute to the District Soviet, where he was active in his sections ... he also worked as a member of the Presidium of the VNITO mechanical engineering plant. ... He understood perfectly the role and responsibilities of a Soviet scientist ... Ivan Nikolaevich was a solid character, a public figure both passionate and steadfast in his views. His fundamental characteristic was always to follow his highest feelings as a Soviet patriot. From this stemmed such traits as adherence to principles, stoicism, a sense of duty, and unshakeability in his decisions and convictions. [12]

After her husband's death, A.M. Voznesenskaya recalled his criticisms of the Institute of Automation and Remote Control:

> Ivan Nikolaevich struggled against this erroneous initiative of the 1930s both inside and outside the Institute, throwing himself into the fight with all the conviction of his personality. [The work of the IAT researchers] aimed to strengthen their position regarding the role of mathematical computation; precisely because of this, their work lost sight of the essence of the phenomena under examination, and met with severe criticism from Ivan Nikolaevich. His criticism, however, for all its harsh words, was very kindly meant towards the researchers. Time after time he took the trouble to set out all the mathematical arguments and computations; he checked the conclusions and proposals, and he repeatedly suggested to the researchers, in particular to Academician Kulebakin, that a joint analysis of the work should be carried out in order to establish a mutual understanding. [13]

Voznesenskii and a number of his Leningrad colleagues were present at the 1940 conference, although he himself seems to have kept a low profile. Although he does not appear to have presented a paper or to have featured largely in the discussion, the background sketched above must have caused considerable tension, even before the conference began.

The Conference

The conference certainly drew participants from a whole range of institutions concerned with control engineering in the Soviet Union at the time. Apart from the IAT and the TsKTI, the most important of the other institutions represented were the All-Union Power Engineering Institute (VTI) and the All-Union Electrotechnical Institute (VEI) [14]. Interestingly, however, the title "First All-Union Conference" seems to have been accorded to the 1940 meeting only in retrospect: the few copies of the proceedings still in existence, duplicated from typescript on paper now yellow with age, bear the rather clumsy title "Propositions and summaries of the papers presented at the scientific conference on the theory of regulation" [15]. So even the *title* of this conference, as referred to in later Soviet publications, is not quite what it appears to be!

The conference took place at a time when control engineering in the Soviet Union, as elsewhere, was gaining recognition as a vital element in modern industrial society, yet still lacked the theoretical and conceptual framework that was to give it independent status as an engineering discipline in the postwar period. It is worth quoting at some length from Kulebakin's introductory address, given on the evening of 2 December 1940:

> Comrades! The 18th Party Congress in its historic resolution gave clear and precise instructions concerning the development of automation and remote control in our country. This historic resolution also emphasized the importance of instrument technology, and the creation of the technical infrastructure needed to support automation and remote control. At the present time automation and remote control are taking root in all sectors of industry, in the national economy, in transport, and in the military. … Automatic control now finds many and various applications: it is used in boiler-houses for the control of combustion processes; in electricity generating stations and sub-stations to maintain constant voltage and frequency of the supply, and for the control of power. Automatic control finds wide application in thermal installations, in the chemical industry, and so on. Indeed, it is difficult now to find any sector in which automatic control is not employed. ...
>
> If we turn to the current position of both the theory and the practice of automatic control, then we should note the following: five to six years ago in those technically most advanced countries, such as the United States, it began to be said that, notwithstanding practical success in the design of controllers, the Americans were falling behind the Germans. The Americans understood that in the development of any area of technology there comes a moment when it is the development of *theory* that can shed light on the further improvement of the technology. ... In recent years theoretical questions have never left the pages of technical journals: not only in American but in all technical literature. Here in the Soviet Union, too, there has been great enthusiasm in recent years for the question of the theory of automatic control … a whole se-

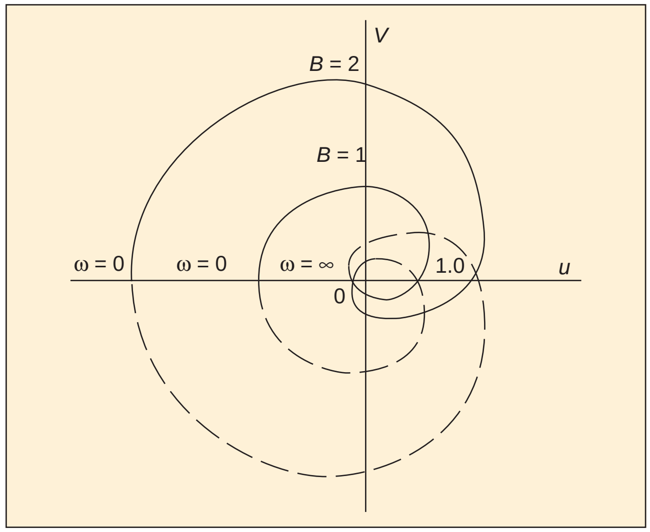

Figure 1. *Nyquist diagram from Solodovnikov's paper.*

Proceedings of the 1940 Moscow Conference

"Principles of automatic control system design," Z. Ya. Beirakh (TsKTI)

"The development of mechanisms for connecting regulators and servomotors from the point of view of a general theory of regulation," Yu. V. Dolgolenko (TsKTI)

"The work of the automatic control laboratory of the VTI in the field of the theory of regulation," E.G. Dudinkov (VTI)

"The influence of machine characteristics on the statics and dynamics of their regulators," S.A. Kantor

"Analytical theory of intermittent regulation," Yu. G. Kornilov (TsKTI)

"On the regulation of turbines with steam extraction," B.A. Kudrov

"General foundations of automatic control," V.S. Kulebakin (IAT)

"On foreign trends in the development of automatic control of steam boilers," A.N. Limarenko (TsKTI)

"Method of investigating the aperiodicity of automatically controlled systems," A.V. Mikhailov (VEI)

"Method of investigating the stability of automatically controlled systems," A.V. Mikhailov (VEI)

"Realization of outline designs for indirect regulators," V.D. Piven (TsKTI)

"Theory of automatic control and its application to electrical drives," E.K. Popov

"On a method of calculating the stability of control systems incorporating components with distributed parameters," A.A. Sokolov

"Operator methods in control theory," V.V. Sodolovnikov (VEI)

"Regulation of steam turbines by speed and acceleration," M.Z. Khefets

"Some questions relating to the theory of regulation," L.S. Goldfarb (VEI)

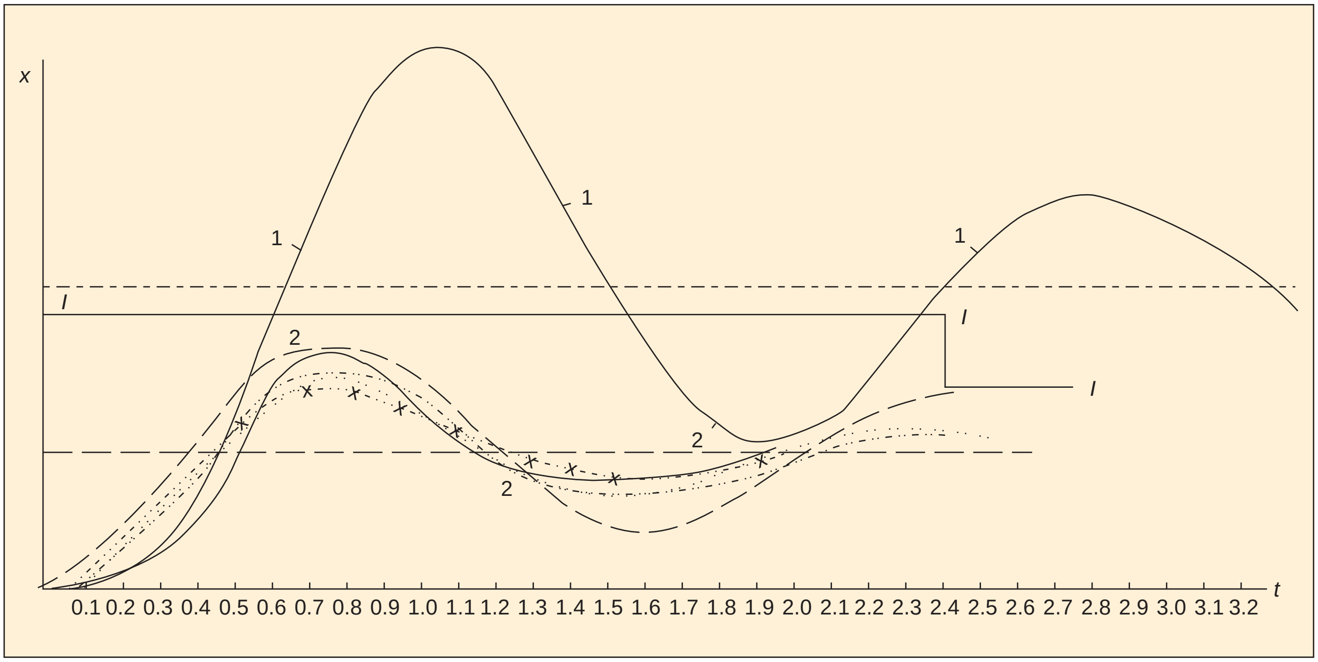

Figure 2. *Transient response curves from Solodovnikov's paper.*

> ries of articles have already appeared. ... But we must insist upon an intelligent combination of theoretical and mathematical research with the practical results that theory should deliver, so that our theory becomes a means for solving practical problems. [16]

A closer reading of these remarks is instructive. Following the formulaic introduction (a necessity for any such meeting in the Soviet Union at that time), Kulebakin makes some uncontroversial remarks about the ubiquitous nature of automatic control, remarks that could have been made in any industrialized country in the late 1930s. But when he comes to justify the 1940 conference on theory, particularly in light of the attacks being made upon what his critics viewed as an overly mathematical and theoretical approach of some researchers at the IAT, he chooses his words carefully. Judicious development of theory is vital to the development of practice, he reminds his listeners, the implication being that the Soviet Union must not fall behind the West [17]. But theory must know its place: there is no room for theoretical work that is not firmly rooted in practical application; in other words, control theory has to advance the material socialist cause.

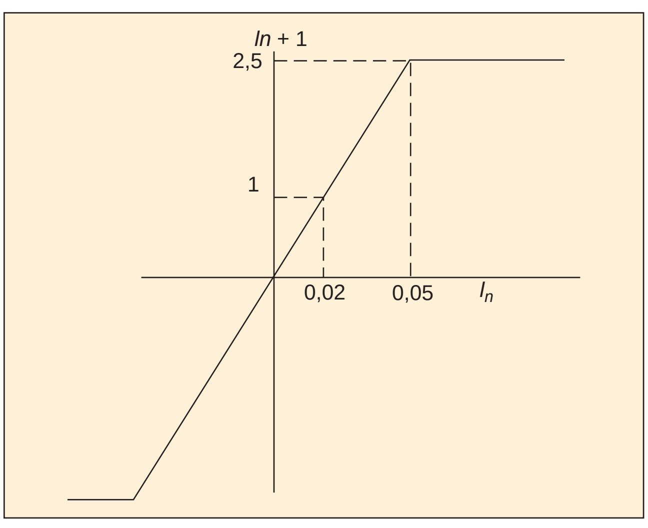

Figure 3. *Nonlinearity analyzed by Goldfarb.*

A list of papers published in the *Proceedings of the 1940 Moscow Conference* is given on pg. 17. This is not a full list of papers presented, however, since we know that Kulebakin gave a second paper and Luzin also presented a paper on matrix methods, neither of which were published in the *Proceedings* (both papers were attacked in the discussion, as indicated below). The published papers are an interesting mix of approaches and fall into two main categories: those which used conventional methods of analysis and description (mainly based on differential equations) and those which were clearly moving toward what we now call classical control, with the emphasis on frequency-response and operator methods. One striking feature of several papers is how well aware the authors were of both the historical development of control theory in Russia, Western Europe, and the United States and of contemporary developments outside Russia. Solodovnikov's paper on operator methods, for example, used block diagrams and applied the Nyquist criterion for assessing stability, presenting classic Nyquist diagrams (Fig. 1 [18]). He also considered transient response (Fig. 2) in some detail.

The Nyquist criterion, and especially an alternative formulation of it [19], was also featured in the two short papers by Mikhailov. It seems likely that the conference offered a good opportunity for participants to become familiar with such novel methods, which were initially perceived as highly mathematical and "difficult" by engineers, many of whom were used only to comparatively simple differential equations or even static models.

There had also been progress in Russia on modeling sampled-data systems and systems with delay (dead time). So, for example, Solodovnikov used the exp($-pT$) operator to model a pure time delay (although he did not take the next logical step of incorporating the linear phase shift into a Nyquist diagram), while Kornilov applied a similar approach to discrete time systems, stating the stability criterion $|z|<1$. Goldfarb, too, demonstrated a good understanding of the emerging features of classical control, applying the Nyquist criterion and harmonic approach to a system containing a nonlinearity (Figs. 3 and 4). He was to develop this approach fully over the next few years and is generally credited with the describing function technique in Russia.

The conference took place at a time when control engineering in the Soviet Union, as elsewhere, was gaining recognition as a vital element in modern industrial society, yet still lacked the theoretical and conceptual framework that was to give it independent status as an engineering discipline in the postwar period.

The Technical Discussion

The printed proceedings of the 1940 Conference do not incorporate any discussion. However, the papers of the Academy of Sciences Investigating Commission do include several pages of verbatim comments [20] on the presentations. It may be that these are selective, quite possibly having been chosen explicitly to support the case against the Institute of Automation and Remote Control. But even if this is so, these comments do give a flavor of the debate. For example, Dolgolenko (TsKTI) dismissed the contributions of both Kulebakin and Luzin:

> I should like to say a few more words about the papers presented at the conference by Luzin and Kulebakin from IAT. As for Kulebakin's papers, I would say that I learned absolutely nothing new from either the first or the second ... I do not want to judge the mathematical aspect of Luzin's paper since I am simply an engineer—but it seems to me personally that the paper is outside the realm of engineering problems, for it considers the solution of linear differential equations, yet in our applications we don't happen to deal with linear differential equations, in so far as we have to deal with objects described by *non*linear differential equations.

And L.N. Mikhailov [21] remarked:

> It must be said that mathematical errors—and elementary enough errors at that—can be found in [Kulebakin's] paper. In this respect the reported work has much in common with that theory of automatic control which bears the title "the theory of the complete compensation of disturbances" [22]. I shall not give examples of the mathematical errors—some of them have already been referred to—but it would be very easy to do so if required.

A number of other participants also criticized the contributions of the IAT in similar terms. But some chose to concentrate on technical issues of rather greater importance. Aizerman, for example, responded to Solodovnikov's paper on operator methods with some perceptive general remarks:

> It seems to me that one of the greatest misfortunes of the theory of control is that theory in its current state is cut off both from practice on the one hand, and its scientific basis in mathematics and physics, from which it has always derived, on the other. Yet control theory has always lagged behind practice. Practice has always run on ahead, unable to wait for a theory of control, but the cause of this has been an inadequate theoretical foundation. Tolle and Zhukovskii [early German and

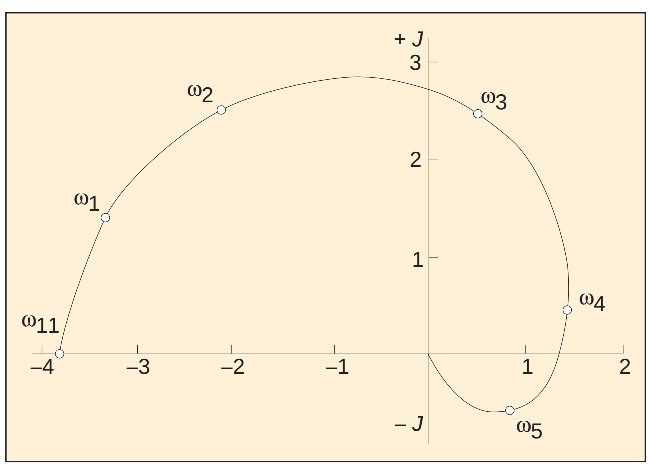

Figure 4. *Harmonic response curve used by Goldfarb.*

> Russian writers on control from around the turn of the 19-20th centuries] were unable to see beyond their mathematical and physical conceptions. And, so far, neither have we. This is why I believe that those papers that appealed for the continuing imitation of classic work, and reliance on experience, are mistaken. And it is why I welcome those papers which call for novelty, and which take up new mathematical tools.

The Resolution

Resolutions and recommendations were an important conclusion to Soviet scientific meetings. In theory, they fed into macro-economic planning, so there was always a highly political context to the final statement. The resolution of the 1940 Conference noted, for example:

- Most current approaches to control, based on linear differential equations with constant coefficients, were inadequate;
- There was a lack of coordination between work at the various Soviet centers of expertise;
- There was a need for work on terminology in the field of automatic control;
- The Hurwitz and Nyquist stability criteria were well known, but a knowledge of absolute stability was insufficient for the design of a control system to fulfill its purpose;
- The work of Luzin and Kulebakin was to be welcomed [despite the criticism] and should be continued.

One participant, in a comment on the draft resolution that echoed Aizerman's remark above, criticized the lack of novelty in the papers from the IAT, TsKTI, and elsewhere and was keen that the final resolution should emphasize the importance of the work being done at other centers, such as the All-Union Electrotechnical Institute, where Mikhailov, Solodovnikov, and Goldfarb were based. And indeed the papers by these latter researchers on operator and frequency response methods, including Goldfarb's approach to nonlinearities, were precisely the ones that were pointing to the techniques of what we now call classical control.

Postscript

The anti-IAT forces at the TsKTI were not long in criticizing the 1940 conference. Just a week later, Piven, Beirakh, Kornilov, Dolgolenko, and Limarenko wrote to a management committee of the Academy of Sciences:

> The work of the Academy of Sciences [in effect, therefore, the Institute of Automation and Remote Control] in organizing the scientific conference on the theory of automatic control, anticipated with great interest by practitioners, did not live up to our legitimate expectations. Practitioners did not receive from the Academy of Sciences any proper guidance in the contemporary state of automatic control theory, its likely development, and methods of analysis. Indeed, practicing engineers were convinced of the feebleness of the methods recommended by the Academy of Sciences, even for control problems long solved theoretically and mastered in practice... [23]

The complaint, however, was not upheld by the relevant committee of the Academy of Sciences, which noted that the conference's final resolution had been approved by the vast majority of the delegates (with only one dissenting voice), and that the authors of the letter had been party to the discussions and should therefore have made their views known then. The official investigation still had some months to go before it pronounced definitively against Kulebakin and his colleagues.

Conclusion

The 1940 Moscow conference on the theory of automatic control was an important landmark in the history of control engineering in the former Soviet Union. The participants came from a variety of backgrounds, but the majority were experienced in turbine control, related process control applications, or the regulation of electrical machines. The documents examined so far by the author have not revealed representatives of communications engineering or high-performance servos at the conference [24]—two sectors which proved to be so seminal in the development of classical control in the West.

The scientific-political infighting over research directions at the IAT seems to have affected the conference atmosphere and discussion quite considerably, and later portrayals of the Moscow meeting in the Russian literature certainly suppressed this aspect, portraying the event as an unproblematic exchange of scientific and technical expertise. Nevertheless, returning to the comments of Aizerman quoted at the beginning of this article, fruitful contacts were indeed made at the meeting, some of which were of great significance later. As described elsewhere [25], Andronov and Voznesenskii were to collaborate during the mid-1940s, and after the war Andronov was to play a major role in a revitalized IAT, when there were very good relations and close academic links between Moscow and Leningrad. It is quite likely that many participants in the 1940 conference refrained from any comment on the Kulebakin/Shchipanov dispute and concentrated on the technical aspects of their work.

Just a few months after the December 1940 conference, the Soviet Union entered the war, and both the Moscow and Leningrad groups were evacuated outside the war zone. (Exactly what the final consequences of the Kulebakin/Shchipanov affair might otherwise have been for the fledgling Institute of Automation and Remote Control must remain speculation.) The wartime exigencies in Russia were very different from those in the West, and wartime and postwar control engineering in the Soviet Union took on a very different character.

It is worth making a final point about the continuing availability of documentary sources for research of this nature. The documents relating to the 1940 conference and the Shchipanov affair held in the Archives of the Russian Academy of Sciences are comparatively secure, apart from problems of conservation and restoration (although it must be said that over the last decade or so, significant documents have indeed been missing from Russian archives). The situation is rather different, however, for sources such as conference proceedings held in academic libraries. Libraries are increasingly under pressure to dispose of such "out-of-date" material and are often unwilling to accept as gifts such materials from individuals. Yet these sources are vital for the history of technology and need to be preserved.

Acknowledgments

Archival research in Moscow and St. Petersburg archives in October 2000 was funded by the British Academy/Russian Academy of Sciences exchange scheme. I am particularly grateful for the assistance of Russian colleagues Prof. Aleksandr Pechenkin, Prof. Boris Polyak, and Dr. Ilya Polushin.

Notes

[1] Naturally, some Western meetings were informal, undocumented, or restricted, particularly those held in wartime. An intriguing example is the series of Gibson Island meetings of the American Association for the Advancement of Science; participation was by invitation only, and no record was kept of the meetings. The 1942 meeting was devoted to instrumentation and brought together a number of instrumentation and control experts. See S. Bennett, *A History of Control Engineering 1930-1955*. Stevenage, UK: Peter Peregrinus, 1993, p. 62.

[2] Political and ideological considerations were important in the West, too, however. Some aspects of the U.S. context are considered briefly in C.C. Bissell, "Textbooks and subtexts," *IEEE Contr. Syst. Mag.*, vol. 16, pp. 71-78, Apr. 1996.

[3] B.N. Petrov et al., "Razvitie teorii avtomaticheskogo regulirovaniya v SSSR [Development of the theory of automatic control in the USSR]," in *Proc. 2nd All-Union Conf. Automatic Control Theory*, Moscow, 1955, pp. 13-50.

[4] For an outline of Andronov's contribution to control engineering in the former USSR, see C.C. Bissell, "A.A. Andronov and the development of Soviet control engineering," *IEEE Contr. Syst. Mag.*, vol. 17, pp. 56-62, Feb.1998.

[5] M.A. Aizerman, "Teoriya avtomaticheskogo regulirovaniya i upravleniya v Institute Avtomatiki i Telemekhanikins—Institute Problem Upravleniya (1939-74) [Theory of automatic control in the Institute of Automation and Remote Control/Institute of Control Sciences (1939-74)]," in *Problemy upravleniya*, Izdat. Inst. Problem Uprav. AN SSSR, Moscow, 1975, pp. 9-19.

[6] Documents in the RAS Archives are indexed numerically in the following way: *Fond* [archive] /*Opis* [list] /*Delo* [file] / *List* [sheet]; sources will be indicated in this format below.

[7] *Institut Avtomatiki i Telemekhaniki (IAT)*. In what follows, Soviet institutions will often be referred to in the transliterated form of their Russian abbreviations or acronyms.

[8] Luzin, an exceptional mathematician, had already been subject to severe criticism and narrowly escaped imprisonment or even execution, in the mid-1930s. Details can be found in S.S. Demidov and C.E. Ford, "N.N. Luzin and the affair of the 'National Fascist Center,'" in *History of Mathematics: State of the Art*. New York: Academic, 1995, pp. 137-148; and S.S. Demidov and B.V. Levshin, *Delo Akademika Nikolaya Nikolaevicha Luzina [The Case of Academic N.N. Luzin]*. St. Petersburg, Russia: RKhGI, 1999.

[9] C.C. Bissell, "Control Engineering in the former USSR: Some ideological aspects of the early years," *IEEE Contr. Syst. Mag.*, vol. 19, pp. 111-117, Feb. 1999.

[10] For an English overview see L. Finkelstein, "The theory of invariance," *Contr.*, pp. 96-98, Nov. 1960, and A.G. Ivakhnenko, "New methods of control-system investigation," *Contr.*, pp. 96-99, Dec. 1960.

[11] M. Granovskaya, 3 July 1940, RAS: $2/1^{a}$1940/60/44-48.

[12] *Ivan Nikolaevich Voznesenskii. Vydayushchiisya sovetskii inzhener i uchenyi [Ivan Nikolaevich Voznesenskii. An outstanding Soviet engineer and scientist],* Mashgiz. Moscow, 1952, pp. 30, 32.

[13] Typescript in St. Petersburg branch of the Archives of RAS: 964/2/48.

[14] Papers from these four institutions are indicated in the panel.

[15] *Tezisy i Konspekty Dokladov na nauchnom Soveshchanii po Teorii Regulirovaniya*, Izd. Ak. Nauk, Moscow/Leningrad, SSSR, 1940.

[16] RAS: 1603/2/48/1-6.

[17] Exactly what Kulebakin was referring to when he claimed that the United States was falling behind Germany in the mid-1930s is unclear. (If anything, much American control technology was "reverse-engineered" by the Germans at that time!) Certainly, researchers in the United States and the United Kingdom in the 1930s noted the need for theoretical developments; but at this time, the most significant theoretical work in the United States appears to have come from industry rather than academia. See S. Bennett, *A History of Control Engineering 1930-1955.* Stevenage, U.K.: Peter Peregrinus, 1993.

[18] At this time, the convention was for the critical point to be $1+j0$, rather than the later $-1+j0$.

[19] See A.B. Mikhailov, "Metod garmonicheskogo analiza v teorii regulirovaniya [Method of harmonic analysis in control theory]," *Avtomatika i Telemekhanika*, vol. 12, no. 6, pp. 27-81, 1938. A brief explanation in English can be found in C.C. Bissell, "A Russian life in control: Yakov Tsypkin," *IEE Rev.*, pp. 313-316, Sept. 1992.

[20] RAS: $2/1^{a}$1940/61.

[21] Not to be confused with A.B. Mikhailov (Note 19)!

[22] In other words, Shchipanov's "theory of invariance" discussed in Bissell, 1999 (Note 9).

[23] RAS: $2/1^{a}$1940/61/61.

[24] This is not to say, however, that workers in these sectors were not present. The author has not been able to locate a full list of delegates.

[25] Bissell, 1998 and 1999 (Notes 4 and 9).

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