

Personalia

IN MEMORY OF S. A. YANOVSKAYA (1896 – 1966) ON THE CENTENARY OF HER BIRTH¹

B. A. TRAKHTENBROT

School of Mathematical Sciences
Sackler Faculty of Exact Sciences
Tel Aviv University
Tel Aviv 69978, Israel
email: trakhte@math.tau.ac.il

Currently there are already comprehensive papers which document and analyze the heritage of S. A. Yanovskaya, and her contribution to history and philosophy of mathematics as well as to the formation and defense of mathematical logic in the USSR (see [Anellis 1987]). Yet the centenary of her birth is an appropriate opportunity to raise recollections which may add some traits to the image of this eminent scholar and superb human being.

Below I am going to publish and comment on (the translation of) some documents, among them letters addressed to me by S.A. From a personal perspective these documents, dated 1951, tell the story of how I was accused of “bourgeois idealism”, and how, due to the guidance and support of my mentors and especially of S.A., I managed to overcome the danger of these accusations in an era of persecution of idealists”, “cosmopolites”, and others. But beyond my personal affairs the documents apparently present some additional evidence on the general atmosphere surrounding mathematical logic at that time and to the struggle of S.A. for its legitimacy and consolidation.

§1. The Seminar.

My first acquaintance with S.A. occurred in 1947 at the seminar she ran together with P. S. Novikov. Having graduated from the Chernovtsy University, I had just started my Ph.D. studies at the Kiev Mathematical Institute of the Ukrainian Academy of Sciences. The director of the

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institute, M. A. Lavrentiev, approved my petition to specialize in mathematical logic under P. S. Novikov, who held a permanent position at the Moscow Mathematical Institute of the USSR Academy. It was also agreed to grant me long-term scientific visits to Moscow where I would stay with my advisor. At that time departments of mathematical logic did not yet exist in the USSR and the Yanovskaya-Novikov research seminar "Mathematical Logic and Philosophical Problems of Mathematics" was the main medium in which research and concomitant activities in the area were conducted.

In particular the seminar was the forum where mathematical logicians from the first post-war generation (mostly students of P. S. Novikov, S. A. Yanovskaya, A. N. Kolmogorov) joined the community, reported on their ongoing research, and gained the primary approval of their theses; and that is also what happened to me. During the years of my Ph.D. studies (1947-50) I actively (though not regularly) participated in the seminar meetings. The results which made up my thesis *The decidability problem for finite classes and finiteness definitions in set theory* were also discussed there. S.A. offered the official support of the Department in the forthcoming defense at the Kiev Institute of Mathematics; the other referees were A. N. Kolmogorov, A. A. Lyapunov and B. V. Gnedenko.

The atmosphere dominating the meetings of the seminar was democratic and informal. Everybody, including the students, felt and behaved at ease without strong regulations and formal respect for rank. For me — a graduate of a provincial university, this seemed quite unusual; I was happy to acquire these habits and later to promote them at my own seminars.

Starting with those years I always benefited from S.A.'s kind attention, I could even say — from her motherly care (after all, she was about the age of my parents). Conversations with S.A. equally covered the general situation in the mathematical and philosophical community, concrete research topics, and even common technicalities. "Be careful about writing adjacent formulas; they can be misinterpreted as one long formula. Hence separate them by an appropriate text!" was an everyday wisdom I learned from S.A. Since then I always passed this "Yanovskaya principle" along to my students when requiring readability of their writings.

Actually this seminar was the successor of the first seminar in the USSR for mathematical logic, which was founded by I. I. Zhgalkin (1869 – 1947). After Zhgalkin's death it became affiliated with the Department of History of Mathematical Sciences of the Moscow University, whose founder and head was Yanovskaya. Its exceptional role in the development of mathematical logic in the USSR is a topic of its own and I will touch on it only very briefly.

The seminar usually engaged in a very broad spectrum of subjects from mathematical logic and its applications, as well as from foundations and philosophy of mathematics. We learn for instance from S.A.'s letter, dated

December 4 1951, that at that time the following topics were on the agenda:

- (i) Discussions on the articles "Intuitionism" and "Conventionalism", submitted to the *Soviet Encyclopedia*;
- (ii) New results in the theory of relay-switching circuits;
- (iii) A critical survey (to be delivered by S.A. herself) of Lorenzen's paper "The Consistency of Classical Analysis".



Figure 1
"The Seminar", 1951

(Front row, beginning second from left to right:

V. I. Shestakov, P. S. Novikov, S. A. Yanovskaya, D. A. Bochvar;

Back row, beginning fourth from left to right:

A. V. Kuznetsov, A. S. Esenin-Vol'pin, S. I. Adyan, V. A. Uspenskii)

Those early years were a period of fierce struggle for the legitimacy and survival of mathematical logic in the USSR and S.A. was at the very epicenter of the battle. Therefore the broad scope of the agendas was beneficial not only for the scientific contacts between representatives of different trends. In the face of ideological attacks it helped S.A. to consolidate an effective defense line, and to prevent the isolation and discredit of

mathematical logic. For us, the junior participants of 'The Seminar', it was also a time when we watched the tactics our mentors — and especially S.A., adopted to face or to prevent ideological attacks. Their polemics were not free of abundant quotations from official sources, controlled self-criticism and violent attacks on real and imaginary rivals.

It was disturbing then (and even more painful now) to read S.A.'s notorious prefaces to the 1947-1948 translations of Hilbert and Ackermann's *Principles of Mathematical Logic* and Tarski's *Introduction to Logic and the Methodology of Deductive Sciences* in which Russell was blamed as a warmonger and Tarski as a militant bourgeois. Alas, such were the rules of the game and S.A. was not alone in that game. I remember the hostile criticism of Tarski's book by A. N. Kolmogorov (apparently at a meeting of the Moscow Mathematical Society): "Translating Tarski was a mistake, but translating Hilbert was the correct decision" he concluded. This was an attempt to grant some satisfaction to the attacking philosophers in order to at least save the translation of Hilbert-Ackermann's book. I should also mention that S.A. was vulnerable — she was Jewish — a fact of which I was unaware for a long time. I learned about it in the summer of 1949 during Novikov's visit to Kiev. He told me then with indignation about official pressure on him "to dissociate from S.A. and other cosmopolitans".

However difficult the situation was, we — the students of that time — were not directly involved in the battle which we considered to be only a confrontation of titans. As it turned out later this impression was wrong.

In 1950 (December 5, a date easy to memorize, it being the anniversary of the Stalin Constitution), after the defense of my thesis, I moved to Penza, about 700 km. SE of Moscow, for a position at the Belinski Pedagogical Institute. As visits to Moscow had become difficult it seemed that the former intensive contacts would be seriously affected. But very soon a threatening situation arose which urgently required even more intense contacts, especially with S.A.

In retrospect I am aware that only thanks to these contacts did I manage to survive then; otherwise, most likely I would not be writing these recollections now.

§2. The Penza Affair.

This affair started with my denunciation as "an idealist of Carnapian variety" in the course of a talk on mathematical logic I delivered for my fellow mathematicians. My opponents extracted this "precise diagnosis" from current philosophical publications (ironically — it may well be from one of S.A.'s papers). In that era of Stalin paranoia such accusations were extremely dangerous, much more than just destroying careers. In order to de-

fend myself, I engaged in more lectures and discussions at the mathematical department in the hope of clarifying my point and dissipating potential misunderstandings. But in vain! Once started, the disputes boiled the whole of 1951 with the threatening prospect of further aggravation and even of extension beyond the meetings of the mathematical department. The reason was that after Stalin's relatively recent "A Letter on Marxism and Linguistics" there was a general call to study and to apply Stalin's great teaching, to engage in criticism and self-criticism and to accordingly reorient the ideological work. For this purpose special conferences were also scheduled in all academic institutions. The ongoing preparations for the conference at the Penza Pedagogical Institute did not bode well, and I expected new attacks and denunciations from a more influential forum. In view of that I appealed to my mentors in Moscow; I sent them copies of material concerning discussions in my department, asking their expertise and advice for the forthcoming struggle. At various stages of the further developments, P. S. Novikov and A. A. Lyapunov (Steklov Mathematical Institute) and to some degree A. N. Kolmogorov and A. G. Kurosh (Moscow Mathematical Society) were all involved in my defense. But S.A. took on the main burden through direct correspondence with me, and by putting my case on the agenda of the seminar. Below in this section some minor quotations from (the abstracts of) my lectures and from the departmental discussions are presented. The complete texts of the letters and protocols I received from Moscow can be found in the next section. These messages arrived at the right time and I took full advantage of them at the forthcoming conference sessions and at a special meeting of the Department of Marxism-Leninism. Together with my accompanying comments, this material will hopefully suffice in restoring the general picture. But let me proceed now with some details concerning the very beginning of the story. The aim of my first talk entitled "The Method of Symbolic Calculi in Mathematics" was to explain the need and the use of exact definitions for the intuitive concepts "algorithm" and "deductive system". Most naturally, I illustrated the subject with the classical "negative" results:

(1) Church's theorem: the first order logic of predicates is undecidable,

and

(2) Gödel's incompleteness theorem.

Nowadays, and already for a long time, such talks are a matter of routine, but over 45 years ago it must have seemed quite unusual. I also intended on that occasion to mention the two main results of my Ph.D. thesis which sound similar to the classical theorems above. Namely:

(1) There is no algorithm which decides for an arbitrary given formula of the first-order logic of predicates whether this formula has or does not have a finite interpretation.

(2) In each “elementary axiomatizable” set theory there exist two definitions of finite set whose equivalence can neither be proved nor refuted in this theory.

Finally, I speculated on some philosophical aspects of the above negative results which (quotation follows):

“. . . strikingly illustrate the irreconcilability between science and idealism with respect to the following two fundamental questions:

(a) The theorems on algorithmic undecidability refute Leibniz’s idealistic hypothesis about the existence a general deciding algorithm for whole mathematics (the universal mathematical machine).

(b) The incompleteness theorems are ‘obviously’ directed against Hilbert’s ideas to replace material truth with formal provability.”

Most of the details of my talks are immaterial to the understanding of the dispute which arose since the critics focused on a single point: what is logical formalism and what should it be. Actually I used “symbolic calculus” instead of “logical formalism” in order to avoid connotations with the odious term “formalism”. My opponents discovered idealism in the following definition and comments (a quotation from my abstracts follows):

“A *symbolic calculus I* is defined by:

(a) a finite set of symbols and rules for constructing formulas from these symbols;

(b) a finite set of basic formulas (“axioms”) and rules of formula transformations.

The least set I^* which contains the basic formulas and is closed under the transformation rules is called the class of regular (“good”) formulas of the calculus I . The essential point about I^* is that the generation of the regular formulas can be mechanized. For this mechanization no particular interpretation of the symbols or of the formulas is relevant.”

The following quotations characterize to some degree the development of the dispute.

From the Statement of G.O.² — 26.6.1951.

The lecturer circumvents philosophical problems. He does not criticize the bourgeois idealists, who parasitize on mathematical logic (Russell, Tarski and others). The lecturer persistently holds a neutral position in the dispute between materialism and idealism in mathematical logic, deliberately avoiding the subjects broached in his lectures.

The very term “*good formula*” is not appropriate: a formula which is good for the capitalist is bad for us. I asked you several times and you always replied that you chose basic formulas and arbitrary transformations, that you did not apply any restrictions. What drives you to cling to this freedom? Holding on to it you reveal yourself as an idealist.

Read *Voprosy Filosofii* for a ocean of open options. You as well as other idealists adopted this ocean of open options

Yours is an idealistic muddle but pride prevents you from admitting it, even though Yanovskaya was able to do so.

From B. Trakhtenbrot’s Response to G.O.

G.O.’s claim that I avoid philosophical problems is not fair. On the contrary, through scientifically established facts, I prove that some of Leibniz’s and Hilbert’s views are not well-grounded. It is unclear why I had to criticize Tarski and Russell just in these delivered lectures. The criticism of Tarski and Russell, as well as other methodological issues, are of great interest. However the selected theorems don’t provide a sufficient basis for such critics, whereas these theorems directly undermine the views mentioned of Leibniz and Hilbert.

G.O. concludes with the categorical claim that I am an idealist. In my opinion, G.O.’s speech reveals great confusion concerning the essence of mathematical definitions (clearly this is not an idealistic jumble, but simply — a jumble) and a certain vulgarization in methodological issues. As for the obtrusive defamation by G.O. of his opponents as idealists, it seems similar to what comrade Stalin writes:

“N. Ya. Mahr and his followers incriminate all linguists who do not share Mahr’s new theory with formalism. That is neither serious nor wise I think that ‘formalism’ was invented by the authors of the *New Theory* to facilitate their struggle with their rivals.”

² Here and elsewhere in the quotations full names are omitted for ethical reasons.

Project of Resolution of the Mathematical Department.

After hearing and discussing comrade Trakhtenbrot's talks (three) on symbolic calculi the department notes the following:

In his talks comrade Trakhtenbrot did not sufficiently explain the real needs which stimulated the emergence of the method of symbolic calculi in mathematics and of its applications.

As for fundamental premises, the lecturer allows absolute freedom in the choice of basic formulas (axioms) and of rules to infer new formulas from them. In that way he adopts the position of Carnap-like idealists. Hence it is not by chance that he did not criticize the idealistic views of the bourgeois experts in the area of mathematical logic (Russell, Carnap, Hilbert, Tarski and others).

In this connection the department finds it necessary for Trakhtenbrot B.A. to give a talk on the essence, tasks and methods of mathematical logic with the critics of idealistic distortions in this area.

On behalf of the department head: G.O. and N.S.

B. Trakhtenbrot's Own Opinion.

1. A number of colleagues discerned idealistic confusion in the definition I gave for Symbolic Calculus. That is reflected in the resolution of the department: "In the fundamental premises the lecturer allows absolute freedom In this way he adopts idealistic positions."

I called attention to the erroneousness of the colleagues' opinion and therefore to the unjustifiability of the corresponding point in the resolution.

There is a full analogy here with grammatical concepts, for instance, with the concept "sentence". A sentence is a set of words which expresses a completed "thought". In this definition nothing is assumed about the concrete essence of the thoughts expressed as sentences. From a grammatical point of view both of the following sets of words are sentences and moreover they have the same grammatical structure:

"The student Ivanov resides in the dormitory".

“The archangel Michael resides in paradise”.³

It is possible to formulate grammatically correct both precious, deep thoughts as well as most absurd claims. Similarly one can formulate and automate as calculi both precious mathematical theories as well as the most absurd “theory of an idealistic obscurantist”. But this does not imply that the grammatical rules formulated for arbitrary sentences with arbitrary subjects and predicates or the theorems about arbitrary symbolic calculi with arbitrary basic formulas and allowed transformations, lose their significance and become idealistic.

The fact that I use terms like “basic formulas” and “rules of allowed transformations” is no reason for the colleagues to confuse these concepts with “initial scientific premises”, “rules of logical inference”. This confusion led the colleagues to incorrect conclusions

2. The clauses of the resolution which claim that in the talks there is little elucidation of the real issues which gave rise to the emergence of the method of symbolic calculi and their applications, and that there are not enough critics of the bourgeois experts — these clauses do not take into account the goals of the lecturer. Of course in those talks it was impossible to give an exhaustive elucidation of all the issues. The lecturer restricted himself to consideration of those issues closer to his own research topics. It would be in place to precisely point in the resolution to what issues should be considered in order to help the lecturer orient himself with concrete demands. It is absolutely unclear to me why such meticulousness was avoided.

From the Statement of V.Sh. (Dept. of Marxism-Leninism).

In the definition of the concept “symbolic calculus” the symbols are considered, without connection to material reality, as absolutely arbitrary signs and hieroglyphs.

The arbitrary choice of basic formulas and of allowed transformation rules is understood as an arbitrary choice which does not reflect reality; no content nor meaning is assigned to the formulas. Hence the formulas may be understood and interpreted arbitrarily (“G-d is eternal”).

³ The prompt criticism of this humorous comparison was “religious propaganda!”

§3. Messages from Moscow.

Dear Boris Abramovich,

I now live in a country cottage quite far from Moscow, so your letters reached me only after a lengthy delay. I enjoyed reading your lecture especially the second part, but not the other material you sent me. I think that your lecture after the inclusion of some further detail should be published in *Uspekhi*⁴ with a short preface by Piotr Sergeevich⁵ or by our seminar. Its whole structure testifies that your point of view contrasts radically with the idealistic views of the formalists and Carnap. Unlike Carnap who identifies logic-mathematical calculi with logic, you distinguish between them so much that you even use the special term “regular” (instead of “inferred”) formula. Further, your proclaimed task is to clarify what is not reducible in mathematics to pure automatic solution by means of computers and, in contrast, to determine what advantage may be gained from these techniques in order to construct computing mechanisms and devices.

On the other hand, your opponents — even though not ill-intentioned — rather adopted Carnap’s view, as far as they agree with him that logic-mathematical calculi *per se* are not simply *auxiliary devices* for mathematics, but each on its own is a mathematical theory with specific pithy axioms and concepts and is intended to substitute for mathematics. I would only like to reproach you for an excessive abstractness of the exposition which renders your lecture difficult for a broad audience and also for the vain addition of the word “good” to the term “regular” (formula). My own experience convinced me more than once that the addition of words which are already loaded with specific meaning often does not help to clarify but rather obscures the issue for the audience; hence it is the wrong method.

At the moment, as it happens, I am in town but have to leave within 2 hours, which is why I cannot write a detailed reference now. If you need it, let me know using my country address.

With hearty regards,

Yours, SY

4.VIII.51

⁴ An abbreviation of *Uspekhi Matematicheskikh nauk*. The journal is translated into English under the title *Russian Mathematical Surveys*.

⁵ P. S. Novikov

Дорогой, уважаемый Борис Иванович,
 Я живу теперь на Даче, Говорино Восток от
 Москвы, а письма Ваши поюти посылу ко
 мне с доброй доставкой. Письмо Вашего
 сына, особенно второй его части, доставило
 мне большое удовольствие, что увидя много
 сказать о Вуде присланных Вами материа-
 лях. Долга Вам мне представляется некото-
 рыми, несколько раскрыть его, а именно
 в «Земляк» с каталогам аргументов Петра
 Вереева или нашего семинара. Все по сути
 уже свидетельствуют о том, что Ваша точка
 зрения в корне противоположна идеалистиче-
 ской взглядам российского и Карнапа.
 Виде Карнап аргументы лишь матери-
 алы и критика с логикой, а во итоге
 их можно, что между Вуде и Карнапом
 термин — "реализм" (а не "идеализм") про-

Figure 2

-2-

ну и сравните своей задачей ~~вспомогательную~~, ~~то~~ ~~это~~
 несводимо в марксистские ~~формализмы~~ к чему-то
 абстрактно-марксистскому ригоризму средств "машинно-
 марксистских" и это, наоборот, может быть исполь-
зовано в ригоризме для популярных сложно-решаю-
щих механизмов и приборов. Вам же продумали
~~механизмы~~ хотя близко не по этой воле,
Зеленый господи вспомогательный Карнап, и связаны с ним
~~формализмы~~ в том, что механи-
цистские механизмы как заказ сог но было
логический аппарат для марксистских, суть
взять само по себе, суть самостоятельная содержательная
марксистская теория, включая содержательные
аксиомы и понятия и привычные идеи
марксистские. Единственно, в чем мне все-таки
 хотелось бы указать Вас, это некоторая чуждая
как абстрактность изложения, Федотов или Филипп
недостаточно для широкой аудитории, а такие
напрямое применение слова "хорошее" к ге-
мии "регулярное" (формула). На содержательном

Figure 3

Page 2, Yanovskaya to Trakhtenbrot, 4 August 1951

- 3 -

еще мне неоднократно приходилось убеждаться в том, что присоединение таких слов, уже названных собственными сокращениями, чаще всего не повышает, а замедляет вопрос для слушателя и даже еще больше потому, что многие незнакомы с этими сокращениями. Поэтому лучше избегать в речи и письме на этот счет еще не разработанных сокращений. Если таковой Вам все же нужен еще, напишите мне по адресу:

ул. «Турция», Савиловской див. Др., Введенское
 №/о, Дер. Старомоново, Львовской.

С сердечными приветиями,

Ваша

Л. Любская

4-III-51

Figure 4

Page 3, Yanovskaya to Trakhtenbrot, 4 August 1951

* * * *

4 December 1951

Dear B.A.,

I feel rather guilty about you. Not because I haven't written to you, but rather because I don't have what to write about. As a matter of fact I suggested to P.S.⁶ to discuss your paper at the seminar meeting, after its remittance to us for reference from the Steklov Institute. But those in the Steklov Institute claim that cannot be done, since the Institute is a sufficiently competent scientific body to have its own opinion. My answer was that I received a copy of your abstract directly from you and that in my opinion it is high time that a paper on the topic you consider appear in *Uspekhi*, that your abstract testifies your deep knowledge of the material, that you can expose it in a deep and lucid way, and that you follow the correct dialectic materialist position which should, however, be exposed in a more extended format. Both Piotr Sergeevich and Alexei Andreevich⁷ expressed their full agreement with this view on your work and we decided that I would present the content of your paper at the seminar for a broad follow-up discussion.

Nevertheless, since then several weeks have already passed. Each Saturday I bring your abstracts to the seminar, but the current talks — well, you know that each talk lasts a few Saturdays — consumes all the time of the meeting and the participants then disperse. Meanwhile, each time, the next item on the agenda, including your abstracts is postponed for a future meeting. *Uspekhi* is still looking for our reference on a paper submitted by Gokieli, and *The Soviet Encyclopedia* [is looking] for our references on the papers "Intuitionism" and "Conventionalism". There is permanent pressure on me concerning these references and each time I bring this material to the seminar. But since it is always the second point on the agenda and since the main talks undergo animated discussion, we never pass to the second item on the agenda. This past Saturday, influenced by your letter, I intended to deliberately shorten the first part of the agenda; unfortunately I was sick and could not attend the seminar.

In connection with your forthcoming lecture at the conference, I strongly advise you to read the remarkable paper of Yu. Zhdanov in the last issue of the *Bolshevik* concerning critics and self-critics in science.

In our seminar we are presently discussing the following talks:

1. B. I. Shcherbakov: "Auto-oscillating relay-switching schemes"

⁶ See footnote 5.

⁷ A. A. Lyapunov.

2. A. V. Kuznetsov: "Bridge connections". (Necessary and sufficient conditions for relay schemes of a certain class which are specified by functions of n independent contacts to admit representation via superposition of functions with fewer arguments; in particular, this is crucial in deciding whether a given bridge in the scheme is eliminable. Sasha⁸ proposed a very simple and witty solution.)

3. Finally my talk — a critical discussion of the work of the German mathematician, Lorenzen, with the high-flown title "The Consistency of Classical Analysis" whose actual content is some generalization of Weyl's old work (1918) on the continuum.

At the past meeting of the Moscow Mathematical Society, A. N. Kolmogorov presented a great survey talk on relay-schemes, based on the works of Lunts on the matrix representation of schemes.

I am afraid I have already written too much. Next Saturday we will finally discuss your abstracts.

Heartiest regards and best wishes,

S.Y.



Figure 5
1966, One of the last photos

(Front row, left to right: A. V. Kuznetsov, S. A. Yanovskaya, B. A. Trakhtenbrot)

⁸ Alexander V. Kuznetsov

4 декабря 1952 г.

Дорогой, уважаемый Борис Абрамович,

Я счастливо очень выговариваю перед вами. Не хотелось бы из-за
 пошлости, что во сих пор не написана вам книга, сколько быду
 что мне кидать написана вам. Я действительно предложил Г.С.
 обсудить вашу статью на заседании семинара, прислав ее к
 нам на отзыв из Стеклового института. Но в Стеклове со-
 сказали, что этого нельзя делать, так как институт достаточно
 авторитетная научная организация, чтобы иметь собственное мн
 ение. На это я ответил, что имею экземпляр ваших тезисов
 непосредственно от вас, читая статью на заседании ваш
 тезисы вполне необходимы для "Ученых изобретательских наук",
 а ваши тезисы свидетельствуют о том, что вы глубоко
 владеете материалом, знаете также и много взглядов его и
 стоите на правильных философско-методологическо-позитивных
 позиции. И Герт Сервант и Анисей Андреевич Вирский вполне со-
 гласны с этой точкой зрения на вашу работу, а не речитатив,
 что я только подтверждаю ваши статьи на семинаре о том,
 что подтверждают это письмом от вас. С тем же являю
 уже прислано несколько книг. Я надеюсь эту книгу прочитать вы
 или кто-то на семинаре, но очередные материалы - а вы знаете,
 что каждый материал у нас заготавливается на ряд субботы -
 занимают все время, и угадывать рассуждал. Между тем
 у нас вернутся - и непосредственно ^(каждый раз) ~~посредством~~ на след
 щий раз - непосредственно не только ваши тезисы.

"Ученые" имеют нашего отзыва на статью Тасман, которую

Figure 6

Page 1, Yanovskaya to Trakhtenbrot, 4 December 1951

Вспомню, что и в том время "разделася" в партийном аппарате, а также, что там, где были, как правило, в основном партийные работники.

он пришел в дни отъезда в Б.Д. - на съезда "Интернационал" и "Самвишьяло". Желая во время отъезда с другим отъездом, я каждый раз приношу ~~то~~ материал на семинар, но поскольку отлучение их является втроем путем и отъездом: и в том, а по мнению Фомашова у нас сейчас будут очень серьезные проблемы, было так и не удалось до отъезда. В первую же субботу, как выходящая вонючая лодка, я хотела сделать первую часть заранее, но, к сожалению, дело и не могла быть на семинаре.

В связи с предположением Васи Фомашова на конференцию очень советуем вам отозвать замминистра погран. К. Жданова в последняя полка "Большая" по вопросу с критикой и самокритикой в науке.

На семинаре мы обсуждали доклад В.И. Черкасова по вопросу методологии реального-конструктивного стиля, А.В. Кузнецова о методах самонаблюдения (речь шла о наблюдении в эксперименте уловчивого, чтобы ^{для} конструктивного реального-конструктивного ~~стиля~~ некоторого класса, представляющего функцию от ~~и~~ независимых ~~координат~~ ~~параметров~~, или представляем в виде суперпозиции функций от меньшего числа аргументов; в частности, об установлении такого критерия для решения вопроса о том, является ли в данной системе "мощно" установленными или нет. Сама проблема очень проста и отработанное решение), наконец, мой доклад - критическое обсуждение работы коллектива математиков и физиков под редакцией названного "Критерий работы коллектива математиков и физиков анализа", за которым на семинаре был сделан доклад о критерии работы коллектива (1918г.) о самонаблюдении.

На семинаре заведующий лабораторией по физике А.В. Волковичев сделал доклад о работе Фомашова о реального-конструктивного стиля, в котором критическое обсуждение работы Фомашова о реального-конструктивного стиля.

Figure 7

Extract from the protocols of the Research Seminar "Mathematical-Logic and Philosophical Problems of Mathematics" attached to the Department of History of Mathematical Sciences, M. V. Lomonosov Moscow State University. Protocol of the meeting of 22 December, 1951

On the agenda: The report by the head of the department, Prof. S. A. Yanovskaya on B. A. Trakhtenbrot's letter requesting discussion at the seminar of the views stated in the enclosed text of B. A. Trakhtenbrot's own opinion concerning the resolution approved by the Conference⁹ of the Penza Belinski Pedagogical Institute.

At the seminar meeting the following opinions, supported by all the participants, were expressed.

(a) A symbolic calculus, considered as a device used when solving tasks or proving theorems, in itself is not yet a scientific theory which claims to describe some objects of material reality.

Confusing initial formulas of any symbolic calculus with axioms of a scientific theory, and confusing allowed rules for formula transformations (rules for pure symbolic and formal inference of some formulas from others) with rules of a scientific proof, is characteristic of the logical positivists of the Carnap style and presents an idealistic distortion of mathematics and logic. By means of computing devices (among them also common Russian abaci) it is possible to solve different mathematical tasks; yet a computing machine in itself is nothing but a device used for the implementation of various mathematical operations, and is by no means a mathematical theory. The same holds for a symbolic calculus which allows, starting with some easily visualizable class of strings (the class of all formulas of the given calculus), to pick out successively elements of a certain subclass, which is in general more difficult to visualize. The elements of this subclass may conventionally be called "good" formulas or "regular" formulas, or something else, for instance "inferable in the given calculus". However by no means should one confuse the concept "symbolic calculus" (related currently to the concept of algorithm) with the concept of mathematical theory. B. A. Trakhtenbrot is absolutely right when he objects to such a confusion.

(b) A mathematical theory as, in general, every other arbitrary scientific theory, cannot be built on arbitrary assumptions and on arbitrary rules of logical inference. However, on the contrary, when deciding which symbolic calculi (for instance, a symbolic mechanism for a given algebra) can be used in a given scientific theory and which are not suitable for these purposes,

⁹ Actually, by the mathematics department.

the ability to survey all possible calculi from a general perspective is necessary. This is especially important in answering the question of whether there exists a calculus which suffices for the solution of all (in some sense) tasks of the given theory, or alternatively, whether such a calculus does not exist (impossibility of a complete calculus); this is because for a proof of nonexistence (impossibility), and even its very formulation, one needs in some way to survey, to define, the totality of all possible calculi. So, in order to settle such a survey and proof by mathematical means, it is necessary to disengage from the concrete interpretation of the symbols and formulas of the calculus; that is, similar to how when solving equations one ignores the concrete meaning of the symbols a, b, c, \dots ; 7, 9, 8, . . . which occur in the equations, and surveys the set of all possible equations and their systems (including inconsistent systems), with no commitment to those tasks to which they may be or eventually will be applied. Hence, B. A. Trakhtenbrot is right in admitting that in the general theory of symbolic calculi we should consider and survey arbitrary possible initial formulas and rules of formula-transformations; that is similar to how we consider arbitrary sentences in grammar, independent of their concrete subjects and concrete predicates. We learn this from I. V. Stalin's works of genius on the problems of linguistics.

(c) At the same time, B.A. Trakhtenbrot is obviously wrong when he claims that it was not his task to elucidate the real needs, which challenged the method of symbolic calculi and the field of their application, as well as the extensive criticism of the idealistic aims of the bourgeois experts. One cannot report on a more special topic unless:

(i) as a preliminary the importance of the topic is clarified;

(ii) the real needs and tasks which require just the given method, the corresponding concepts, and the given degree of abstractness are explained;

(iii) the original methodological aims are clarified, in light of which one can proclaim the genuine scientific character of the theories presented in the talk and the legitimacy of the abstractions used in these theories.

It goes without saying that an extensive critique of all the idealistic distortions in the field under consideration is always needed.

The seminar resolved that:

1. Already long ago, problems concerning the essence of symbolic calculi and of algorithms as well as their theory were in need of elucidation in our mathematical literature. Since B. A. Trakhtenbrot is a skilled expert in this theory, the seminar finds it desirable that B. A. Trakhtenbrot write a survey paper on this topic for *Uspekhi Matematicheskikh nauk*. Of course, the paper should contain clarification of the most important results in the field, the fundamental concepts and methods from the positions of Marxism-Leninism.

2. A request be made to the management of the Penza V. G. Belinski Pedagogical Institute to grant B.A. Trakhtenbrot a scientific trip to Moscow to report on and discuss his work at the seminar.

The heads of the seminar:

Prof. S .A. Yanovskaya (Head of the Department of History of
Mathematical Sciences)

Prof. P. S. Novikov

Secretary of the seminar:

A. V. Kuznetsov

27 XII 1951

The Academy of Science of the USSR

The V. A. Steklov Mathematical Institute.

Penza, Chkalov Str 56, B. Trakhtenbrot

The Mathematical Institute sends you a referee report on your Abstracts, composed by experts of our Institute.

Director: Academician I. M. Vinogradov

Scientific Secretary: K. V. Borozdin

Выписка из протоколов научно-исследовательского семинара по математической логике и философии математики при кафедре истории математических наук Московского Государственного университета им. М.В. Ломоносова.

Протокол заседания от 22 декабря 1951 года.

Случаи:

Сообщение зав. кафедрой проф. С.А. Яновской о письме В.А. Трактенброта с просьбой обсудить на семинаре тезисы, написанные в приложении к письму гексте обшего имени В.А. Трактенброта по поводу резолюции, принятой на конференции Пензенского педагогического института им. В.Г. Белкина.

На семинаре были высказаны следующие мнения, поддержанные всеми его участниками:

(а). Символическое исчисление, рассматриваемое как аппарат, используемый при решении задач или доказательстве теорем, само по себе еще не является научной теорией, претендующей на описание каких-нибудь объектов материальной действительности. Символики (базисных) формул любого символического исчисления с аксиомами научной теории, а также допустимы прообразованной формул (правила часто символического и формального вывода на одних формул других) с правилами научного доказательства, характерное для полноты познания типов Кармана, есть символическое изображение математик и логики. С помощью счетных машин (здесь тем числе обыкновенных русских счетов) можно решать множество различных математических задач; но само по себе счетная машина есть только аппарат, используемый для выполнения тех или иных математических

Figure 8

Page 1, Protocol of the meeting of 22 December, 1951

- 2 -

опораний, а не научная математическая теория. Так же обстоит дело и с символическим исчислением, позволяющим лишь из некоторого легкого обзорного класса формул вывести (класс всех формул данного исчисления) выделяясь независимо элементарно определенному подклассу, в общем случае более трудно обзорно. Элементы последнего могут называться, условно, "хорошими", или "регулярными" формулами, или еще как-нибудь иначе, например, формулами, "выходящими в данном исчислении". При всех условиях безусловно одинако символизировать понятие "символическое исчисление" (связывая в настоящее время с понятием алгебры) с понятием и тематической теории. Вад. Трактенбром совершенно прав возражая против такого отождествления.

(б). Математическая теория, как и всякая вообще научная теория, не может строиться на произвольных допущениях и произвольных правилах логического вывода. Но при решении вопроса о том, какие допущения символически исчисления (или, булевый аппарат той или иной алгебры) могут быть применены в данной научной теории и какие недостаточны для ее целей, возможно, возможно упомянуть обзореть или с общей точки зрения все возможные исчисления. Особенно не это важно для решения вопроса о том, существует ли такое исчисление, и того же достаточно для решения всех (в определенном смысле) задач данной теории, или же, наоборот, такого исчисления не существует (невозможность полного исчисления), так как для доказательства невозможности (невозможности) и даже для самой постановки вопроса о этом требуется или-то обзореть, определить совокупность всех возможных исчислений. Z.

Figure 9

- 3 -

А для того, чтобы такое обозрение и доказательство можно было провести математическими методами, необходимо нельзя отвлекаться от всякого вообще конкретного истолкования (интерпретации) знамен и формул исчисления, подобно тому, как, например, при решении уравнений мы отвлекаемся от того, что именно обозначают входящие в них буквы $a, b, c, \dots; x, y, z, \dots$, и обозреваем всю вообще совокупность возможных уравнений и их систем (в том числе и несовместных) независимо от тех задач, к которым они затем будут или могут быть применены. Б.А. Трахтенброт прав, поэтому, считая, что в общей теории символических исчислений мы должны рассматривать и обозревать любые возможные исходные формулы и правила преобразования формул, подобно тому, как мы рассматриваем в грамматике любые предложения, независимо от их конкретного подлежащего и конкретного сказуемого. Этому учат нас гениальные труды И.В. Сталина по вопросам языкознания.

(в). В то же время Б.А. Трахтенброт явно неправ, когда он говорит, что в его задачу не входило освещение реальных потребностей, вызвавших возникновение и тогда символических исчислений, и области их применений, а также развернутая критика идеалистических установок буржуазных специалистов. Нельзя делать ~~доклад~~ доклад на более специальную тему, если значение этой темы ^{еще} предварительно не выяснено, ^{не} раз'яснены те реальные потребности и задачи, которые требуют именно данного метода, и соответствующих понятий, и именно данной степени абстрагирования; если не выяснены исходные методологические установки, в свете которых толь-

Figure 10

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и можно претендовать на настоящую научность тех теорий, которые в докладе излагаются, и законность тех абстрактных идей, которые в этих теориях употребляются. Само собой разумеется, что всегда необходима развернутая критика всяких измышлений в данной области идеалистически извращенной.

Семинар постановки:

1^o Вопрос о сущности символических вычислений и алгоритмов и их теория давно изучаются в освещении в научной математической литературе. Поскольку Б.А. Трахтенброт является квалифицированным специалистом в этой теории семинар ~~института~~ считает весьма желательным, чтобы Б.А. Трахтенброт написал для "Успехов математических наук" обзорную статью на эту тему. Статья должна содержать, конечно, освещение важнейших результатов в этой области, ее основных понятий и методов с пометкой марксистско-ленинизма.

2^o Просить дирекцию Пензенского педагогического института им. В.Г.Белинского дать Б. А. Трахтенброту научную командировку в Москву для доклада и обсуждения на семинаре вышеупомянутой Б.А. Трахтенбротом работы.

Руководители семинара:

зав. кафедрой истории

математических наук

проф. *В.И.Алексеев* (С.А. Яковлев)

проф. *А.М.Михайлов* (М.С. Новиков)

Секретарь семинара (А.И. Кузнецов)

А.И. Кузнецов

27 / XII 1951.

Figure 11

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Resolution.

On the abstract of the talk by B. A. Trakhtenbrot on fundamental problems of mathematical logic, given at the Mathematical Department of the Penza Pedinstitut.

The abstracts of comrade Trakhtenbrot's talks present a correct, to-the-point, and interesting exposition of fundamentals in contemporary mathematical logic and theory of algorithms as well as comrade Trakhtenbrot's own results in these fields. Unfortunately there is too little literature on these topics, and besides that, the basic ideas of these trends and the basic results obtained with these methods are insufficiently known. It is therefore very desirable to publish comrade Trakhtenbrot's lectures in *Uspekhi Matematicheskikh nauk*.

At the same time it is advisable to somewhat extend the introductory part of the lecture, giving enough consideration to the critics of the philosophical views which circulate among some of the foreign scientists — idealists who misinterpret the fundamentals of mathematical logic. In this context one should also emphasize that no symbolic calculus is able to replace human thinking. We should also mention the progressive role of scientists of our country.

It is impossible to give a conclusion concerning the discussions which arose, because they were too sketchy.

27.XII-1951

* * *

Dear Boris Abramovich,

Forgive me for fulfilling your request so late — continual ill-health.

I recently met P. K. Rashevski and told him about your "affairs". He claims that I. E.¹⁰ makes a more and more strange impression. I think that there is a connection between the above and his behaviour in Penza.

I have not yet seen Kolmogorov but Alexandrov, to whom I spoke, promised me to inform him.

If you will be asked to present explanations related to I. E.'s declarations, insist on the expertise of the Mech-math (faculty) of the Moscow

¹⁰ Full name omitted for ethical reasons

University. Make sure that you name the faculty, to avoid sending it to the philosophers.

How is your health? Hearty regards,

A. Lyapunov

20.XII. 54

§4. Epilogue.

The Penza affair culminated with the conference "Marxism and Linguistics" after which the main danger had dissipated. Nevertheless, minor upheavals still occurred (see A. A. Lyapunov's letter dated December 1954). Actually the conference did not raise specific new ideas; as expected it followed the ordinary ritual of attacking "idealists" and of "Against idealism in axiomatic foundations of mathematics" had to be based on "Stalin's statements on abstract thinking in grammar and geometry". Its list of "idealists" was most impressive: Kant, Cantor, Hilbert, Poincaré, . . . (and who not!) At the same time (and relying again on Stalin's instructions) I fiercely "unmasked" vulgarizers of Marxism in general and my opponents — especially. In the second lecture — "Against Vulgarization in Methodology of Sciences", delivered at the Department of Marxism-Leninism, I based my offensive on Yu. Zhdanov's article, as suggested by S.A. (letter of December 1951). Later one of my opponents confessed that he wavered in view of my militant and offensive behaviour which he attributed to my hidden support by some powerful OFFICIAL body! Well, it seemed that I had mastered a new genre which might even have appeared amusing had the situation not been so gloomy. My health was undermined by permanent tension, dread and hard teaching load (often more than 20 hours weekly). It goes without saying that for about two years I was unable to dedicate time to research. In those circumstances it was the selfless care and support of my wife Berta that saved me from collapse. I should also mention the beneficial and calming effect of the charming central Russian landscape which surrounded our dwelling. But despite all these troubles I remember this period first of all for its happy ending. In the summer of 1992, forty years after this story took place, Berta and I again visited those regions. The visit to Penza was especially nostalgic. Most of the participants of those events had already passed away. Only the recollections and of course the beautiful landscape remained.

Let us now return to the Moscow documents. All of them stressed the lack of lucid exposition of the fundamentals of symbolic calculi and of

algorithms for a broad mathematical community. As a remedy they insisted on the preparation of a survey paper on the topic for *Uspekhi*. It was also suggested that the survey “should be based on the positions of Marxism-Leninism and contain criticism of the foreign scientists-idealists”. Finally there was an appeal to me to undertake this work which would also demonstrate my philosophical ideological loyalty. No doubt, this was a tempting proposal for a researcher at the beginning of his career, so let me explain my reaction to it.

(1) First, I did not feel competent to engage in a work which covers both a mathematical subject and official philosophical demands. These demands were permanently growing and changing; they could bewilder people far more experienced than myself. So it seemed reasonable to postpone the project until more favorable circumstances would allow one “to separate logic from philosophy of logic and from philosophy of mathematics, to treat mathematical logic as a science rather than a methodology of idealist philosophy of mathematics” (quotation from [Anellis 1987]). Indeed, such a change in attitude took place gradually and S.A. contributed enormously to it by taking advantage of the critics to which the adepts of Mahr were subjected by Stalin in his “Letter on Marxism and Linguistics.”

(2) To what audience should the intended survey (or book) be addressed? S.A. and the other referees regarded *Uspekhi* as the appropriate journal. Well, *Uspekhi* is a very prestigious journal but it is addressed mainly to mathematical researchers. At that time I already felt that a popularization of the subject would be expedient and useful for a much broader public, say for school teachers of mathematics. Hopefully, through them the flavour of the new developments in logic and computability might even reach school students. (The famous series *Popular Lectures in Mathematics* started by A. I. Markushevich’s *Recursion Sequences* was an encouraging experience.)

(3) In my Penza lectures I focused on symbolic calculi and their role in the formalization of deductive theories which culminated with the incompleteness theorems. A similar story could be told about algorithms and undecidability results. Despite the close relationship between the two avenues the second one seemed to be more understandable (and of course more fundamental). Also the growing and exciting awareness about computers suggested the ultimate decision.

In 1956 the journal *Mathematics in School* published my tutorial paper “Algorithms and Automated Problem Solving”. Its later revisions and extensions appeared as books which circulated widely in the USSR and

abroad. (Throughout the years I was flattered to learn from many people, including prominent logicians and computer scientists, that this tutorial monograph was their own first reading on the topic as students and it greatly impressed them.) An explicit (though very mild) hint to the circumstances under which it was written, namely, to the Penza Affair, appeared only in the foreword to the 1974 edition (quotation follows).

“A few words about the origin of this book. The initial stimulus to publish a popularization of the Theory of Algorithms is rooted in the difficulties and distress the author experienced with his first attempts at oral popularization. This happened more than 20 years ago when the author addressed this topic at meetings with his fellow mathematicians of the Penza Pedagogical Institute. Concepts and results in the Theory of Algorithms like formalization of computational processes, existence of algorithmically un-solvable problems, not only seemed unusual but even frightening. It was, therefore, apparent to the author that a popular paper or small book which could help the reader to reach theorems about algorithmic unsolvability in the shortest possible way was urgently needed. As a result the paper “Algorithms and Automated Solving” was published. Its extended versions were published twice (1957, 1960) as books with the same title and were also translated into several foreign languages.”

Hence, the troubles were not completely in vain! Indeed, as the saying goes — every cloud has a silver lining.

Acknowledgment. Professor B. A. Kushner encouraged me to write these recollections and contributed with useful remarks and suggestions. I benefited from Mrs. Diana Yellin’s help in polishing the English and formatting the text. It is my pleasure to express my gratitude to both of them.

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