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Descendentes Vermelhos de Apolo:

Construindo as Bases para o Laser Soviético (1939 - 1961)

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Red Descendants of Apollo

The Making of Soviet Laser Physics (1939 - 1961)

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Co-orientador: Prof. Dr. Alexei Kojevnikov

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Contents

Acknowledgements

I sat down to write these acknowledgements intending to be brief, but along the way I was seized by a feeling that this dissertation is the materialization of the striking transformation which I went through in the last years, which I own to many people who have helped to put together the mosaic that forms who I am now. I failed in my intent of being brief, but if you are interested in a glimpse of my experiences during my doctoral studies here is where you may find it. The richness of the experiences, the remarkable people and places made it impossible to limit these acknowledgements to few paragraphs. I swear, though, I did my best to make them concise, yet faithful to my gratitude.

My first acknowledgement is to my family, in the broad meaning of the word, more common in Brazilian Portuguese, which helped me to build the solid foundation for all the growth and transformation that were to come. As I look back in my initial years a I see clearly the influence of my parents, brothers, grandmothers, uncles, aunts, and cousins in my upbringing and education. Although I am no longer able to participate in most of family gatherings, I will always be grateful for their unconditional love that gave me the strength and confidence necessary to explore distant lands and face the most daunting challenges.

Olival Freire Junior's professional behavior and scholarship have been an inspiration for me since I knocked his door in 2006 interested in doing research in physics teaching. My interest in history of physics was kindled by the lively discussions on history of quantum physics and history of physics in Brazil going on in the meetings of his research group LACIC (*Laboratório de Ciência como Cultura*). He received with enthusiasm my decision of going for a master's degree in history of physics, but with prudence my decision of working on history of Soviet physics for my doctorate. I am thankful for his trust and hopeful that this dissertation and my subsequent career will reward all the time and attention he has invested in me in almost a decade of mentorship, in which he has been much more than an advisor.

My colleagues of LACIC have been source of inspiration and motivation for me during all this period. In my first years, it was at once daunting and stimulating to witness the talks and discussions of the old guard: Elder Sales Teixeira, José Eduardo Clemente, Mario Ferreira, Fabio Freitas, Frederick Santos, and Caio Fernandes. Special thanks to Elder, who was my co-advisor in my first project of *iniciação científica* and sat patiently for hours to teach me the basics of writing a research paper. He has been my good friend since then. Also to Indianara Silva and professor José Fernando Moura Rocha who have given important contributions to this work with their comments and suggestions. While Indianara and Mayane Nóbrega motivated me to be pragmatic to conclude my master's degree in 18 months, Gustavo Rocha, Thiago Hartz (two learned men with impressive erudition for their age), and my soul sister Leyla Joaquim offered constant support and stimuli for me to aim at a broader education. One way or another, they all have contributed to my academic education. With Virgile Besson I had several discussions that sharpened my knowledge of Soviet history and politics. It was quite a stimulating intellectual exercise to challenge his trotskyist views.

Alexei Kojevnikov is the main responsible for my interest in history of Soviet science and has been a role model of historian throughout my doctoral studies. It is clear in the chapters of this dissertation how his work has guided my questions and supported my arguments, what needs to be acknowledged is that our frequent discussions during my stay in Vancouver shaped a large part of this dissertation, specially the third chapter, the gist of which was written in Vancouver. I highly appreciated his informal advisory style and constructive criticisms. Our meetings were a quite international gastronomic tour. I am also indebted to him for a substantial parcel of my archival sources, as well as for making possible a second trip to Russia. In short, without him I could not have written this dissertation.

My greatest debit of all is to Daria Chusovitina, my wife. Without her I would not have had the guts, nor the means, to write a dissertation on history of Soviet science. Her heroic support and stoic attitude toward life has been a mainstay of my personal and professional growth. We are strikingly different, yet complementary. While I am the kind of dreamer who is constantly elaborating lofty plans for the most distant future, she is totally down to earth and has the most admirable quality of living the present. She can hardly get excited about anything that is not happening today. While I theorize about healthy, simple living, she shows me how to do it in practice with incredible ease. I have learned a lot about how to live a meaningful and happy life with her. She is the best companion for travels, long walks, and in the quest for what really matters (not a trivial task in a society in which people increasingly believe that to be happy one needs a new car or the latest iPhone). She has given me the most precious treasures of my life - our two gorgeous little daughters Vitória and Gabriela. With them I can go anywhere. So long as we are together, any place feels like home. Thanks my love, love you, always, Dasha.

My older daughter has taught me a lot about writing a dissertation as well. A child needs a lot of daily attention, so does a dissertation. Raising a child is a lot about selfless giving. You do it for love. Sometimes it can be boring, annoying, but she's your child, you love her. You take a deep breath and do what you are supposed to. You nurture her with care and love. And suddenly you find yourself living the next moment of happiness. A similar dynamics happened quite often in the writing of this dissertation.

I own my first adventures in Russia to my sister-in-law, Viktoria Murykina, and her husband, Sergei Murykin, who equipped me to face the Russian winter. When I first arrived in Russia I stayed for one month with them in Odintsovo, a city on the outskirt of Moscow. During that period they gave me a taste of winter barbecue in the woods (in which I experienced the warming power of vodka), running on the snow, and introduced me to several sorts of Russian beers, spirits, and zakuski (salty bites highly recommended to accompany shots of vodka).

My subsequent adventures and cultural immersion were provided by my parents-in-law Olga Chusovitina and Yuri Chusovitin. Altogether I have lived about 8 months with them in the city of Magnitogorsk. They taught me cross-country skiing, and made sure that I experienced Russian *banya* (sauna), Soviet-style parties and holidays. Their stories about life in Soviet Union helped me to understand how Magnitogorsk have changed since the fall the the Soviet Union but that it is still a quite Soviet city. Naturally, it has preserved much more of the Soviet lifestyle than big cities such as Moscow and Saint Petersburg. Even the economy and politics of Magnitogorsk is still ruled by the steel industrial complex around which the city was built in the 1930s and is the main revenue source. Nowadays the complex is regarded as the city's blessing and curse. On the one hand, it helps to maintain high-quality public facilities and services ranging from amusement parks to a world-famous hockey team (*Metallurg*). On the other hand, it has put Magnitogorsk on the top of the list of most polluted cities in the world.

I am thankful to several librarians and archivists who have been extremely helpful in the process of research and writing this dissertation. During the period I lived in Magnitogorsk I worked daily in the Central City Library. Its librarians received me with open arms, gave me an ideal working place, shared many stories of life in Russia and the Soviet Union, and asked a host of questions about life in Brazil, forcing me to use all my language skills. Thanks to them my Russian language skills developed extremely fast during the period. By the end of my stay in Magnitogorsk I already felt myself as one of them. I took part in several of their events, and even recited Brazilian poetry, but the most memorable was the *subbotnik,* a day of volunteer work for cleaning and fixing public spaces or doing other community services, a living Soviet tradition created by Lenin in 1919 that has survived in Russia and other ex-Soviet republics. In Moscow, the staff of the Russian Academy of Science (ARAN) was as thoughtful as to allow me access to the reading room during two weeks of the summer of 2012, when it was supposed to be closed. Irina Tarakanova, the archivist of the reading room, was incredibly kind and patient guiding me through the complex bureaucratic procedures of ARAN and watching me to decipher my first archival material. Likewise, in the United States, the staff and librarians of the American Institute of Physics, specially Gregory Good and Stephanie Jankowski, were extremely helpful during my stay there in April 2014.

I am indebted to the American Institute of Physics, for two travel grants and one grant-in-aid that allowed me to attend two conferences in the United States and to work in the Niels Bohr Library & Archives; and to the University of British Columbia, for the tickets that allowed me to travel from Canada to Russia to conduct archival research, and spend the holidays with my family, in the winter of 2013-2014.

I am a child of a fortunate combination of affirmative actions and consistent public support for research began after PT (Worker's Party) arrived in power in Brazil. I entered university in the year the quota for black people from public schools was effected, the steady increase of funding for high-education and research allowed me to have scholarships from the second to the last year of my undergraduate studies (*bolsas de iniciação científica* CNPq), I had CAPES scholarships throughout graduate school, and a CAPES-PDSE (*Programa de Doutorado Sanduíche no Exterior*) scholarship which allowed me to work in Vancouver with Alexei Kojevnikov at the History Department of the University of British Columbia for 9 months. Those programs have been promoting an impressive phenomenon of social mobility in Brazil and are changing the demographics of Brazilian academe. Unfortunately, the government has began to withdraw its support, signaling large budget cuts that are threatening the continuity and consolidation of those changes. I am currently working in one of the newest federal universities in Brazil that is seriously threatened by the cuts. Opportunely, I would like to thank my colleagues of the small, young, yet promising Campus of Barra of the Universidade Federal do Oeste da Bahia, who have welcomed me with open arms.

A doctorate is not made only in libraries, universities, and archives. I have lived impressive experiences during these four years that have influenced my world view and therefore the writing of this dissertation. My bike trip from Saint Petersburg to Stockholm, crossing Estonia and part of Finland, was particularly important in this regard. Crossing few very different countries at the slow pace of a bike ride for more than two weeks gave me plenty of opportunities to observe and think. My most remarkable finding was the understanding of how social inequality is the root of Brazil's major problems, and since then I have made the fight against inequality a life purpose. I am very thankful to Julia Jakovleva (Estonia), Mikko Sovijärvi and Eija Lappalainen (Finland), Thiago Froes and Emma Li (Brazil/Sweden), Michael Gaebler and Leyla Joaquim (Germany/Brazil) for their company and hospitality during that trip.

My stay in the diverse and international Vancouver was a likewise remarkable experience. I am grateful to my Brazilian Friends in Vancouver, Alexandre, Flávia, Júlia, Marina e Phillipe and to other awesome friends and fellows of hikes, walks, and bike rides. They have helped to make memorable what otherwise would be an unbearable six-month stay far from my family enforced by the Citizenship and Immigration Canada, which denied visas to my wife and daughter. In Alexandre, my flatmate, I found a friend with whom to discuss history of science, the meaning of life, and how to change the world, but who also knew how to appreciate simple things like sunsets, trees, and salmons; a friend with whom to bike under the rain in the dismal days of Vancouver winter, or to lay on the grass to appreciate the spring sun; with whom to play and compose Bossa Nova songs or have endless arguments (just for the sake of it) while pilling, cutting, and cooking potatoes (Thanks dude!).

I am grateful to some people who have helped and inspired me to practice Ashtanga yoga with discipline and devotion. All began when Dasha suggested me to try Ashtanga and since my first practice I was impressed by its power. The teachers Tatiana and Lida from Namaste, in Magnitogorsk, and Tania from Ashtanga Yoga Vancouver, welcomed me in their classes and gave me the basis for my home practice. During one of her classes Tania proffered the words that sealed my commitment to Ashtanga: "Yoga gives you the self-knowledge and discipline necessary to find what you are good at, do it well, and do it with love." Indeed, as I mature in my daily practice, I see more clearly how Ashtanga yoga gives me the fortitude, discipline, and concentration so invaluable in writing. The most famous and emphasized remark by Sri Pattabhi Jois, the founder of Ashtanga yoga, applies equally well to writing: "Practice. All is coming."

Around the time I took up Ashtanga I was stricken by a sudden passion for writing. It was in my second Russian winter, as if I had been bitten by a writing bug. Since then writing has been my daily drug, without which my day feels empty, and a feeling akin to angst hunts me until the next time I sit down to write. Thus, my thanks to that writing bug, actually the book Writing is My Drink by Theo Pauline Nestor. But with that passion I also got a tendency to write on and on, and texts that were supposed to be short have become twice, three times, longer. In the end I had to force myself to murder my darlings, to cut out what does not contribute to the argument, regardless how much I love the fragment. This advice, “murder your darlings”, given by George Orwell in On Writing Well, is also know as Chekhov’s gun, because Anton Chekhov has supposedly warned that a writer should never to put a gun on the stage if s/he is not going to use it. Had I been willing to follow it through, I would have cut out half of this paragraph, but this time I allowed myself to ignore Orwell and Chekhov in honor to all those lines, paragraphs, and sections that I have murdered ruthlessly. I take full responsibility. For that and for other poorly constructed paragraphs and sentences in the text that follows, because notwithstanding my love for writing and good writing, I am just beginning to master this subtle and powerful craft.

Resumo

Essa tese é uma narrativa de como masers e lasers foram inventados na União Soviética no começo da Guerra Fria que busca explicar como aquelas invenções ocorreram simultaneamente naquele país e nos Estados Unidos. Para tanto, ela é dividida em dois períodos. O primeiro começa no final da década de 1930 e se estende até meados da década de 1950, quando os primeiros masers foram postos em funcionamento. Esse período é marcado pela crescente militarização da ciência na URSS nos EUA que estimulou o desenvolvimento de pesquisas na direção de tópicos com potencial para aplicações militares e, argumento, levou duas tradições científicas marcadamente distintas a convergirem na invenção do maser na primeira metade dos anos 50. O segundo período tem início por volta de 1955, indo até 1961, e é marcado por transformações profundas na ciência e sociedade soviéticas, entre as quais as mais importantes para essa narrativa foram o reestabelecimento de laços entre os físicos soviéticos e seus pares em países capitalistas e a extensão da competição entre os blocos para além da corrida armamentista. Naquele período, cooperação científica internacional passou a ser vista tanto como uma ferramenta para promover *détente* quanto como uma forma de obter informações sigilosas sobre pesquisas com aplicações militares em potencial sendo conduzidas no outro lado da Cortina de Ferro. Aquele contexto não apenas estimulou os físicos soviéticos a desenvolverem novos tipos de maser, incluindo o maser ótico (laser), em competição com os Estados Unidos, mas foi também foi explorado por eles para desenvolver suas agendas de pesquisas. Se a Guerra Fria influenciou os rumos da física, ela influenciou também as concepções de observadores ocidentais sobre a ciência soviética, e sobre a União Soviética de maneira geral. Isto se vê de forma clara na evolução dos estudos sobre a URSS, em si produtos da guerra fria, ao longo da segunda metade do século vinte. Pensando no leitor pouco familiarizado com os estudos sobre a União Soviética, essa tese traz também um ensaio bibliográfico que discute a evolução da literatura sobre a URSS e sua relação com a Guerra Fria.

Palavras chaves: Masers, Lasers, Guerra Fria, União Soviética, Internacionalismo Cientifico.

Abstract

This dissertation tells a history of how masers and lasers came to be invented in the Soviet Union in the beginning of the Cold War that helps to understand how those devices were invented simultaneously on both sides of the Iron Curtain. It is divided in two periods. The first spans from the end of the 1930s to mid 1950s, when the first masers were launched. That period was marked by the increasingly militarization of science, both in the USSR and the USA, which fostered the development of physical research toward research with potential to yield military applications and, I argue, led Soviet and American physicists, formed in remarkably different scientific traditions, toward the invention and development of the maser in the early 1950s. For their different backgrounds, American and Soviets physicists had different conceptual understandings of the maser, but found common ground on experimental practice. The second period begins circa 1955 and ends in 1961. It is marked by a significant transformation in Soviet science and society, of which the most important for this narrative are the reestablishment of ties between Soviet scientists and their foreign peers from capitalist countries and the extension of the competition between the two blocks beyond the arms race. In that period, international scientific cooperation could be both a diplomatic tool to promote détente and a channel of information gathering. That context not only stimulated Soviet physicists to develop new types of masers, including in optical masers (laser), in competition with the United States, but was also exploited by physicists to promote their own research agendas. In addition, this dissertation also contains a bibliographical essay which discusses how the Cold War influenced Western perspectives on Soviet Union and its Science.

Keywords: Masers, Lasers, Cold War, Soviet Union, Scientific Internationalism.

1 Introduction

"The horizons before us are truly open to infinity. But the responsibility before us is huge, for the level of laser technology and the rate and breadth of its distribution depend on our work."[1]

That was how the director of the Scientific Research Center for Technological Lasers of the Academy of Sciences of the USSR, G. A. Abilsiitov, concluded an interview in 1984, after presenting a very long list of technological applications of lasers his institute was pursuing. Abilsiitov's assertion may sound exaggerated, but it reflects the optimism regarding laser research and development (R&D) in the Soviet Union at the time. The Soviets believed, justifiably so, that in the 1980s they were years ahead of the Americans in laser R&D. The problem was that most of the technology they had developed was directed toward military ends. The Center for Technological lasers was created to shift the balance toward the development of civilian technology. It would concentrate exclusively on civilian technological applications of lasers to close the gap between research and industry, "expanding scientific-research work with the purpose of satisfying the inquires of the economy for very different kinds of technological lasers".[2] Although an academic institution, it was modeled after industrial production centers, and its activities would range from research to manufacturing of technological equipment. Visiting the Center in 1989, when it had a staff of more than 1200 people, a Chinese delegation "felt deeply that this is an ideal model that [their] country need[ed]".[3]

[1] A. Pankov, 'Hyperboloids of the 21st Century (Interview with G. A. Abilsiitov, director of the Technological Laser Scientific Research Center)', *Leninskoie Znamia* oct (1984). Translated and republished in *USSR Report (Physics and Mathematics)*, (Foreign Broadcast Information Service, 1986) (February). – Technical report ⟨URL: `http://www.dtic.mil/dtic/tr/fulltext/u2/a360706.pdf`⟩.

[2] Pankov (as in n. 1).

[3] Guo Zhenhua, Li Zaiguang and Han Yansheng, 'Randon Notes on Visiting the USSR', *Jiguang Jishu (Laser Technology)*, 13 (1989):4 ⟨URL: `http://www.dtic.mil/get-tr-doc/pdf?AD=ADA309984`⟩, p. 5. This publication, as the one cited above, was translated to English by the translation services of the US National Air Intelligence Center. Apparently the archive of

The creation of the Center for Laser Technology reveals that by the late 1970s Soviet physicist had realized that there was something wrong with Soviet laser physics. If, as they have claimed, initially “All [their] thoughts were about mastering the physics in full measure in order to develop our national economy”,[4] when push came to shove, surfing the Cold War rivalry, they promoted attempts to use lasers for the national defense that more strained than helped the economy. By 1978, they had lost track of how much time and money they had spent trying to develop reliable laser-based systems to track and shoot down Intercontinental Ballistic Missiles, airplanes, and satellites. And although those programs provided a better understanding of the physics of high-energy lasers, they failed in their ultimate goal of developing practical and reliable laser defense systems. To make up for that, the minimum they could do was to promoted the translation of the knowledge and expertise developed in military-laser programs into civilian, economically profitable technology.[5]

Civilian applications of lasers are innumerable, and the list continues to grow every year, but it was for the potential military applications that lasers were invented and developed in the early years. The laser was “a remarkable gift” from the Department of Defense (DoD), Ian Hacking concluded.[6] But while I agree with Hacking’s remark regarding the role of military funding in the invention of the laser, giving all the

the Defense Technical Information Center (DTIC) of the US Department of Defense (DoD) has more documents on history of lasers in the USSR than any single Russian institution. To keep US scientists abreast of the laser developments in the USSR the DoD consistently translated papers, interviews, and surveys regarding the Soviet laser research. For example, the periodical **Bibliography of Soviet Laser Developments,** began in 1970, was in its 94th number in 1988, all volumes I have seen contain over 120 pages. Many of them are digitized and available at www.dtic.mil. Unfortunately, I discovered this wealth of sources too late to take advantage of it while writing this dissertation.

[4] N. G. Basov, *Interview with Dr. N. G. Basov by A. Guenther, on 14th September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.* 1984.

[5] 1978 was the year the Terra-3, a project which aimed to develop antimissile defense system with laser weapons, was halted. But one of the leading figures of the Soviet scientific-military complex, Peter Zarubin argued that much earlier “it occurred to many researchers that the implementation of a [laser-based] AMD system... was a practically insurmountable problem (technically and economically). However, it was hard to admit this, and there was no firm evidence to support such a conclusion, while an error of judgment in this case could be catastrophic for the country.” P. V. Zarubin, ‘Academician Basov, high-power lasers and the antimissile defence problem’, *Quantum Electronics*, 32 (2002):2, p. 1049. That project, began in 1964, was the Soviet version of the Strategic Defense Initiative (SDI) proposed by US president Ronald Regan, and promoted by many scientists. Likewise, the SDI spent much but had little success trying to develop a laser AMD system. Nigel Hey, *The Star Wars Enigma: Behind the Scenes of the Cold War Race for Missile Defense*, (Potomac Books Inc, 2006); Joung Cook, ‘High-energy laser weapons since the early 1960s’, *Optical Engineering*, 52 (2013):2.

[6] Ian Hacking, *The Social Construction of What?* (Harvard University Press, 1999), p. 179.

credit to the DoD downplays the role of scientists elsewhere, specially in the Soviet Union, in the invention of the laser.

In her book *The Laser in America* historian Joan Bromberg has acknowledged that

> even the historian who looks at the American work alone sees continually the impact of advances made in Europe and the Soviet Union in the 1950s and 1960s. Clearly we need histories of the maser and laser in other contexts. Then we may compare effects of varying environments on the research directions ad attack a range of supranational questions such as the diffusion of scientific information between nations and within nations.[7]

This dissertation is partly an answer to Bromberg's call, which was very influential in my initial conception of the project that brought me here. It is an attempt to tell a history of how masers and lasers came to be invented in the Soviet Union in the beginning of the Cold War that helps to understand how those devices were invented simultaneously on both sides of the Iron Curtain. As assumed here, Cold War is the indirect confrontation between the Soviet Union and the United States which lasted from shortly after the end of the World War II until about 1991. That context is essential to explain both the excessive militarization of laser physics and how a research program initiated by a young physicist and his first two graduate students around 1953, in a little more than a decade, was transformed in one of the most successful fields of Soviet physics.

The remaining of this introduction has the following structure. In the next section I make a brief presentation of the maser/laser principle and its applications, which was written to stand alone and be published as an essay in a journal of physics teaching.[8] Next, come two sections with historiographical reflections on the concept of Cold War and on writing history of contemporary science, respectively. I conclude with comments on sources and the structure of the dissertation as a whole.

[7] Joan Lisa Bromberg, *The Laser in America, 1950-1970*, (Cambridge, Mass: MIT Press, 1991), p. XII.

[8] That section will be translated to Portuguese and expanded to be submitted to Revista Brasileira de Ensino de Física as a small contribution to the celebration of the year of light (2015).

1.1 A gift from Apollo: maser, laser and their applications

Apollo is one of the most complex and versatile deities of the classical Greek and Roman mythology. He was considered the God of light, knowledge, medicine and healing, music, arts, and more. Laser lights, likewise, turned out to be a very versatile tool, being often called “the tool of thousand functions”, or a “solution in search for problems”. Hence, the parallel between laser and Apollo is not restricted to the fact that He is the God of light. Lasers have performed many roles that were attributed to Apollo. They have reveled new knowledge in several branches of science. Fields as nonlinear optics and quantum optics originated from the study of lasers, its properties, or applications of lasers to study or clarify known phenomena. Lasers found a wide range of application in medical diagnoses, treatments, therapies, and is currently being used as tool for procedures ranging from surgery to wound healing. The French composer, performer, and producer Jean Michel Jarre has enchanted millions of fans with laser music displays and laser harps that have been a high profile feature of almost all his concerts since 1981. And you need only to input “laser art” in a search engine to see all sorts of artwork based on lasers. The connections could go on and on, but I will just to give one more. Apollo is most often represented with a harp or a bowl with and arrow of light, what is perhaps the remotest representation of a directed-energy weapon (DEW), the most advanced of which nowadays are high-power lasers.[9]

What is this heaven-sent gift that permitted so many possibilities? We may say that it is not the device in itself but the ability to harness two quantum mechanical effects called spontaneous and stimulated emissions of radiation. As evident from its full name, Light Amplification by Stimulated Emission of Radiation, the laser uses stimulated emission to amplify the power of radiation. And the process of amplification is so powerful that a single photon in a fraction of a second may generate a high-power beam.

The basic physics of spontaneous and stimulated emissions was settled in the first decades of the 20th Century with the development of the old quantum theory and was explained in 1917 by Albert Einstein, but physicists need almost 40 yeas more

[9] All applications of laser I mentioned in the paragraph are well described in wikipedia.org. For DEWs see Cook (as in n. 5).

to harness and use those effects for practical purposes.[10]

According to quantum mechanics, atomic and molecular systems have discreet and well-defined energy states. Within an atom, for example, an electron can only occupy certain orbitals associated with well-defined energy levels. To transit from one orbital to another it must absorb or emit energy to give a quantum jump, because the region between the energy levels is a forbidden territory according to quantum laws (see figure 1.1). To move into an orbital of higher energy (upper state), the electron must absorb energy; to move into an orbital of lower energy (lower state), the electron must emit energy. The same is valid for other quantum systems. Molecules, to give one more example, have well-defined energy levels associated with the rotation and/or vibration of their atoms, and the transition from one state to another occurs with emission or absorption of radiation, usually microwave or infrared radiation.

Figure 1.1: In this simplified scheme each ball represents an electron (not realistically) and E_n represent the energy associated with the orbital n. According to the atomic model proposed by Niels Bohr, electrons occupy only orbitals with well-defined energy levels. To transit between orbitals they must absorb or emit energy in form of a quantum of radiation given by the expression $E_2 - E_1 = h\nu$, where h is Plank's constant and ν the frequency of the radiation. The balls could also represent molecules, and the process of transition is analogous, what changes is the energy difference and therefore the frequency ν of the radiation absorbed. Illustration by Daria Chusovitina.

When atoms and molecules are in the upper state, also known as excited state, they may move to the lower state either by spontaneously emitting radiation or by being stimulated to do so. Spontaneous emission occurs naturally because the

[10] A. Einstein, 'On the Quantum Theory of Radiation', *Physikalische Zeitschrift* 18 (1917):121, English translation in D. ter Haar, *The Old Quantum Theory*, (New York: Pergamon Press, 1967). A recent, didactic explanation of Einstein's paper can be found in Daniel Kleppner, 'Rereading Einstein on radiation', *Physics Today*, 58 (2005):2.

excited state is unstable and after a period, called relaxation time, the electron spontaneously decays to the lower state, emitting a photon of radiation. In some situations, however, the relaxation time is too long and its necessary to stimulate the transition recurring to the effect of stimulated emission. The easiest way to do so is by irradiating the atoms with radiation of frequency equivalent to the desired transition (see figure 1.2).

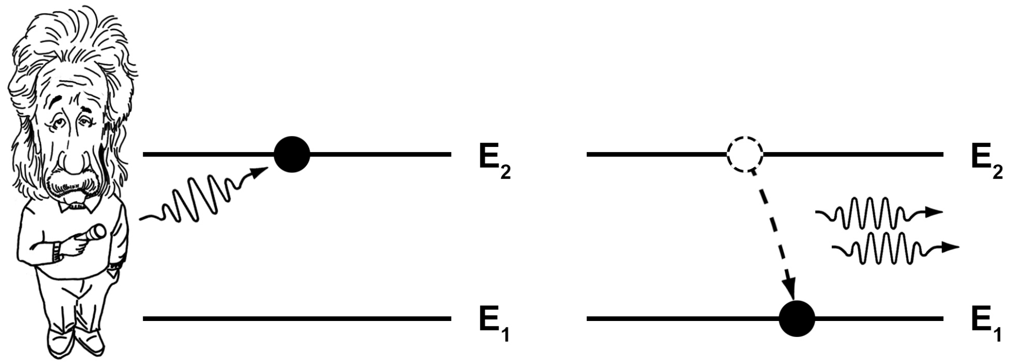

Figure 1.2: This scheme, analogous to the one presented in Figure 1.1, represent the effect of stimulated emission predicted by Albert Einstein in 1917. The arrow followed by a wave-pack represents a photon (again not realistically). When a photon hits an electron in the excited state it stimulates the transition into the ground state and the emission of another photon identical and in phase, i.e. coherent, with the stimulating photon. The energy of the photons has to be equivalent to the energy of the transition, in accordance with the expression $|E_1 - E_2| = h\nu$. Illustration by Daria Chusovitina.

The "Eureka" behind the maser-laser invention was the realization that if one manages to obtain a medium where most of the atoms are in the excited state, spontaneous and stimulated emissions could be used to create a powerful beam of radiation in which virtually all the photons have the same frequency and oscillate in phase with one another, in physicist's language, that is called a monochromatic and coherent radiation.

To give an idea of how powerful the amplification process can be, imagine a simple set-up made of a medium in which almost all the atoms are in the excited state placed between two parallel mirrors separated by a distance of one meter. Once the first photon is emitted it stimulates other atoms to emit other photons, and each of those new photons also stimulate the emission of other photons, and so on. A chain reaction takes place and the number of photons grows exponentially. But that is not all. If the mirrors are good enough, each time the radiation bounces back and forward between the mirrors its intensity increases a little. And as it is moving at the speed of light, in a single second the radiation beam travels the way between the mirrors about 300 million times. Thus, it does not matter how small is the increase in a single trip, in a very short time the power of the radiation will increase up to the limit permitted by the set-up.

This vision of a single photon generating a powerful light beam in much less than a second captured the imagination not only of scientists and the military, but of the general public as well. Laser weapons became the favorite death ray of science fiction writers since the 1960s, as evidenced by its presence in the first pilot episode of Star Track (1965), in the series Lost in Space (1965-1968), and in the epic Star Wars.

But such a system is easier said than done. There are two major problems to devise a setup as the one described above. The first is that to obtain a medium in which almost all the molecules are in the excited state is not an easy task. That situation is called population inversion precisely because in nature the population of atoms or molecules in the lower state is almost always larger than the population of the upper state. Thus, to produce a laser it is necessary to devise an efficient way to create this artificial population inversion.

The second major problem is to find an effective way of confining the radiation within the system, making it bounce back and forth in the medium, to ensure that most of the excited molecules are stimulated to emit radiation and the energy gain will be superior to the energy loss. This system is called resonator. In the example given above the resonator is the pair of parallel plates. For it to be efficient enough the mirrors should reflect almost 100% of the light and be perfectly aligned with each other; a small inclination of one of the mirrors and the light leaks away after a few reflections.

Hence, although this general maser-laser principle may sound simple, and today it is indeed relatively easy to solve those two problems, the knowledge and technology necessary to use stimulated emission to amplify radiation ripened only after the World War II, and thanks to the war. The first person who thought of using stimulated emission to generate light was the Soviet physicist Valentin Fabrikant in the late 1930s, but he could not come up with an efficient scheme to obtain population inversion and did not elaborate on the resonator. It is very unlikely that any other could have done so even with the most up to date knowledge and technology before the WWII. [11]

[11] Several reasons for Fabrikant's failure can be given, but the limited knowledge on population

During the war physicists in many countries, but specially in the United States, England, Germany, and Soviet Union, devoted themselves to the task of developing the technology of radio communication and radar systems to detect enemy airplanes, ships, and submarines. The key task was improving the technology of generation and detection of short radiowaves both to make it more sensitive and stable and to generate shorter and shorter waves, approaching the inferior limit of a spectral region referred to as microwaves (waves of length ranging from 30 centimeter to 1 millimeter). Working on those tasks physicists learned to combine their theoretical knowledge of quantum mechanics with cutting-edge engineering knowledge and technology of generation, stabilization, and amplification of microwaves.[12]

Figure 1.3: The inventors of the maser. From left to right: Hebert Zeiger, Nikolai Basov, James Gordon, Alexander Prokhorov, and Charles Townes. USA 1959. Courtesy of Alexander K. Prokhorov.

That combination of physics and engineering skills, associated with new technology of generation and detection of microwaves, was what permitted American and Soviet physicists to successfully harness the effect of stimulated emission to amplify radiation in the first half of the 1950s. The American team was composed by the

inversion (his thesis is one of the first works on the subject) and resonance phenomena were serious drawbacks. Svetlana G. Lukishova, 'Valentin A. Fabrikant: negative absorption, his 1951 patent application for amplification of electromagnetic radiation (ultraviolet, visible, infrared and radio spectral regions) and his experiments', *Journal of the European Optical Society: Rapid Publications*, 5 sep (2010), ISSN 1990–2573.

[12] Bromberg, *The Laser in America, 1950-1970* (as in n. 7); Paul Forman, '"Swords into ploughshares": Breaking new ground with radar hardware and technique in physical research after World War II', *Reviews of Modern Physics*, 67 apr (1995):2.

Columbia University physicist Charles H. Townes (1915–2015), his PhD student James P. Gordon, and the postdoctoral fellow Herbert J. Zeiger. The Soviet team was initially composed by the physicist Alexander M. Prokhorov (1916–2002) and his graduate student Nikolai G. Basov (1922–2001) working at the Physical Institute of the Academy of Science of the USSR (FIAN). Although among them only Charles Townes worked in the radar project, in the postwar engineering skills were highly valued among American and Soviet physicists. Both teams were well familiar with the latest microwave technologies in their countries and were on the forefront of a new field called microwave spectroscopy, which used microwave radiation to study the structure of molecules and atomic nuclei. Not by chance, they came up with the same solution to the problems of obtaining population inversion and of devising a resonator good enough to make the radiation gain overcome the loss.[13]

Charles Townes hit upon an idea to obtain population inversion to make a generator of microwave based on spontaneous emission in the spring of 1951 while preparing for a meeting in which he would discuss with a few prominent US physicists alternative ways of generating short microwave. His idea was to make a beam of ammonia molecules with roughly the same number of molecules in the upper and in the lower state and then to separate the molecules according to their energy state using a non-homogeneous magnetic field. Once the molecules were separated, he could direct only the excited molecules into a cavity resonator. That would permit to obtain a medium in which virtually all the molecules were in the excited state, ready to transit to the lower state and emit radiation.[14]

The Soviet had had basically the same idea by the end of 1952, the difference was that instead of ammonia they suggested to use a beam of cesium fluoride, and use a non-homogeneous electric field, instead of magnetic, to sort the molecules according to their energy state. They all understood from the beginning that if the molecules in the beam could spontaneously emit while passing through the cavity, and if the cavity were good enough to retain almost all the radiation, they would have a generator of microwave based on a completely original principle.

Yet, the implementation of the idea turned out being not so easy. As Basov and Prokhorov understood since their first theoretical calculations, and Townes and his group would discover while trying to build the device, the time of flight of the

[13] Bromberg, *The Laser in America, 1950-1970* (as in n. 7). The case of Soviet physicists will be discussed in more details in this dissertation.

[14] Ibid.; Paul Forman, 'Inventing the Maser in Postwar America', *Osiris*, 7 (1992):Science after '40.; Charles H. Townes, *How the Laser Happened*, (New York: Oxford University Press, 1999). The details of the technique and apparatus will be given in chapter 2.

molecules inside the cavity was shorter than the relaxation time of the molecules, what meant that the molecules could remain excited all the way through the cavity. Another drawback was that the best cavities they could make with the available knowledge and technology was not so good to confine the radiation efficiently enough to make a generator.

Luckily, those two drawbacks had a single solution: to introduce radiation in the cavity. Introducing radiation would at once stimulate the molecules to radiate and reduce the quality requirements for the cavity, but the device would no longer be a generator, it would be an amplifier. The Soviets proposed that alternative in the first time they presented the idea in a classified meeting in January 1953; and the Americans, independently, in the same year, after many unsuccessful attempts of making a generator based on spontaneous emission.[15] With that change the Americans succeeded in obtaining maser action for with a beam of ammonia for the first time in early April 1954, and shortly after they christened the device after the elegant acronym MASER (Microwave Amplification by Stimulated Emission of Radiation).

The Soviets had began their work toward the maser with a more theoretical approach. They first focused on making a theoretical description of the molecular generator (as they initially called the maser), which they published in1954, and only after that they began the practical work. They launched their first ammonia-beam maser the following year.[16]

Studies of the first masers already revealed them to be very promising devices. The unprecedented stability of their frequencies made them the most precise time standards and their very low noise made them the most sensitive amplifiers. However, the first masers were big, cumbersome devices that could hardly be used outside university laboratories. In the subsequent years, physicists in the United States, Soviet Union, and few other countries, dedicated themselves to the task of developing new types of masers that could amplify radiation of various wave lengths, devising new schemes to obtain population inversion using different materials and effects.

Physicists began seriously considering the possibility of maser action in the optical region around 1957. In the following year Charles Townes and his collaborator Arthur Schawlow discussed the possibilities of infrared and optical masers in a paper

[15] Forman, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14).

[16] N. G. Basov and A. M. Prokhorov, 'Application of molecular beams for radiospectroscopic investigation of rotational spectra in molecules.' *Zh. Eksp. Teor. Fiz.* 27 (1954):4.

that has become a classic of laser physics, and is considered the salvo that started the race to make the first laser.[17]

The winner of the race was the American engineer and physicist Theodore Maiman in the spring of 1960. Maiman obtained laser action with a surprisingly simple setup (Figure 1.4) made of a ruby rod at the center of a coiled flash lamp. Both ends of the rod were coated with silver layers, but in one of the ends the layer was semi-transparent to let the light out.[18] However, the laser was not an invention of a single man, or a single country, and the history of the laser can be seen as a story of a transnational competition between men committed to opposing ideologies that resulted in a joint contribution to invent one of the most remarkable devices the mankind has ever invented.

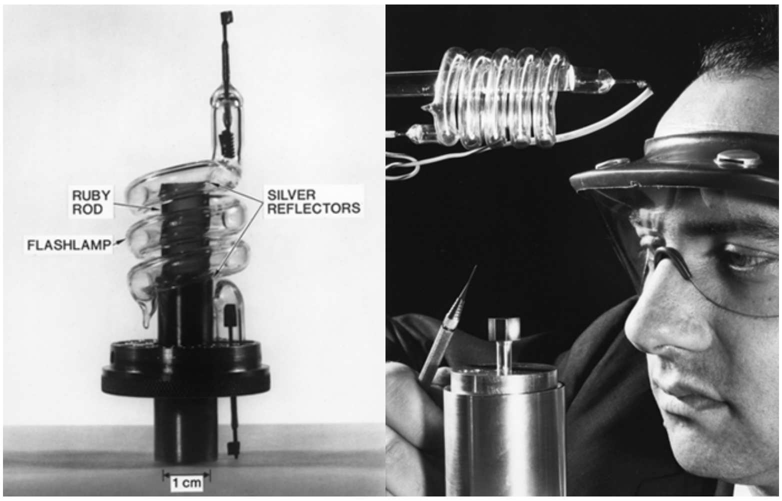

Figure 1.4: On the Left: The first laser. Maiman used a coiled flash lamp to energize atoms in a ruby crystal (aluminum oxide "doped" with traces of chromium). On the right: Theodore looking at a ruby rod, the heart of his laser. Source https://www.aip.org/history/exhibits/laser/sections/therace.html.

Scientists naturally compete with their peers in occasions where there are few groups working on the same problem, but in the context of Cold War the competition between American and Soviet physicists acquired new meanings. It became entangled with the arms race and the perceived need of displaying superiority of their political systems. Thus, masers and lasers were more intensively developed

[17] A. L. Schawlow and C. H. Townes, 'Infrared and optical masers', *Physical Review*, 112 (1958):6

[18] T. H. Maiman, 'Stimulated Optical Radiation in Ruby', *Nature*, 187 (1960):4736, ISBN 0028–0836.

in the Soviet Union and the United States not only because those countries had the largest physics communities in the world, but also because those devices were products of the orientation of science toward the development of advanced military weapons and defense technologies, and that orientation was clearer in those countries than in any other.

Figure 1.5: Alexander Prokhorov, Charles Townes, and Nikolai Basov at FIAN, Moscow, 1965. They were key actors of the defense research establishment of their respective nations, but maintained friendly scientific relations throughout the Cold War.

American and Soviet physicists quickly learned how to exploit the context to advance their own agenda, which arguably overlapped the agenda of the military to a large extent. They caught the attention of military sponsors with promises that lasers would be the base for a new high-tech weapon which could destroy incoming missiles carrying atomic bombs before they could reach their targets. Such a system would definitively shift the balance of power of the Cold War.[19] But lasers could also be used to make more precise radars, to separate isotopes, and could even be the key to dominate nuclear fusion. Hence in the three years following the first demonstration of lasing, the US Department of Defense alone dumped more than 220 million 2015 dollars into laser R&D, and the DoD was not the only military agency funding physical research in the US.[20] Soviet physicists claimed that the Soviet Union invested even more than the United States in laser R&D during the

[19] Jeff Hecht, *Beam : the race to make the laser*, (New York: Oxford University Press, 2005).
[20] Paul Forman, 'Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960', *Studies in the Physical and Biological Sciences*, 18 (1987):1.

1960s.[21]

That pattern, however, was not restricted to the Cold War superpowers. In 1960 the Brazilian physicist Sergio Porto moved to the United States to work at Bell Laboratories and made a successful career in the United States working with lasers. At the end of that decade the Brazilian military government began a negotiation to convince Porto to return to Brazil to develop a method of isotopic enrichment of uranium based on lasers. This was a move toward mastering the technology necessary for Brazil to enrich uranium and possibly to make its own atomic bomb.[22] In the terms of the deal, successfully closed, Porto received a world class laboratory, a team of 30 doctors, mostly his collaborators in the US, and two million dollars, an impressive investment for Brazilian standards at the time. Although Porto's team apparently failed in their ultimate goal –devising a method of isotopic separation –they developed a series of laser technology, specially applied to medicine, that permitted Brazil to become a reference in medical applications of lasers.[23]

Thus, as nuclear and high-energy physics, we may say that laser physics advanced in the Cold War because of its military potentials, but unlike atomic bombs, which became a symbol of men's capacity for self-destruction, lasers found mostly peaceful civilian applications. With compact disc (CD) readers, lasers have brought music to our homes; with diverse kinds of medical procedures, lasers have improved the life quality of numerous people; and with optical-fiber communications, lasers have connected most of the world at the speed of light. Numerous applications of laser in industry, as cutting and hardening of metals, have decreased the price and increased the quality of production. Ultimately, lasers transformed our society. A heaven-sent gift indeed.

1.2 Physics and the Cold War

The concept of Cold War is a mainstay of this narrative. And what a surprise I had when, in the middle of the writing, I was asked whether there really had been such thing as a Cold War. I must confess, I had taken it for granted. I had read hundreds of works framed by the concept of Cold War. Since the beginning of my

[21] Zarubin (as in n. 5).

[22] Olival Freire Junior et al., 'Nuclear Weapons in Regional Contexts: The Cases of Argentina and Brazil', jun (2015) ⟨URL: `http://arxiv.org/abs/1506.05859`⟩.

[23] Walker Antonio Lins and Olival Freire Jr., 'Contribuição do físico brasileiro Sergio Porto para as aplicacões do laser e sua introdução no Brasil', *Revista Brasileira de Ensino de Física*, 32 (2010):3.

doctoral studies I had been following a very diverse and active virtual community of scholars, mostly historians, whose only common ground seems to be that they all study Cold War. "Is it possible that all those pages, all those people, are about something that does not correspond to an actual historical phenomenon?" That was my first thought. In principle, yes. However, I saw little reason to question that an indirect confrontation between the Soviet Union and the United States existed beginning from the late 1940s and that it influenced not only the course of the history of the major nations involved, but of many nations that played a secondary role in the conflict. Yet, I allowed myself to reflect on the question, and thinking critically about the concept and how it influenced the scholarship on Soviet Union produced during the period turned out to be an exciting exercise that resulted in the Chapter 2 of this dissertation.[24]

The answer I gave when asked whether there had been a Cold War was based on a distinction made by Ian Hacking in the book *Social Construction of What?* between interactive and non-interactive concepts. To clarify some issues at stake in polemics regarding the social construction of various things, Hacking argues that we have to distinguish between concepts that interact with the object or person they are meant to describe (interactive kinds), and those that do not interact (non-interactive kinds). For example, "stone" is a non-interactive kind, because the object, stone, is not affected by the fact that we classify it as a "stone", while "'woman refugee' (as a kind of classification) can be called an interactive kind because it interacts with things of that kind, namely people, including individual women refugees, who can become aware of how they are classified and modify their behavior accordingly."[25] This is a fundamental difference between the natural and the social sciences. The classifications and concepts of social sciences are of the interactive kind, while the classifications of the natural sciences are not. Based on this distinction I answered that as the concept Cold War is an interactive kind, once it began to be used by historical actors themselves, it began to shape their actions and interactions. When politicians, scientists and their fellow citizens began to use the term "Cold War" to describe the indirect confrontation between the Soviet Union and the United States following the Wold War II, they began to see themselves as living in a Cold War and to act accordingly. In short, Cold War, the concept, shaped the conflict and

[24] The question was made by my colleague Bruno Martins Boto Leite during the contest for the position of professor of history and philosophy of science I currently hold at Universidade Federal do Oeste da Bahia.

[25] Hacking, *The Social Construction of What?* (as in n. 6), p. 32.

period it was used to describe.[26]

To underpin this point I can give two examples of how the perception of living a cold war influenced the actions even of people living in countries not directly involved in the conflict. The history of Brazil in the second half of the last century would be quite different if it were not for the fears of Western leaders, shared by local elites, that Brazil could be "infected" by the virus of communism and become 'unwilling to complement the industrialized economies' with cheap labor and raw materials.[27] Those fears were the basis for the articulation of the civilian-military coup, backed by the United States, that to defend Brazilian 'democracy' from the 'claws of communism' interrupted it for more than two decades.[28]

The Cold War and the anti-communist rhetoric was also used by US officials to justify anti-democratic interventions in Latin American countries even when there was no shadow of the Soviet Union in events. Historian David Ryan argued that to justify intervention in Guatemala Washington claimed that a 'Communist type of terrorism' existed in Guatemala that served the 'the evil purpose of the Kremlin to destroy the inter-American system'. The real purpose of the actions, however, was to prevent that

[26] The term "cold war" was first used by George Orwell in the essay "You and the Atomic Bomb" published October 19, 1945, little more than two months after atomic bombs were dropped in Hiroshima and Nagasaki, in the British newspaper Tribune. In this prophetic essay Orwell discussed the prospect of "two or three monstrous super-states" possessing a powerful weapon as the atomic bomb and he saw a "permanent state of 'cold war"' as the mostly likely outcome.

"Had the atomic bomb turned out to be something as cheap and easily manufactured as a bicycle or an alarm clock, it might well have plunged us back into barbarism... If, as seems to be the case, it is a rare and costly object as difficult to produce as a battleship, it is likelier to put an end to large-scale wars at the cost of prolonging indefinitely a 'peace that is no peace'." George Orwell, *You and the Atomic Bomb*, (London, oct 1945) ⟨URL: `http://orwell.ru/library/articles/ABomb/english/e{\_}abomb`⟩.

However, the first use of the term in the sense of a war waged through indirect conflict to describe the post–World War II geopolitical tensions between the USSR and its satellites and the United States and its western European allies is attributed to Bernard Baruch, an American financier and presidential advisor, on April 16, 1947. The term caught on and since then became widely used not only in the US. Wikipedia has a good discussion on the origins of term (one of the best I could find online): https://en.wikipedia.org/wiki/Cold_war_(general_term).

[27] That was how the problem of communism was perceived by US planners. "Western leaders feared that Russia's economic growth would inspire 'radical nationalism' elsewhere, and that others too might be stricken by the disease that infected Russia in 1917, 'when it became unwilling to complement the industrial economies of the West'." Noam Chomsky, *Profit Over People: Neoliberalism and Global Order*, ebook edition. (Seven Stories Press, 2011), pp. location 339.Chomsky is discussing the US foreign policy in Latin America with Brazil, since 1945 regarded as a "testing area for modern scientific methods of industrial development based solidly on capitalism", as a major case in point.ibid., pp. location 300.

[28] Elio Gaspari, *A ditadura envergonhada*, Volume 1, (Sao Paulo: Companhia das Letras, 2002).

> 'radical' and 'nationalistic regimes' responsive to popular pressures for 'immediate improvements in the low living standards of the masses' and the development of the domestic needs" conflicted with US demands for 'a political and economic climate conductive to private investments with adequate repatriation of profits'.[29]

As Ryan concluded, "Cold War constructs facilitated Washington's use of covert means to subvert the democratic process, and various economic, diplomatic and military means to support the subsequent brutal 'anti-communist' regime."[30]

Those two examples show that in some cases, as that of Brazil, the fear of the influence of the Soviet Union in Latin America, the fear that Brazil could become another Cuba, was indeed experienced by many historical actors (the local elite, which would lose all their privileges, for example). But even when the fear was not real, the Cold War and its social constructs were used to justify actual action that did shape the history of the period.

On physics, the impact of the Cold War is undeniable. In the early postwar physics became a pillar of the defense industry, both in the Soviet Union and the United States. Mindful of the imminence of a new war, and that a new high-tech weapon more powerful than the atomic bomb could shift the balance of power, policy makers sought to direct science toward knowledge and devices that could lead to new military technologies.[31] In the postwar, historian Paul Forman convincingly argues, "American physics [...] underwent a change in purpose and character, an enlistment and integration of the bulk of its practice and practitioners in the nation's pursuit of security and ever more advanced military technology."[32]

It was based on the history of the maser in the United States that Forman forged the thesis that the presence of massive military funding set the directions of the physical research in the United State, turning physics from basic to applied topics, specially to the development of gadgets with probable military applications. This

[29] David Ryan, *US Foreign Policy in World History*, (Routledge, 2014), p. 153. Ryan is quoting a 1953 National Security Council's top secret document.

[30] Ibid..

[31] See the volume Forman, Paul and Sánchez-Ron, José M., editors, *National military establishments and the advancement of science and technology*, Dordrecht. The size of the literature on science in the Cold War is daunting, and continue to grow fast. A nice bibliographic essay, which is already somewhat outdated, can be found in Audra J Wolfe, *Competing with the Soviets: Science, Technology, and the State in Cold War America*, (The Johns Hopkins University Press, 2012), pp. 143-159.

[32] Forman, 'Studies in the Physical and Biological Sciences, No. 1, vol. 18, 1987' (as in n. 20), pp. p. 150.

Forman's thesis became know in science studies and history of science as the "distortion thesis", or the Second Forman Thesis, and was widely discussed in the literature of those disciplines.[33]

However, although the wide use of the term *distortion* in the literature is telling of how Forman's papers were received by a significant parcel of historians of science, that term is nowhere to be found in any of Forman's papers, and as does not translate well his thesis. The term distortion has led to misinterpretations of Forman's second thesis such as the one criticized by Historian Audra Wolfe, who argued that the term distortion

> ...implies that there exists some sort of pure, undistorted science devoid of political or cultural intention. A generation's worth of scholarship in the history of science has demonstrated that, to the contrary, scientists are participants in the culture in which they live. Their choices and opportunities have always been shaped by the ideological assumptions, political mandates, and social mores of their times. Indeed the idea that American science ever operated in a "free zone" outside of politics is itself a legacy of the ideological Cold War.[34]

Wolfe's criticism may apply to a large parcel of the literature on Cold War science, and it is interesting because it draws attention to how the Cold War affected the image of science. However, if directed toward Forman's work, the criticism is misguided. Forman does not assume that prior to the marriage between physicists and the military science was pure, undistorted, "devoid of political or cultural intention", but, instead, that scientists had control of their discipline, even if to direct it to promote national interests, as German physicists did between the wars.[35] The core of Forman's argument is that while believing that they were dictating the rules,

[33] The Second Forman thesis is articulated in a series of paper on Quantum Electronics in early Cold War: Forman, 'Studies in the Physical and Biological Sciences, No. 1, vol. 18, 1987' (as in n. 20); idem, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14); idem, 'Into quantum electronics: Maser as 'gadget' of Cold-War America', in: Paul Forman and J.M. Sanchez-Ron, editors, *National military establishments and the advancement of science and technology*, (Dordrecht, 1996); idem, 'Atomichron®: the atomic clock from concept to commercial product', *Proceedings of the IEEE*, 73 (1985):7 ⟨URL: `http://ieeexplore.ieee.org/xpls/abs{\_}all.jsp?arnumber=1457534`⟩. Second because Forman became famous for another thesis, that the quantum theory is a result of attempts by physicists to accommodate their discipline to the *zeitgeist* of Weimar Germany.Idem, 'Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment', *Historical Studies in the Physical Sciences1*, 3 (1971), ISBN 9788578110796.

[34] Wolfe, *Competing with the Soviets* (as in n. 31), p. 4.

[35] Paul Forman, 'Scientific Internationalism and the Weimar Physicists: The Ideology and Its Manipulation in Germany after World War I', *Isis*, 64 (1973):2.

in the early Cold War, US physicists lost the control of their discipline. They were lured with astronomical budgets, 'purchased on the install installment plan', to do the physics the military judged to be strategic.[36] Thus, the Second Forman thesis seems to be better translated in terms of "militarization", rather than "distortion", of science.

However important was military funding on influencing the directions of physical sciences, we ought to be careful not to put to much emphasis on pecuniary interest alone. Among the researchers who profited from the novel working mode of physics in the postwar, a host of them were physicists inclined to engineering, or the other way around, and were genuinely excited by the challenges and potentialities of well-funded, practically oriented research.[37] After the Soviet Union obtained nuclear weapons, specially with the outbreak of the Korean War in the early 1950s, many were also moved by the international tension (some, as the iconic Eduard Teller, to an unreasonable degree). As Joan Bromberg puts it, "at the time, it seemed right and normal to most of US scientists to pursue fundamental research and military applications"[38]. Forman took in account all this excitement as well as this sense of patriotic duty among physicists as part of a host of factors that overdetermined the directions of physical research toward device making and the "pursuit of security through ever more advanced military technologies".[39]

Bromberg, on the other hand, convincingly argues that the skills physicists learned and the devices they produced during their military-oriented, device-making activities helped to develop fundamental research as well. She shows how the involvement of the physicist Marlan O. Scully in military research helped him to devise important experiments in foundations of quantum mechanics. Ironically, this is the reverse of the formula propagandized by scientists to military agencies in order to increase the share of basic research sponsored by military contractors, according to which basic science naturally advances military technology. In this case we have military technology advancing basic science.[40]

[36] Forman, 'Studies in the Physical and Biological Sciences, No. 1, vol. 18, 1987' (as in n. 20), p. 229.

[37] See chapter 9 Timothy Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines*, (Stanford, Calif.: Stanford University Press, 1997).

[38] Bromberg, *The Laser in America, 1950-1970* (as in n. 7), p. 15.

[39] Forman, 'Studies in the Physical and Biological Sciences, No. 1, vol. 18, 1987' (as in n. 20), p. 150.

[40] Joan Lisa Bromberg, 'Device Physics vis-a-vis Fundamental Physics in Cold War America: The Case of Quantum Optics', *Isis*, 97 (2006):2.

What we may learn from all this is that, regardless of how scientists try to compartmentalize, its hardly possible to draw a line between applied and fundamental research in most of physical research done in the second half of the twentieth century. Yet, in the discourse of scientists fundamental and basic research do seem to be distinct categories, and this distinction often oriented the actions and demands of scientists. In this regard, Konstantin Ivanov has drawn attention to an interesting phenomenon that took place in the early Cold War and is crucial for our narrative. While among American scientists after the late 1950s there occurred a ideological shift from pure, or fundamental, to applied research; in the Soviet Union the shift was from applied to fundamental research. In practice they were somewhere in the middle of those two ideological extremes. They did both fundamental and applied science and had to compartmentalize not to deal with the conflict between their words and deeds.[41]

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Military funding and research became conspicuous in the Soviet Union as well, and for similar reasons. Soviet planned economy was from its outset a war economy, namely, an economy where certain targets are fixed in advance and then plans to achieve them by allocating recourses whatever the short-term cost are launched; and it would remain so at least until Stalin's death.[42] Science was a central part of that economy, playing a major role in governmental planning throughout the Soviet period. Since its conception in the 1920s and early 1930s, the Soviet scientific establishment, structured in form of large-scale research institutes was designed to be centrally planned. When the WWII loomed in Europe, those circumstances made military mobilization of Soviet science a relatively easy endeavor.[43]

By 1950, Soviet science was as engaged in military research as American science, if not more. And during the Cold War science in those two countries was entangled with society to the same effect, well expressed by Alexei Kojevnikov:

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[41] Konstantin Ivanov, 'Science after Stalin: Forging a New Image of Soviet Science', *Science in Context*, 15 jan (2002):02. For compartmentalization see conclusion of Forman, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14).

[42] E. J Hobsbawm, *How to Change the World*, (New Haven and London: Yale University, 2012), ISBN 9780300188202, p. 9.

[43] Alexei Kojevnikov, 'The Making of the Soviet Bomb and the Shaping of Cold War Science', in: Cathryn Carson and David Hollinger, editors, *Reappraising Oppenheimer: Centennial Studies and Reflections*, Volume 1956, (Berkeley: University of California, 2005).

> On both sides of the Iron Curtain, militarization proceeded along to main path: the movement of scientists into secret, military oriented research centers, and the increased penetration of massive military funding and classified research into ostensibly civilian institutions. Probably the majority of academic physicists in both U.S.S.R. and the U.S. during the postwar decade did at least some military research. Many moved between classified and academic settings and ran parallel projects in both, often within the same laboratory buildings and with the same or similar instruments.[44]

As we will discuss in more details in chapter 2, Soviet Science came to resemble its North American counterpart not only in its engagement with military research, but in content as well. In their first big postwar challenge – developing the atomic bomb – Soviet leaders and scientists deliberately followed the Americans; an strategy well expressed in the slogan "to catch up and to surpass" promoted by Stalin. Later, as the competition with the west extrapolated the arms race, the same strategy was extended to other research fields.

Thus, there are good reasons to believe that Forman's militarization thesis may be, without loosing its strength, extended to Soviet science as well. In fact, the uniformity of American and Soviet research agendas in physical sciences, what is implied in extending Forman's thesis to Soviet Science, should be the starting point to understand the parallel development of masers and lasers in the United States and the Soviet Union.

1.3 Learning to write history of contemporary science

After obtaining a bachelor's degree in physics, the decision of going for a master's degree in history of science for me meant a substantial change. A move which implied learning new methodologies and disciplinary values, it entailed a conscious, deliberate effort to break out of my training overtly focused on natural sciences, since high school, and to adopt a discipline in which there are no theoretical or experimental proofs, but plausible versions of past events, in which we do not create theoretical models, but reconstructions of the processes that produced whatever structure it is we're seeking to explain based on evidences the past has bequeathed us. That is to say that learning to write history of science implied mastering what

[44] KOJEVNIKOV, 'The Making of the Soviet Bomb and the Shaping of Cold War Science' (as in n. 43), pp. 142-143.

for historian John Lewis Gaddis "is one of the most sophisticated of all methods of inquiry", with which historians are well familiar –the narrative.[45] This section is my personal reflection on this process of learning to write history of contemporary science.

That historical accounts are narratives which, notwithstanding being one of the most sophisticated methods of inquiry, ought not to be taken as descriptions of a real past as it actually happened was a common ground of the discussions of a discipline of Social History[46] and of the book *The Landscape of History* by John Lewis Gaddis. That discipline, which I attended during my master studies, and that book, which I read in the same period, were bases of my first systematic studies of historiography and had strong influence on how I looked at historical narratives afterwards. Later I came to regard that conception of history as a reaction to, or a result of, the threat to the tenets of historical objectivity posed by the late arrival of the 'linguistic turn' and the postmodern attacks on the discipline of history.

The 'linguistic turn', began early in the 20th century with the emphasis on the relationship between philosophy and language, to history meant questioning the assumption that the past can be described as it actually happened, detached from the ideological perspective of the historian who is describing it. Proponents of the linguistic turn argue that the past does not exist outside our textual representations of it, and that these representations cannot be separated from the ideological baggage that historians bring to them. Although this was the basis for the post modern attacks that questioned the point and possibility of writing history and generated a general sense of crisis in the discipline in the 1980s and 1990s, some historians embraced the 'linguistic turn' as an opportunity to rethink the foundations of the

[45] John Lewis Gaddis, *The Landscape of History: How Historians Map the Past*, (New York: Oxford University Press, 2002), ISBN 0195066529, p. 88. This idea that I had to learn the methods and internalize the mores of the discipline of history was rooted in the assumption that history of science, as history of art, wars, or slavery, was a sub-discipline of history. This assumption was evident in Olival Freire Jr.'s works, courses, and the composition of his group, LACIC, which included students with basic training in history and natural sciences, and I initially took it for granted. But still in my first year as a master candidate I learned that it was not to widely held. As I became more familiar with the community I realized that not all historians of science paid due attention to history and that historians *tout court* were not so willing to acknowledge history of science as a sub-discipline of history. This latter realization came in the the first ENAPEC, a National meeting for young scholars in history of science and technology, held in Belo Horizonte 2011, when one of the main talks, delivered by a historian *tout court,* reflected upon the question "Is history of science history?". The talk raised general discomfort in the audience and was the center of polemics among my fellow students for the following weeks.

[46] Discipline offered by Fátima Pires for graduate students of the Department of History of UFBA in the first semester of 2011.

discipline and reformulate the concept of historical objectivity.[47] In practice, and institutionally, the arrival of the 'linguistic turn' to history marked the dawn of cultural history and is often referred to as 'cultural turn'.

Among the first to embrace the implications of the linguistic turn to historiography, the French philosopher Paul Ricoeur argued that the associations between history and narrative was for a long time denied by the dominant historiography that, aiming to establish history as a scientific discipline modeled after the natural sciences, denied any link between history, supposedly a faithful description of real facts, and fictional narratives, free creation of the minds of their authors. For Ricoeur, "the problems posed by the epistemological break between fiction and history, or between myth and history, turned attention to the question of evidence, at the expense of more fundamental question of what accounts for the interest of a work of history".[48]

But far from aiming to blur the distinction between history and fiction to undermine history, what Ricoeur shows is that looking at history as narrative allows historiography to drawn from the rich literature on narrative analysis and literary theory.[49] Elaborating on Aristotle's concept of *mimesis* (sometimes translated as "representation", sometimes as "imitation") in *Time and Narrative* Ricoeur develops what at first seems a pure exercise of literary criticism, analyzing how literary works represent and re-configure the world. However, after convincingly arguing, that history is ultimately a form of narrative,[50] he applies his framework to historiography and the resulting analysis is plenty of novel historiographical insights, many of which would be assimilated by adepts of cultural history.

[47]For a discussion of the impact of the linguistic turn on the discipline of history and a call to reflect upon its implications to move "Toward a History of the Social" see James Vernon, 'Who's Afraid of the 'Linguistic Turn'? The Politics of Social History and Its Discontents', *Social History*, 19 (1994):1. A discussion of postmodernist crises in history is also is presented in Paul Forman, 'On the Historical Forms of Knowledge Production and Curation: Modernity Entailed Disciplinarity, Postmodernity Entails Antidisciplinarity', *Osiris*, 27 (2013):1. However, Forman is far less optimistic about that crisis as an opportunity for reformulating the foundations of the discipline.

[48]Paul Ricoeur, *Time and Narrative*, Volume 1, (Chicago: University of Chicago Press, 1990), p. 151.

[49]That Ricoeur, a philosopher, not a historian, was among the first to explore the implications of the linguistic turn to historiography is symptomatic of the unwillingness of most of historians to accept the implications of the linguistic turn in history. That's one of the reasons why the linguistic turn arrived in history much later than in other disciplines of humanities. Vernon (as in n. 47).

[50]Nowadays its common to read historians referring to their histories as stories, but the careful way that Ricoeur builds his argument to defend that history is ultimately narrative suggests that this was not taken for granted among his potential readers.

From the perspective of analysis of narrative, historians, as poets or novelists, to make their narratives depend on an understanding of the world which is grounded not only on temporal structures and knowledge of basic elements as agents, means, ends, circumstances, and so on, but also on knowledge of the symbolic system that gives meaning to actions and discourses produced in a given culture. The comprehension of those actions or discourses require historians to make a conscious effort of interpretation of their symbolic meaning in the cultural context of their object of study, which, as a rule, is foreign to them. Historian Marc Bloch seems to have graped this intuitively when he warns that history if full of "states of mind which were formerly common, yet which appear peculiar to us because we no longer share them",[51] but Bloch still lacked the theoretical foundations for a more systematic reflection on those "state of mind" as a culture with a symbolic system that gives meaning to particular actions and discourses. Only later, based on the linguistic turn and its implications to the interpretations of culture discussed by Clifford Geertz, could Ricoeur argue that "To understand a ritual act is to situate it within a ritual, set within a cultic system, and by degrees within the whole set of conventions, beliefs, and institutions that make up the symbolic framework of a culture."[52]

To give a concrete example from history of Soviet science, Alexei Kojevnikov drew on Wittgenstein's notion of language games to forge an anthropological approach to the ideological disputations in Soviet Science in the late 1940s. Rejecting the notion that their results were determined from above, he investigated the rituals of criticism and self-criticism, borrowed from political live, used by scientists to settle the disputations. Kojevnikov concludes that although the rules of the rituals were determined the results were open and dependent on the performance of individuals or groups of scientists. Thus, what looked like an anti-democratic influence of ideology in science for some historians, when understood from a symbolic framework, may be regarded as a democratic, if idiosyncratic, way of settling scientific disputes.[53]

Historians, of course, unlike poets and novelists, have strong constrains while writing their narratives. As John Gaddis put it, historical narratives "vary in purpose,

[51] Marc Bloch as quoted in Gaddis (as in n. 45), p. 106.

[52] Ricoeur (as in n. 48), p. 58. Cultural anthropologist Clifford Geertz was one of the first to explore the implications of the linguistic turn to anthropology in his classic essay Clifford Geertz, 'Thick Desccription: Toward an Interpretive Theory of Culture', in: *The Interpretation of Cultures: Selected Papers by Clifford Geertz* (New York: Basic Books, 1973).

[53] Alexei Kojevnikov, 'Games of Stalinist Democracy: Ideological discussions in Soviet Sciences', in: Sheila Fitzpatrick, editor, *Stalinism: New Directions*, (New York: Routledge, 2000).

but not in their methods. For in all of them, we ask ourselves: ‘How could this have happened?’ We then proceed to try to answer the question in such a way as to achieve the closest possible fit between representation and reality.”[54] Thus, although they often follow a chronological order, historical narratives are most often written having an ending as a lighthouse that guides the author and the reader, and along the way to that ending the narrative should be as coherent as possible with reality, namely the historical sources, records of the events that may be included in the narrative.

However, as we may infer from the discussion above, historical sources never talk for themselves, and that “closest possible fit to reality” may be less tangible than it sounds. The interpretation of the meaning of actions and events registered in the historical sources quite often requires the historian to explicit or translate in the narrative the symbolic cultural framework that makes them meaningful (otherwise his or her narrative cannot be intelligible to readers who do not share that framework). And this process of translation, as well as the preconditions necessary for historians to understand the sources, necessarily involves preconceptions deeply rooted in their own culture. Therefore, a historical narrative may be regarded as a bridge that links the culture of the historian with the culture the period and place s/he interprets, and the fit to reality might change as the culture of the historian changes.

In this perspective to write a historical narrative is not to describe the past in its own terms, as historians once intended, because that is impossible; instead, it is to dialogue with the past from the present. The preconceptions of the world inherent in the cultural tradition of the historian, far from being negative, offer the basis for new forms of comprehending the past. Hence, each historiographical tradition, each generation of historians, will dialogue differently with the past and perceive it differently.

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Contemporary history profited twofold from the crisis and reconfiguration of history initiated by the linguistic turn. First, questioning the possibility of writing the past as it actually happened, including of the most distant past, weakened the main objection of historians of earlier periods against contemporary history, that

[54] Gaddis (as in n. 45), p. 105.

the proximity of the historian to the event s/he is studying does not allow detachment. Contemporary historians were already aware of their subjectivity when the post modern attacks on history, and the ensuing sense of crises, pushed historians of earlier period rethink their notion of historical objectivity.

Second, with its characteristic abundance of sources for analysis of discourse (oral-history interviews, memoirs, and the abundant bureaucracy of modern governments), contemporary history became one of the main locus of historians prone to cultural histories. Far from being regarded as a setback, the familiarity of the historian with the culture of his object actually came to be seen as a head start to understand the meaning of action and discourse in the symbolic framework of the culture in question. Historians of present (this quite vague and flexible timeframe) usually have an easier time to understand the "state of mind" of his or her period.[55] But in any case, "It's always dangerous to exalt 'to the level of the eternal observations necessarily borrowed from our own brief moment in time.'"[56]

A major challenge for contemporary historians lies on the relationship between the historian and his or her subject. Contemporary historians write about people who can write back, and those people have their own narratives of their lives that quite often conflicts with the narratives written by historians. This problem was brought into sharp relieve by David Carr in his book *Time, Narrative, and History.* Scrutinizing the relationship between life experience and narrative, Carr argued that for events we have lived to become life experiences they must be configured in a narrative that becomes part of our life story, which in turn may be regarded as a broad narrative that follows principles of coherence, unity, and structure, typical of narratives. Along our lives, we are constantly revisiting our life story. From time to time we engage in some kind of reflection about our lives, regardless whether we write about it or not. Carr argued that although "we may think of autobiographical reflection as being performed in the present and directed entirely toward the past, more often however, it is concerned with the past in order to render it coherent with or comprehensible in terms of a present and a future."[57] As he concludes (his emphasis), "Life can be regarded as a constant *effort*, even a struggle, to maintain

[55] Contemporary historians often argue that they have their own sense of the "atmosphere of the time" to guide them through the sources. There are histories in which the historian is at once the author and witness of his narratives. Martin Johnes, 'on Writing Contemporary History', *North American Journal of Welsh Studies*, 6 (2011):1.

[56] Marc Bloch as quoted in Gaddis (as in n. 45), p. 106.

[57] David Carr, *Time, narrative, and history*, (Bloomington: Indiana University Press, 1986), p. 75.

or restore the narrative coherence face of an ever-threatening, impending chaos at all levels."[58]

The implications of this to contemporary historians, who relies heavily on autobiographical accounts and oral histories, are tremendous. To illustrate this I would like to bring a lengthy, precious quotation from an interview with Charles Townes, one of the main characters of this dissertation, regarding the role of the military in the invention of the maser:

> It is interesting –there are a couple of people who write about laser history that I think have a very doctrinaire political point of view. One of them is this woman, [Joan] Bromberg, who's written a book on lasers that has just come out, and another one is a historian at Smithsonian [Paul Forman]. And they both keep trying to sell the idea that the navy was very interested in shortwaves, it had important uses for them, and they were very interested in the possibility of generating energy and hence they did this, and they had it all in mind and this was the reason we did what we did, I did what I did. And not only the military, but also industry. That shortwaves were very important to industry, and so industry was very interested in pushing us in this direction.
>
> That's pretty far from the truth. There was one industrial person who had some interest, and I'll mention that. For the navy, this was kind of exploratory. They didn't really see a lot of uses, but they were hoping there would be some uses, and it was really exploratory for them. Another part of the historians story is that the military was very interested in pushing me on the maser and so on. Well, they were completely uninterested in the maser when I started it. It was just one of the things they tolerated, along with the rest of my science, and in order to have the laboratory there, they would give us freedom, and okay, if I thought I wanted to do this, why it was all right, but they were quite uninterested. Now, on the other hand, they did sponsor this committee to explore the field, and I think that is a very good and sensible thing that they did. But we basically didn't get anywhere very much, and we just didn't find any particularly good methods for doing it.[59]

"[T]he committee to explore the field" was the Advisory Committee on Millimeter Wave Generation of the Office of Naval Research (ONR), which had Townes as the chairman. Townes conceived the idea of a millimeter-wave generator based on

[58] Carr (as in n. 57), p. 91. For Carr, "the most striking occasion for such reflections are those radical conversions, usually religious or political, in which a new view of life, of oneself, of one's future, requires a break with or a reinterpretation one's past".

[59] Charles H. Townes, *Inteview with Charles H. Townes by Suzanne B. Riess in 1991 and 1992*, 1992 ⟨URL: `http://content.cdlib.org/view?docId=kt3199n627{\&}brand=calisphere{\&}doc.view=entire{\_}text`⟩.

spontaneous emission that led to the maser while preparing for a meeting of that committee, which was one of "a panoply of advisory committees" created by the Navy to foster physical research in particular directions.[60] We may see that Townes does not deny the factual existence of the committee, he reinterpreted it as a "very good sensible thing" the Navy did "to explore the field", as if the ONR were a philanthropic organization willing to promote the advance of knowledge for its own sake.

It is hardly questionable that military interests, funding and strategies for directing physical research toward particular strategic problems were major causal agents in the invention of masers and lasers. This is clear not only in Bromberg's and Forman's work, but in most of historical narratives on history of masers and lasers, including this dissertation. [61] But if we look at Townes's statement as an effort to restore narrative coherence things suddenly fall into place. Townes was "a scientists and active church member", who at the end of his life became a "leading advocate for the convergence of science and religion"; so much so that he was awarded the 2005 Templeton Prize for Progress Toward Research or Discoveries about Spiritual Reality, as described in its webpage, "the world's best known religious prize and the largest annual monetary prize given to a single individual" ($ 1.5 million).[62] With the memories of the Cold War fading away, his past role of cold warrior was hard to accommodate in a life narrative coherent with his new role of leading religious figure.[63]

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What does it take to become a good historian of science? One of the founding fathers of the discipline, living in an epoch that disciplines were on the high, George Sarton set high and clear standards for its practitioners. Just to illustrate, the knowledge he considered essential for a "real" historian of science included "'A knowledge of the European languages, paleography, scholastic philosophy, political

[60] Forman and Sánchez-Ron (as in n. 31), pp. 282-284.

[61] Forman, 'Studies in the Physical and Biological Sciences, No. 1, vol. 18, 1987' (as in n. 20); idem, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14); Forman and Sánchez-Ron (as in n. 31); Bromberg, *The Laser in America, 1950-1970* (as in n. 7); Hecht (as in n. 19); Mario Bertolotti, *The History of the Laser*, (Bristol, UK; Philadelphia, PA: Institute of Physics Publishing, 2005); Hey (as in n. 5).

[62] *Charles Townes Wins 2005 Templeton Prize - Archived News*, 2005 ⟨URL: `http://www.templetonprize.org/tpctwtp.html`⟩ – visited on 2015-09-13.

[63] A similar denial of connections between the invention of the maser and military research was made by Nikolai Basov in the 1980s. That will be discussed in the chapter 3.

history, ecclesiastic history' as well as a basic training in one of the natural sciences".[64] He believed that "no greater service can be rendered to the history of science... than by relentlessly insisting upon the necessity of raising the standards of scholarship".[65]

History of science have changed dramatically since Sarton's times, and those standards are clearly in need of a reformulation. Sarton understood science as an international enterprise in the quest for a true, context-independent knowledge. International, in that context, meant mostly European; hence the need of knowing the European languages. But the development of science after the World War II removed Europe from the center of the scientific activity, or created new centers. The United States, propelled by an inflow of illustrious scientist-refugees, quickly gained prominence. The Soviet Union also consolidated itself as a major scientific center. Many other nations that had been neglecting their scientific communities, with the growing awareness of the importance of science to their national sovereignty and economy, began to foment science and support the formation and growth of their scientific communities. Thus, we may say that in the second half of the 20th century science has become truly international, and as historians turn their attention to contemporary science, those developments left the "language requirements" of the discipline in need of revision.

The most important disciplinary changes in history of science, however, were not due to changes in science itself, but on how science was perceived. Landmarks as Thomas Kuhn's *The Structure of Scientific Revolutions,* Paul Forman's article on quantum mechanics in Wiemar Germany, and the Strong Program of Sociology of Science, nowadays essential part of the training of historians of science, each in its way have transformed the discipline and how its objects, science, and scientists, are perceived and represented. Science is no longer regarded as an international enterprise in the quest for truth, but as social practice situated in a well defined context. Scientists are no longer regarded as geniuses working in relative isolation with the sole purpose of advancing knowledge, but generally as people, if distinguished, involved in a cultural milieu, and more often than not, concerned with questions prominent in their time and societies. The result was a shift from macro histories encompassing many nations and centuries to micro histories concerned with

[64] James Conant, 'George Sarton and Harvard University', *Isis*, 48 (1957):3, p. 305. Sarton's odyssey to establish history of science as an academic discipline with high standards is discussed in Aydin Sayili, 'George Sarton and the History of Science', *Turkish Review*, 25 (1996), ISBN 00211753; see also Forman, 'Osiris, No. 1, vol. 27, 2013' (as in n. 47).

[65] Quoted in Idem, 'Osiris, No. 1, vol. 27, 2013' (as in n. 47), p. 62.

well-defined temporal and geographic settings; a shift from the global to the local.[66] If on the one hand, those changes eased the language requirements and made disciplines as paleography and ecclesiastic history almost irrelevant to most historians of science, on the other hand, they increased the dependency on other disciplines as sociology, anthropology, economics, political sciences, and even literary studies.

That focus of the discipline on the local in the last decades has produced a host of insightful historical narratives that scrutinizes the development of science in all sorts of local contexts, but how does science look like on a global scale? After all, science is still a supranational institution. To answer that question a tendency toward the global has emerged recently in history of science. Following globalization, and ensuing developments in the discipline of history, transnational approaches have been maturing among historians of science as a promising ways to think the history of science on a global level and to articulate the results of local studies in order to "come up with truly integrated narratives bringing together alternative and conflicting viewpoints".[67]

For the training and practice of historians of science, this new approach brings new challenges. It has not only risen once more the language requirements, but also entails the need of forming a broader historiographical perspective and establishing connections and collaboration between historians of science on an international scale (if we are to avoid the need for accessibility to archives in many countries that may make transnational history a luxurious and elitist approach). Reflecting upon the methodology of transnational history, Pierre Saunier argues that historians inclined to that approach ought to conduct 'research in different languages, to become familiar with several archival systems, historiographical traditions, and questions,

[66] Although both helped to direct history of science toward micro histories, Forman and the sociologists of the Strong Program had totally different and conflicting goals. While Forman's history was a denunciation of Weimar physicists' betrayal of their discipline to accommodate it to the *Zeitgeist* of Weimar Germany, and can be thus seen as a defense of the discipline, the goal of the Strong Program was anti-disciplinarian, meaning that they aimed at showing that scientists are guided not by disciplinary mores, but by factors external to their discipline. A good collective reflection of the impact of Forman's work on history of science can be found in Carson, Cathryn, Kojevnikov, Alexei and Trischler, Helmuth, editors, *Weimar Culture and Quantum Mechanics: Selected Papers by Paul Forman and Contemporary Perspectives on the Forman Thesis*, (London; Singapore, 2011). For a discussion of this change as a shift from the global to the local see Simone Turchetti, Néstor Herran and Soraya Boudia, 'Introduction: have we ever been 'transnational'? Towards a history of science across and beyond borders', *The British Journal for the History of Science*, 45 may (2012):03.

[67] Idem (as in n. 66), p. 17. That paper is a review essay on transnational history of science. See also the other essays in that special issue of the British Journal for History of Science dedicated to historiographic reflections on the approach.

to learn how to imagine the sources which can help to answer his questions'. He also invites us to avoid being 'complacent with limits and habits inherited from the historian's linguistic domain and/or from training'. Those recommendations, in the opinion of Simone Turchetti and collaborators, point in the right direction.[68]

What then are the requirements for becoming a good historian of science today? What standards should the young scholar and graduate student conform to? In the beginning of my graduate studies, I participated in valuable discussions that approached those questions in a course on "advanced topics in history of sciences", lectured by Olival Freire Jr., largely based on the book *Positioning the History of Sciences.* That book is a remarkable starting point to understand what is the discipline nowadays. However, the variety of opinions, sometimes contradictory, presented by its contributors may be both evidence of the heterogeneity of the discipline, a positive thing, or the lack of consensus about what are the underlying disciplinary mores of the history of science.[69] As I conclude this doctoral dissertation and look back upon my readings and experiences within the community, it seems to me that the answers to those questions posed at the beginning of the paragraph are largely dependent on approaches and idiosyncrasies of particular groups and/or scholars. My own standards were mostly shaped by Freire Jr. and a few scholars he presented to us, his students, as role models of historians of science. And as in the last year I have become more inclined to transnational history, I keep Saunier's suggestions of skills to be developed by transnational historians in mind. But is it possible nowadays to define clear disciplinary standards and mores for the discipline as Sarton once aimed?

To conclude this section I would like to make a few remarks on the process of producing this dissertation. In accordance with what was discussed above, I have not tried to hide myself behind a pretense objectivity. Instead, I have tried to be aware of my position in the text, of how my political views were affecting my interpretation of the events. This exercise made me reconsider some points and to substantiate others with more arguments. I have also made myself tone down the writing, of even the argument itself, in respect for the views and opinions of other historians, because self-awareness led me to understand that my position, likewise, is likely to be targeted by historians writing from elsewhere. Besides, although I

[68] Saunier is quoted in Turchetti, Herran and Boudia (as in n. 66), p. 17.

[69] Kostas Gavroglu and Jiirgen Renn, 'Positioning the History of Science', 248 (2007) ⟨URL: http://www.springerlink.com/index/10.1007/1-4020-5420-3⟩, ISBN 978–1–4020–5419–8.

have done my best to be intellectually honest and to render a narrative as faithful to my sources as I possibly could, my current limitations of knowledge, language, and time were naturally translated in limitations of the narratives presented in this dissertation. I intend to look at criticism as a way of overcoming some of those limitations and improving the overall quality of present and future works.

Writing a dissertation can be daunting. Few times I was paralyzed by the realization that to write a good dissertation, besides learning Russian language, what was happening at a pace much slower than expected, I felt I had to study economics, political science, sociology, philosophy, psychology, a little of German, and etc.. The solution to get out of my paralysis was to keep repeating to myself two phrases that were my mantras of the last years: "keep calm, and write on;" and "you don't have to write *The Dissertation*, but you *Have* to write a dissertation." It worked. But the most important thing I have learned while writing this dissertation was that discipline, patience, and practice are essential elements on the long, long journey to becoming a "real" historian of science. Whatever it may be.

1.4 Notes on sources, archives and the structure of the dissertation

The influence of Forman's work and his militarization thesis is widespread in this dissertation. I was awed when I first read Paul Forman's paper on quantum mechanics in Weimar Germany. The way he rallied elements of the intellectual environment of Weimar Germany to show their influence upon the pioneers of quantum mechanics, and on the very content of that theory, made me raise my eyes from the page and stare at the white wall in thoughtful amazement a few times. Naturally, his work on postwar American science has become a mainstay of my comparison between American and Soviet Science. I have read some of his papers over and over again, and every time I have found new nuances that had escaped my understanding, such is the depth of those works. No doubt, military funding and the strategies adopted by military agencies to lure scientists into the kind of research they wanted, which Forman has skilfully revealed, were central aspects of the laser history.

Joan Bromberg's *Laser in America* has also been my constant companion since I decided to work on history of lasers. Writing her book while the Soviet Union was collapsing, Bromberg had almost no access to sources for the history of the Soviet laser, and limited her narrative to the United States. Yet, her book, together with

Paul Forman's works, was for me both a model and the scholarly reference on laser research in the United States. Through her history of lasers Bromberg was able to present a comprehensive and detailed picture of the research establishment in the United States which involved its major institutions: Universities, professional associations, and the military and civil industries. For her, laser research "became a 'hot topic' precisely because its properties were well matched to the reward structure in a large number of sectors of the U.S. high-technology establishment as it was constituted in the early 1960s."[70] Therefore, working on lasers was an effective way to develop not only a successful career in physics, but a profitable business as well.

My understanding of the history of Soviet science has been largely shaped by Alexei Kojevnikov and his works. *Stalin's Great Science* [71] was the seed of my interest in Soviet science, which has since been nourished by our interaction that began with his first visit to Brazil in 2010 and was intensified during my nine-month fellowship at the University of British Columbia under his supervision. The revelation of how concepts of freedom and collectivism formed by Soviet physicist living under Stalin shaped the collectivist approach to solid state physics, today incorporated in mainstream of physics, had upon me an impact similar to that of Forman's thesis on quantum mechanics in Weimar Germany. His book was also the starting point of my exploration of Soviet history. Therefrom I was led to the works of Sheila Fitzpatrick and other revisionists, and in turn, to post-revisionists as Stephen Kotkin, whose work *Magnetic Mountain*,[72] one of the milestones of the historiography on Stalinism, to my surprise focused on the building of Magnitogorsk, My wife's home city, where we lived during most of my stay in Russia. The late Stalinist period is well described in more recent works published by Juliane Fürst.[73] Those works allowed me to understand Soviet life and culture beyond totalitarianism.

Following the emphasis on science as part of culture, which has been central in Olival Freire Jr.'s work and teaching, I have tried to build my narrative of the history

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[70] Bromberg, *The Laser in America, 1950-1970* (as in n. 7), pp. p. 224.

[71] Alexei Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists*, (London: Imperial College Press, 2004).

[72] Sheila Fitzpatrick, 'The Soviet Union in the twenty-first century', *Journal of European studies*, 37 (2007):1, ISSN 0047–2441, 1740–2379; Stephen Kotkin, *Magnetic Mountain: Stalinism as a Civilization*, 1st edition. (Berkeley, Los Angeles, Oxford: University of California Press, 1995).

[73] Juliane Fürst, *Stalin's Last Generation: Soviet Post-War Youth and the Emergence of Mature Socialism*, (Oxford University Press, 2010), ISBN 9780199575060; idem, 'Late Stalinist society: history, policies and people', in: *Late Stalinist Russia: Society Between Reconstruction and Reinvention* (London and New York: Routledge, 2006).

of masers and lasers in close dialogue with the general historiography of the USSR, conveying how science affected, and was affected by, events in Soviet history.

A few books have been written about the history of lasers in the United States; some of them briefly discuss the developments in the Soviet Union. Besides the academic history by Joan Bromberg, I highlight three other historical accounts by Mario Bertolotti, Jeff Hecht, and Nigel Hey for they also helped me to form new perspectives on historical events and scientific concepts.[74] Additionally, there are narratives focused on protagonists of the laser history, as the autobiographies of Charles Townes[75] and Theodore Maiman,[76] Nick Taylor's chronicle of Gordon Gould's patent war,[77] and Edwin Brit Wyckoff account of Maiman's invention. [78].

Bertolotti, Hecht, and Hey profited from a few secondary sources made available in English language recently to discuss some of the main developments in the Soviet Union. Hecht focuses on three years between the proposals of the laser by Charles Townes and Gordon Gould and the recognition of Theodore Maiman as the winner of the race to make the first laser, including Valentin Fabrikant, Nikolai Basov and Alexander Prokhorov as competitors. Bertolloti's is a history of the laser which begins with the ancient Greeks and includes short accounts of the work of Soviet physicists. It offers particularly clarifying conceptual discussions. However, both authors largely disregard the Soviet social and cultural contexts. Nigel Hey have made a much more careful study on Soviet laser. He personally interviewed few prominent Soviet laser physicists and paid some attention to the Soviet social context, but apparently he did not conduct archival research and he focuses mostly on the development of laser Weapons.

In the last decade, with the celebrations of the 50 years of masers and lasers, Russian physicists who were among the pioneers of laser research in the Soviet Union have published their historical accounts of laser research in their institutions. Sergei Bagayev, Oleg Krokhin, and Alexander Manenkov were working at FIAN under the supervision of Alexander Prokhorov or Nikolai Basov in the late 1950s. In 2005 they published a paper about the history of quantum electronics in the USSR discussing the main steps of Basov's and Prokhorov's way to the laser and

[74]Bertolotti (as in n. 61);Hecht (as in n. 19);Hey (as in n. 5).
[75]Townes, *How the Laser Happened* (as in n. 14).
[76]Theodore Maiman, *The Laser Odyssey*, (Laser Press, 2001).
[77]Nick Taylor, *Laser: The Inventor, the Nobel Laureate, and the Thirty-Year Patent War*, (Simon & Schuster, 2000).
[78]Edwin Brit Wyckoff, *Laser Man: Theodore H. Maiman and His Brilliant Invention*, (Enslow Elementary, 2007).

the research performed in some Soviet research institutes.[79] A similar paper was published in 2010 by Nikolai Karlov, Oleg Krokhin, and Svetlana Lukishova, where they chronicled the history of quantum electronics at FIAN and at the General Physics Institute (later named Prokhorov General Physics Institute).[80]

A different account of the history of the laser in the Soviet Union began to emerge in the last years with the release of new archival material and the publication of a short paper by Inna Belousova about the laser research performed at the Vavilov State Optical Institute, known by its Russian acronym GOI. The institute was part of the Defense Industry Ministry and most of its research was classified. It had a large budget and a long research tradition in optics, competing with FIAN in that field. Inna Belousova began working at GOI in 1960 under the supervision of Leonid D. Khazov, who was already involved in the race to make the first laser. According to her account, after the news that a ruby laser was made to oscillate in the United States they focused their attention on ruby, and by the summer of 1961 they had successfully built the first laser in the Soviet Union. However, as the research was classified, the first public announcement that a laser was put to operate in the Soviet Union came from FIAN.[81] As with the early proposal by Valentin Fabrikant, this episode will remain to be explored in future works.

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A significant parcel of primary sources related to the history of the lasers and masers in the Soviet Union are found in form of selected papers and documents of Nikolai Basov and Alexander Prokhorov. This is the case of a publication by the archivist of the Archives of the Russian Academy of Science which gathers a selection of Basov's papers that includes the early presentations and reports related to his molecular-generator (maser) proposal.[82] Prokhorov's papers are found in two publications. The first is a selection of papers on quantum electronics, published in 1996.[83] The second contains a short memoir written by Prokhorov's wife

[79] Sergei Bagayev, Oleg Krokhin and Alexander Manenkov, 'Pages from the history of quantum electronics research in the Soviet Union', *Journal of Modern Optics*, 52 aug (2005):12, ISSN 0950–0340.

[80] N V Karlov, O N Krokhin and S G Lukishova, 'History of quantum electronics at the Moscow Lebedev and General Physics Institutes: Nikolai Basov and Alexander Prokhorov', *Applied optics*, 49 sep (2010):25, ISSN 1539–4522.

[81] Inna M Belousova, 'The laser in the USSR: the first steps', *Physics-Uspekhi*, 54 jan (2011):1, ISSN 1063–7869.

[82] A. N. Starodub, 'Zapiski Arkhivariusa (Notes of Archivist)', *Izdanie Arkhiva Fizicheskogo instituta im. P. N. Lebedeva RAN* 2 (1997):1.

[83] Bunkin, F V et al., editors, *A. M. Prokhorov, Quantum Electronics, Selected Papers*, (IzdAT, 1996).

Galina Alexeevna Prokhorova, recollections by some of his former collaborators, documents, and interviews that were invaluable aid in understanding Prokhorov's life and work.[84]

Recently, important sources for the history of quantum electronics in the USSR were made available in English. With the historical interest steered by the celebration of the 50th anniversary of the first maser, Soviet pioneering works on masers and laser were and translated and published in English. According to the editors, the papers were selected by their pioneering nature and scientific impact.[85]

Those sources mentioned in the paragraphs above, most of which can be found on the Internet, plus a thick pile of scientific papers published by Soviet physicists in the 1950s and 1960s that Alexei Kojevnikov brought me the first time he visited Brazil would be enough to write an original dissertation. However, a dissertation work in history cannot be complete without archival research. I have also collected documents in two visits to the Archive of the Russian Academy of Science (ARRAS), in Moscow. That archive houses most of FIAN's institutional bureaucracy. I have particularly profited from reports and yearly plans of the laboratory of oscillations, letters regarding international conferences, and the documentations of the promotions of Alexander Prokhorov and Nikolai Basov to members of the Academy of Science.

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A dissertation on history of Soviet physics written in English in a Brazilian university is likely to let the reader wanting some explanation, but it is not difficult to understand the rationale behind my choices. The Soviet Union was a key actor in the history of the 20th century, an obligatory subject in books and classes of general history. The Russian Revolution and its ensuing development, often called the Great Soviet Experiment, is a key chapter in the history of our civilization. Because if we regard history was a discipline essential to understand who we are and how we have gotten here, Soviet history is an essential topic for all modern societies. For that reason, and for the its role of superpower in the Cold War, the Soviet Union was arguably the country that attracted most attention of scholars world wide in the

[84] Shcherbakov, I. A., Mikhailova, G. N. and Prokhorov, A. K., editors, *Alexander Mikhailovich Prokhorov. Vospominaniia, Stati, Interviu, Dokumenty*, (Moskva: Fizmatlit, 2006).

[85] S.N. Bagaev et al., *Beginning of the Laser Era in the USSR (Collected Papers)*, 2010.

second half of the last century. And even nowadays communities of scholars dedicated to historical studies on Russia and other ex-republics of the USSR in Europe and North-America continues to be large and active. Apparently, interest on Soviet and Russian history is rising once more with the increase of the tensions between Russia, European Union and the United States, as reflected in a "prognosis in the world of academia, where funding proposals and demand for Slavic specialists has begun to increase since the start of the Russia-Crimea debacle."[86]

As for history of physics, likewise, one can hardly write a book on history of contemporary physics without discussing achievements of Soviet physicists or the influence of the Soviet Union and the Cold War on the development of physics. Any physics graduate student nowadays is acquainted with many contributions of the Soviets to modern physics as the Cherenkov effect, Hartree-Fock method, Fock states, and so on. They even might have studied with some of the books of the influential *Course of Theoretical Physics* by Lev Landau and Evgeny Lifshitz,[87] and probably know a few anecdotes about Russian physicists.

The Soviet Union was present in almost all presentations of the last conference for early career scholars promoted by the American Institute of physics held in 2014. During that conference I realized with satisfaction that I had made a good choice for a dissertation topic, one that would permit me to dialogue with a broad spectrum of the community of upcoming historians of physics.

My intentions of establishing a dialogue with the international community of historians of science was the main factor behind the decision of writing a dissertation in English. As I mentioned before, I had chosen the topic influenced by my readings of the works of Alexei Kojevnikov and Joan Bromberg, two close collaborators of Olival Freire Jr., and writing a dissertation in English seemed to be a condition to establish a dialogue with them; and not only with them. It greatly increased the number of my potential collaborators. I know of only very few historians of science in Brazil with whom I could dialogue and they all can read and write in English. Hence, It was worth to face the challenge of learning to write in English. Even if I find no interlocutor outside Brazil, the learning process itself was well worth.

[86] *Interest in Contemporary Russian Literature Grows in US / Global City NYC*, 2015 ⟨URL: `http://globalcitynyc.com/2014/10/21/interest-in-contemporary-russian-literature-grows-in-us/`⟩ – visited on 2015-09-13.

[87] Karl Hall, '"Think Less about Foundations": A Short Course on Landau and Lifshitz's Course of Theoretical Physics', in: David Kaiser, editor, *Pedagogy and Practice of Science: historical and contemporary perspectives*, (Cambridge, Ma: MIT Press, 2005). – chapter 8.

To conclude, a few words about the structure of the dissertation. Each of the following chapters was written to be a separate paper. The advantages of this format is that they are almost ready to be submitted to publication (the first has already been submitted). The inconvenience is that the narrative is not fully integrated and to make them independent I had to repeat some information and quotations. I have tried to minimize those inconveniences, but in any case, I believe, the pros outweigh the cons.

The Chapter 2, the Cold War and Western Perspectives on Soviet Science, as mentioned above, is a reflection on how the Cold War, the conflict, fostered and influenced the development of studies on history of the Soviet Union and its science. It is thus a meta-historical account that looks at history of Soviet science as a discipline at the intersection of Soviet history and history of Science. It will be specially helpful for those who are not familiar with historical studies on Soviet Union to understand the major trends and historiographical interpretations in the field as it evolved in the second half of the last century and to situate this work in relation to them.

Chapter 3 is an attempt to explain how the maser could be invented simultaneously in the Soviet Union and the United States in a period of restrained scientific exchange between Soviet and American physicists, mostly limited to journal publications. It begins in the late 1939 with the graduate training of Alexander Prokhorov and ends circa 1955 with a comparison of the understanding of the maser by the Soviets and the Americans. Along the way, key topics in history of physics are approached, namely militarization, secrecy, and the dynamics between theory and experiment.

The fourth and last chapter is about the process of internationalization of Soviet science in mid-1950s, after Stalin's death. It spans roughly from 1955, year of Prokhorov's first foreign trip, to the launching of the first Soviet lasers in 1961. The chapter shows how Prokhorov and Basov exploited the context of competition with the United States to promote their agenda and to create one of the most successful branches of Soviet physics. It also reveals the multiple roles scientists could play on the stage of the Cold War.

2 The Cold War and Western Perspectives on Soviet Science

Science was a central element of the Soviet Union throughout its existence, regarded by official ideologues as essentially entangled with society. Not only its direction, but its structure and demographics were supposed to be determined by social needs and policies of the Communist Party, which in turn, offered unprecedented financial support for science. Along the history of Soviet Union scientists and government developed a complex and multi-faced relationship that resulted in the world largest scientific establishment, equally complex and multi-faced. For those and other reasons, Loren R. Graham, the dean of history of Soviet Science in the West, has argued that one can hardly imagine a case better suited for testing our contemporary ideas and theories regarding science, technology, and society than the historical experience of Russia and the Soviet Union.[1] Alexei Kojevnikov, echoing Graham, highlights that "the Soviet case was characterized by, on the one hand, an exceptionally high development of science and, on the other hand, a distinctive social and cultural milieu artificially isolated from most international contacts by political barriers. This exceptional combination offers historians a perfect opportunity for genuinely comparative studies of science and society."[2]

Hence, history of Soviet science certainly deserves the attention of the communities of historians, sociologists, and philosophers of sciences at large, and above all of those interested in the relationship between science, politics and ideology. More than in any other country, perhaps with the exception of the United States, science in the Soviet Union has received a large share of attention of foreign scholars, specially

[1] Loren R. Graham, *What Have We Learned About Science and Technology from the Russian Experience?* 1st edition. (Stanford University Press, 1998)

[2] Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), p. 277

since the beginning of the Cold War, and the ensuing literature indeed offers valuable insights on the nature of scientific knowledge. However, as Soviet science itself, that literature has been strongly influenced by the tides and political passions of the Cold War. With this in mind, I endeavored to write this historiographic essay with an overview of the evolving perspectives on Soviet science in the last half century. My strategy was to look at the dominant trends in the Western historiography on Soviet Union along the Cold War inquiring how they affected the perspectives on history of Soviet science.[3] Hopefully this paper will stimulate other students of science to become more familiar with Soviet science and embrace this "perfect opportunity for genuinely comparative studies."[4] In any case, there are valuable lessons to be learned from the evolution of the historiography of Soviet science.

History of Soviet science is at the intersection of two sub-disciplines, namely the history of science and Soviet history, that have evolved along similar lines in the last half century and in the last decades have relied on methods of social and cultural history. The similarity is not surprising, though, if we consider that both sub-disciplines have been affected by the cultural and intellectual climate of the Cold War and key events such as the anti-communist hysteria of the late 1940s and 1950s, which favored a scholarship framed in terms of the binary opposition between the "totalitarian East" and the "democratic West"; and the protests of the late 1960s, which favored contentious scholarships as the revisionists in history of Soviet Union and the constructionists in history and sociology of science. But perhaps it was the end of the Cold War, and the ensuing integration between the Eastern and Western communities of historians, added to the influence of developments in the academic discipline of history (the linguistic turn) that caused the most clear convergence between Soviet history and history of science.

[3]This review essay is not purported to be exhaustive. For this methodological choice of discussing the dominant historiographic trends, I set aside works that do not dialog with the historiography of Soviet Union. Many of then, as those by historian Helge Kragh, are more history of specific theories or disciplines, cosmology in his case, than history of Soviet science. Some of these works were discussed recently by ALEXANDRE BAGDONAS, JOÃO ZANETIC and IVAN GURGEL, 'Controvérsias sobre a natureza da ciência como enfoque curricular para o ensino da física: o ensino de história da cosmologia por meio de um jogo didático', *Revista Brasileira de História da Ciência*, 7 (2014):2. That work was already an effort published in this journal to explore the lessons on the nature of science that we can learn about the nature of science from the Soviet experiment. Likewise, another recent paper presents an overview of Russian science from 1917 to 2010. SANTOS JUNIOR (2012). This essay differs substantially in content and approach. While Santos Junior (2012) is a *longue dureé* synthesis, this essay is more concerned with historiographic trends in history of Soviet science as a discipline.

[4]KOJEVNIKOV, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), p. 277

Assuming that the reader is more familiar with the evolution of history of science and science studies, which have been the subject of a dossier in a recent issue of this journal,[5] in this essay I dedicate more attention to developments in Soviet history and its impact on history of Soviet science. Then, in the next section I discuss the totalitarian interpretation of Soviet Union, which resonated with Robert Merton and Karl Popper's claims that science best develops in democratic societies. Next, I turn to the challengers of the totalitarian school, a group of social historians self-professed revisionists which came of age in the 1960s and resembled some schools of the sociology of science both in their motivation to subvert the dominant perspective in their discipline and in their methodological choices. In the section 3 I discuss the post-Cold War and post-revisionists perspectives on Soviet history and history of Soviet science, and how they challenged widely held beliefs on Soviet science and society that underpinned many Cold War-era works on Soviet Union. I conclude the essay discussing how the historiography of the Soviet Science resonates with Christopher Hill's claim that history needs to be rewritten at every generation.

2.1 Totalitarians and Mertonians

In the aftermath of the World War II, as Cold War began to take shape and the Soviet Union replaced Nazi Germany as the enemy number one of Western civilization in the view of European and North-American tk , Western scholars began studying attentively what was going on behind the Iron Curtain. Kremlinology and Sovietology acquired strategic significance and were highly stimulated in North American Universities, attracting a few bright minds. Then, the order of the day was "know your enemy".[6] An emblematic case of this period was the creation of the Harvard University's Russian Research Center. In 1947 the Carnegie Corporation, diagnosing a shortage of "serious work on Soviet behavior in Washington," convinced Harvard professors to accept money to create a research center dedicated to Russian studies. The first head of the Center, the anthropologist Clyde Kluckhohn, knew almost nothing about Russian language and culture at that time. However, he was an old acquaintance of military agencies. During the WWII he had worked at the Foreign Morale Analysis Division of the Office of War Information studying the Japanese

[5] See Revista Brasileira de História da Ciência, vol. 6, 2013:2. pp. 151–248.

[6] David C. Engerman, *Know Your Enemy : The Rise and Fall of America's Soviet Experts*, (Oxford: Oxford University Press, 2009).

culture. Afterwards, considering the wartime work a great success, Kluckhohn energetically promoted their methods of studying "culture at a distance".[7]

The Harvard team and other similar centers then created to promote Soviet studies developed ingenious methods to interpret what was going on inside the Kremlin and how it maintained pervasive ideological control on the population, the most likely means being terror and coercion. Soviet Union, despite having led the anti-fascist resistance, under this light came to resemble the fascist states and had its similarities with Hitler's Germany emphasized under the label "totalitarian state". Those studies forged the so called totalitarian framework that dominated the studies on Soviet Union until the late 1960s. They formed a sophisticated, although one-sided, view of Soviet Union "from above", which focused on high politics and the ideological foundations of the regime, paying due respect to Marxism-Leninism as the motor behind the Soviet project, but neglected agency to Soviet society.[8]

Much of the scholarship done in the heydays of the totalitarian school, 1950s and 1960s, was framed in terms of the binary opposition between the "totalitarian East" and the "democratic West". In history and philosophy of Science, the thesis that science and democracy reinforced each other forged by Robert Merton and other intellectuals in the late 1930s, in the context of the rise of fascism in Europe, was endorsed by the philosopher of science Karl Popper in the 1950s. The purported separation between science and ideology of the "democratic West", often seen as an uniform whole, was contrasted with the supposedly dangerous mix of science and Marxist ideology practiced in the Soviet Union.[9]

In the late 1950s the totalitarian framework began to crack from within. David Engerman in his essay on social sciences in the Cold War gave two instances of military projects whose results questioned the basic assumptions of the totalitarian

[7] David C. Engerman, 'Social Science in the Cold War', *Isis*, 101 jun (2010):2, ISSN 0021–1753, p. 397. Indeed the work was considered to be so successful that the so called area studies, such as Brazilian studies, Latin American studies, Asian studies and so on, proliferated in North American universities nourished by military money. Nowadays, when the Cold War passions are past, some US social scientists still feel embarrassed about their role in producing work that was either sponsored by or proved to be useful to US defense and intelligence operations. A diagnose of the embarrassment and a defense of Cold War social sciences is presented in Audra J Wolfe, 'Cold War Social Science', *berfrois.com* (2013).

[8] The totalitarian framework is discussed in Sheila Fitzpatrick, 'New perspectives on Stalinism', *Russian Review*, 45 (1986):4; Igal Halfin and Jochen Hellbeck, 'Rethinking the Stalinist Subject: Stephen Kotkin's" Magnetic Mountain" and the State of Soviet Historical Studies', *Jahrbücher für Geschichte Osteuropas*, 3 (1996):44

[9] See introduction of Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71)

model.[10] First, in the early 1950s the Air Force paid $1 million (almost $55 millions in current dollars[11]) to Harvard's Russian Research Center to make a "Working Model of the Soviet Social System" based on interviews with refugees. In publications and classified reports, Kluckhohn and his team concluded that the "Soviet regime had wide if not deep support from its citizens, and was not teetering on the blink of collapse; American forces attacking the Soviet Union, in short, would not be greeted as liberators." Contrary to expectations, they concluded that the Soviet Union was a stable industrial society, in important ways similar to the United States.[12]

The second military project whose results questioned the basic totalitarian assumptions was the Smolensk Archive, the first archive available to Western social historians of the Soviet Union. In 1941, when the German Army invaded the city of Smolensk, close to the Western borders of Russia, it managed to take a large part of the local archive of the Communist Party, containing a wealth of documents and letters from citizens and Party officials, which offered a window into a micro-cosmos of the USSR. Upon the defeat of Germany, the American Army found and claimed the archive, which in turn, passed through the CIA, the US Army Air Force, and was finally given to RAND (Research ANd Development) Corporation, a Global policy think tank created by the Air Force "to help to improve policy and decision making through research analysis".[13] The first analysis of that archive, published in 1958 by Harvard political scientist Merle Fainsod in the book *Smolensk under Soviet Rule,* "offered a devastating empirical challenge to the notion that the Soviet Union was under total control of a small group of schemers in the Kremlin; 'the totalitarian facade', Fainsod concluded, 'concealed a host of inner contradictions.' Indeed he drew attention to 'surging energies from below' that would become 'the seedbed of tomorrow's political debates."'[14]

[10]One of the purposes of Engerman's essay is to show that work produced in area studies, even those directly financed by military agencies, were not always aligned with military needs and expectations, and produced valuable knowledge on cultures around the world. Soviet history, as developed in North American universities, is a case for this argument.

[11]Economy cost of a project is measured using the relative share of a project as a percent of the output of the economy. It reveals the importance of the project to society as a whole. See http://www.measuringworth.com/uscompare/relativevalue.php

[12]Engerman, 'Isis, No. 2, vol. 101, 2010' (as in n. 7), p. 399. The partial results of the project were presented in Raymond Augustine Bauer, Alex Inkeles and Clyde Kluckhohn, *How the Soviet System Works: Cultural, psychological, and social themes*, (Harvard University Press, 1956) Summaries of the interviews are available on line in the page of The Harvard Project on the Soviet Social System: http://hcl.harvard.edu/collections/hpsss/

[13]www.rand.org. Acessed on Dec 27, 2014.

[14]Engerman, 'Isis, No. 2, vol. 101, 2010' (as in n. 7), p. 399

In addition, the totalitarian framework, which already had difficulty to explain how the Soviet Union had won a total war that required an enormous sacrifice from its population based on terror and coercion alone, became even more fragile when, after Stalin's death in 1953, the Soviet Union exhibited itself as a prosperous economic, scientific, and military power under the more loose policies of Khrushchev's Thaw.[15] The difficulties faced by the totalitarian framework, added to the subversive mood set by the protests against the Vietnam war, favored the emergence of alternative interpretations of Soviet history.

2.2 Breaking paradigms: The revisionist challenge

The main challenge to the totalitarian school and works based on its premises came from outsiders, the cohort of social historians self-professed *revisionists* led by Sheila Fitzpatrick, an Australian based in the USA, coming of age in the subversive 1960s. Then, the Vietnam War was making North American intellectuals painfully self-conscious about the imperialist policies of the United States and the role that science was playing in the defense establishment. In that atmosphere of growing discontentment, the differences between the North American "liberal democracy" and Soviet "totalitarian autocracy" seemed no longer clear cut, and sciences (including social and human sciences) surrounded by a veil of secrecy, no longer seemed so democratic.[16]

[15] Khrushchev took a series of actions to signal to the world that Soviet Union was heading on a path different of that settled by Stalin and to amend the damages caused by the impact of his Secret Speech and the subsequent crack down on Hungarian protests. For instance, during the 1957 World Youth Festival more than 30000 foreigners where received in Moscow in a peaceful and open atmosphere. The festival is regarded as a mark of the opening of the Soviet Union to the outside world, a redefinition of relationship between Soviet people and foreigners, and part of the revival of Soviet cultural relations that climaxed with the signing of an agreement on cultural exchange between the United States and the Soviet Union in 1958. See Pia Koivunen, 'The 1957 Moscow Youth Festival: propagating a new peaceful image of the Soviet Union', in: Melanie Ilic, editor, *Soviet State and Society under Nikita Khrushchev*, (London and New York: Routledge, 2009). – chapter 3.

[16] Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945-1975*, (Princeton: Princeton University Press, 2013). As an indication that in the 1960s the two great powers increasingly resembled each other for contemporaries, in the late 1950s and early 1960s there appeared a number of convergence theories, according to which all developing industrial societies, independent of their political composition, are confronted principally with the same social prerequisites and challenges, and would converge in their social, political and economic systems because of the determinant effects of technological development. For a summary of some convergence theories see Jörg Requate, 'Visions of the Future: GDR, CSSR, and the Federal Republic of Germany', in: Heinz-Gerhard Haupt and Jürgen Kocka, editors, *Comparative and Transnational History: Central European Approaches and*

More inclined to methods and themes of social history and the view "from bellow", the revisionists objected the "Cold War bias" of the totalitarian school and criticized their framework for being too focused on the Kremlin and its high politics and for treating the population as a formless mass that could be easily controlled by an all powerful regime, like lab rats on the hands of experimenters. They claimed that on the basis of terror and coercion alone the Soviet Union could not have remained stable for so long, it could not have achieved the impressive industrial growth it did, and it could not have won a war that required an enormous sacrifice from its population.[17]

In works that became classics of the revisionist school, Sheila Fitzpatrick shed light on the impressive social mobility in Soviet Society and the humble origins of the new Soviet elite, arguing that they were loyal supporter of the regime precisely because the regime had created them by a combination of "Cultural Revolution",[18] term of her creation, and Great Purges. Likewise, based on a study of the lowbrow Stalinist literature of the immediate postwar, another historian, Vera Duham concluded that there had been a *big deal* struck by the Stalinist regime and the emergent middle class, by which the regime provided privilege and accommodation of middle class values in return for loyalty and support. Taking on Duham's big deal, Fitzpatrick in later work used it loosely to describe the terms of the relationship between the Russian intelligentsia and the regime in the 1930s:

> If one hypothesizes something like a deal between the Russian intelligentsia and the Stalinist regime in the 1930s, it would involve the intelligentsia's pledge of loyalty and service to the regime in exchange for privilege and social status for themselves and the regime's support for

New Perspectives, (Berghahn Books, 2012). – chapter 8, pp. 181–183.

[17] Fitzpatrick, 'Russian Review, No. 4, vol. 45, 1986' (as in n. 8)

[18] From 1918 the Bolsheviks loosened requirements for university admission, abolished formal titles and diplomas, and created a series of affirmative actions to populate universities with students from formerly unprivileged or discriminated groups – peasants, workers, women, and racial minorities. Fitzpatrick called Cultural Revolution the apex and end of that process, raging approximately from 1928, when young radical students were stimulated to challenge the authority of their professors, until 1934, when the expansion was reined in, titles were restored, and the new system became focused in quality and discipline. See: Idem, *The Cultural Front: Power and Culture in Revolutionary Russia (Studies in Soviet History and Society)*, (Cornell University Press, 1992). An example of the impact of those policies on the lives of Soviet scientists is given in Alexei Kojevnikov, 'A Grande Ciência de Stalin: Tempos e Aventuras de Físicos Soviéticos no Exemplo da Biografia Política de Lev Landau', *Revista Brasileira de História da Ciência*, 4 (2011):1.

> traditional institutions such as the academy of sciences; and an agreement that the two sides would cooperate in disseminating a popularized form of the intelligentsia's culture among the masses.[19]

The revisionists had much in common with the Edinburgh school of sociology of knowledge, with which students of science are more familiar. Two of the basic explanatory principles of the Edinburgh school, namely the emphasis on interest and the symmetry principle, are conspicuous in the revisionist historiography, which explained both the social support, or lack thereof, to Soviet regime in terms of the interest of those groups in society that were identified as its beneficiaries.[20]

Loren Graham, an undisputed pioneer among Western historians of Soviet science, brought a revisionist perspective to history of Soviet science. In his influential book *Science, Philosophy, and Human Behavior in the Soviet Union*, he claimed that the natural sciences played a major role in the ideology of the Russian revolution, unparalleled with other revolutions of modern times; and that Marxist philosophy, in turn, was ubiquitous in Soviet science. "Even good science bears the mark of Marxist philosophy, including 'hard' sciences such as physics", he concludes.[21] In Graham's historical frame of Soviet science there was only one major dark spot - Stalinism. For him, Stalin "converted this interest in philosophy of science into a dogmatic interpretation of natural phenomena that rivaled the scholastic system of the Catholic church in the Middle Ages".[22] In that and later works, Graham tended to view Stalinist period as harmful to the development of Soviet science[23] – this,

[19] Fitzpatrick, *The Cultural Front: Power and Culture in Revolutionary Russia (Studies in Soviet History and Society)* (as in n. 18), p. 9

[20] This is not so surprising, given that they were contemporaries and came of age in a golden age of social sciences. For a brief discussion of the revisionist historiography and their emphasis on interest see Halfin and Hellbeck (as in n. 8). The symmetry principle, for Edinburgh school, implied that the same factors should be used to explain both the success and failures of scientific enterprises. For a discussion of interest and symmetry as guiding principles of the Edinburgh school see Hacking, *The Social Construction of What?* (as in n. 6), p. 90. Another common ground between revisionists and sociologist of science seems to be their debit to Thomas Kuhn's influential *The Structures of the Scientific Revolution* (1962). Fitzpatrick later wrote: "many natural scientists – believing their disciplines to be cumulative and their generalizations to be falsifiable in principle and in practice by new experimental data – have problems with the Kuhn argument, whereas social science and humanities people love it." Fitzpatrick, 'Journal of European studies, No. 1, vol. 37, 2007' (as in n. 72), p. 66

[21] Loren R. Graham, *Science, Philosophy, and Human Behavior in the Soviet Union*, (New York: Columbia University Press, 1987), p. xi

[22] Ibid., p. x

[23] Loren R. Graham, *Science in Russia and the Soviet Union: A Short History*, (Cambridge University Press, 1993)

under the light of the subsequent historical studies, turned out to be a dark spot in Graham's nonetheless enlightening work.

In other remarkable revisionist works on history of Soviet science, one of Graham's pupil Kendall Bailes relativized the negative impact of Stalinism on Soviet science and went further in his acknowledgment of the role of scientists and engineers in shaping Stalinist scientific policies. He stressed that the relationship between scientists and the Soviet government were more complex than the totalitarian model suggests. For Bailes, the prewar Soviet "technostructure" did not simply follow orders issued by the "power structure" but played a large role in shaping the Soviet social and cultural landscape.[24] In his last book, about the Soviet mineralogist and geochemist Vladimir Vernadsky and his scientific school, Bailes revels nuances of the compromise between the old Russian intelligentsia and the Bolsheviks, showing that Vernadsky's scientific stand, his tactful dealing with Soviet officials, and the combination of theoretical and applied research characteristic of his school permitted him to remain an unabashed critic of the official ideology until the end of his life, while promoting his scientific agenda and school under Stalin. Bailes conclusions show that the regime had only limited control over groups of Soviet society, scientists and engineers in this case, and that the regime's policies were liable to be modified in practice through processes of informal social negotiation.[25]

By the late 1980s the revisionists had become the dominant Western school of Soviet history. Studying various social groups and showing how their interest agreed or conflicted with the regime's policies, they painted a richer picture of Soviet society, in comparison with the totalitarian school. However, their historiography had problems of its own. One problem, identified by the revisionists themselves with the evolution of their agenda, was that while bringing in the perspective "from bellow" and new themes as social mobility and social support to the regime, they still framed their questions in "Sovietological terms", in the sense that they maintained the focus on a binary relation between society and the government. Studies of social support or resistance took for granted a strict devision between society and the state, downplaying the role of social groups as builders, by means other than support or resistance, of Soviet society.[26]

[24]Kendall E Bailes, *Technology and Society under Lenin and Stalin: Origins of Soviet technical intelligentsia, 1917–1941*, (Princeton: Princeton University Press, 1978)

[25]Idem, *Science and Russian Culture in an Age of Revolitions: V. I. Vernadsky and his scientific school, 1863–1945*, (Bloomington and Indianapolis: Indiana University Press, 1990), pp. 160–178, chapt. 5.

[26]For a self-critical acknowledgment of the revisionist's inability to abandon the Sovietological

Other two problems of the revisionist historiography surfaced only with the opening on new archives in the 1990s and with the arrival of cultural history to the field Soviet studies. The first is the adoption of a Trotskyist framework to understand the Russian revolution.[27] In the context of the 1960s many leftist intellectuals who sympathized with the Russian revolution gladly embraced the idea of a discontinuity between Lenin and Stalin put forward by Trotsky, and later endorsed by Khrushchev. Following this trend, the revisionists tended to see Stalinism as a return, under conditions of great stress, to nonrevolutionary traditions under a conservative bureaucracy. However, in the 1990s, as newly open archival sources reveled "Lenin's though-mindedness and willingness to shed blood", and his "more neurotic and sensitive personality", on the one side; and Stalin as an "intellectual who continued to read seriously even in power" and "dominated his associates partly simply by intellectual power as well as political skills" on the other, it became increasingly difficult to maintain claims that their rule would be qualitatively different, specially the myth of a Lenin gentler than Stalin.[28] This undermined the Trotskyist framework, and the revisionists were later reproached for adopting a framework to understand the Russian Revolution proposed by Trotsky, "the revolution's greatest loser". [29]

The second problem was that building their historiography to a large extent in opposition to the totalitarian school, which emphasized ideology, the revisionists neglected the role of Marxism-Leninism in shaping Soviet society. This too, under the light of new archival sources made available in the 1990s and methods of cultural history turned out to be a problematic feature of much of the revisionist historiography. Based on a wide variety of sources, the following generation, the post-revisionists, criticized the revisionists for neglecting ideology. They would persuasively show the significance of Bolshevik ideology and how it fashioned life in Soviet Union.[30] It is remarkable, however, that although the younger post-revisionist generation announced itself as critic of the revisionists, the latter wisely accepted most of their substantial criticism and welcomed and promoted the post-revisionist historiography.[31]

questions see:Fitzpatrick, 'Russian Review, No. 4, vol. 45, 1986' (as in n. 8)

[27] A good criticism of revisionist historiography can be found in Halfin and Hellbeck (as in n. 8)

[28] The new portraits of Lenin and Stalin based on archives open in the 1990s are discussed by Fitzpatrick, 'Journal of European studies, No. 1, vol. 37, 2007' (as in n. 72), p. 54.

[29] Kotkin, *Magnetic Mountain: Stalinism as a Civilization* (as in n. 72), p. 6

[30] Halfin and Hellbeck (as in n. 8)

[31] Fitzpatrick, 'Journal of European studies, No. 1, vol. 37, 2007' (as in n. 72)

2.3 Post-Cold War and Post-revisionists Perspectives on Soviet Science

The 1990s was a turning point for the history of Soviet science not only due to the opening of new archives, but for a convergence of many factors that affected simultaneously the disciplines of history of science and Soviet history. In the West, specially in the United States, history of science and Soviet history have been strongly influenced by the changes in the political and cultural climate that followed the end of the Cold War, by the integration into the Western communities of Soviet historians who moved west bringing the perspectives of those who had just lived through a revolution,[32] and last, but not least, by developments in the academic discipline of history as the cultural or linguistic turn of the 1980s, which arrived in those disciplines with a certain delay.[33]

The reaction of historians of Soviet science to the perestroika was two fold. While some followed the mantra of specialists who dealt with Soviet studies, Westerns and Russians alike, that could be summarized as "What went wrong with the Soviet Union" and "How the Soviet System Failed to Work", seeing nothing but bad in the diseased regime;[34] others have followed the dominant trend among historians of using the less polarized political climate to develop less biased historical accounts. The latter group will be our main concern here.

The embrace of the cultural turn in the history of science and history of Soviet Union occurred somewhat simultaneously and led to new insights on the power relations in Soviet society and science. The generation of historians formed in the 1990s, the post-revisionists, incorporated elements from both conflicting schools of totalitarians and revisionists adding elements of cultural history to the mix, specially ideology in form of discourse. Challenging "the paradigms of both the parents and the grand-parents,"[35] they escaped the historiographic dichotomy "from above"/"from below" by framing the interaction between Soviet state and its citizens in terms of

[32] Alexei Kojevnikov, 'A New History of Russian Science', *Science in Context*, 15 (2002):2.

[33] For the new interpretation of Soviet history see Fitzpatrick, 'Journal of European studies, No. 1, vol. 37, 2007' (as in n. 72); Halfin and Hellbeck (as in n. 8).

[34] Fitzpatrick, 'Journal of European studies, No. 1, vol. 37, 2007' (as in n. 72), p. 52. In works in history of Soviet science this tone can be found for example in Gennady Gorelik, 'Lev Landau, Prosocialist Prisoner of the Soviet State', *Physics Today*, 48 (1995):5 ⟨URL: `people.bu.edu/gorelik/Landau{\_}PhysicsToday{\_}1995.htm`⟩; Paul R. Josephson, *Physics and politics in revolutionary Russia*, (University of California Press, 1991).

[35] Fitzpatrick, 'Journal of European studies, No. 1, vol. 37, 2007' (as in n. 72), p. 60. Ideology was already an essential element of the totalitarian framework. However, it was seen and imposed by the state on its population. The post-revisionists, in turn, view ideology as shaped

a constant renegotiation of power. Learning to "speak Bolshevik",[36] or to play the "games of Soviet democracy",[37] Soviet citizens actively participated in the making of Stalinist society. From this perspective, Stalinism was not only a project imposed on Soviet citizens, but also a project of Soviet citizens themselves.

Stephen Kotkin's *Magnetic Mountain: Stalinism as a Civilization* was a landmark work which set the main agenda of the post-revisionist generation. Based on the creation of the city Magnitogorsk, which he saw as "the encapsulation of building socialism", Kotkin argued that far from retreating from the original ideals of the Russian Revolution, as suggested by trotskyist interpretations, in the early 1930s Stalin launched a forward-looking and progressive project of building Socialism that transfixed and inspired Soviet people as well as foreign observers. As ideologically formulated, "Stalin's revolution seemed like the second, and potentially more lasting, dawn of a just, merry, and beautiful Russia, where he who has nothing would become everything."[38] Kotkin thus proposed to "shift the focus from what the Party and its program *prevented* to what it *made possible,* intentionally and unintentionally", "without denying the heavy coercive force of the Communist project".[39]

The more permissive cultural and political climate of the Post-Cold War permitted the appearance of other works with similar approaches to Stalinism and similarly provocative titles, such as Alexei Kojevnikov's *Stalin's Great Science* and Terry Matin's *Affirmative Actions Empire*, that in earlier years would be enough for their authors to be accused of being Stalinists.[40] Those works, and others of the same generation, have reveled a side of Soviet history and some of the legacies of the Soviet Experiment that had been shadowed by the ideological climate of the Cold War. They have argued, for instance, that the USSR set the tone for policies that would shape modern societies as the welfare state, affirmative actions, and Big

by both state and its citizens. This way they combined elements of both frameworks Halfin and Hellbeck (as in n. 8). Seeing the work of entire generations in general trends we risk committing some injustices. I would like to amend at least one by acknowledging that Loren Graham, discussed here as a revisionist, as I said above payed due attention to the role of Marxism-Leninism in Soviet science.

[36] Kotkin, *Magnetic Mountain: Stalinism as a Civilization* (as in n. 72).

[37] Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), pp. 186–216, chap. 9.

[38] Kotkin, *Magnetic Mountain: Stalinism as a Civilization* (as in n. 72), p. 19.

[39] Ibid., p. 22.

[40] I have drawn this conclusion from Fitzpatrick's comment: "Kotkin's subtitle, *Stalinism as a Civilization*, was one that in earlier years no revisionist would have dared to use for fear of being accused (however inaccurately and unfairly) of being a Stalinist." Fitzpatrick, 'Journal of European studies, No. 1, vol. 37, 2007' (as in n. 72), p. 61.

Science.[41]

In the last decades, developing this post-revisionist framework, a younger generation of historians have constructed highly detailed and nuanced pictures of Soviet society under Stalin and have advanced on studies of later periods. Resorting to diaries, oral history interviews, and other printed sources as press articles and memoirs as texts for discourse analysis, they have taken the post-revisionist agenda a step further by including analysis of Soviet subjectivity.[42] The claims of social support to the regime made by the revisionists, which raised much controversy during the Cold War, have been taken to a new level as historians have emphasized the internalization of Soviet values and code of behavior to understand how and why particular groups have become supportive of the regime (*Komsomol* youth, Jews before the WWII,[43] and war veterans, for instance). But in addition to that, they have outlined a gradient of attitudes toward the Bolshevik ideology and policies, including the passive resistance by withdrawing from the Soviet project (as the *stiliagi* counterculture in the 1950s, often compared to hippies) and the appearance of the first dissenters.[44] For instance, studying youth culture of *Stalin's last Generation,* Juliane Fürst makes a comprehensive account of youth participation patterns. She argues that they "ranged from ideological commitment to apolitical apathy, from professional careerism to drifting into alternative spheres", being chiefly defined in

[41] For Big Science see KOJEVNIKOV, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), pp. 23–46, chap. 2. Kojevnikov convincingly argues that the Soviet model of science can be seen as the precursor of what would be later labeled Big Science, and influenced science in countries as France, England, and indirectly, the United States. When British physicists began working on the radar project at the beginning of the WWII they called their weekly meetings the "Sunday Soviet". Idem, 'The Phenomenon of Soviet Science', *Osiris*, 23 (2008) For a discussion of welfare state see KOTKIN, *Magnetic Mountain: Stalinism as a Civilization* (as in n. 72), p. 21. Kotkin argues that under Stalin the state-guaranteed social welfare was developed to a greater extent than had previously been the case anywhere, and that "the Soviet example, as showcased in Magnitogorsk, could be said to have exerted a direct and profound influence on the rest of the world's industrialized countries. In a word, the USSR decisively shaped part of the bedrock of the world in which we live, a bedrock that today is coming apart everywhere." Affirmative actions are discussed in TERRY MARTIN, *The Affirmative Action Empire: Nations and Nationalism in the Soviet Union, 1923-1939*, (Ithaca: Cornell University Press, 2001).

[42] FITZPATRICK, 'Journal of European studies, No. 1, vol. 37, 2007' (as in n. 72)

[43] Yuri Slezkine has challenged the Western assumptions that the history of Jews in Soviet Union is purely a victim story, showing that the upward mobility of Jews after the revolution constituted an important source of support for the regime. However, when antisemitism grew in the late 40s an increasing number of Jews withdrew their support, some of who would later dissent. See: YURI SLEZKINE, *The Jewish Century*, (Princeton University Press, 2006)

[44] For war veterans, stiliagi, and the attitude of other groups of Soviet society to the regime and its policies in late Stalinist Soviet Union see essays in the volumeFÜRST, 'Late Stalinist society: history, policies and people' (as in n. 73).

dialogue with the state and its policies. From hindsight she saw in late Stalinism the seeds of the overturn of the Soviet Union. Up to the end of Stalin's time open dissidence was rare, but disengagement with the official ideology and drifting into alternative, private spheres was enough to destabilize the Soviet system.[45]

The fruitfulness of this perspective in history of Soviet science is evident in Alexei Kojevnikov's *Stalin's Great Science: The Times and Adventures of Soviet Physicists*, an insightful account that presents a series of case studies that scrutinize the lives of some physicists in Stalinist society on social, political, and ideological levels to make sense of what Kojevnikov called the paradox of Soviet science: "The worst decades of Stalin's dictatorial rule were also the period of arguably the greatest progress achieved by science and technology on Russian soil since the time of Peter the Great". Along his narrative Kojevnikov dismantle many stereotypes largely held during the Cold War such as that the mix of science and ideology is always harmful to science, or that science and democracy develops in tandem and one cannot develop without the other.[46] Showing how physicists and other scientists engaged with the official ideology and internalized the mores and rules of Stalinist society Kojevnikov illuminated episodes as the ideological battles of the late 1940s (including the polemic Lysenko affair), and how scientists, specially the president of the Soviet Academy of Sciences Sergei Vavilov, worked to create one of the World's largest scientific empires.

Another example of work developed in close dialogue with post-revisionist historiography that challenges formerly held views on Stalinist science is Slava Gerovich's studies on the Soviet space program. Gerovich argues that the professional culture of space engineers, with its emphasis on stability, control, and authority, epitomized values of the Stalinist society in which those engineers were formed. And, more surprisingly, that "In the folklore of Soviet rocketry... even the fear and oppression of the Stalin's era were often remembered fondly as productive mechanisms for instilling a strong sense of personal responsibility." Thus, for Soviet space engineers, a group that claimed world-wide attention during the Cold War with groundbreaking achievements, far from being harmful, Stalinism fostered Soviet science.

[45] Fürst, *Stalin's Last Generation: Soviet Post-War Youth and the Emergence of Mature Socialism* (as in n. 73), p. 339

[46] Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), pp. xi– xii. This approach is presented also in Idem, 'Revista Brasileira de História da Ciência, No. 1, vol. 4, 2011' (as in n. 18). For a review of *Stalin's Great Science* see Olival Freire Jr., 'RESENHA: Stalin's Great Science: The Times and Adventures of Soviet Physicists (Alexei Kojevnikov)', *Revista Brasileira de História da Ciência*, 3 (2005):2

The works of Alexei Kojevnikov and Slava Gerovich, both Russians based in North American universities, are part of an ongoing agenda which can be seen as the aftermath of the integration of Russian historians of Science in North America, and likewise, points to the integration of the history of Soviet science with the general history of science and technology.[47] For example, Kojevnikov has called for, and developed, studies that compare the history of science in Russia to history of science in other national contexts, specially the United States, and cast light on how information, ideas, and scientific approaches have crossed and influenced scientists on both sides of the Iron Curtain.[48] Konstantin Ivanov, studying the changes in Soviet science after Stalin's death, have claimed attention to the convergent attitude of Soviet and American physicist after the 1950s. While in the Soviet Union "Since the mid–1950s, Soviet scientists embraced the notion of 'fundamental science, or research on fundamental questions with no requirement of direct practical applications...."', in the West

> Since the 1960s, the notion of 'pure science' has been quickly losing popularity in the West, being replaced instead by another concept that viewed science as deeply immersed and responsive to the society's needs and values... Both ideological near-reversals occurred relatively quietly, and both had serious implications for the practice of science. The consequences of those changes for the fate of late Soviet science – and for the fate of Soviet Union herself – still await their historians.[49]

Thus, ideologically, Soviet physicists moved from the Marxist view that science is, or should be, closely connected to the needs of Society toward the view toward the ideology of pure science, while physicists in the West moved on the opposite direction. In practice, as historians of science have convincingly shown, on both sides fundamental and applied science evolved in tandem.[50]

Kojevnikov and Ivanov's suggestions are in resonance with more recent works that advocate for a transnational approach in history of science as a way of articulating

[47] Kojevnikov, 'Science in Context, No. 2, vol. 15, 2002' (as in n. 32).

[48] Idem, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), pp. 276– 300, Chapter 11.

[49] Ivanov (as in n. 41). Chapter 2 discuss the convergence of Soviet and American physics under an atmosphere of increasing militarization in the early Cold War, approaching themes that have been widely discussed in the historiography of science as the militarization, secrecy, and the compartmentalization of knowledge.

[50] Bromberg, 'Isis, No. 2, vol. 97, 2006' (as in n. 40); Forman, 'Into quantum electronics: Maser as 'gadget' of Cold-War America' (as in n. 33); Kojevnikov, 'Osiris, vol. 23, 2008' (as in n. 41).

the results of many local narratives and to think the history of science on a global level.[51] With the renewed interest in debates about science, politics, and ideology that followed the Cold War many studies have scrutinized the development of science and the role of scientists as political players in contexts as diverse as Soviet Union, United States, East and West Germany, Brazil under military rule, and communist China.[52] Those works have forged sophisticated historical narratives that mobilize ideological, political, and cultural factors to account for the historical development of science in specific contexts.[53] They illustrate Olival Freire Jr.'s conclusion that "the job of the historian is therefore to disentangle the role played by each factor in each local and temporal context."[54] As a whole, they reflect the emergence of the cultural and social history of sciences, and after all, the increasingly self-identification of historians of sciences as historians.

2.4 Rewriting history

The evolution of perspectives on Stalinism in the last half century seems to reflect Christopher Hill's assertion that "History has to be rewritten in every generation, because although the past does not change the present does; each generation asks new questions of the past, and finds new areas of sympathy as it re-lives different aspects of the experiences of its predecessors."[55] Perhaps the way we look at history tells us as much about our own times as about the historical moment in question.

The need of rewriting history became clear to me in my own work on history of lasers in the USSR when I first read Loren Graham's last book, *Lonely Ideas*,

[51] See the special issue of the British Journal for History of Science dedicated to historiographic reflections on the approach: Turchetti, Herran and Boudia (as in n. 66).

[52] Collective efforts by historians of science from different countries was a strong evidence of the prominence of the topic. See essays in the special issue *Physicists in the Postwar Political Arena: Comparative Perspectives,* Historical Studies in the Physical and Biological Sciences (HSPS), vol. 30, 1999:1 ; And also chapters in the book Mark Walker, editor, *Science and Ideology: A Comparative History*, (New York: Routledge, 2003).

[53] Examples of works in this line may be found in Olival Freire Jr., *The Quantum Dissidents: Rebuilding the Foundations of Quantum Mechanics (1950-1990)*, (Springer, 2015); Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71). I have also to pay due tribute to a longer historiographic tradition dating back to the 1970s represented by historians such as Paul Forman and John Heilbron. Historians of both old and new generation are well represented in the volume Carson, Kojevnikov and Trischler (as in n. 66).

[54] Olival Freire Jr., 'Quantum dissidents: Research on the foundations of quantum theory circa 1970', *Studies In History and Philosophy of Science Part B: Studies In History and Philosophy of Modern Physics*, 40 dec (2009):4, ISSN 13552198, p. 281

[55] Hill (1984, p. 15)

about invention and innovation in Russia, which contains a chapter on lasers.[56] It's difficult to say anything but prizes about a book that is "based not only on a study of the relevant sources but also on long-term residence in Russia, visits to dozens of Russian universities, research institutes, and industrial establishments, and conversations with thousands of scientists and engineers."[57] The book is indeed masterfully written and reflects Graham's life-long experience and solid knowledge of Soviet sciences in the broad sense of the Russian word *nauki*, which includes all fields of scholarship. However, while acknowledging and respecting the authority and quality of the work, we need not agree with all of its underpinnings. Graham's book, besides being based on his lifelong experience as historian of Soviet science, is also based on a "rich literature on innovation"[58] – I would add neoliberal– which underlies many of his conclusions along the book. For instance, he gives as an evidence that the Soviet laser research lagged behind the statistics that

> By the year 2000, approximately $ 200 billion worth of lasers and laser systems had been sold. Yet the Russian share of the world laser market at this time, thirty-six years after two Russians and an American were awarded the Nobel Prize for the invention of the maser and laser, was merely 1–1.5 percent.[59]

While it may seem all right, given the purpose and intended audience of the book, to use dollar-based revenue to evaluate the success of laser innovation in Russia, it is problematic to use it to access laser innovation in the Soviet Union. First, Soviet laser research and development was based on a network of state run, non-market institutions. The state was at the same time sponsor, producer, and the main consumer of laser-based technology. Using market indicators to access non-market institutions is, at best, misleading. How were the financial transactions accorded between those institutions? Were the price fixed to cover the costs of production or to generate profit? Can we estimate the revenues raised by laser R&D in the Soviet Union without including state investments on scientific and technological institutes and goal-oriented projects for specific applications of laser technology, a significant parcel of which was directed to classified projects? There are no easy answers to these questions. We need a more careful and refined analysis, which

[56] Graham (2013, pp. Chapter 9, Lasers: Genius and Missed Opportunities)
[57] ibid., pp. location 118
[58] ibid., pp. location 97
[59] ibid., pp. location 1653

aims to understand the patterns of innovation in Soviet Union in its own terms to access how successful was laser research and development in the Soviet Union.

Russia's transition into a market economy did not happened overnight. As historians have argued, the collapse of Soviet Union did not end in 1991, but petered out at least until 2000, comprising much more than the emergence of civil society and adoption of neoliberal reforms. For Stephen Kotkin, for example, "What happened in the Soviet Union, and continued in Russia, was the sudden onset, and then inescapable prolongation, of the death agony of an entire world comprising non-market economics and anti-liberal institutions."[60]

Besides, for Graham the impossibility of becoming super rich with inventions, what happen with laser inventors in the United States, was a set back to innovation in the Soviet Union. "Because of its centralized economy the Soviet Union could not develop laser companies in the individualistic, competitive, and, yes chaotic way in which they arouse in the United States." He gives the example of "Valentin Gapontsev, whose story comes strikingly close to a start-up tale."[61] A Soviet physicist specialist on light and lasers, when the Soviet Union collapsed, Gapontsev, illegally, established a private business in the basement of a small laboratory in the Institute of Radio Engineering in Friazino, a state institute near Moscow. Gapontsev began to make business with Italian companies and moved to Italy to open a company of his own. Soon his company was manufacturing high power fiber lasers and amplifiers in Italy and Germany. After his business run into trouble around 2000, Gapontsev decided he "had to move to the US, because a lot of business is based there." By 2006 his company, headquartered in Oxford, Massachusetts, had grown to $143 million.[62]

The story above can be held as a prove that the American Dream, however seldom, does come true. In fact, many inventors, even Soviet ones, may have been motivated by the prospect of fast enrichment[63]. However, that is by no means the only way of fostering innovation, and it is debatable whether it is the most beneficial to society as a whole. A living example that there are effective ways to foster innovation besides profit-driven entrepreneurship is found in the story of the Soviet physicist Zhores Alferov.

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[60] Stephen Kotkin, *Armageddon Averted: The Soviet Collapse, 1970-2000*, (Oxford University Press, 2008), p. 2.
[61] Graham (2013, pp. location 1686)
[62] ibid.
[63] See for example the case of Gordon Gould in Hecht (as in n. 19), pp. Chap. 4

Alferov was awarded the 2000 Nobel prize in physics for the invention of semiconductor heterostructures that permitted the miniaturization of electronics and the creation of the first laser to operate continuously at room temperature, an invention to which we should be thankful whenever we use our compact devices or listen to CD or DVD players.[64] In any account, Alferov's life is plenty of episodes that illustrate how his commitment to communist values drove him toward innovations. For example, in the 1950s, he and other students of the Leningrad Physico-Technical Institute helped to design a power station and worked in several brigades in competition with each other to push the project forward, a strategy typical of the times of building socialism in the 1930s (when whole cities were built from scratch), then it was being employed to build communism. Efforts like that resonated with Alferov's faith in the power of science and technology to transform society. To this day, he is still an active communist and champion of science and technology. For him, communism now means primarily the defense of social welfare, public education and healthcare, and last, but not least, the revival of Russian science and hi-tech industry. Probably, Alferov's story is as rare as Gapontsev's.[65]

Graham's book is a remarkable guide of how to bring Russia closer to an idealized model of American liberal society. A model which, when seen against the background of works of critical thinkers as Noam Chomsky, seems very far and getting is increasingly further from reality. It is unlikely that young historians nowadays, feeling the effects of neoliberalism under their own skin, would take up such agenda. On the contrary, the historiography of Soviet Union in the twenty-first century seems to be going on the opposite direction.[66]

Whereas the totalitarians, writing in a moment of anti-communist hysteria of the 1950s, were excessively concerned with demonstrating the ideological power and ruthlessness of Stalinism, the revisionists, experiencing the power of popular mobilization exhibited in the protest of the 1960s, understood that Soviet people ought to have played a role larger then had been granted to them in totalitarian accounts;

[64] Alexei Kojevnikov, 'A Nobel for communism', *Physics World*, (2011):March

[65] For Alferov's biography see: Paul R. Josephson, *Lenin's Laureate: Zhores Alferov's Life in Communist Science*, (MIT Press, 2011); Zhores I Alferov, *Nauka i Obshchestvo*, (Sankt-Peterburg: Nauka, 2005); idem, *Zhores I. Alferov - Biographical*, 2014 ⟨URL: `http://www.nobelprize.org/nobel{\_}prizes/physics/laureates/2000/alferov-bio.html`⟩. For a description of the enthusiastic building of socialism in the 1930s see Kotkin, *Magnetic Mountain: Stalinism as a Civilization* (as in n. 72). The importance of Alferov's invention and his communist views are discussed by Kojevnikov, 'Physics World, No. March, 2011' (as in n. 64).

[66] Fitzpatrick, venturing to discuss how the Soviet Union may look like in the twenty-first century, guesses that "'what was right with it' may be next on the agenda."Fitzpatrick (2007, p. 64)

the post-revisionists, in turn, writing from a moment of increasing social insecurities, amid talks of crisis of academia due to the unrivaled influence of neoliberalism, have turned their attention to some of the legacy of the Soviet Union that had been taken for granted, such as the welfare state, a "bedrock of the world in which we live that today is coming apart everywhere,"[67] as Stephen Kotkin perceived in 1995.[68]

Perhaps one more lesson may the taken from the evolution of historical works on Soviet Union. Acquiring professional maturity in times of collective awareness about the shortcomings in their own societies, seems to have helped both the revisionists and post-revisionists to assume a less condescending and more perceptive historiographical stances. Being self-critical, it seems, we are more inclined to adopt a more understanding, less judgmental stance in our analysis of other times and societies, and hence be more open to see their positive sides, as well as the negative ones. This, no doubt, has had positive impact on historical studies of Soviet Union, as we can see by the rich, insightful, and interesting accounts of Soviet culture and society produced by both schools so far.

[67] Kotkin, *Magnetic Mountain: Stalinism as a Civilization* (as in n. 72), p. 21

[68] The North American academia is increasingly aware of the negative impact of neoliberal policies in academic intuitions. Willem Halffman and Hans Radder, 'The Academic Manifesto: From an Occupied to a Public University', *Minerva*, (2015). In the last decade there appeared a host of articles and blog posts about the harmful influence of neoliberal policies in North American colleges and universities. For a highly discussed article on this issue see: Deresiewicz (2011). Professors from top US universities such as Columbia have given this kind of advice to students: "I won't talk to students about graduate school anymore... Going to grad school is a suicide mission."

3 Convergence of Cold-War Science: (Co)inventing the Maser in Postwar Soviet Union

In the early 1950s, after years of restrained personal contact with Western scientists, Soviet physicists resumed their participation in international conferences. In 1955 the physicist Alexander Mikhailovich Prokhorov (1916–2002) left for his first conference outside communist borders. It was the meeting of the Faraday Society held in England from April 4 to 6. His presentation, whose title was theretofore unknown to the participants, took the US physicist Charles Hard Townes (1915–2015) aback. He presented the "Theory of the Molecular Generator", a device that had been recently put into operation by Townes in the United States and would became know by the acronym MASER (Microwave Amplification by Stimulated Emission of Radiation). Townes described the episode as a "revealing", "eye-opener" encounter. "After the presentation", he recalled, "I got up and said, Well, that is very interesting, and we have one of these working."[1] Years later, in 1964, the Nobel Prize of physics would be shared by Charles Townes and the Russian Physicists Alexander Prokhorov and his former student Nikolai Gennadievich Basov (1922–2001) as a recognition of their 'fundamental work in the field of quantum electronics, which has led to the construction of oscillators and amplifiers based on the maser-laser principle'.[2]

It came as a surprise to most of the audience that the same device was developed in parallel on both sides of the Iron Curtain in one of the most tense periods of the Cold War, marked by the Korean War. However, as we will defend bellow, that

[1] Townes, *How the Laser Happened* (as in n. 14), pp. 76-78.

[2] The internationalization of Soviet Science, began around 1954, will be discussed in the next chapter. For the changes in Soviet Physics that led to it see Ivanov (as in n. 41), pp. 322-325. "The Nobel Prize in Physics 1964," accessed February 03, 2014, http://www.nobelprize.org/nobel_prizes/physics/laureates/1964/.

episode is symptomatic of a more general trend of convergence between Soviet and American sciences after World War II.

As a device that blurred the borders between applied and basic, military and civilian research, the maser itself was a product of the new orientations of postwar physics in the post-World War II.[3] Its parallel invention in the United States and the Soviet Union suggests that physics in those countries evolved along similar lines in the period. Our main goal in this paper is to reveal that despite political and cultural particularities, in this early period of Cold War, physics in the Soviet Union shared many features of its North American counterpart, and how those shared features can be mobilized to explain the simultaneous and independent invention of the maser in those countries and the convergence of Cold War science.

The Soviet Union and the United States are often portrayed in such dichotomic ways that writing about convergence between them may sound strange for the contemporary reader. However, in the late 1950s and early 1960s the two great powers increasingly resembled each other. This is evident in a number of convergence theories created in that period. The theories are diverse, made by intellectuals on the left and on the right, each giving the primacy to his political view, but the common ground among them is the idea that all developing industrial societies, independent of their political composition, are confronted principally with the same social prerequisites and challenges, and would converge in their social, political and economic systems.[4] A number of factors contributed to this perception. First, the Soviet economy from the end of the World War II through mid-1950s grew at such rates that worried even the CIA.[5] Second, the first broad sociological studies on Soviet society promoted by American military agencies, contrary to expectations, concluded that the Soviet Union was a stable industrial society, in important ways similar to the United States.[6] And third, more to the point of our narrative, science,

[3] Forman, 'Studies in the Physical and Biological Sciences, No. 1, vol. 18, 1987' (as in n. 20).

[4] For a summary of some convergence theories see Requate (as in n. 16), pp. 181-183.

[5] "At a Congressional hearing in 1959, regarding a comparison between the US and Soviet economies, CIA Chief Allen Dulles testified that the growth rate of industrial production in the USSR in the last years had been twice as great as in the USA. If the growth rate of Soviet Industry remained at 8 or 9 percent for the next decade and the US economy continued to grow as it had, the industrial gap between the economies of both countries nearly would be closed."Ibid., p. 182.

[6] In the early 1950s the Air Force gave generous funding for Harvard's Russian Research Center to make a "Working Model of the Soviet Social System" based on interviews with refugees. In classified reports, researchers of the Center concluded that the "Soviet regime had wide if not deep support from its citizens, and was not teetering on the blink of collapse; American forces attacking the Soviet Union, in short, would not be greeted as liberators." quoted in

specially physics, was enjoying unprecedented prestige and government support in both countries due to the alliance between scientists, engineers, and the military on the pursuit of advanced military weapons, such as the atomic bombs, radars, and missile systems. In this regard, the launching of the Sputnik in 1957 was a major boost to the reputation of the Soviet Union abroad.[7]

Grasping the extent of the convergence requires awareness of the distinctive model of science established in the USSR by mid 1930s. Key features of that model were government funding, emphasis on useful research, and a structural organization which privileged research institutes instead of universities as the main sites of scientific research. As adherents of the Enlightenment ideal of using science to perfect society, the Bolsheviks were almost as strongly pro-science as scientists themselves, but they insisted that science had to be connected to the needs of society. Basic, uninterested science was berated as 'bourgeois science'.

Thus, still during the Civil War, the Bolshevik government endorsed most of the essential elements of a reform proposed by scientists of various political orientations that aimed to bridge the gap between science and industry, perceived by some prominent scientists as one the main reasons of Russia's defeat in the First World War. Among the leading proposals, the creation of a network of large state-sponsored scientific institutes directed simultaneously to advanced research and industry was the one adopted by the Bolsheviks as the basis for a revolutionary model of research. Once the revolution was consolidated, the few existing Russian research institutes were scaled up and many others were built throughout the country. In

Engerman, 'Isis, No. 2, vol. 101, 2010' (as in n. 7), p. 399. The first studies of the Smolensk Archive, published from 1958, had similar conclusions. This is the archive of the Communist Party stolen by the German Army from the city of Smolensk and later found by the CIA and transferred to the US. For the impact of these finding on the Western perspectives on Soviet Union see C. P. Silva Neto, *Red Descendants of Apollo: The making of Soviet laser physics.* Ph. D thesis, (Universidade Federal da Bahia/Universidade Estadual de Feira de Santana, 2015), pp. chapter 1.

[7] For the alliance between scientists, engineers and the military in the US see Forman, 'Studies in the Physical and Biological Sciences, No. 1, vol. 18, 1987' (as in n. 20); Forman and Sánchez-Ron (as in n. 31); for the similar alliance in the USSR see Kojevnikov, 'The Making of the Soviet Bomb and the Shaping of Cold War Science' (as in n. 43); idem, 'Osiris, vol. 23, 2008' (as in n. 41). For the Sputnik and the image of the USSR abroad see Idem, 'The Cultural Spaces of the Soviet Cosmos', in: James T. Andrews; Asif A. Siddiqi, editor, *Into the Cosmos: Space Exploration and Soviet Culture*, (Pittsburgh: University of Pittsburgh Press, 2011); Asif A Siddiqi, 'Cosmic Contradictions: Popular Enthusiasm and Secrecy in the Soviet Space Program', in: James T. Andrews and Asif A. Siddiqi, editors, *In Into the Cosmos: Space Exploration and Soviet Culture,*, (Pittsburgh: University of Pittsburgh Press, 2011). – chapter 3.

those institutes, fundamental science and industrial application were pursued by large, multidisciplinary groups of scientists (freed from teaching duties), engineers and technicians, often directed to a particular branch of industry. The perceived success was such that one of the main Soviet ways of developing a particular branch of industry became to create a research institute and a related factory, and the number of such research institutes evolved from dozens to hundreds. Later directed by the Soviet Academy of Science, this impressive network embodied the ideal of planned science, forming the gist of the "Soviet (or socialist) model of research". By mid-1930s the Soviet Union was investing a larger share of its GDP in science than any other country in the World.[8]

That phenomenon attracted attention of European observers. Specially in France and Great Britain, left wings scientists began demanding a similar support for scientific research from their governments. With a Socialist government in France from 1932, and the Popular Front in 1936, influential, left-wing scientists managed to create and establish the Caisse Nationale de la Recherche Scientifique (CNRS) as the leading scientific institution in France. In 1939 the CNRS employed about half of the French academic scientists. In Britain, the Communist John D. Bernal succeeded in promoting some Marxist, Soviet-inspired proposals of scientific reforms when he began serving the British government mobilizing science for the war. The influence of the Soviet model on the British wartime mobilization was such that "When British scientists working on the development of radar technology decided to meet regularly for informal discussions with representatives from different branches of the military, they even called their gathering the 'Sunday Soviet.'"[9] Those examples show that Soviet model of science exerted direct influence on major European scientific powers.[10]

Meanwhile, on the other side of the Atlantic, the circumstances were also favorable to the implementation of Soviet-inspired changes in science. Since the start of the

[8] Alexei Kojevnikov, 'The Great War, the Russian Civil War, and the invention of big science', *Science in Context*, 15 (2002):2; idem, 'Osiris, vol. 23, 2008' (as in n. 41); Loren Graham, 'Big Science in the Last Years of the Soviet Union', *Osiris*, 7 (1992):in Science after '40, edited by Arnold Thackray,.

[9] Kojevnikov, 'Osiris, vol. 23, 2008' (as in n. 41), p. 124.

[10] Since its conception the Soviet experiment was followed by curious eyes of foreign observer and exerted considerable influence abroad, not only in science. Discussing the state-guaranteed social welfare in the Soviet Union, consolidated under Stalin, Stephen Kotkin argues that "the Soviet example... could be said to have exerted a direct and profound influence on the rest of the world's industrialized countries. In a word, the USSR decisively shaped part of the bedrock of the world in which we live". Kotkin, *Magnetic Mountain: Stalinism as a Civilization* (as in n. 72), p. 21.

New Deal in the early 1930s, US citizens were getting used to the presence of their government in several sectors of society. The period between the start of the New Deal and the early 1970s marked the largest expansion of government in the history of the United States. During that period, as many other areas of American society, scientific research relied upon federal support. The watershed in state support for science was the beginning of the World War II, when the antifascist mobilization that forged an unlikely alliance between anticommunist military leaders, such as General Leslie Groves, and left-wing scientists, such as Robert Oppenheimer, permitted the creation of large state-sponsored scientific enterprises as the Manhattan and the Radar projects –the latter building on results of the 'Sunday Soviets' began in Britain.[11]

In the postwar, as an aftermath of the wartime scientific effort, American scientists became used to state-sponsored science and to working in big, multidisciplinary teams of scientists, engineers and technicians on research connected with "societal and market needs".[12] Thus, what used to be regarded as distinguishing features of Soviet science, became characteristic of American science as well. Kojevnikov has drawn attention to the similarities between the phenomenon that happened in the US after the Second War and what happened in the Soviet Union as a result of the First Wold War:

> Elsewhere, most notably in the United States, comparable changes occurred later, typically as a result of another World War, and survived afterward in the form of national laboratories and other institutions, under the unofficial name "big science" (the label "socialist" could not be used during the Cold War for obvious reasons). Despite the difference in names, the two phenomena are homologous and apparently are parts of the same social process. They share most of the essential features, such as gigantomania, state support, the cult of science in society, (con)fusion

[11] Wolfe, *Competing with the Soviets* (as in n. 31); Kojevnikov, 'Osiris, vol. 23, 2008' (as in n. 41). In 1940, to join forces with the United States in the wartime scientific effort, the UK sent a delegation led by Henry Tizard to share top secret information on the British atomic bomb and radar projects that helped to boost the American projects. The Tizard mission brought to the US a cavity magnetron, which advanced by years the US radar effort, and helped to set up a joint US-British radar team to develop microwave radars. See chapter on Militarization of Science in José M. Sánchez-Ron, *El Poder de La Ciencia*, (Barcelona: Crítica, 2007), pp. 707-899.

[12] The literature on the changes in the working style of American physicists enormous and continues to grow. A good illustration of the changes highlighted here is can be found inLenoir (as in n. 37), pp. 239-292. Quotation from p. 257.

> between science and engineering, multidisciplinary research, collective or team work, complex bureaucracy, and militarization.[13]

Physicists, both in the US and the USSR, emerged from the WWII with unprecedented social prestige, translated in symbolic and material terms, thanks to their participation in strategic programs developing radars, rockets, and most important, the atomic bomb. The experiences, skills, prestige, and technology sprung from the physicists's wartime efforts shaped the physics done in the Cold War. Research funding rose to levels unimaginable before the war, physicists acquired celebrity status and came influence political decision making through advisory boards or direct connections to top politicians and officials.[14]

The privileges, however, came with demands and compromises. Physicists were expected to foster research in topics that matched national interests. It is not so surprising, thus, that when the Cold War took more defined shapes in the late 1940s, and American and Soviet governments governments channeled their resources and recruited scientists to strengthen the military capabilities of their states, scientists dutifully put their skills to advance that goal. What was unpredictable was the impact this would have on physics.

Pioneer, and ground-breaking, studies on the impact of the involvement of American physicists in the Cold War military buildup were conducted by Paul Forman in a series of papers on postwar American physics, concerned above all with the war-time radar development and related fields such as microwave spectroscopy and quantum electronics. As will discuss below, "Quantum electronics" was a label created in the late 1950s to unify several efforts of research and development of atomic clocks and masers, electronic devices based on quantum transitions, under a single field. Forman evidenced that American physicists welcomed military funding and support believing that they could use it to advance their own interest, maintaining control of their discipline. Part of the formula to do so involved an strategy of compartmentalization based on the assumption that basic and applied, military-related

[13] Kojevnikov, 'Science in Context, No. 2, vol. 15, 2002' (as in n. 8), p. 270.

[14] In the Soviet Union the social status approached that of *aparatiki*, top party officials, and generals. Likewise, in the US physicist came to be regarded as one of the most prestigious profession, second only to judges of the supreme court. The influence of scientists on decision making in the US through the presidential advisory board is discussed in Norman Kaplan and Don K. Price, 'The Scientific Estate.' *Political Science Quarterly*, 81 jun (1966):2, ISSN 00323195. In the USSR, after the WWII there happened a striking approximation between science and political power. See Alexei Kojevnikov, 'Dialogues about Knowledge and Power in Totalitarian Political Culture', *HSPS*, 30 (1999):1.

science could be kept in different compartments, and therefore physicists could conduct their "basic" or "pure" research, i.e. research driven by their own personal interest in the production of knowledge for its own sake, while doing some military research to satisfy their military patrons.[15]

Ideas, or creativity, however, turned out being recalcitrant towards compartmentalization, and the consequences of that recalcitrance showed up conspicuously in the development of physics in the second half of the 20th century. The most serious of them, highlighted by Forman himself, but scrutinized, debated, and contested by many students of science afterward, was that in the long run physics was reoriented toward fields judged by the military to be strategic, namely, fields in which knowledge produced even in basic researches could lead to new military technologies. Fields such as nuclear and high-energy physics, microwave spectroscopy, and solid state physics thrived in the period. In common they had the close link to high-tech military technology. A second, and closely related, consequence was the tendency to judge the valued of scientific research in terms of its potential to produce new gadgets, a phenomenon which Forman christened as "gadgeteering".[16]

The invention of the maser and its subsequent development into a laser, both gadgets of the Cold War, simultaneously in the United States and the Soviet Union calls up important issues related to the dynamics between science and society in that period. The most prominent issues to be addressed here are militarization and convergence of science, and their consequences to physical research in the early 1950s. As scientists formed in indifferent scientific traditions began to ask the similar questions and aim at similar goals they began to move in the same direction, toward masers and similar devices. They, however, had different perspectives. Thus questioning the convergence of American and Soviet Physicists in the invention of masers we are led to question as well what happens when two different scientific traditions begin to probe the same problems, treading the same path? The history of the maser reveals that although Soviet and American scientists invented the same device, they had different understanding of it, differences marked above all by their original training.

In what follows, in a narrative mostly focused on the invention of the maser in the Soviet Union, we reveal some of the overlapping features in the practice of physics

[15]Forman, 'Studies in the Physical and Biological Sciences, No. 1, vol. 18, 1987' (as in n. 20); idem, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14); idem, '"Swords into ploughshares"' (as in n. 12); idem, 'Into quantum electronics: Maser as 'gadget' of Cold-War America' (as in n. 33).

[16]Idem, 'Into quantum electronics: Maser as 'gadget' of Cold-War America' (as in n. 33).

in the United States and the Soviet Union and how they helped to settle a tendency of convergence between physics in those countries. To do so, we have organized this article in the following way. The next section is about a scientific school established in the USSR before the WWII called the Madelstam's school of oscillations, which was crucial to how the Soviets understood and formulated the theory of the maser. Thanks to its emphasis on applied research, and its mix of physics and engineering, that school thrived in the Soviet Union of the 1930s, and remained one of the leading schools of Soviet Physics thereafter. In the 1960s Alexander Prokhorov and Nikolai Basov became major leaders of that school. Section 2 discusses the militarization of Soviet science in the late 1930s, its impact on the school of oscillations, and its influence on the careers of young graduate students as Prokhorov and Nikolai Basov, who were formed in a period in which Soviet science was militarized to a very high degree. Section 3 addresses how the "slogan to catch up and to surpass", which expressed the strategy of following the American's path to build an atomic bomb, was adopted beyond nuclear physics and influenced Prokhorov and his graduate students to take up research in microwave spectroscopy, a field which flourished as an aftermath of radar research, specially in the US. Section 4, finally arriving at the invention of the maser, is a comparison of the Soviet and American approaches to the maser, discussing how their different backgrounds influenced the way they conceived and perceived the device. To conclude we reflect on what the narrative tells about the questions of militarization and convergence, drawing attention to the similarities and differences on the American and Soviet paths to masers.

3.1 Mandelstam's School of Oscillations

In the summer of 1939, after graduating from Leningrad[17] University, Alexander Prokhorov was invited to apply for graduate school at the Lebedev Institute of Physics, known as FIAN, the Russian acronym for Physical Institute of the Academy of Science.[18] The institute had been founded in 1934 by Sergei Vavilov, an exceptional administrator, who managed to protect the physicists working at FIAN even

[17]Today Saint Petersburg.

[18]Being a research institute without direct ties with university, FIAN recruited its graduate students among leading universities throughout the Soviet Union. One of the main channels of recruitment was through physicists who combined their research at FIAN with teaching positions in universities, but in addition to that, FIAN sent requests to universities asking them to recommend their best graduates to continue their studies at the institute. G. A. Prokhorova, 'Luch Nadezhdy', in: I. A. Shchgerbakov, G. N. Mikhailova and K. A. Prokhorov, editors, *Alexander M. Prokhorov: vospominaniia, stati, interviu, dokumenty*, (Moskva: Fizmatlit,

in the worse period of the Great Purges and raise funds to secure the growth of the institute, transforming FIAN into one of the largest physics institutes of the USSR.

From its conception FIAN was a hotbed for the scientific school of Leonid Isaakovich Mandelstam, a prominent physicist of the Moscow State University who led and ambitious program of development and application of a general approach to physics based on nonlinear oscillations. They aimed to created an "international language" in physics, namely a theoretical framework that could be applied across disciplinary frontiers. Three out of the five initial laboratories that formed FIAN – oscillations, optics, and theoretical physics – were headed by Mandelstam's closest collaborators. His life-long collaborator, Nikolai Papaleksi, headed the laboratory of oscillations, whose name reflects their ambition of studying oscillation as the fundamental phenomenon behind a wide range of physical problems. It was in the laboratory of oscillations that Alexander Prokhorov and Nikolai Basov were trained and developed their initial career. The scientific school institutionalized in that laboratory was crucial to how they understood and formulated the theory of the maser. For that reason, to reveal the specificities of the Soviet approach to masers, we need to discuss the origins and some characteristics of Mandelstam's school.[19]

The origins of the oscillatory approach to physics can be traced back to the work of the German physicist Karl Ferdinand Braun, winner of the Nobel prize of 1909 for his work on wireless telegraphy, with whom Leonid Mandelstam and Nikolai Papaleksi graduated and worked in Strasbourg and Berlin. Under Braun, Mandelstam and Papaleksi worked not only in radiophysics, but in optics as well, and they assimilated his ambition of developing a common oscillatory approach to optics and radiophysics. When they returned to Russia in 1914, Mandelstam and Papaleksi continued to pursue programs began in Strasbourg, maintaining intermittent connection with Ferdinand Braun and other collaborators, but during the Russian Revolution and

2006), p. 29.

A large part of the biographical information on Prokhorov presented in this paper is based on the recollections written by his wife in 1992, but published in 2006. Climério Silva Neto would like to thank Alex Prokhorov, Alexander M. Prokhorov' grandson, for this and other valuable sources.

[19] For the foundation of FIAN see Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), pp. 166-168. Alexander Prokhorov headed the Laboratory of oscillations for almost half century (1954-1998). He later organized a new institute called General Physics Institute (GPI) whose "history [...] is inextricably linked to the laboratory of oscillations of the Lebedev Institute of Physics". The institutional history of GPI portraits Mandelstam, Papaleksi and Mikhail Leontovich as the forerunners in the tradition that led to its creation. http://www.gpi.ru/history.php, accessed in 21 March 2015.

the Civil War their contacts were almost completely interrupted. Although working in relative isolation, Mandelstam and Papaleksi found institutional support to train young cadres and develop old and new research programs, and from then on their route diverged from that followed by Braun and his pupils in Germany.[20]

The turn toward nonlinearity

Perhaps the main bifurcation point between Mandelstam and Papaleksi's research program and those that continued to be developed in Germany by Braun and his pupils was their move toward nonlinearity. Oscillatory phenomena had long been a fundamental component of many physical theories, however, for the sake of simplicity, systems are often idealized to be described by a linear equation. In a simple pendulum, for example, this can be achieved by limiting the amplitude of the oscillation to small angles – a strategy that simplifies the solution, but strongly limits its applicability to real physical phenomena. The development of electronic devices as vacuum tubes oscillators, and the importance they acquired for their applications, shed light on a number of systems that could not be described satisfactorily in terms of a linear equation. Studying radio devices Mandelstam and Papaleksi convinced themselves that the world is nonlinear, and so should be a fundamental physical theory. They began to advocated for a nonlinear theory of oscillations which would offer a sophisticated and rigorous mathematical treatment to phenomena in fields as diverse as optics, mechanics, acoustics, and radiophysics, with close ties with practical applications.[21]

The outstanding mathematical tradition originated in Czarist Russia was instrumental to develop Mandelstam's program. It was based on that tradition that Alexander A. Andronov, one of Mandelstam's first pupils, married to the mathematician Evgeniia Leontovich and closely related to many other mathematicians, gave a remarkable contribution to advance the project of a nonlinear theory of oscillations. Studying the oscillations in an electric circuit fitted with an electric switch,

[20] For Mandelstam and the development of his school see the comprehensive biography Alexander Pechenkin, *Leonid Isaakovich Mandelstam - Research, Teaching, Life*, (Springer, 2014).

[21] Idem, 'The concept of self-oscillations and the rise of synergetics ideas in the theory of nonlinear oscillations', *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 33 jun (2002):2; idem, *Leonid Isaakovich Mandelstam - Research, Teaching, Life* (as in n. 20); Amy Dahan Dalmedico, 'Early Developments of Nonlinear Science in Soviet Russia: The Andronov School at Gorkiy', *Science in Context*, 17 jun (2004):1-2, ISSN 0269–8897.

a problem assigned by Mandelstam, he identified and conceptualized an inherently nonlinear phenomenon that he called self-oscillation.[22]

Nonlinear oscillations in electric circuits had already received a mathematical treatment given by the German Balthasar Van der Pol, however, for lack of precision and the strenuous calculations entailed, his methods failed to attract attention of the scientific community.[23] To develop an alternative approach, Andronov resorted to methods developed by the French Henri Poincaré and the Russian Alexander Lyapunov to solve the three-body problem of celestial mechanics. From Poincaré, Andronov took the geometrical methods of describing the properties of the solution of differential equations. From Lyapunov, he took the methods of studying the stability of motions. The combination of these methods permitted him to put forward a rigorous pictorial description of nonlinear oscillations on the phase space.[24]

Self-oscillations became the core of the theory of oscillations. As conceptualized by Andronov, they are undamped oscillations in real, i.e. non-idealized, systems with resistance or friction that are maintained by an energy source, whose characteristic parameters (like amplitude and frequency) are determined by inner features of the system and are independent of the initial conditions (phase). An energy source is necessary to compensate losses, but it does not need to be periodical, and usually it is not. They can be better understood in contrast with the more familiar forced oscillations. While forced oscillations are maintained by an external periodic source and depend on the initial condition, self-oscillations are maintained by a non-periodic source and do not dependent on the initial conditions.[25] As Dalmedico observed, Andronov "immediately attributed to self-oscillations an universal character, seeing them at work in radiophysics, acoustics, mechanics, chemistry (periodic reactions), and biology. Self-oscillations provided the basis for the paradigm of nonlinear physics that Mandelstam had called for".[26]

Radiophysics, applications and transdisciplinarity

At the center of the activities of Mandelstam's school, as we can seen by the discussion above, was radiophysics, a discipline which was born applied. Shortly after

[22] Dalmedico (as in n. 21). For consolidation of a mathematical tradition in Czarist Russia see Alexander Vucinich, 'Mathematics in Russian Culture', *Journal of the History of Ideas*, 21 (1960):2.
[23] A. M. Prokhorov, 'Sovetskaia Radiofizika', *Nauka i Zhizn*, 11 (1947).
[24] Pechenkin, 'Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics, No. 2, vol. 33, 2002' (as in n. 21), pp. 137-140.
[25] A. A. Andronov and S. E. Khaikin, *Teoriia Kolebanii*, (Moskva, 1937).
[26] Dalmedico (as in n. 21), p. 237.

Heinrich Hertz's experiments of generation and detection of electromagnetic waves in late 19th century Germany, there was no shortage of inventors dedicated to experiments in wireless telegraphy, the prehistory of radio. Some of them were acknowledged scientists as Karl F. Braun,[27] but many were like the Italian Guglielmo Marconi, "a self-taught man [who] had not the theoretical basis and the mental habit of the researcher... and went on his way with a pure pioneer spirit of adventure." [28] Practically-minded inventors as Marconi promoted the development of the technology of generation and detection of electromagnetic waves in the last decades of the 19th and beginning of the 20th century, accumulating a wealth of practical, experimental knowledge on production, transmission and reception of radio waves. However, it was for the work of practically oriented physicists such as Karl Ferdinand Braun, who became director of the Physical Institute of the University of Strasbourg in 1895, that wireless telegraphy received a scholarly formulation and entered academic institutions as radiophysics, a sub-discipline of physics independent of, but closely related to, electromagnetism and optics. As students of Braun, Mandelstam and Papaleksi pioneered radiophysics in Russia, where the engineer Alexander Popov had given his own remarkable contributions to develop radio technology, and where radiophysics found highly fertile grounds.

By the end of the 1930s the scientific results obtained by the school of oscillations were already presented in textbooks. The first textbook was published in 1937, written by Andronov in collaboration with Semion E. Khaikin and Alexander A. Vitt, two other pupils of Mandelstam based at Moscow State University. Another, *Oscillations and Waves*, was published in 1950 by Gabriel S. Gorelik, one of Andronov's collaborators in Gorky. The textbooks may be taken as windows to understand what has been called by historians the paradigm of self-oscillations and how the concept was employed to solve real problems. According to Gorelik, "as self-oscillating systems we understand any real device that is source of undamped oscillations. The word 'real' means that it excludes ideal (and with them the trivial) cases, when the system does not have friction or resistance".[29] Among the paradigmatic examples in the early textbooks are musical instruments, pendulum clock, and vacuum tube generators. The first question that arises from the study those

[27]For the long list of men who proposed wireless telegraphic systems see Wikipedia's article: http://en.wikipedia.org/wiki/Invention_of_radio.

[28]Bertolotti (as in n. 61), p. 125.

[29]G. S. Gorelik, *Kolebaniia i Volny - vvedenie v akustiku, radiofiziki i optiku*, 1st edition. (Moskva: Gosudarstvennoe Izdatelstvo Tekhniko-teoreticheskoi Literatury, 1950), p. 106.

systems is how unilateral, constant blowing, lowering of weight, or constant tension from a power supply can maintain oscillations in air, watches, or oscillatory circuits, respectively. The second question is what factors determine the form and period of the oscillations; in particular, in which cases sinusoidal oscillations occur and, when occurring, on which depends their amplitude.[30]

That textbook shows that by 1950 not only a theory of nonlinear oscillations, but a world-view, which can be loosely called paradigm or disciplinary matrix, centered on the study of oscillatory phenomena in various fields,[31] with special emphasis on radiophysics, had been institutionalized and embedded in the training of physicists. Some features of that paradigm are clearly illustrated in Gorelik's postwar textbook, which attempts to see from a single viewpoint, at the level of a general undergraduate physics course, oscillatory and wave phenomena found in mechanics, acoustics, optics, and electromagnetism. According to the author, the fruitfulness of that approach to vibrational and wave phenomena had long become clear to physicists and engineers. Most of the book, however, was dedicated to radiophysics. For Gorelik this was justified not only for technical importance of radiophysics, but also for the application of radiophysics methods in the most varied fields of scientific research – from nuclear physics to astronomy, from biology to geophysics. Accordingly, the book presents the students with example of application in a wide range of disciplines.[32]

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The research program in which Alexander Prokhorov was involved when he enrolled for graduate school at FIAN in 1939 illustrates the kind of work in radiophysics developed by Mandelstam's school in the USSR. It was a broad study on propagation of radio waves on the surface of the earth, the last large program Mandelstam took part in his life.

[30] Andronov and Khaikin (as in n. 25), pp. 193-194; Gorelik (as in n. 29). A condensed English translation of Andronov's textbook was published as part of the work on the project on nonlinear differential equations under contract with the Office of Naval Research. A. A. Andronov and C. E. Chaikin, *Theory of Oscillations*, (Princeton University press, 1949).

[31] Physicists who identified themselves with this school went as far as to consider applying the concept of self-oscillation to describe social phenomena. The historian Alexander Pechenkin argues that the self-oscillations became an ideology adopted by many scientists in the Soviet Union. For a discussion of self-oscillations as a paradigm and ideology see Pechenkin, 'Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics, No. 2, vol. 33, 2002' (as in n. 21).

[32] Gorelik (as in n. 29).

The evolution of radio technology and the increasing distances of radio transmissions revealed that radio waves could contour the surface of the earth, but the theoretical explanation for this phenomenon remained for years a challenging problem for physicists. Even in the most simple case, considering flat the frontier between air and earth and that both of those media are homogeneous, the problem was still very complex due to the proximity of the radio-wave source (antenna) to the intersection between the media.[33] The first steps toward the solution of that problem were given by Jonathan Zenneck, like Mandelstam a student of Karl F. Braun, and Arnold Sommerfeld in Germany. Zenneck calculated plane and cylindrical wave solutions of Maxwell's equations, now know as Zenneck waves, in the presence of a planar boundary separating free space from a half space with finite conductivity.[34] The problem with this model is that those waves could be produced only by infinite, therefore inexistent, sources. Arnold Sommerfeld extended Zenneck's analysis to the case of a punctual source, a Hertz dipole, over a conducting spherical surface. His main motivation was the analysis of the propagation of radio waves around the earth.[35]

Mandelstam first got involved with that problem in 1916 when he extended Sommerfeld's analysis to optics. While in optics the distances between the light source and boundary surface are usually far greater than the radiation wavelength, in radiophysics the distances between wave source and the boundary are often smaller than the radiation wavelength, which may exceed 1000m. Thinking in analogy with radiophysics, Mandelstam considered the phenomenon of refraction of light when the light source is very close to the boundary, concluding that in this case new optical phenomena would rise. He showed that in the situation where the source of light is located nearby the interface, the usual laws of refraction are violated and should be replaced by the other laws.[36]

Later, however, when the problem of propagation of radio waves on the surface of the Earth had acquired new significance due to the development of radio communication and radiolocation, Mandelstam and Papaleksi followed the opposite way.

[33] Prokhorov, 'Nauka i Zhizn, vol. 11, 1947' (as in n. 23).

[34] Jonathan Zenneck, 'Über die Fortpflanzung ebener elektromagnetischer Wellen längs einer ebenen Leiterfläche und ihre Beziehung zur drahtlosen Telegraphie', *Annalen der Physik*, 23 (1907).

[35] Arnold Sommerfeld, 'Uber die Ausbreitung der Wellen in der drahtlosen Telegraphie', *Annalen der Physik*, 28 (1909). For a literature review on surface waves see David Reiss, *The Advance of Physics: Special Report No. 1. Electromagnetic Surface Waves.* 1996 ⟨URL: web.mit.edu/redingtn/www/netadv/zenneck.html.⟩.

[36] Pechenkin, *Leonid Isaakovich Mandelstam - Research, Teaching, Life* (as in n. 20), p. 70.

They used interferometry, a well-know technique in optics, in radiophysics to devise a method to measure distances and the velocity of propagation of radio waves in different conditions with high accuracy. Experiments employing those methods helped to test and improve the theory of propagation of radio waves being developed in concerted effort by Soviet physicist such as Boris Vvedensky, Vladimir Fock and Mikhail Leontovich, the latter Mandelstam's student.[37]

As a first year student of FIAN in 1940 Prokhorov was given the task of operating a range-finder (dalnomer), devised based on the interferometry methods proposed by Mandelstam and Papaleksi, in experiments conducted by another student of Mandelstam, Vladimir Migulin, to study the ionosphere and its influence on the propagation of radio waves. Prokhorov did not play his part in the experiments for he was mobilized for the war the following year, but Migulin and his collaborators obtained important original results in field experiments conducted during the summer of 1941 in a village on the outskirts of Moscow.[38]

The radio-interferometry methods devised by Mandelstam and Papaleksi also found useful applications in geodesy, cartography, radio navigation and so on. Prokhorov, writing in 1947, exalted the practical applications of apparatuses based on radio-interferometry methods, and in accordance with the nationalist mood of the postwar, considered "worth to mention that abroad radio navigation with help of radio wave interference began to develop during the Second World War, while in our country it was practically applied long before the war".[39]

For our narrative the importance of those methods, and their subsequent translation into gadgets, is that they illustrate a style of research in which cross-field borrowing and analogies, and concerns with translation of knowledge into gadgets were central even before the Wold War II. Whereas in the United States a similar style was incorporated by physicists during their wartime work, in the Soviet Union when the war began they were already assimilated in the practice and training of physicists. The close ties between theory and devices, transdisciplinarity, and the application of radiophysics methods characteristic of the school of oscillations, settled a solid basis which permitted that school to develop successfully in many of

[37] A technical summary of those works in presented in James R. Wait, *Radio Wave Propagation in an Inhomogeneous Atmosphere*, (Washington, D.C.: U. S. National Bureau of Standards, 1959).
[38] Prokhorova (as in n. 18), pp. 29-30.
[39] Prokhorov, 'Nauka i Zhizn, vol. 11, 1947' (as in n. 23), p. 31.

fields of physical research that flourished after the World War II as the aftermath of the wartime radar development.

In a thorough survey of research fields that after the war were benefited by applications stemmed from the wartime radar work, Paul Forman argues that one of the most important effects of the radar development in the US was the creation a group of physicists with good engineering skills that were applied to create and develop new fields of physics. He classified the “applications” of radar to physical research in two sorts. First, those in which radar hardware, techniques of radar, or developed to radar, were directly employed to yield new knowledge. And second, those in which the experiences, or familiarities, “that physicists acquired through their involvement with radar research, engineering, and operations during the war resulted in new ways of thinking about nature of scientific investigation”. Among these new ways of thinking about the nature of scientific investigations Forman lists the approximation between theoreticians and experimentalists that resulted in a “shift from theories of general, abstract, perfect objects to theories of particular, real, imperfect materials”,[40] and the support for “programs of knowledge production which were less discipline delimited”[41] As we can conclude from the discussion presented above, those changes brought American physics closer to its Soviet counterpart. To make a more specific case, they made the working style of American physicists involved in radar research and development similar to that cultivated in Mandelstam’s school.

Scientific schools in Soviet Physics

Mandelstam’s school quickly grew in size and importance. By 1936 it had consolidated itself in a network of six “strongly connected” research institutions in Moscow, Leningrad, and Gorky, which jointly conducted research on nonlinear oscillation as an “unified scientific policy”. The institutions involved were FIAN and the Moscow State University, in Moscow; the Leningrad Electro-Physical Institute (LEFI), the Industrial Institute and the Central Radio Laboratory, in Leningrad; and the Gorky State University, in Gorky.[42] Those institutions were bound together by the fact that some of their leading researchers were previous students and close collaborators of the Leonid Mandelstam or Nikolai Papaleksi. Mandelstam and Papaleksi were

[40] Forman, ‘"Swords into ploughshares"’ (as in n. 12), p. 411.
[41] Ibid., p. 415.
[42] Mandelstam, 1936, quoted in Pechenkin, *Leonid Isaakovich Mandelstam - Research, Teaching, Life* (as in n. 20), pp. 147-149.

central figures coordinating the efforts of all those institutions, but their agency was not limited to the conceptual formulation and discussion of scientific problems. As Pechenkin highlighted, Mandelstam "was anxious for practical applications of his student's results" and "took care of the social status of their research".[43] Here we can discriminate two features of Mandelstam's school that permitted it to prosper in the Soviet Union.

First, it was closely connected with practical and industrial applications and as such it matched well, and was favored by, the official vision of science promoted by the Bolsheviks. As adherents of the Enlightenment ideal of using science to perfect society, from the beginning of their rule, the Bolsheviks aligned with scientists, of all political orientations, who were willing to use their skills and knowledge to foster the industrialization of Russia. This was the core of the early compromise between the Bolsheviks and "bourgeois" scientists that permitted the rise of the leading schools of Soviet science.[44]

Second, the hierarchical structure of the school, with physicists from several institutions orbiting around the central authority of Leonid Mandelstam, favored the development of "an unified scientific policy", as defined by Mandelstam himself, or a method of "collective work", as defined later by Sergei Vavilov. This hierarchical structure proved to be convenient to the pursuit of goal-oriented R&D programs as those related to the industrialization drive, as well as military programs that were set in motion in the second half of the 1930s. In his celebratory speech on the 30th anniversary of the Revolution Vavilov boasted this as an innovation of Socialist science:

> A new method, applied more and more frequently, was that of collective work, in which the solution of a problem would be undertaken, not by a one individual, but by a group of scientists, usually headed by a prominent specialist in the field. This method of work made it possible to undertake intricate and laborious research which had formerly seemed impossible.[45]

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[43] Pechenkin, *Leonid Isaakovich Mandelstam - Research, Teaching, Life* (as in n. 20), p. 149.

[44] Other examples are found in the schools of Abram Joffe, Josephson, *Physics and politics in revolutionary Russia* (as in n. 34); and of the geochemist Vladimir Vernadsky, Bailes, *Science and Russian Culture in an Age of Revolitions: V. I. Vernadsky and his scientific school, 1863–1945* (as in n. 25).

[45] S. I. Vavilov, *Soviet Science: Thirty Years*, (Moscow: Foreign Language Publishing House, 1948) ⟨URL: `https://www.marxists.org/archive/vavilov/1948/30-years/x01.htm`⟩.

Perhaps part of the formula of Vavilov's successful administration of FIAN involved the convenient choice of the scientific approach the institute would house and promote. Approaches as that of Mandelstam school, which dealt with fundamental, or basic, questions while concerned with the application of knowledge, resonated simultaneously with the values of their scientific tradition and the Bolshevik ideal of science.

In his biography of Vladimir Vernadsky, historian Kendal Bailes has described a similar combination of emphasis on applied research and clustering around a powerful figure who guided the group both through conceptual and social problems. Bailes argued that Vernadsky's scientific stand, his tactful dealing with Soviet officials, and the combination of theoretical and applied research characteristic of his school permitted it to prosper even during the most difficult period of Stalin's rule, even though Vernadsky remained an unabashed critic of some of the official policies.[46]

In the postwar the clustering of the academic community into "scientific schools", which typically did not mix, became a common social phenomenon of Soviet science. That structure came to be perceived by Soviet scientists as a natural and necessary feature of science itself, beneficial for the very progress of knowledge.[47]

In addition, this centralization of several scientific institutes around a chief leader permitted in practice to get closer to the Soviet ideal of planned science coordinating the effort of several institutions to solve challenging scientific problems. This can be exemplified by the research on propagation of radio waves discussed above as well as by the program that contained Prokhorov's postwar work, to be discussed in the next section. That feature, which we may call scientific centralism, was instrumental for the mobilization of Soviet science that preceded the World War II.

3.2 Militarization and Secrecy in Soviet Science

As the war drew closer, and the Red Army summoned Soviet citizens, Alexander Prokhorov volunteered to military service. In 1941 Lieutenant Prokhorov interrupted his doctoral research to serve in the infantry division in which he performed reconnoitering activities. His final training before being sent to the front ended in

[46] Bailes, *Technology and Society under Lenin and Stalin: Origins of Soviet technical intelligentsia, 1917–1941* (as in n. 24), pp. 160-178.

[47] Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), pp. 264-265.

Kazan, a city 800km east of Moscow, on the Volga river, to where many plants and industries were moved in the first years of the war, which became the center of the Soviet military industry, producing tanks, airplanes, and other advanced military weapons. The Lebedev Institute of Physics, as many other institutes of the Academy of Sciences, had been moved to Kazan. During his short stay in the city, Prokhorov visited the installations of FIAN to meet some of his close friends that had continued working at the institute. His visit was in vain. His friends were all doing war work in installations on the outskirts of the city. For the duration of the war most the usual scientific activity of FIAN stagnated. Those who could, employed their expertise in military projects of immediate applications; those who were still not mature enough, as Prokhorov and other graduate students, volunteered to the battle field. Such was the impact of World War II on the Soviet Union that one could hardly continue his usual scientific research, unless it could yield an tangible, immediate contribution to the war effort.[48]

Massive militarization and mobilization of Soviet science was obvious by 1935, when major academic institutions acquired classified laboratories and projects with corresponding secrecy and security procedures. The prospect of the looming war and threat from Nazi Germany in the mid 1930s diverted ever more thoughts and activities in the Soviet Union towards military preparations.[49] Similarly to what would happen in the United States during and after the war, Soviet military opted for contracting for research in industrial laboratories and academic institutions rather than seeking to do it all by themselves. Several civil institutions were involved in the early military projects, with requests for specific devices or line of research coming from military institutions, as the Ministry of Defense and the Navy, from industrial organizations, or from completely obscure contractors identified only by a postbox number.[50]

By 1945 Soviet science was already mobilized for military related work to the highest degree, but the war emergency imposed rather strict parameters on its work. For the duration of the war, Soviet science had to focus primarily on weapons

[48] Galina Prokhorova, Prokhorov's wife, described richly the atmosphere of the war, how they were involved in it, and how her husband, on the week of their wedding, left Moscow for his training in Kazan and later was sent to the front. PROKHOROVA (as in n. 18), pp. 31-36.

[49] KOJEVNIKOV, 'The Making of the Soviet Bomb and the Shaping of Cold War Science' (as in n. 43).

[50] The choice of American military of resorting to industrial and academic institutions to develop military technology is discussed in FORMAN, 'Studies in the Physical and Biological Sciences, No. 1, vol. 18, 1987' (as in n. 20). For how civilian institutions as Gorky State University received military contracts see DALMEDICO (as in n. 21).

and projects that were immediately useful and crucial for the ongoing battles, such as improving the mainstream technology of tanks, artillery, automatic guns and aircraft. Fancier forward-looking proposals of radically new weapons of the type of atomic bomb and long-range missiles could receive only secondary attention, if any at all. Pursuing expensive and long term goals, which held large but uncertain promises for the future, in the midst of an all-consuming battle for immediate survival was seen as a distraction and a waste of critically short resources. Only after the war's end would such strategic projects rise in priority, with a major restructuring of resources into their favor.[51]

Engaging on the scientific front

Prokhorov's prewar work on propagation of radio waves through the ionosphere, discussed above, as well as his postwar theoretical work on nonlinear oscillations began in 1944, were connected to one of the major tasks of the war effort – developing radars and radio technologies.

The study on propagation of radio waves on the surface of the earth coordinated by Leonid Mandelstam and Nikolai Papaleksi had been instrumental to argue for the feasibility of using radio waves for aircraft detection and location, and for the advantages of radiolocation in comparison with acoustic and infrared-radiation systems. The Academy of Sciences got involved in radar development in 1933, when the Air Defense Command consulted with its president Alexander Karpinskii and other academicians on the possibility of using radio methods for air defense, as alternative to acoustic and infrared systems that were then being developed and presenting serious limitations. They called a conference with leading physicists of the country, including Nikolai Papaleksi and Sergei Vavilov, which concluded that radiolocation was feasible, but due to the novelty and technical challenges involved in developing the necessary technology of ultra-short waves (decimeter and centimeter waves) it would be recommend to continue advancing acoustic and infrared technologies as well.[52]

At the beginning of 1934 the radio range-finder developed by Mandelstam and Papaleksi was used by engineers of the Central Radio Laboratory to investigate the reflection of electromagnetic waves from the surface of aircrafts and to demonstrate

[51] Kojevnikov, 'The Making of the Soviet Bomb and the Shaping of Cold War Science' (as in n. 43), p. 135.

[52] John Erickson, 'Radio-location and the air defense problem: The design and development of Soviet radar, 1934-40', *Science Studies*, 2 (1972), pp. 247-250.

to military officials the feasibility of radiolocation. Radio technology thereof proceed as the main solution for the air-defense problem and important progress was made in the technology of generation and detection of ultra-short waves specially at the Ukrainian and Leningrad Physical-technical Institutes, but in 1940 the whole radiolocation effort was called into question due to attacks from acoustic specialists in an attempt to denigrate radio methods and promote acoustic methods. In the summer of that year a meeting was called to discuss whether the military investment in the "radio-technical" program was justified. Among the specialists summoned, Papaleksi, then head of FIAN's laboratory of oscillations, intervened to point out that the claim made by acoustics experts that "radio-detection obviously had no future was 'scientifically groundless'"; it was acoustics, rather than radio-detection, which had little or no future. Nikolai N. Andreev, the head of FIAN's acoustics laboratory, sided with Papaleksi. After those "authoritative interventions" the meeting was closed down and its proceedings ended without any particular resolution.[53] In spite of controversies, by 1940 a poorly-coordinated radar program had yielded some important results and prototypes of radars were tested by the Red Army as early as 1939. A well-coordinated and well-funded radar program, however, was set in motion only in 1943, after radars acquired status of "state significance".[54]

When Prokhorov resumed his scientific work at FIAN in 1944, the radar-related work was in a second stage. Then radiolocation had moved up in the scale of priorities. The main focus of the laboratory of oscillations was on generation, modulation, and application of short radio waves.[55] This appears in the prospective plan for theory of oscillations and radiophysics written by Sergei Vavilov, director of FIAN and president of the Academy of Sciences of the USSR, in august 1944:

> In the field of oscillations the central problems are questions regarding oscillations in systems with "large" nonlinearity and "deep modulation" of parameters, as well as oscillations in systems with many degrees of freedom... Closely connected with those problems are extremely relevant present questions on generation, modulation, and application of super-high-frequency oscillations, which acquired extremely important meaning not only to radio-communication strictly speaking, but also to

[53] Erickson (as in n. 52), pp. 259-260.
[54] Ibid., p. 262.
[55] S. I. Vavilov, *Perspektivnyi plan rabot po teorii kolebanii i radiofizike. ARAN, 532-1-90, p. 10*, 1944, p. 10.

> a series of other applications of radio wave (radio navigation, radiolocation, teleautomation and etc.) Therefore, it is extremely relevant the creation of a theory of super-high-frequency oscillations, connected with the development of new nonlinear methods, and the execution of wide experimental research in that field.[56]

In accordance with those guidelines, Prokhorov's first Russian dissertation was a theoretical work on stabilization of frequency of lamp generators, with which he obtained his degree of candidate of sciences (kandidat nauk, roughly the equivalent of a PhD) in 1946.[57]

We cannot reduce, however, the scope of applicability of that research to radars and other military devices. Much before radio technology began to be applied for aircraft detection and location, even before Europe began mobilizing for the War, the Soviet radio industry was highly regarded and accordingly funded. In a country as large as the USSR radio was a powerful tool of communication that could deliver the messages of the Party to the most remote corners of the country. In the words of Sergei Vavilov: "Radio was very timely development for the socialist, Soviet land. It has become a powerful means of information and propaganda, a means of uniting the people in labor, struggle, and festivity".[58]

From celestial mechanics to frequency stabilization

With the country's economy burdened by the ongoing war, his work on stabilization of frequency had to follow a theoretical, therefore less costly, approach, Prokhorov recalled during an interview.[59] However, within the framework of the theory of nonlinear oscillations this did not mean that it would be less grounded on demands of the on-going war. As discussed above, the theory of oscillations had been conceived to deal with real devices and practical problems. And the work developed by Prokhorov under the guidance of Sergei M. Rytov, a former student of Mandelstam, is a good illustration of that. As Rytov would later recall, "The appeal of the work

[56] Vavilov, 'Perspektivnyi plan rabot po teorii kolebanii i radiofizike. ARAN, 532-1-90, p. 10' (as in n. 55), p. 10. There is no direct mention to war in the plan, but the repetition of the adverb "extremely" (chrezvychaino) express a sense of urgency. And what, in 1944, could be more "extremely relevant" than the problems directly connected with the demands of the on going war?

[57] *Stenograma zasedaniia Uchenogo soveta ot 10 Ianvaria 1946. ARAN, 532-1-122. pp. 1-10*, 1946.

[58] Vavilov, 'Soviet Science: Thirty Years' (as in n. 45).

[59] A. M. Prokhorov, *Interview with Dr. A. M. Prokhorov by A. Guenther, on 14 September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.*.

on stabilization of frequency was not occasional, but dictated by the 'social needs' of that time: radiolocation, radio-communication, television, all demanded generators with more and more stable frequencies".[60]

The work on stabilization of frequency also illustrates how members of Mandelstam's school working in different institutions cooperated to advance large scientific programs. In the same year that Prokhorov resumed his academic work, Alexander Andronov had started a well-attended seminar in Moscow in which he presented the latest developments in the theory of nonlinear oscillations at work in priority areas as control engineering, crucial to improve missiles and aircraft technology, and frequency stability.[61] Perhaps influenced by that seminar, Sergei Rytov set about applying the theory of nonlinear oscillations to stabilization of frequency. He developed further Andronov's method of small parameters, a perturbation method, to make it suitable to study the frequency stability.

As a follow-up, under Rytov's guidance, Prokhorov and another graduate student Mark E. Zhabotinskii employed the method of small parameters to study the stability of a lamp generator with quartz stabilizer[62]. Besides showing how the method had to be used to convey the essential peculiarities of stabilizers, they specified the very meaning of the concept of stabilization in the theory of nonlinear oscillations and discussed the behavior of basic stabilizing circuits: pulling circuit and circuit grid with quartz.[63] That theoretical treatment predicted original phenomena that was later verified experimentally as, for example, the existence of islands of stability amid mismatching intervals in some specific conditions. Overall, the work yielded results relevant for the making of stabilized generators that were presented in a simplified way in a paper addressed to engineers with an intuitive picture of the stabilization process and a summary with calculated formulas for stabilized frequencies that could be used to produce more stable generators.[64]

Not by chance, for those works Rytov, Prokhorov and Zhabotinskii were awarded the Prize Leonid I. Mandelstam of best work in radiophysics for the year of 1947.[65]

[60] Rytov, as quoted in Prokhorova (as in n. 18), pp. 48-49.
[61] Dalmedico (as in n. 21), pp. 242-244.
[62] 'Stenograma zasedaniia Uchenogo soveta ot 10 Ianvaria 1946. ARAN, 532-1-122. pp. 1-10' (as in n. 57).
[63] S. M. Rytov, A. M. Prokhorov and M. E. Zhabotinskii, 'K teorii stabilizatsii chastoty: 1', *JETF* 15 (1945):613; idem, 'K teorii stabilizatsii chastoty: 2', *JETF* 15 (1945):557.
[64] M. E. Zhabotinskii, 'O teorii stabilizatsii chastoty', *Radiotekhnika*, 1 (1946):19.
[65] S. M. Rytov, A. M. Prokhorov and M. E. Zhabotinskii, *O stabilizatsii chastoty lampovykh generatorov (avtoreferat rabot, udostoennykh premii AN SSSR im. L. I. Mandelstama za luchshuiu rabotu v oblasti radio za 1947)*, 1947.

Their program exhibits a research pattern that was common and desirable according to the disciplinary mores of Mandelstam's school and of Soviet science in general. It followed from the development of sophisticated mathematical methods, to its application to solve a practical, relevant problem, and to the translation of the results into engineering language or, alternatively, into gadgets. The same pattern can be seen in the research program led by Mandelstam himself discussed in the previous section and other programs developed by Mandelstam, Andronov, their collaborators and disciples discussed by Pechenkin and Dalmedico.[66] It shows a tendency to define and value scientific projects in terms of the special gadgets they produce rather than knowledge per se, also apparent in Rytov's statement that appeal of their work was "dictated by the 'social needs' of that time"[67] as well as in Vavilov's prospective plan, both cited above. It is arguably the same tendency found in postwar American physics, which Paul Forman has labeled "gadgeteering".[68]

The quest for new microwave sources

In 1948, Prokhorov began an experimental work on synchrotron radiation that would be presented in 1951 to obtain the degree of Doctor of Sciences (doktor nauk), the highest degree a Soviet scientist could achieve, being comparable to the German Habilitation. Pursuing a degree of Doctor of Sciences, the scientist must conduct independent research, hence without a supervisor, and may be supervising PhD students. It was at this point that Prokhorov began advising Nikolai Basov, with whom he would establish a prolific collaboration. Basov was a second-year student of the Moscow Mechanical Institute (MMI), and an exemplar Soviet youth, when he began working at FIAN as an engineer of the Laboratory of Oscillations in the autumn of 1948. He was a member of the Komsomol (All-Union Leninist Young Communist League), the youth branch of the communist party, since 1940, and had been in the front for two years until he was demobilized at the end of 1945. He was first assigned to Prokhorov's group as an engineering laboratory assistant, and joined Prokhorov in the study of synchrotron radiation for his diploma work.[69] "The

[66] Pechenkin, 'Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics, No. 2, vol. 33, 2002' (as in n. 21); Dalmedico (as in n. 21).
[67] Prokhorova (as in n. 18), p. 48.
[68] Forman, 'Into quantum electronics: Maser as 'gadget' of Cold-War America' (as in n. 33).
[69] M. C. Rabinovich, *Materialy o vydvizhenii N. G. Basova. ARAN, 532-1-368*, 1962. Unlike the United States or Bologna process model, Russian higher education was traditionally not divided into undergraduate (bachelors) and graduate (masters) levels. Instead, tertiary education was undertaken in a single stage, typically five or six years in duration, which resulted in a specialist

gifted, purposeful, and tenacious young physicist soon adapted to the laboratory and found a common language with Prokhorov", becoming his main collaborator in the following years.[70]

As long-term and forward-looking military projects like the atomic weapons began to receive attention and funding after the war, they fostered research topics in related fields, including expensive research with uncertain results. In 1949, FIAN obtained a particle accelerator, betatron, which Alexander Prokhorov used in his experimental work on synchrotron radiation. Together with the betatron came Alexander I. Barchukov, a student from the Bauman Moscow State Technical University who was already familiar with the accelerator.[71] At an intersection of atomic and radio physics, the research on synchrotron radiation was useful to understand the functioning of the synchrotron accelerator itself, then one of the newest technologies in high-energy physics, and, more important in their case, to study its potential as source of microwaves. Basov would later recall: "In our investigations we aimed at creation of such radiation sources that would continuously cover a wide range of centimeter waves (just with that purpose we studied the synchrotron radiation)".[72]

As the Americans, Soviet physicists were looking for new schemes of generating radio-waves at smaller wave lengths, coming into the millimeter region, where the existing technology for generating microwave met its limit. According to Joan Bromberg, reliable sources of microwaves were being sought both by the military and the new growing cohort of microwave spectroscopists formed as a consequence of the wartime radar development in the USA. The first because the supposedly more compact size of millimeter-wave sources would be useful in reducing the weight of guided missiles and radars, and promised greater secrecy in short-range communications. The latter because good millimeter-wave sources were essential to study the absorption spectra of many molecules whose more intense spectroscopic lines were around 1 millimeter.[73]

Besides the synchrotron radiation, physicists at FIAN bet on Cherenkov radiation, their indigenous discovery, as a promising program for generating short microwaves. Cherenkov radiation, also known as Vavilov-Cherenkov radiation, is produced when charged particles travel through a dielectric medium faster than light

degree. Specialist degrees were perceived equal to Western MSc/MA qualification. For more details see: http://en.wikipedia.org/wiki/Education_in_Russia.

[70] Prokhorova (as in n. 18), p. 48.

[71] Ibid..

[72] Basov, 'Interview with Dr. N. G. Basov by A. Guenther, on 14th September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.' (as in n. 4).

[73] Bromberg, *The Laser in America, 1950-1970* (as in n. 7), pp. 13-14.

in that medium, but still slower than light in vacuum. It was discovered by Pavel Cherenkov in 1934 working in FIAN under the guidance of Sergei Vavilov, who developed a clever experimental method which required more than an hour in total darkness to increase the eye sensitivity and proposed the first interpretation of the results. Two other physicists of FIAN, Ilya Frank and Igor Tamm, developed a theory of this effect within the framework of Einstein's special relativity theory by 1937, for what they shared the 1958 Nobel Prize with Cherenkov. But it was only after the war, with the advent of high-sensitivity photomultipliers, which substituted the visual observation, and the development of radar technology that the effect attracted attention of physicists worldwide as method of detection of charged particles and as promising method for microwave generation.[74]

In the United States, Charles Townes pioneered the exploitation of Cherenkov effect for microwave generation. Working at Columbia Radiation Laboratory, and as the chairman of the Office of Naval Research's Advisory Committee on Millimeter Wave Generation, Townes was sponsored by the military to advance the microwave technology towards shorter wavelengths. Additionally, as a pioneer in microwave spectroscopy, he had his personal research interest added up to those of his military patrons. All this placed generation of short microwave amid his top priorities. In his laboratory, Townes and his pupils were conducting a research program named "New Schemes for Millimeter Wave Generation", in which they explored two alternatives for generating microwave, namely Cherenkov radiation and Molecular Generator. Even though the latter would prevail, leading to the invention of the maser, in the first years of 1950s most of Townes time and attention was dedicated to Cherenkov radiation.[75]

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Prokhorov's research on synchrotron radiation was therefore one of few promising lines in the quest for new microwave sources. Synchrotron radiation was predicted in 1944 by the Russian physicists Dimitri Ivanenko and Isaak Pomeranchuk in a paper concerning the limits of energy attainable in a particle accelerators.[76] It was initially seen as an unwanted, but unavoidable effect limiting the energy particles

[74] B. M. Bolotovskii, 'Vavilov-Cherenkov radiation: its discovery and application', *Physics-Uspekhi*, 52 nov (2009):11.

[75] Forman, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14).

[76] D Ivanenko and I Pomeranchuk, 'On the maximal energy attainable in betatron,', *Physical Review*, 65 (1944).

could achieve. Only later the potential advantages of synchrotron radiation began to be explored and became widely used as x-ray sources in hospitals and laboratories.[77]

In his work, Prokhorov showed that while the power of non-coherent radiation emitted by electron beams in the synchrotron orbit was proportional to the number N of electrons, the power of coherent radiation, due to electrons moving in bunches within the synchrotron, could be proportional to N^2 for electron beams with high degree of bunching. In a synchrotron, electrons revolve with a frequency equal or multiple of that of the high-frequency external field used to accelerate them. The selection of the multiple of the frequency, know as harmonic, permits to generate waves of different lengths. In Prokhorov's case, the frequency of electrons corresponded to the 16th and 24th harmonics and generated 3 cm and 2 cm wavelength, correspondingly. In the experiment the achieved power output was 10^{-6} watt. Estimation was made that it could be increased up to 10^{-2} watt with waves of 1 mm and 10^{-4} with waves of 0.1 mm, "considerable values that [then] could hardly be achieved by other methods".[78] Although the results were of some use for the analysis of accelerators, they were not very inspiring as far as the task of microwave generation is concerned. The synchrotron could not compete with other generators in the centimeter-wave region. Based on the estimated power for millimeter waves Prokhorov expressed hopes that with a synchrotron which permitted a significant increase in the number of particles one could achieve a power high enough to use it as source of radiation in spectroscopy.[79]

If we assume that the main groups interested in developing microwave sources were the military and microwave spectroscopists, and the that up to 1951 microwave spectroscopy were still nonexistent in the USSR.[80] we are led to the conclusion that Prokhorov's research was motivated mainly by military needs. And indeed, it would remain classified until mid-1950s. The results of that research were published only in 1956, but by then it was clear that they would not lead to any military application, and they were still original and relevant as knowledge on synchrotron radiation.[81]

[77] Philip Willmott, *An Introduction to Synchrotron Radiation*, (John Wiley and Sons, 2011).

[78] *Stenograma zasedaniia Uchenogo soveta. ARAN, 532-1-194a.* (Moscow, 1951).

[79] Ibid..

[80] N. G. Basov, 'Otchet za 1951 god po rabote "Postroika Radiospektroskopa s elektricheskoi molekuliarnoi moduliatsei"', in: *Zapiski Arkhivariusa T. 2, Byp. 1, 1997* (Moskva: Izdanie Arkhiva Fizicheskogo Instituta im. P. N. Lebedeva, 1952), p. 17; A. M. Prokhorov, *Otchet po sektoru radiospektroskopii laboratorii kolebanii FIAN za 1955 god. ARAN, 532-1-251. pp. 83-39*, (1955), p. 83.

[81] Idem, 'Kogerentnoe izluchenie elektronov v sinkhrotrone v oblast santimetrovykh voln', *Radiotekhnika i elektronika*, 1 (1956).

Thus, as Charles Townes and Joseph Weber, the two Americans who conceived the working principle of masers, the former a physicist who worked in the American radar program during the war and the latter an engineer who became physicist in the postwar, Prokhorov was involved in the development of radar and other radio technologies. However, while this connection between maser and radar research has been long explicit in the American cases, it has so far been absent in accounts of the maser invention in the Soviet Union.[82] Perhaps one of the reasons for this absence is that the connection between masers and military research has been downplayed by Soviet physicists themselves, in accounts produced when military research had lost its appeal. In those accounts the maser was invented from an attempt at solving problems originated in apparently fundamental research in spectroscopy. For instance, being interviewed by American historian Arthur Guenther in 1984,[83] Basov gave the following declaration on the connections of military research and his initial work that led to masers:

> I joined FIAN in 1948, a few years after the war, and I didn't find the presence of any military investigation at the Institute. All our thoughts were about mastering the physics in full measure in order to develop the national economy. As far as masers and lasers are concerned, then of course, one should mention that centimeter waves used in radiolocation had been the creation of the war. But we were dealing with those problems without any connection with military investigations... I'm somewhat younger than Aleksandr Mikhailovich [Prokhorov]. He too was in the army during the war, but he was not involved in radio engineering at the wartime either. We, therefore, were not radar investigators. In this respect we and the American scientists have some different approaches to the development of quantum electronics.[84]

[82]The importance of the radar work as source of both expertise and hardware for the invention of the maser is widely acknowledged in the historical literature on masers and lasers.Bromberg, *The Laser in America, 1950-1970* (as in n. 7); Bertolotti (as in n. 61); Hecht (as in n. 19); Forman, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14).

[83]In 1984 Soviet physicists were publicly opposing Regan's Strategic Defense Initiative. In interview recorded in the same day Prokhorov shows his position to military application of lasers: "I wish that the dream of ah... the war by lasers, I think so, it's rather silly thing, and that one way in which we must not go." Prokhorov, 'Interview with Dr. A. M. Prokhorov by A. Guenther, on 14 September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.' (as in n. 59).

[84]Basov, 'Interview with Dr. N. G. Basov by A. Guenther, on 14th September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.' (as in n. 4).

There are, no doubt, differences between the American and Soviet approaches to the development of quantum electronics, as we will see in the next sections, but the absence of a link with military investigations in the Soviet case is not one of them. Regarding the perception of involvement with military research, being involved in an organized and concentrated effort to develop radars, as happened with Townes in the US, and being a graduate student working in a civilian institution doing a small portion of broader research program closely connected with radar development are remarkably different. In the first case, no matter how meaningless the research appears to be, one is certainly aware of being involved in military research; in the second, if the student does not have a clear picture of the project as a whole, as often happens in big-science projects, he may not see how his small strokes affect the entire composition. Thus, one may argue that Prokhorov and Basov at the time were not aware of the connection of their work with radars. Even that, however, is unlikely. Among Prokhorov's first publications after the war are a short paper in the popular science magazine *Nauka i Zhizn* (Science and Life)[85] and a booklet explaining the fundamentals of radiolocation[86] that show his familiarity with radar physics and an understanding of the importance of his work on stabilization of frequencies and of new methods for generation of short microwave for radar technology.

Secrecy and compartmentalization

In a time when secrecy and spymania had reached levels of paranoia in the Soviet Union, significantly exceeding that of the United States, Prokhorov's work on synchrotron radiation was, naturally, classified. Then, many Soviet scientific institutes employed security officers who reviewed and monitored personal. Personal contacts, correspondence, and informal channels with foreign colleagues all became very risky; library purchases of officially published scientific literature was the chief remaining channel of information exchange across the Iron Curtain. In many academic disciplines, such a publication required a clearance by a security officer in the institute, especially in fields at least nominally related to nuclear matters.

However, for physicists of Prokhorov's generation, who acquired professional maturity in the years between 1940 and mid 1950s, classified work on military research was normal, prestigious, and rewarding. Most of those physicists were part of the *frontovik* (war veteran) generation, defined by their participation in the war, often

[85] A. M. Prokhorov, 'Fizika Radiolokatsii', *Nauka i Zhizn*, (1946):8-9.
[86] Idem, *Chto takoe radiolokatsii*, (Moskva: Goskultprosvetizdata, 1948).

glorified as heroes in the cult of war promoted by official propaganda in the late 1940s. *Frontoviks* could be seen walking on Gorky Street, a major street leading to the Moscow Kremlin, sporting war medals, and were, in large majority, loyal supporters of the regime.[87] Prokhorov could boast three war medals and would affirm his loyalty joining the communist party in 1950.[88] As a secretary of the Komsomol organization at the Leningrad Electro Technical Institute would later recall, people of his time were "deeply influenced by the spirit of the *frontovik* generation" characterized by war-related virtues such as loyalty, sacrifice and obedient collectivity, which dominated the landscape of the institute until mid-1950s.[89]

Physicists of that generation acquired professional maturity in a period in which science and scientists were mobilized for the war, working in the *modus operandi* of defense research – mission oriented work under security and secrecy regime. Seeing the example of their elders, they were inured to militarized working discipline and security regulations from the outset of their careers. Leading Soviet physicists as Sergei Vavilov and Igor Kurchatov, the director of the atomic bomb, aka General Kurchatov, were exemplars of the virtues characteristic of the *frontovik* generation. They saw their contribution to military research as a continuation of the wartime effort, necessary to protect the country from a military aggression, requiring comparable discipline and self-sacrifice.[90]

To be sure, there could be certain uneasiness about working on military research, especially among physicists formed before the war. An emblematic example is found in the case of Lev Davidovich Landau, who worked in the atomic bomb not out of patriotic duty, but out of fear of political prosecution. He belonged to the first Soviet generation of scientists, educated immediately after the revolution. From 1929 to 1931 he visited leading European laboratories in Berlin, Leipzig, Zurich, Cambridge,

[87] Mark Edele, 'More than just Stalinists: The political sentiment of victors 1945-1953', in: Juliane Fürst, editor, *Late Stalinist Russia: Society Between Reconstruction and Reinvention*, (London and New York: Routledge, 2006); Monica Rüthers, 'The Moscow Gorky Street in late Stalinism', in: Juliane Fürst, editor, *Late Stalinist Russia: Society Between Reconstruction and Reinvention*, (London and New York: Routledge, 2006)

[88] Shcherbakov, Mikhailova and Prokhorov (as in n. 84). Both Prokhorov and Basov became members of the Communist Party in the early 1950s and remained faithful to the Soviet Leadership to the end of the Soviet Union. Later Basov and Prokhorov would assume leading roles in strategic defense research in the Soviet Union. Hey (as in n. 5). From 1974 to 1989 Basov was a deputy of the Supreme Soviet of the Soviet Union.I. G. Bebikh, N. Ia. Goncharova and L. M. Zhukova, *Biobibliografiia Uchenykh: Nikolai Gennadievich Basov*, 1993.

[89] Fürst, 'Late Stalinist society: history, policies and people' (as in n. 73), pp. 224-225.

[90] Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), pp. 126-184.

and Copenhagen, working closely with the physicists who developed quantum mechanics in its early years. Back in the USSR, Landau established himself at the Ukrainian Physico-Technical Institute (UFTI) in Kharkov, Northeastern Ukraine, where he created his school of theoretical physics. The UFTI, fostered by industrialization drive of the 1930s, became one of the leading physics institutes in the Soviet Union, and its early pioneering work on magnetrons secured it a key position in the Soviet radar development. When the institute became increasingly militarized in the second half of the 1930s, Landau voiced dissatisfaction. He and some of his institute friends protested the new security regulations and the redirection of some the laboratories towards classified projects. For those protests Landau had to flee Kharkov to scape arrest. He was arrested in 1938, at the end of the Great Purges (1936-1938), for other radical pronouncements, but released a year later to work on the problem of superfluity of liquid helium.[91] In the postwar, in a precarious position after his arrest, Landau agreed to work on the atomic bomb because "he felt that classified research provided additional protection. He still kept his involvement rather limited and withdrew at the first available opportunity after Stalin's death".[92]

Landau's felling of safety from his involvement with military research had grounds. To be involved in military research in that period, although requiring self-sacrifice and entailing other risks, could offer a degree of personal security that diminished the possibilities of political prosecution. For example, Yuri I. Neymark, a physicist of Gorky State University was attacked by local representatives of the Communist Party of Gorky and charged at the local level. However, he seemed mysteriously protected by higher authorities who canceled locally-taken decisions. The reason was that he was doing classified work, judged elsewhere to be strategic.[93]

Secrecy was a trait of Prokhorov's research since his training, and the same was true for his students. His first students were formed working on microwave spectroscopy, to be discussed in the next section, in a program that was compartmentalized from its conception, divided in open and classified parts. The public face of their research was the making of the spectroscope, measurements of rotational spectra of molecules, and the possibility of using molecular transitions for frequency stabilization. Those works produced results that were published in scientific journals and

[91] For more details on Landau's biography and his arrest see Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), pp. 114-120.
[92] Ibid., p. 245.
[93] Dalmedico (as in n. 21), p. 258.

in an international conference.[94] The secret face of their research – measurements of nuclear spins and moments of radioactive nuclei – because of its relevance for theoretical calculations in nuclear physics, and possibly, the development of nuclear weapons, was published only in classified reports and discussed in closed meetings.[95]

Thus, secrecy and compartmentalization, essential elements of the history of the maser in the United States,[96] were also traits of Soviet physics. We can see this similarity as a matter of form, of the way scientific research was organized, stemming from the entanglement between physics and military research during and after World War II. However, the postwar developments in Soviet physics would extend similarities to matter of content as well.

3.3 Exploring nuclei: catching up, and surpassing

> "[N]o doubt... if we render the necessary assistance to our scientists they will be able not only to overtake but also, in the very near future, to surpass the achievements of science outside the boundaries of our country." [97]

While working on his doktor nauk dissertation, Prokhorov's living standard improved dramatically. His job as a senior worker at FIAN in material terms meant not only a significant rise in pay but also came attached to privileges as access to special grocery stores with better quality food and a plot of land on the outskirts of Moscow. For a while, however, at home he still had to work sharing the single table and the scarce space of the tiny 15.5 m^2 room where he lived with his wife, son, and mother-in-law. In summer he could have more privacy on an improvised writing desk made of a piece of plywood nailed at a corner of the balcony. That

[94] A. M. Prokhorov and A. I. Barchukov, 'Metod Izmereniia Koeffitsientov Poglochsheniia v Microvolnovoi Radiospektroskopii', *JETF*, 26 (1954):6; N. G. Basov and A. M. Prokhorov, 'The Theory of Molecular Oscillator', *Discussions of the Faraday Society*, 19 (1955).

[95] Idem, 'Primenenie Molekuliarnykh Puchkov v Radiospektroskopii.' in: *Soveshchaniia po magnitnym momentam iader.* (ARAN, 1522-1- 59, pp. 36-47, 1953). This conference paper and the internal report were later declassified and published in a collection of Basov's early papers in Starodub (as in n. 82). Basov particularly emphasizes the importance of precise measurements of nuclear moments to develop the nuclear shell model, which then did not fit recent experimental data of nuclear moments. N. G. Basov, *Opredelenie Iadernykh momentov radiospektroskopicheskim metodom*, Published in: Zapiski Arkhivariusa T. 2, Byp. 1, 1997 (FIAN, Moskva, 1953).

[96] Forman, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14).

[97] I. V. Stalin, *New five-year plan for Russia. Election address*, (Moscow, 1946) ⟨URL: http://digitalarchive.wilsoncenter.org/document/116179⟩.

situation changed in 1950, when they moved to a new three-rooms apartment in a condominium built by Sergei Vavilov specially for FIAN workers located in front of the new building of the institute.[98]

Besides being associated with his new academic title, Prokhorov's new living standard reflected the new status of science in postwar Soviet Union. When the war was over, the atomic blast over Hiroshima and Nagasaki made clear that for the years to come military superiority rested on scientific superiority. Stalin understood it clearly. In the early postwar years, he helped to elevate science and its representatives to the level of social prestige approaching that of the political and military elites. The new status was not limited to nuclear physics and other fields related to military projects, but embraced all fields of scholarship, *nauki* in the Russian sense. Scientists, in this wider sense, came to form an elite social group next to party *apparatchiks*, industrial administrators and the military, and became more privileged than engineers. Not only resources for research, but also individual salaries were raised higher than in any other time in Soviet history. For several thousand senior scientists this status meant sufficient quantity and somewhat better quality food from special rations and grocery stores and, with some luck, a separate apartment for the family. Although contradictory to the egalitarian Soviet mentality, these privileges in practice were highly valued in a poor country ruined by the war, with burnt-out villages and leveled cities and factories, with peasants starving and big-city dwellers typically living in shared, one-room-per family apartments.[99]

With increased privileges came increased responsibilities. In exchange for their new status, Stalin expected scientists, specially nuclear physicists, to commit to the slogan he put forward in the speech quoted above - "to catch up and to surpass". The slogan was not restricted to nuclear physics, though. It was adopted in most of branches of postwar Soviet physics.[100]

[98] Prokhorova (as in n. 18), pp. 45-53.

[99] Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71).

[100] This may seem contradictory for some, given that this period coincides with the Zhdanovishchina, the campaign against western cultural influence, and the apex of Lysenkoism, which resulted in the banishment of genetics from Soviet science. However, despite being the most widely studied case in history of science in postwar Soviet Union, the outcome Lysenkoism is not representative of what happened in most of disciplines. In the last years of Stalin's rule several debates similar, even inspired by Lysenko's campaign, were held in different academic disciplines, with outcomes quite different from the one in Biology. In the case of physics, the participants adopted "to catch up and to surpass" as the official slogan of the meeting. The debate in physics never took place; bureaucratic maneuvers postponed and finally canceled the meeting. However, the documentary records of its preparation and its slogan reflect the particular choice of developing

Thus, Soviet physicists could not overlook one of the newest and fastest growing fields in the West - microwave spectroscopy. Investigations in the field had began in several US laboratories well before the end of the war, driven by the K-band problem. Pushing the development of radars toward shorter microwaves, the military and scientists discovered that the k-band radars, with wavelengths between 1.66 cm and 1.11 cm, had a surprisingly limited reach. As it turned out, the water vapor present in the atmosphere was absorbing the radiation because the energy associated with that frequency matched the energy of a rotational transition of water molecules.

A setback to military usage of the new radars, the absorption of the radar radiation by water vapor in the atmosphere was a major boost to microwave spectroscopy. First, because the urgent motivation to find what was the problem with K-band radar and to provide the means of avoiding such problems in the future drove the attention of physicists to the phenomenon of absorption of microwaves by molecules. Second, because once those radar sets were not of much use for the military, they were discarded and became available to physicists devalued or at no cost at all. Radar transmitters and receivers were the basic, and most expensive, parts of spectroscopes. With physicists armed with expertise and hardware, the quite practical orientation of US physics in the postwar warranted the pursuit of microwave spectroscopy in industrial as well as academic settings.[101] For Forman, microwave spectroscopy became the "premier example of a flourishing field of physical research created – in every sense– by radar". Its impressive growth in the late 1940s is expressed by the numbers of publications, which surpassed a thousand papers in less than a decade.[102]

The growth of microwave absorption spectroscopy in the postwar was closely linked to atomic and nuclear physics. Its object of investigation is the radiation absorbed or emitted during transitions between energy states of molecules, atoms, or nuclei (see figure 3.1 for an overview of absorption spectroscopy). Each of this entities has specific energy states defined according to quantum mechanical laws. Molecules, for instance, possess discreet and well defined energy states associated with rotation and vibration of their atoms. To switch from one energy state to

Soviet physics along western lines. For the preparation of the physics meeting see Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), pp. 245-248; For the ideological battles in Soviet science in the period see Idem, 'Games of Stalinist Democracy: Ideological discussions in Soviet Sciences' (as in n. 53); and Ethan Pollock, *Stalin and the Soviet Science Wars*, (Princeton: Princeton University Press, 2006).

[101]Forman, '"Swords into ploughshares"' (as in n. 12); Bromberg, *The Laser in America, 1950-1970* (as in n. 7).

[102]Forman, '"Swords into ploughshares"' (as in n. 12), p. 422.

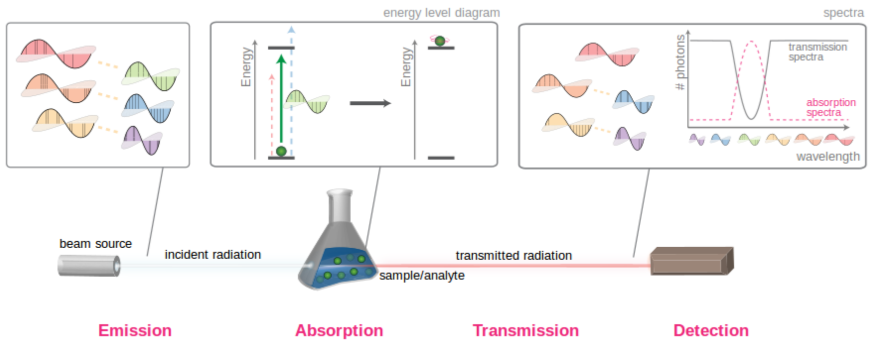

Figure 3.1: An overview of electromagnetic radiation absorption. This example discusses the general principle using visible light as a specific example, but the same occurs in microwave absorption spectroscopy. In the example a white beam source – emitting light of multiple wavelengths – is focused on a sample (the complementary color pairs are indicated by the yellow dotted lines). Upon striking the sample, photons that match the energy gap of the molecules present (green light in this example) are absorbed in order to excite the molecule. Other photons transmit unaffected and, if the radiation is in the visible region (400-700nm), the sample color is the complementary color of the absorbed light. By comparing the attenuation of the transmitted light with the incident, an absorption spectrum can be obtained. Source: wikipedia.org.

another the molecule must absorb or emit a quantum of radiation, usually in the microwave or infrared region, equivalent to the energy gap between those energy states. If the transition is to a state of higher energy, the molecule absorb radiation; if the transition is to a state of lower energy, the molecule emit radiation. The absorbed or emitted radiation yields information about the physical properties of the molecules in question. The accuracy of the measurements produced in the postwar had a remarkable repercussion in theoretical physics, forcing theoreticians to reformulate theories to fit the new experimental data.[103]

Prokhorov was aware of the new developments in microwave spectroscopy when his group took up research in microwave spectroscopy. By many accounts, the choice of new field was suggested by Sergei Vavilov,[104] but if that was the case Vavilov's

[103] Silvan S. Schweber, 'Writing the Biography of Hans Bethe: Contextual History and Paul Forman', *Physics in Perspective*, 16 jun (2014):2, ISSN 1422–6944.

[104] Prokhorov, 'Interview with Dr. A. M. Prokhorov by A. Guenther, on 14 September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.' (as in n. 59); Basov, 'Interview with Dr. N. G. Basov by A. Guenther, on 14th September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.' (as in n. 4).

suggestion was probably oriented by his knowledge of classified information from high-priority projects. According to a report that summarizes the activities of the laboratory beginning from 1952 at least a third part of the research of the laboratory was the "determination of nuclear moments, carried on by request (*po postanavleniu*) of the Council of Ministers of the USSR", a high-administrative body which, when it comes to science, interfered only in high-priority topics. [105]

To catch up in the new field they followed closely North American publications, familiarizing themselves with the latest challenges and developments. According to Basov's recollections, publications in microwave spectroscopy appeared mainly in Physical Review and Journal of Chemical Physics,[106] what in fact reflected in the bibliography of his dissertation, mostly composed of foreign papers.[107] The first internal report on microwave spectroscopy concerning the work to build a spectroscope "described in general terms in foreign literature" emphasizes that as far as they knew, besides their group at FIAN, there was no one else conducting similar research in the USSR.[108]

A later report revels not only that the group was familiar with foreign research, but that they measured the development of their own research against it:

> A new field, physical radiospectroscopy of molecules, started developing abroad (especially in the USA) after 1946. More than 400 foreign researchers are currently involved in the work in radiospectroscopy. At FIAN, the work on radiospectroscopy of gases started in 1952... Research done in the unity is not inferior to foreign works in its quality, but the amount and the speed of development are significantly lower. This is due to the small number of people involved (1 doctor of science,

[105]Prokhorov, 'Otchet po sektoru radiospektroskopii laboratorii kolebanii FIAN za 1955 god. ARAN, 532-1-251. pp. 83-39' (as in n. 80), pp. 85-86. The Council of Ministers was highest executive and administrative body of the USSR, subordinated only to the Central Committee of the Communist Party. It directed the Academy of Sciences and the ministries of Education and Industry through its State Planning Commission (GOSPLAN). In practice, the Academy had autonomy to manage its research within the budget allocated by the GOSPLAN. According to Loren Graham, the Council of Ministers and the Central Committee interfered only in high-priority topics. Graham (as in n. 8).

[106]Basov, 'Interview with Dr. N. G. Basov by A. Guenther, on 14th September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.' (as in n. 4).

[107]Idem, 'Opredelenie Iadernykh momentov radiospektroskopicheskim metodom' (as in n. 95).

[108]Idem, 'Otchet za 1951 god po rabote "Postroika Radiospektroskopa s elektricheskoi molekuliarnoi moduliatsei"' (as in n. 80).

> 5 candidates and 5 engineers), lack of space and inadequacy of technical supply.[109]

The first challenge Prokhorov and his group faced entering the new field was to build a spectroscope with high sensitivity and resolution. A high-resolution and high-sensitivity spectroscope is imperative to record precise data. The higher the resolution, the more detailed is the data provided. The sensitivity dictates the limit of the measurement. The first seconds after you turn off the light in a closed room at night you are immersed in total darkness, the sensitivity of your eyes is not enough to detect the weak light reflected by the objects around you. As your eyes adapt, and become more sensitive, you begin to discern shapes of the furniture and identify some objects around. In this example the resolution determines the smallest details of the object you can identify.

When they began studying microwave spectroscopy in 1951, spectroscopic measurements were made by sending radiation through a container, called absorption cell, with gas of the substance under study. The main components of spectroscopes were the source of radiation (transmitter), the absorption cell, and a detector (receiver) to accuse the absorption of radiation. The microwave source was the main limitation to the sensitivity of their spectroscope. They employed a reflex klystron that generated radiation of wavelength between 5 cm and 2,6 cm. With help of a frequency multiplier they explored the second harmonic of the klystron's basic frequency to generate waves of wavelength from 2,5 cm to 1,3 cm. In that case, however, the radiation power at the output of the multiplier was reduced more than a thousand times. The power obtained was far bellow the required for the optimal functioning of the spectroscope, what diminished its sensitivity. They initially considered using the third harmonic to produce millimeter waves, but the output power would be still weaker. With that klystron, the best radiation source available to them, their spectroscope could not study many important molecules whose absorption spectra laid in the microwave region bellow 1,3 cm. Hence, they claimed, "for the successful development of microwave spectroscopy it is necessary that the domestic industry release wide-range centimeter and millimeter wavelength klystrons.[110]

[109] Prokhorov, 'Otchet po sektoru radiospektroskopii laboratorii kolebanii FIAN za 1955 god. ARAN, 532-1-251. pp. 83-39' (as in n. 80), pp. 83-84. This report, and others written in the second half of the 1950s, were part of an strategy to promote microwave spectroscopy exploiting the competition with the West, which will be discussed in the chapter 3.

[110] Basov, 'Otchet za 1951 god po rabote "Postroika Radiospektroskopa s elektricheskoi molekuliarnoi moduliatsei"' (as in n. 80), p. 35.

Comparing the microwave source they used with those available to microwave spectroscopists in the US, Basov and Prokhorov had reasons to be upset. The transmitters of the k-band radars widely available to US physicists after the war could generate radiation of wavelength from 1.66 cm to 1.11 cm with enough power to be employed in radars that had higher power requirements than spectroscopes. Therefore, with the technique that Basov and Prokhorov had to use to reach wavelengths from 2.5 cm to 1.3 cm, US physicists could go all the way to 0.6 mm and investigate many more molecules.

As for the spectral resolution, the Soviets and Americans had similar problems. The main limiting factor was the Doppler broadening[111] due to the random movement of molecules in the gas. While the molecules moving toward the source absorbs radiation of a frequency slightly lower than the actual frequency of the transition, the molecules moving away from the source absorbs radiation of a frequency slightly higher than the actual frequency of the transition. Therefore, there is a broadening of the spectral line registered by the detector. And the spectral linewidth matters because if it is wider than the distance between two absorption lines, those lines will be detected as a single one.

From molecular-beam spectroscopy to masers

To eliminate Doppler broadening, Basov and Prokhorov opted for a new kind of spectroscope in which instead of irradiating a gas they would irradiate a molecular beam[112] of the substance to be analyzed. By 1952 physicists had understood that

[111] Doppler effect is the change in the frequency of a wave due to the relative movement between the source and the observer. A good illustration of this effect is the change in the frequency of the sound of the siren of a police car when it passes by us. The sound becomes lower because when the source (police car) begins moving away from the observer (person) the frequency of the sound decreases.

[112] A molecular beam is produced by allowing a gas at higher pressure in an oven to expand through a small orifice into a chamber at lower pressure. The molecules enters the chamber through the orifice moving at approximately equal velocity (which is determined by the temperature of the oven) with very few collisions between themselves. This technique was employed in the famous 1922 experiment by Otto Stern and Walther Gerlach, which is considered the first experimental demonstration of the quantization of magnetic moments of atoms, or 'space quantization'. As we said above, according to quantum mechanics electrons and nuclei can assume only well defined, quantized values of magnetic moments. The transition from one value to another occurs only with absorption or emission of radiation. What is shown in the Stern-Gerlach experiment is that when passing through a non-homogeneous magnetic field electrons are deflected in two opposing directions according to the orientation of their magnetic moment (spin). For a discussion of the Stern-Gerlach experiment and its relation to the old and new quantum theory see Friedel Weinert, 'Wrong theory–Right experiment: The significance of the Stern-Gerlach experiments', *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 26 (1995):95.

once the Doppler broadening was due to the random movements of the molecules, it could be eliminated by making all molecules move in the same direction, as in a focused beam, and the measurement on a plane orthogonal to that of the movement of molecules. This method was first proposed by the Princeton physicists George Newell and Robert Dicke, and soon others in the United States were constructing and operating molecular-beam absorption spectroscopes.[113]

Molecular beams were also being employed in spectroscopy since the late 1930s, although in a very different way, by Isidor Isaac Rabi's group at Columbia University. Upon returning from a two-year fellowship in Europe, where he worked with Otto Stern and Walther Gerlach, Isidor Rabi established a group of young physicists at Columbia to develop and employ molecular beam resonance to determine spins and magnetic moments of atomic nuclei, a program which would win him the 1944 Nobel prize. In 1937 Rabi and collaborators decided to employ molecular-beam techniques to radio-frequency resonance spectroscopy, creating a new method christened molecular-beam magnetic resonance spectroscopy.[114] To extend the resonance method to study molecules with large dipole momentum Harold Hughes, working in Rabi's group, proposed the molecular-beam electronic resonance spectroscopy, which was analogue to the magnetic resonance method, but used non-uniform electric field instead of magnetic to manipulate the beam.[115]

The main difference between the resonance methods and the traditional spectroscopic methods is in the way the absorption of radiation is registered. While in traditional spectroscopy the measurement is made directly over the radiation, and the absorption is accused by a decrease in the intensity of the radiation, in molecular-beam magnetic resonance spectroscopy what is measured is the intensity of the molecular beam, and the absorption of radiation is accused by a decrease in the intensity of the beam.[116]

Basov and Prokhorov gave the main steps towards the conception of a maser combining the idea of replacing the gas by a molecular beam, to decrease Doppler broad-

[113] George Newell and R. H. Dicke, 'A Method for Reducing the Doppler Breadth of Microwave Absorption Lines', *Phys. Rev.* 83 (1951):5;Forman, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14).

[114] Jack S. Goldstein, *A Different Sort of Time: The Life of Jerrold R. Zacharias, Scientist, Engineer, Educator.* (Cambridge, Mass, and London: MIT Press, 1992), ISBN 0–262–07138, p. 34.

[115] Harold Kenneth Hughes, 'The Electric Resonance Method of Radiofrequency Spectroscopy The Moment of Inertia and Electric Dipole Moment of CsF', *Phys. Rev.* 72 (1947):7.

[116] For a history of Rabi's group see Goldstein (as in n. 114). Molecular-beam magnetic resonance spectroscopy and its historical significance is discussed in Forman, '"Swords into ploughshares"' (as in n. 12), pp. 404-407.

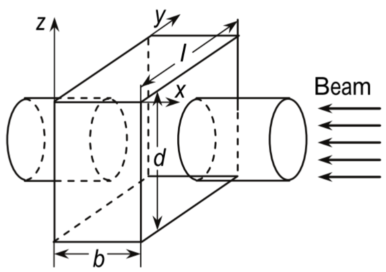

Figure 3.2: Basic scheme of the spectroscope proposed by Basov and Prokhorov. The cavity or absorbing cell is the rectangular box. The cylinder is a waveguide and the arrows on the right represent the molecular beam. Source: (Idem, 'Primenenie Molekuliarnykh Puchkov v Radiospektroskopii.' (as in n. 95)).

ening, with the new method of molecular-beam electronic resonance spectroscopy proposed by Harold Hughes. Their aim was to create a spectroscope unifying the standard spectroscopic method with molecular beam methods. They proposed to use the molecular beam techniques to manipulate the beam, but instead of detecting the absorption measuring the molecular beam, as Rabi and his group, they followed the usual spectroscopic method of irradiating the substance, i.e. the beam, and measuring the outbound radiation. The design of the spectroscope consisted basically of a cylindrical waveguide with a rectangular cavity as an absorbing cell (figure 3.2). Based on calculations, unifying those two methods, they expected to decrease the spectral line width from 60 kHz, attainable with gases under usual conditions, to approximately 8 kHz.[117]

The first presentation of their work on the molecular-beam spectroscope was in a classified conference on magnetic moments of nuclei, which took place from January 22 to 23, 1953. According to the proceedings, they had done the theoretical work and were making the device. That presentation reveals that they went further than American spectroscopists who were using molecular beams when, based on Hughes paper, they suggested to use an non-uniform electromagnetic field to sort out the molecules according to their energy states and to make the measurement on molecules in a single energy state.[118]

[117]Basov and Prokhorov, 'Primenenie Molekuliarnykh Puchkov v Radiospektroskopii.' (as in n. 95).

[118]Basov and Prokhorov cited Hughes's paper only in a later publication: N. G. Basov and A. M. Prokhorov, 'Primenenie Molekuliarnykh Puchkov v dlia Radiospektroskopicheskogo Izucheniia Vrashchatelnykh Spektrov Molekul', *JETF*, 27 (1954):4; but there are indications that Hughes was already influential in this early stage: First, for the calibration of the apparatus

A molecular beam leaves the oven with a slight difference in number of upper-state and ground-state molecules, in their case the number of ground-state molecules exceeded the number of upper-state molecules by a factor 0.001, what means that only 0.1 percent of the molecules could be considered active molecules, namely molecules that can absorb radiation. They understood that separating the molecules according to their energy state would hike up the number of active molecules, and hence the power of the radiation absorbed. If all the molecules that enter the cavity are in the ground state, they would all be active molecules and the sensitivity of the spectroscope would increase up to a thousand times.[119]

That step led them not only to solve the problem with sensitivity but also to a new spectroscopic and amplification methods. “Once the molecules can be separated”, they wrote, “a new spectroscopic method is at hand, namely to study emission spectra instead of absorption spectra”. The advantages of studying the emission spectra is that instead of having to measure a relative decrease in power of radiation of the frequency absorbed in a wide frequency interval, one had to measure the absolute power of the radiation emitted in a very narrow frequency interval, what simplified greatly the measurement process.[120]

To study the emission spectrum, instead of sending ground-state molecules into the cavity, they would send the upper-state molecules, and those had to irradiate during the flight inside the cavity. However, they calculated that once the time of flight of the molecules inside the cavity is much shorter than the time the molecules remain in the upper state, “the radiation studied has to be, of course, induced”[121] Thus, to study the emission spectrum they had to introduce radiation inside the cavity to stimulate further emission of radiation.

They conclude that “Over time, if the cavity has a quality factor (Q) good enough, the energy is stored in the cavity and the probability of emission tends to unity. All

they chose CsF because it had been studied by resonance method, and they used precisely the values obtained by Hughes for the dipole momentum (7.3 Debye) and the rotational constant (0.147cm-1) for molecules of CsF. Second, instead of using a magnetic field for the separation of molecules according to their rotational states, more common in the experiments with molecular beams, they used a non-homogeneous electric field, one of the innovations of Hughes proposal. Hughes (as in n. 115).

[119] Basov and Prokhorov, ‘Primenenie Molekuliarnykh Puchkov v Radiospektroskopii.’ (as in n. 95), pp. 40-41.

[120] In figure 3.1 the third box shows a graph with two curves. The continuous curve represents the transmission spectra and the valley in the curve is what accuses absorption. With an emission spectroscope what has to detected is a peak of radiation which looks like the dotted curve.

[121] Basov and Prokhorov, ‘Primenenie Molekuliarnykh Puchkov v Radiospektroskopii.’ (as in n. 95), p. 41.

the molecules passing through the cavity irradiate".[122] This is basically the process of amplification wherein the radiation introduced into the cavity is amplified by the molecules that irradiate while passing through that cavity. However, in that presentation of January 1953, they did not draw attention to amplification, and limited the discussion of the new device as a spectroscope. Perhaps they thought the amplification would not be significant and that the device could not compete with other off-the-selves amplifiers; or perhaps, short of time, in a conference on magnetic moments of nuclei, they had to limit the discussion to spectroscopy. But, in any case, by then they apparently did not think that they had hit upon something exceptional, because it took them almost one year to submit a paper discussing the device as an amplifier and generator of microwaves.

The first time Basov and Prokhorov presented the device as a molecular generator in print was in a paper submitted on January 19, 1954. "Using a molecular beam in which the molecules in the lower state of the transition under study are absent, we can make a molecular generator", they wrote.[123] That paper concerned the application of molecular beam in spectroscopy; it was an extended, improved version of the 1953 talk discussed above and dedicated the last one and a half page to explain the working principle and calculate some of the basic parameters of the molecular generator. The working principle is summarized in the following way (the emphasis is ours):

> The sorted out molecular beam, in which molecules in the lower state of the transition under study are absent, is passed through a cavity. During the flight into the cavity, part of the molecules undergo transitions from the upper to lower state, imparting their energy to the cavity. If intracavity losses are smaller than the emission power of molecules, *self-excitation* takes place and the radiation power in the cavity increases up to the value determined by the *saturation effect*.[124]

[122] Basov and Prokhorov, 'Primenenie Molekuliarnykh Puchkov v Radiospektroskopii.' (as in n. 95), p. 41.

[123] Idem, 'Zh. Eksp. Teor. Fiz., No. 4, vol. 27, 1954' (as in n. 16), p. 435. This paper was published only in October 1954. According to some accounts it was "submitted in December of 1953; but when this paper was about to be published they discovered that they omitted 2π to some power in the numerical coefficients for the self-excitation conditions. This forced them to withdraw the paper in order to make the necessary corrections. Its submission is now dated 19 January 1954. The paper was published in October of 1954" Karlov, Krokhin and Lukishova (as in n. 80), p. 34.

[124] Basov and Prokhorov, 'Zh. Eksp. Teor. Fiz., No. 4, vol. 27, 1954' (as in n. 16), p. 437.

In that brief account of the molecular generator, Basov and Prokhorov basically calculated the condition of self-excitation and the energy of the stationary state of the oscillator, the latter determined by the saturation effect. Those were the first questions raised in the study of self-oscillating systems in the framework of the theory of oscillations.[125] The condition of self-excitation was calculated simply by making the energy irradiated by the molecular beam bigger than the energy lost through the cavity ($N_{act}h\nu > E_{loss}$).[126] From that expression they calculated the quality factor Q necessary for the device to work as a molecular generator. As for the stationary state, taking in account the saturation effect, they limited its description to the calculation of the maximum power that could be obtained with that generator, estimating it at half of the total energy emitted by the molecular beam ($\frac{1}{2}N_{act}h\nu$).[127] Those calculations therefore indicate that since the early stages of their research, probably since they realized that the spectroscope could be a generator, the new device was included in the class of self-oscillating systems, and understood within the framework of the theory of oscillations.

The conception of the maser by Basov and Prokhorov, as it surfaces in the documentary evidence, was derived from at least three instances. They put together, first, the proposal by Newell and Dicke to eliminate Doppler broadening by making spectroscopic measurements over molecular beams instead of gases; second, the method of manipulating molecular beams using non-homogeneous electric fields developed by Harold Hughes at Columbia University; and third, the knowledge of the theory of oscillations which permitted them to envisage that under specific conditions stimulated radiation would build up inside the cavity and that the device would work not only as an spectroscope, but as a microwave amplifier as well. In the way they understood the maser we can see distinctive traits of the approach to oscillations developed by Mandelstam and his pupils. This can be better appreciated, as we will see in the next section, comparing the Soviet and the American theoretical understanding of the maser.[128]

[125] Gorelik (as in n. 29).
[126] N_{act} is the number of active molecules, molecules that emit radiation; $h\nu$ is a quantum of radiation, h being Plank's constant and ν the radiation frequency.
[127] Basov and Prokhorov, 'JETF, No. 4, vol. 27, 1954' (as in n. 118), pp. 437-438.
[128] Newell and Dicke (as in n. 113).

3.4 Comparing approaches: radiophysics vs. electronics

In 1954, amid important structural changes in Soviet science, Mikhail Leontovich, the head of the Laboratory from 1947 to 1954, left to work at the Institute of Atomic Energy and Prokhorov assumed the leadership of the Laboratory of Oscillations. Leontovich and Sergei Rytov were the last members of the early days of the school of oscillations still active in that laboratory. Thus that change in leadership represented a significant generational change that translated itself in a striking renewal of the problems approached in the laboratory. Radiospectroscopy rapidly became dominant, taking more than two thirds of the annual plans, which also included the research on radio-astronomy and statistic radiophysics led by Sergei Rytov.[129] Back then, radiospectroscopy involved all the maser related research that would soon form a separate field called quantum radiophysics by the Soviets and quantum electronics by the Americans, a difference in labels that reflected different institutional affiliations as well as different perspectives on the physics underling the working of the device.[130] Rytov's recollections reveals how the rapid growth of that field threatened to eclipse all other research programs of the laboratory, including his own.

> The success achieved in the field of quantum radiophysics brought world fame to the laboratory, rendering stronger influence over the institute as a whole. In an environment of rapid success... it was very easy to slide to the fringes all other activities not reducible to quantum electronics. However, this did not happen, and the laboratory management has paid great attention, in particular, to two other important areas - radioastronomy and statistical radiophysics.[131]

However, that old tradition formed a base from which they attacked the new problems. Prokhorov would later acknowledge that the rich experimental and theoretical knowledge accumulated in the laboratory made possible to deduce the possibility of devising a molecular generator.[132]

[129]A. M. Prokhorov, *Problemnyi Plan za 1957. ARAN, 532-1-283, pp. 22-25*, 1957.
[130]A. M. Prokhorov, N. G. Basov and Barchukov, *Otchet o komandirovke v SSHA na 1iu Mezhdunarodnuiu konferentsiiu po kvantovoi radiofizike. ARAN, 471-5-34. pp. 1–23*, 1959.
[131]Rytov as quoted in Prokhorova (as in n. 18), p. 56.
[132]Ibid., p. 55.

The molecular beam spectroscope presented in January 1953 revels how Basov's project had converged with the work led by Charles Townes at Columbia University. Under the supervision of Charles Townes, in 1952 the PhD Student James P. Gordon and the post-doctoral fellow Herbert J. Zeiger began to devise a generator of short microwave envisaged by Townes the previous year. By 1953 they had made some adjustments along the way that made the device a spectroscope and amplifier to ensure that Gordon would have material to defend his dissertation even if the device did not work as a generator.[133] Basov and Prokhorov, on the other hand, began the project as a molecular beam spectroscope and drifted towards an amplifier and generator, and Basov moved on to turn the molecular beam spectroscope into a molecular generator and amplifier upon defending his dissertation of candidate of science in 1954.

The Soviets probably discovered that the same device they were devising had been recently devised in the United States at Columbia University in mid-1954, after the issue 95 of *Physical Review* arrived at FIAN. A short paper in letters to the editor by Gordon, Zeiger, and Townes described in general terms the working principle of the "molecular microwave oscillator" and its successful operation as a high-resolution microwave spectroscope.[134]

It did not take long for them to submit the "Theory of the Molecular Generator and Molecular Power Amplifier" to the Proceedings of the USSR Academy of Sciences.[135] Based on the theory of oscillations they classified the new device as a self-oscillating system, in many ways similar to a tube oscillator, and following an heuristics of comparison and analogies they formulated a theoretical account of the molecular generator. With an English version of the paper in hands,[136] in early April 1955 Alexander Prokhorov flew to England, where he first met Charles Townes and discussed their approaches to the new device face-to-face. In the following sections we chart a comparison between the early maser papers by the Soviet physicists Basov and Prokhorov, on one hand, and by the American physicists Gordon, Zeiger, Townes on the other hand, inquiring how their distinct scientific traditions influenced their understanding of the maser.

[133] Forman, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14).

[134] J. P. Gordon, H. J. Zeiger and Charles H. Townes, 'Molecular Microwave Oscillator and New Hyperfine Structure in the Microwave Spectrum of NH3', *Phys. Rev.* 95 (1954):282.

[135] N. G. Basov and A. M. Prokhorov, 'Teoriia molekuliarnogo generatora i molekuliarnogo usilitelia moshchnosti', *Doklady Akademii Nauk SSSR*, 101 (1955):1.

[136] Idem, 'Discussions of the Faraday Society, vol. 19, 1955' (as in n. 94)

Maser as "a self-oscillating system"

> We call molecular generator a self-oscillating system that uses the energy associated with molecular transitions between different molecular levels. The circuit of the molecular generator is the cavity... Feedback coupling in the generator is made through the resonator's electromagnetic field which, affecting the dipole moments of the molecules, causes induced radiation of the molecules.[137]

That was how Basov and Prokhorov presented the maser for their audience at the meeting of the All-Union Scientific Society of Radiotechnique and Radiocommunication, held in Moscow in October 1954. Its was about that time that they began to work on a systematic theory of the molecular generator. That brief definition, and the presentation as a whole, reveals the keys for Basov and Prokhorov's understanding of the new device, namely the concept of self-oscillating systems, which was part of the theory of oscillations, and an heuristics of analogies and correspondence with a vacuum-tube oscillator. As some of their collaborators would latter write:

> In the Lebedev Institute's Oscillations Laboratory they were traditionally familiar with the theory of oscillations. Since this theory deals mainly with questions of self-excitation, they were able to find the conditions for self-sustained excitation of a beam-cavity system.[138]

As they understood, the cavity and the electromagnetic field are to a molecular oscillator as the circuit and electric charge are to a vacuum-tube oscillator, respectively. These analogies were important because they allowed to transfer relevant knowledge from the long-familiar vacuum-tube oscillators to an unfamiliar device. In particular, as we discussed in a previous section, the Soviets had the theory of oscillations in which the vacuum-tube was a paradigmatic example. Prokhorov himself revealed the importance of the analogy in his Nobel lecture:

> As is well-known from radio engineering, any system able to amplify can be made to oscillate. For this purpose a feedback coupling is necessary. A theory for ordinary tube oscillators is well developed in the radio range... Therefore the condition of self-excitation for the quantum oscillator [maser or laser] should be written in the similar way as for a

[137] Basov and Prokhorov, 'JETF, No. 4, vol. 27, 1954' (as in n. 118), pp. 127-128.
[138] Karlov, Krokhin and Lukishova (as in n. 80), p. 34.

> tube oscillator. According to the analogy with usual tube oscillators, it is quite natural to expect that for a quantum oscillator the oscillations will also be quite monochromatic.[139]

Thus, when they set to write a theory of the maser, they wrote its nonlinear differential equation based on the equation of vacuum-tube oscillators. The implications of the analogy did not end here. The class of self-oscillating systems and the concept of self-oscillation structured the way Basov and Prokhorov investigated the device. The understanding of the maser as a self-oscillating system dictated, for example, what was to be observed (condition of self-excitation and stationary state), what kind of questions were to be asked and probed (what was the quality factor necessary of self-excitation to happen? What is the amplitude of the stationary state?), and how those questions were to be structured.[140]

However, they also stress the particularities of the molecular generator:

> The molecular oscillator is not entirely analog to other feedback oscillators [...] because stimulated emission is an essentially quantum phenomenon. In contrast with other generators, in the molecular oscillator the oscillating energy is not produced in its circuit, but introduced in the cavity by the molecular beam, whose molecules can each be considered an excited oscillating system, i.e., the molecular oscillator is a system with very many degrees of freedom. Thus, [...] we should follow statistical quantum mechanical method.[141]

They developed thus a semi-classical theory that combined the quantum statistics with the theory of oscillations. Their training in the school of oscillations permitted Basov and Prokhorov to understand the molecular oscillator in analogy with radio

[139] A. M. Prokhorov, 'Nobel Lecture: Quantum electronics', (1964) ⟨URL: `http://www.nobelprize.org/nobel{\_}prizes/physics/laureates/1964/prokhorov-lecture.html`⟩, pp. 112-113. For Alexander Pechenkin, "One can read Mandelstam's typical way of speaking his lecture".Pechenkin, 'Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics, No. 2, vol. 33, 2002' (as in n. 21), p. 291. Mandelstam and his pupils often understood phenomena in other fields, as optics and quantum physics for example, using analogies with phenomena well-known in radiophysics. Idem, *Leonid Isaakovich Mandelstam - Research, Teaching, Life* (as in n. 20).

[140] N. G. Basov, *Molekuliarnyi Generator (Tezisy doklada), 1954*, (Moskva, 1997); Basov and Prokhorov, 'Doklady Akademii Nauk SSSR, No. 1, vol. 101, 1955' (as in n. 135); idem, 'Discussions of the Faraday Society, vol. 19, 1955' (as in n. 94);Gorelik (as in n. 29), pp. 107-109.

[141] Basov and Prokhorov, 'Discussions of the Faraday Society, vol. 19, 1955' (as in n. 94), p. 47.

devices as the vacuum tube of oscillator, while taking in account specific features of the system, as it had to be done to describe wind and bow musical instruments.

The heuristic role of the theory of oscillations can be seen as well in earlier stages of their work. In the first paper where they discussed the possibility of devising a molecular generator, they calculated as an example the minimum quality factor (Q) and the maximum power for a molecular generator based on the transition of cesium fluoride (CsF) which would emit radiation of wavelength 3.7 cm. The conclusion was that the state-of-the-art technology could not produce a cavity with the required Q. However, they claimed, the self-excitation regime with practically achievable cavities could be obtained increasing significantly the density of the molecular beam. With the density of the molecular beam they had obtained, and with easily achievable cavities, the device could be used only as a spectroscope of very high resolution and very low noise based on induced emission.[142] Guided by those calculation, along 1954 Basov and Prokhorov were joined by other physicists to work on technical improvements of the apparatus, specially on improving the quality of the cavity and the focuser used to sort the molecules to increase the density of the molecular beam.[143]

Those moves suggest that the invention of the maser in the Soviet Union might be described as theory-guided. We can identify here a pattern similar to Prokhorov's graduate work, in which, together with Sergei Rytov and Mark Zhabotinskii, he used the theory of oscillations to study the stability of generators and suggested ways to build more stable generators.[144] That was possible only because they had a well-developed theory to deal with radio devices.

The theory of oscillation and its central concept of self-oscillation therefore can be seen as central elements of a paradigm. And in fact historians have called them so.[145] However, as Pechenkin emphasized, Basov and Prokhorov "did not work within the paradigm of self-oscillations".[146] What means that, despite following the general heuristics of the theory of oscillations, they did not commit to one of the tenets

[142] Basov, 'Molekuliarnyi Generator (Tezisy doklada), 1954' (as in n. 140).

[143] N. G. Basov, V. G. Veselago and M. E. Zhabotinskii, 'Uvelichenie dobrotnosti Obiemnogo rezonatora pri pomoshchi regeneratsii (pismo v red.)', *Zh. Eksp. Teor. Fiz.* 28 (1955):2.

[144] Rytov, Prokhorov and Zhabotinskii, 'JETF, No. 613, vol. 15, 1945' (as in n. 63); idem, 'JETF, No. 557, vol. 15, 1945' (as in n. 63); idem, 'O stabilizatsii chastoty lampovykh generatorov (avtoreferat rabot, udostoennykh premii AN SSSR im. L. I. Mandelstama za luchshuiu rabotu v oblasti radio za 1947)' (as in n. 65).

[145] Dalmedico (as in n. 21); Pechenkin, 'Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics, No. 2, vol. 33, 2002' (as in n. 21).

[146] Idem, 'Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics, No. 2, vol. 33, 2002' (as in n. 21), p. 291.

of the “paradigm of self-oscillations”, namely that nonlinear equations should be solved using the rigorous, qualitative methods developed by Andronov based on the works of Lyapunov and Poincaré. Instead, they found an approximate expression to calculate the saturation effect, “the nonlinear character which gives the amplitude of stationary oscillations in the molecular oscillator”.[147]

Their work was conducted in an intersection between an indigenous Soviet scientific tradition, a paradigm, we may say, and another scientific tradition shaped above all by the wartime radar development in the USA, which created the field of microwave spectroscopy.[148] Seeing those scientific traditions as paradigms we are led to the most polemic question of inconmensurability. How different were those traditions and how could the results based on the theory of oscillations be communicated to scientists outside of that tradition? Part of the answer to this question can be found in the heavy usage of analogies and comparisons with tube oscillators, a device with which practitioners of both traditions were familiar. This strategy was used by Prokhorov in his talk at the Faraday Society meeting. He began the presentation introducing the device and its working principle, but made no mention to self-oscillating systems. Instead, he began with analogies:

> A molecular oscillator is the name given to a system using energy connected with transitions among energy levels. A cavity resonator is a circuit of the molecular oscillator. The molecules arriving at the cavity are virtually all in the upper states. Back coupling in the molecular oscillator is made through the resonator’s electromagnetic field...[149]

From here, a common ground on experimental practice could be found based on measurable quantities as spectral linewidth and signal-to-noise ratio. This can be better appreciated comparing the first publications on masers by American and Soviet physicists before and after they discovered about each others’ work.

Maser as “experimental device”

“An experimental device, which can be used as a very high resolution microwave spectrometer, microwave amplifier, or a very stable oscillator, has been built and operated.” That was how Gordon, Zeiger and Townes described the maser in their

[147] Basov and Prokhorov, ‘Discussions of the Faraday Society, vol. 19, 1955’ (as in n. 94).
[148] Forman, ‘"Swords into ploughshares"’ (as in n. 12)
[149] Basov and Prokhorov, ‘Discussions of the Faraday Society, vol. 19, 1955’ (as in n. 94), p. 96.

first publication. Lacking a specific family to fit the maser in, they used the very general class of "experimental device". To specify which kind of experimental device they were dealing with, they had to state its functions. An experimental device needs only experimental parameters to be described. Thus, as classifying the maser as a self-oscillating system guided the questions asked by the Soviets, classifying the maser as an experimental device likewise guided the questions asked by the Americans.[150]

The questions they were concerned with can be found in the Physical Review Letter quoted above. Gordon et al. presented the block diagram of the apparatus, describing its main parts, and explained in general terms its working principle and the conditions in which it would work as oscillator, amplifier, or spectrometer. They then discussed the power, stability of oscillation, noise figure, and resolution (spectral linewidth). All those were measurable quantities. The only calculation presented concerned the spectral line width, which they calculated at 4 kHz, concluding that it was close to the observed value of 6-8 kHz. In addition, they presented experimental results obtained using the device to study the hyperfine structure of ammonia's inversion transitions.[151]

When Basov commented that letter at the All-Union conference on radio technique and radio communication held in October 1954, he pointed out that "no theoretical consideration is given in the letter and the estimate of the line width shows that the authors do not understand well enough the working principle of the molecular generator."[152]

The major difference between the estimates of the spectral linewidth by the Americans and the Soviets lay on the role they attributed to saturation effect, which is responsible for the nonlinearity of the system. According to Basov, the spectral linewidth is determined either by the time of flight of the molecules inside the cavity or by their life-time in the excited state, given by saturation effect. In the stationary regime, when the radiation field inside the cavity is intense, the life-time of the molecules is shorter than the time of flight. In that case, saturation effect determines the spectral linewidth. James Gordon calculated the spectral linewidth taking in account only the time of flight of the molecules. His theoretical value was 4 kHz, while his observed linewidth was 6–8kHz. Taking in account the saturation effect,

[150] Gordon, Zeiger and Townes, 'Phys. Rev., No. 282, vol. 95, 1954' (as in n. 134).
[151] Ibid..
[152] Basov, 'Molekuliarnyi Generator (Tezisy doklada), 1954' (as in n. 140).

namely the nonlinearity, Basov obtained a theoretical prediction of 7kHz, in total agreement with the observed figures.[153]

If the Soviets understood well the role of saturation effect in determining the spectral linewidth of the maser was because they had, and were familiar with, a well-developed theory which accounted for that effect. When saturation effect takes place the system enters the stationary state, namely all its parameters remain constant, and according to the theory of oscillations, "stationary self-oscillations can be described only by nonlinear equations".[154]

Later, after they were acquainted with the theory of the maser published by the Soviets, Gordon, Zeiger and Townes published a detailed linear theory of the maser based on first-order perturbation theory. In the paper they acknowledged that when "the molecular transitions begin to saturate" their equation was no longer sufficient for the calculation of the linewidth, and they promised that "the effects of this saturation will be considered in detail in a later paper".[155]

Theory and experiment in the invention of the maser

By all accounts the invention of the maser in the USA was a process of slow experimental progress with few major adjustments and a good deal of tweaking. Between the "early-morning epiphany" on a park bench in Washington D.C. in the spring of 1951, when Townes took the first notes that would lead to the maser, and the moment Gordon broke into a seminar room announcing that he had finally obtained the long-sought oscillations in April 1954, the concept of the device evolved from a generator of 5 mm wave, to a spectrometer and amplifier of 1.25 cm wave. This evolution was costly, tiresome, and unpredictable. So much so that Townes's superiors at Columbia Radiation Laboratory asked him to halt the project. It had spent too much and the chances of success seemed too little. Backed by his tenure, Townes continued, and in the end the successful operation of the device vindicated his stubbornness.[156] Soon the novel device attracted wide attention and some young physicist were going to Columbia University to learn the "maser art".[157] All this

[153] Gordon, Zeiger and Townes, 'Phys. Rev., No. 282, vol. 95, 1954' (as in n. 134); Basov, 'Molekuliarnyi Generator (Tezisy doklada), 1954' (as in n. 140).

[154] Gorelik (as in n. 29), pp. 121-122.

[155] J. P. Gordon, H. J. Zeiger and Charles H. Townes, 'The maser - new type of microwave amplifier, frequency standard, and spectrometer', *Physical Review*, 99 (1955):4, pp. 1268-1269.

[156] Townes, *How the Laser Happened* (as in n. 14); Hecht (as in n. 19); Forman, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14).

[157] Bromberg, *The Laser in America, 1950-1970* (as in n. 7).

shows that their work was an experimental practice, with a life of its own, to use Ian Hacking's way of saying that it was independent of a theory. They did make some calculations along the way, but those were assessment of specific steps or elements of the apparatus and thus subordinate to experimental practice.

To be more precise we have to distinguish between two theory levels in the invention of the maser. On the first level is Einstein's theory of spontaneous and stimulated emission, both included in the framework of quantum mechanics, that explains how molecules absorb and emit radiation.[158] On this level the invention of the maser might be considered theory guided both in the case of the Soviets and the Americans. The second theory level concerns the explanation of the behavior of the radiation once it leaves the molecules and interacts with the cavity. Here we have a different theory-experiment dynamics on different sides of the Iron Curtain.

While the Americans followed an experimental approach, largely based on know-how span off from the wartime radar work, the Soviets followed a theory-guided approach, starting from a general equation and deducing particular elements from it. What seems to be the major factor behind this difference was the availability of a well-developed theory. Philosopher Ian Hacking, drawing attention to the changing dynamics between theory and experimentation over time, has argued that the existence of a mature theory to guide the community heavily influences the relationship between theory and experiment. The existence of such a theory changes even what scientists of a given period considers to be the scientific method. Scientists trained in contexts in which a well-developed theory is available tend to follow a more deductive approach, while scientists trained in context that such a theory does not exist tend to follow a more inductive approach in which the experimentation, or the experimental art, has a life of its own.

3.5 Conclusion

To understand how masers could be invented in parallel, and independently, in the United States and the Soviet Union we have to let go of two dichotomies often present in studies of science and of Soviet Union, respectively: internalism–externalim and East–West. In particular, the East-West dichotomy often renders unalike similar phenomena in Soviet and American histories. The narrative above shows that a convergence trend between the physics in the Soviet Union and the United States

[158] Einstein (as in n. 10).

during the 1940s and 1950s. This mutual approximation cannot be understood without taking in account the broad social context of the period, namely the World War II and the Cold War, nor the role of nature, of the object of the scientific investigation, in directing the course of research. On a macro level, we have states with two reputedly different political systems, structural organizations of scientific research and communities, and (scientific) ideologies, confronted with the same problem, namely the need to strength their defense capabilities by developing high-tech weapons and defense systems. As we zeroed in on the lives and works of scientists, people living in those nations, affected by, and engaged with, the World War II and the Cold War, all those differences attenuate and distinctions become blurry.

In the scientific practice of Soviet and American scientists involved in the invention of the maser we see a similar response to the challenge posed by the context of war and Cold War. They engaged with the kind of research that permitted them to be simultaneously a good scientist and a good citizen, given the conditions and resources available, putting their skills and knowledge to advance useful research and technology while binging in funding and institutional support to advance their science. Several implications of that surfaced in the narrative above, some of which will be revisited in this conclusion.

The convergence was set by a number of factors present both in the US and the USSR, added to the Soviet policy of catching up and surpassing. The major one was no doubt the engagement of physicists in the military buildup started in the USSR in the second half of the 1930s and in the US during the WWII. That engagement triggered a series of transformation in the practice and training of physicists in those countries that, as Forman and many others have written, drifted the course of physics toward particular fields of interest to the military.

On the one hand, the involvement of US physicists in military research led US physics toward more realistic theories and less discipline-delimited research programs, what made it similar to the style of Mandelstam's school and other leading Soviet schools, which were established before the WWII. The model of science established in the decades that followed the revolution to support the all-out industrialization drive favored the rise of Mandelstam's school, which resonated with the ideal image of Soviet Science. Then "pure" science was frowned upon as bourgeois science, and science was regarded as a central element of, and a tool to better, Soviet society. As a result, coveted scientists were those who engaged in the industrialization drive, eager to translate their knowledge into useful technologies and devices, made no distinction between pure and applied science, and saw no boundaries between disciplines.[159] On the other hand, the catch-up-and-surpass incentive drove Soviet physicists toward problems and fields trending in the United States. This is apparent in Prokhorov's research on microwave spectroscopy which began following research in leading American journals and measured their progress against the research conducted abroad, specially in the US. That strategy was in resonance with the slogan to catch up and to surpass, put forward by Stalin himself, which brought the content of Soviet physics closer to that of American physics.

Differences in the structural organization of Soviet and American science required different strategies to foster research in particular directions. While in the US the military created a panoply of advisory committees and directed research funding to areas of their interest,[160] in the USSR the hierarchical structure of the scientific schools, which seems to reflect the power structure within Soviet Society, proved to be fit to coordinate efforts of several institutions in unified programs led by powerful institutional leaders. As a graduate student Prokhorov participated in large research programs involving members of Mandelstam's school from different institutions and, even as a senior worker of FIAN, followed suggestions from above of research topics that were connected with major programs, including military programs as those of radars and nuclear weapons, being pursued by Soviet physicists. It was so with his research on synchrotron radiation as well as with his research in microwave spectroscopy, both connected to radar R&D, and the latter to nuclear physics as well.

Although the knowledge required to build masers was ripe by 1916, historian Joan Bromberg has argued, the invention of the maser required a combination of knowledge of quantum mechanics and technical skills rare prior to World War II. Prior to that physicists had good knowledge of quantum mechanics but lacked the engineering knowledge and skills, while engineers had the technical skills but lacked

[159] Not by chance those were characteristics of the major schools of Soviet science established well before the WWII. In better position to bargain with politicians, they had more funding, more power, and were less vulnerable to political prosecution, even if their political views diverged from the official ones. See, for instance, the schools of Abraham Joffe in Josephson, *Physics and politics in revolutionary Russia* (as in n. 34); and Vladimir Vernadsky in Bailes, *Science and Russian Culture in an Age of Revolitions: V. I. Vernadsky and his scientific school, 1863–1945* (as in n. 25).

[160] Forman and Sánchez-Ron (as in n. 31), pp. 282-284. Charles Townes conceived the idea that led to the invention of the maser while preparing for a meeting of one of those committees, the Advisory Committee on Millimeter Wave Generation, which he happened to be the chairman. Forman, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14).

knowledge of quantum mechanics necessary to make a maser. Only after the war, when there appeared scientists like Charles Townes, a physicist with good knowledge of engineering acquired during the war, and Joseph Weber, an engineer with solid knowledge of quantum mechanics who was driven to physics by his wartime work; only then the maser could be conceived.[161] This narrative suggests that a combination of knowledge of quantum mechanics and engineering skills was a necessary but not sufficient condition for conceiving the maser. Well before the Wold War II that combination was common and appreciated among leading schools of Soviet physics such as Mandelstam's and Joffe's schools. A complex web of factors, which can be seen as causal agents, that existed both in the United States and the Soviet Union led Prokhorov and Basov towards the maser. Among those are many factors that came into play only in the post-World War II discussed here. This may be a key to explain why Valentin Fabrikant, being the first to conceive the idea of using population inversion to generate radiation, did not pursue the idea through.[162]

When Alexander Prokhorov and Charles Townes established direct exchange they had a very different conception of the maser. Townes seemed to have trouble understanding the saturation effect which Basov and Prokhorov stressed to be so important. Basov and Prokhorov had trouble understanding how Townes and his group could continue insisting on a linear theory of a device that for them, the Soviets, was inherently nonlinear. But they had common grounds. Those were the physics of spontaneous emission, the knowledge of radio-devices such as tube oscillators, and the experimental knowledge. Thus this episode supports the claim by philosophers collectively known as New Experimentalists that experimental knowledge serves as a common ground for scientists working within different paradigm or disciplinary matrix, remaining robust through scientific revolutions.

The main promoter of that experiment-based approach to philosophy of physics, Ian Hacking pictured the relationship between theory and experiment in the history of science is a diverse one:

> Some profound experimental work is generated by theory. Some great theories spring from pre-theoretical experiment. Some theories languish for lack of mesh with the real world, while some experimental phenomena

[161] Bromberg, *The Laser in America, 1950-1970* (as in n. 7).

[162] Fabrikant's failure to purse his idea through is still in need for a careful analysis. The most detailed article in English language touches the question superficially. Lukishova (as in n. 11).

> sit idle for lack of theory. There are also happy families, in which theory and experiment coming from different directions meet.[163]

To illustrate this last point Hacking gives the example of the meeting between the theoretical prediction of the background radiation by a group of theoreticians working on Big-Bang cosmology and its serendipitous detection by Arnold Penzias and Robert Wilson using a maser-based radiotelescope. What the history of the maser presented above shows is that in the early Cold War, when Soviet physicists began resuming international scientific exchanges, happy meetings could happen not only between theory and experiments coming from different quarters of science, but also within the same field, with theory and experiment coming from different geopolitical camps.

To conclude, we bring up a hypothetical situation recently debated by philosophers of science. Imagine two isolated community of physicists, starting from the same initial conditions, left to ask their own question, unguided by the work of other scientists. How similar would be the physics developed by those communities? This hypothetical situation, which came up in the Contingency/Inevitability debate sprang out of Ian Hacking's *Social Construction of What?*[164], arguably took place in the 20th century. Having had their training in Germany before the Russian Revolution, Mandelstam and Papaleksi found themselves isolated from their foreign peers, for a long while without access to foreign journals, and in those conditions developed old and new research programs, training young scientists along the way. The ensuing scientific community exhibited a few peculiarities and even created a theory of their own. How different did they become? How equivalent was the nonlinear theory of oscillations to other approaches developed elsewhere? This may be a good opportunity to address the need for clarification of the concept of contingency

[163] Ian Hacking, *Representing and Intervening: Introductory topics in philosophy of science*, (Cambridge University Press, 1983), p. 160.

[164] Idem, *The Social Construction of What?* (as in n. 6). This hypothetical situation was presented by Léna Soler to clarify the issue in the contrast contingentism versus inevitabilism. It is closely related to the argument of an Alien science used by physicists in defense of the inevitability of physical laws that is basically a claim that had intelligent alien developed their own physics it would inevitably be equivalent to our physics. This argument and its shortcomings are discussed by Hacking (1999, pp. 74—76). What Soler does is to reformulate it to clarify what contingency means. "Any nontrivial contingency, Soler contends, requires that two isolated scientific communities starting from the same point produce 'irreducibly different' results, while still satisfying a reasonable set of criteria for success." Joseph D. Martin, 'Is the Contingentist/Inevitabilist Debate a Matter of Degrees?' *Philosophy of Science*, 80 (2013):5, p. 923.

exposed by Joseph Martin and to test some claims on contingency and inevitability in science.[165]

[165] Martin (as in n. 164).

4 Exploiting the Iron Curtain: Lasers, Scientific Internationalism and Discipline Building in Early Cold War

When the young Soviet physicist Alexander Prokhorov and his first two graduate students Nikolai Basov and Alexander Barchukov ventured themselves in the new, foreign-born field of microwave radiospectroscopy in the first years of the 1950s, they could hardly imagine that that venture would be their passport into capitalist countries, let alone that in 1959 they would be posing for pictures at the international airport of New York City with a KLM airplane on the background (see Figure 4.1). Back in 1952, when they officially began working in radiospectroscopy, Soviets physicists had been working in isolation from their foreign peers for many years and the Cold War was going through one of its hottest periods. The Soviet Union and the United States were engaged in the first indirect confrontation since the USSR began to build its atomic-bomb arsenal –the Korean War – and physicists in both countries were busy working on the development of "ever more advanced military technology."[1] Apropos, one of the main tasks of Prokhorov and his students in the new field was to make highly precise measurements of nuclear moments necessary to develop the models of nuclei being employed on the making of the first hydrogen bomb[2]. In that context, talks of academic exchange with American physicists would sound like wishful thinking.

In 1959, however, although physicists were still working on ever more advanced military weapons, the setting was quite different, as far as international relations

[1] Forman, 'Studies in the Physical and Biological Sciences, No. 1, vol. 18, 1987' (as in n. 20), p. 150

[2] See Climério Paulo Silva Neto and Alexei Kojevnikov, 'Convergence of Cold War Science: Co-inventing the maser in the Soviet Union', *(forthcoming)*.

Figure 4.1: A. M. Prokhorov, N. G. Basov and A. I. Barchukov. USA, 1959. Courtesy of Alexander K. Prokhorov.

are concerned. Prokhorov was on his third foreign trip, Basov on his second.[3] Their laboratory at the Lebedev Institute of Physics (FIAN) had been exchanging papers with physicists from "capitalist countries" at least since 1955, and delegations of Soviet scientists in international conferences was becoming routine.

The 1950s were years of broad changes in Soviet Society, which reflected plainly on Soviet science, often more than in other segments of society. Stalin's death in 1953 left the new leadership with the task of redefining the country's political strategies. Recent scholarship have depicted the new leadership as willing to change, but uncertain of what and to what extent, and attentive to signs "from bellow" to redefine official policies.[4] Not very far bellow, with their recently acquired celebrity status,

[3]In the late 1950s Prokhorov took part in the Discussions of the Faraday Society in England (1955) and in an international conference of spectroscopy in France (1958).Shcherbakov, Mikhailova and Prokhorov (as in n. 84). Basov went to Japan in 1957 as a delegate for the USSR in a meeting of the World Peace Council, an organization created by initiative of the Communist Party of the USSR to promote disarmament and campaign against imperialism.Y. M. Popov and V. B. Rozanov, *Biobibliografiia Uchenykh: Nikolai Gennadievich Basov*, (Moscow, 1982).

[4]William Taubman, 'The Khrushchev period, 1953–1964', in: Ronald Grigor Suny, editor, *The Cambridge History of Russia*, (Cambridge: Cambridge University Press, 2006). – chapter 10; Jones, Polly, editor, *The Dilemmas of De-Stalinization*, (London: Routledge,

heightened by the successful test of the first prototype of a hydrogen bomb (1953), scientists had their own agenda for change. Some of them understood that they were facing the opportune moment and the right action would lead to a major reform in Soviet science. In his well-documented study, *Science after Stalin*, Konstatin Ivanov draws on a wealth of archival material from the Central Committee of the Communist Party and the Russian Academy of Science to reveal the terms of the negotiation that changed not only image of Soviet science, but the very terms of the relationship between science and polity in the Soviet Union. Internationalization and more freedom to work on fundamental science rather than applied science were some of the top priorities of scientists. Ivanov highlights that the scientists who succeeded in promoting the reforms did so not by opposing the regime, but playing as insiders who had mastered the rules of Soviet polity[5].

It was thanks to those changes that Prokhorov, Basov and Barchukov could travel to the United States in 1959 to take part in the conference "Quantum Electronics - Resonance Phenomena" organized by the Columbia University physicist Charles Townes. That international conference marked the creation of a scientific field called *quantum electronics* by the Americans and *quantum radiophysics* by the Soviets.[6] Those labels were created to group various research project sprang in the wake of the invention of the maser, simultaneously in the United States, by Charles Townes, James Gordon, and Hebert Zeiger, and in the Soviet Union, by Nikolai Basov and Alexander Prokhorov, circa 1954. The maser used quantized molecular or atomic transitions for practical purposes as frequency standardization, generation, and amplification of microwave radiation. This made it a hot topic in the Cold War. But the big topic of the conference, the one which filled the air with excitement, was the possibility of exploiting quantized transitions to generate and amplify light, that is, of making a laser.[7] The conference "Quantum Electronic - Resonance Phenomena" is thus considered by many as the landmark of the birth of laser physics. Its participants, many of whom later became leaders in laser physics, went home with the feeling that the laser was imminent.[8] The participation of the Soviet delegation

2006).

[5]Ivanov (as in n. 41). A similar case is made in Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71), pp. 291-300.

[6]The different labels reflect the distinctive roots of the field in those countries. See Silva Neto and Kojevnikov (as in n. 2).

[7]In the period discussed here the Soviets called their devices molecular generator and amplifier. In the end they adopted the simple and elegant acronyms MASER and LASER that I will use along the text, aware that my usage of the term is anachronistic.

[8]Hecht (as in n. 19).

and of delegations from seven more countries suggests that laser physics was born international.[9]

Elsewhere I have discussed some of the factors that facilitated, or set the basis for, the internationalization of laser physics that were set in motion much before the first lasers were launched. They were the convergence on topics, questions and goals driven above all by the integration of physics with military research, and experiments that formed the common ground in the talk between scientists who spoke not only different mother languages, but distinct theoretical languages was well.[10] In this article I turn to the reestablishment of international connections between Soviet and American scientists in the second half of the 1950s, the beginning of a competition which, in the words of Zhores Alferov, a pioneer of Soviet laser physics, "was a at that time a rare example of an open and friendly competition between laboratories belonging to the antagonistic Great Powers".[11]

When applied to laser physics as a whole, Alferov's statement captures only partially the terms of the competition, though. For it was indeed friendly, but only partially open. In June 2002, few months after the death of Alexander Prokhorov, Charles Townes expressed his "admiration for Aleksander [sic.] Prokhorov as a great scientist and enjoyable personality", describing their exchange in the following terms:

> "I first met Sasha[12] in 1955 at a scientific meeting in England, and immediately enjoyed his company and talking with him about science. I had already read some of his publication on microwave spectroscopy, but was fascinated to learn at that time of his work towards a maser. His research and my own were quite closely interactive for sometime as he continued to enrich science with important contributions to masers, lasers and quantum electronics. I also enjoyed an early visit to Russia on his invitation, visiting his dacha and collecting mushrooms..."[13]

[9] In her classic history of the Laser in the United States Joan Bromberg acknowledges that "even the historian who looks at the American work alone sees continually the impact of advances made in Europe and the Soviet Union in the 1950s and 1960s. Clearly we need histories of the maser and laser in other contexts." Bromberg, *The Laser in America, 1950-1970* (as in n. 7), pp. p. XII.

[10] Silva Neto and Kojevnikov (as in n. 2)

[11] Alferov, 'Zhores I. Alferov - Biographical' (as in n. 65).

[12] Informal, shortened form of Alexander. In Russia, among adults, used only to address family members and close friends.

[13] Letter read in the International Conference on Quantum Electronics in Moscow, 2002. Reprinted in Shcherbakov, Mikhailova and Prokhorov (as in n. 84), pp. 480–481.

Shortly after his first visit to Moscow, Townes paid back the courtesy during Prokhorov's first visit to the USA, at his invitation. Here is a short account by Prokhorov's wife:

> The relationship [with Townes and his wife] up to now is very good. Townes visited our home and our dacha. In turn, during my husband's trip to America - it was, by the way, his second working trip abroad - Charles invited Alexander Mikhailovich to his farm on the outskirts of New York. It had full hundred hectares. American professors received much money for their big scientific results. But, as it is usual in the States, he used any possibility of receiving money from alternative sources." [14]

On the other hand, Townes and Prokhorov were leading figures of the scientific-military-industrial complexes of their respective nations. By 1955 Townes was already one of the key links between the scientists and military agencies, he would soon become the vice-president of the Institute for Defense Analysis, a think tank to advise the Pentagon on scientific and technical issues related to national security[15]. Prokhorov, in turn, soon discovered new "possibilities of receiving money from alternative sources" as well. By his own account, in his laser research he discovered a "general effect" in physics: "military generals are very interested in physics".[16]

Thus, although they developed close collaboration and might have felt themselves as genuinely part of an international community working on cutting-edge research, they also felt committed to their own homelands and ideologies and not only sought to apply the fruits of their research to develop civil and military technology, but used that collaboration to advance interest of their countries. As we will see in the following pages, this tension, which would be characteristic of laser physics throughout the Cold War, took shape still in the 1950s, when the first lasers as well as the Soviet laser physics was still on the making.

[14] Prokhorova (as in n. 18), p. 67. The recollections are dated 1992. Note that Galina Prokhorova wrote that Prokhorov's trip to the USA was his second international trip. However, the same book in which her recollections are published lists a trip to France in 1958 as he second foreign trip. Shcherbakov, Mikhailova and Prokhorov (as in n. 84), p. 13.

[15] Interview with Charles H. Townes by Joan Bromberg on 28 January 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA. Available at: https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4917-1

[16] Loren R. Graham, *Lonely Ideas: Can Russia Compete?* ebook edition. (Cambridge: MIT Press, 2013), p. 83. Prokhorov reveled his "discovery" in an interview with Loren Graham in the 1980s.

This paper is about the process of internationalization of Soviet science began in mid 1950s, after Stalin's death. It spans roughly from 1955, the year Alexander Prokhorov participated in his first international conference, to the launching of the first Soviet lasers in 1961. The remaining of this introduction is dedicated to discuss general changes on the international policy of Soviet Union after Stalin's death and how Soviet scientists were perceived by their Western peers in the same period. The section 4.1, *The revealing encounter,* focuses on Prokhorov's first international trip. The documentary records of that trip, which began to be discussed in October 1954, reveals that the early stage of internationalization was a careful one. The Academy of Sciences was willing to foster international exchanges, but was still cautious about its extent. Even loyal scientists as Prokhorov, with impeccable records for Soviet standards, were accompanied by "reliable comrades" (*nadezhnyi tovarishch*), KGB handlers sent to make sure that scientists would act according to guidelines. What information about the talks to reveal and when to reveal was also a carefully discussed issue. The subsequent section 4.2, *Not inferior, but smaller,* discusses how Prokhorov's first international experience was reassuring for his small group and stimulated them to scale up their research agenda. In it I discuss the maser research and development carried by groups led by Basov and Prokhorov, comparing with quasi-parallel works in the US, in the second half of the 1950s.

The section *First time in America* (4.3), dedicated to the conference "Quantum Electronics - Resonance Phenomena" held in the US, revels a that the end of the 1950s was a quite different moment for Soviet international scientific exchanges. As the changes in international policy were consolidated, the bureaucracy decreased and scientists gained more autonomy. The reliable comrades were no longer an obligatory figure in delegations of Soviet scientists. Even scientists of questionable loyalty were allowed to travel abroad, notwithstanding being under the watch of other scientists. In the early 1960s Prokhorov traveled to the US with a delegation composed exclusively by his close collaborators. The 22-page report of the trip written by the delegation tells much about the terms of the cooperation/competition established by then between American and Soviet Physicists. The Section 4.4 shows how Prokhorov and Basov exploited that context of competition with the United States to promote their agenda and to create one of the most successful branches of Soviet physics. Finally, I conclude with a discussion of what this early international exchange in laser science across the Iron Curtain reveals about the uses of scientific internationalism and the roles played by scientists in the early days of Cold War.

Beyond Eastern Europe

Among the major demands of Soviet scientists, internationalization was the easiest matter to settle. On this point the interests of scientists and politicians overlapped. Stalin's postwar foreign policy had been mostly concerned with creating a protective belt of friendly nations in Eastern Europe to protect Russia from a hostile, supposedly unified, West. Shortly after his death, the Kremlin, soon controlled by Nikita Khrushchev, acknowledged that "the West" was not an unified hostile camp and extended its foreign relations beyond the communist block. According to William Taubman, author of *Khrushchev: The Man and his Era (2003)*, "the centerpiece of Khrushchev's diplomacy was a campaign for what a later era would label détente".[17] In that context, the internationalization of Soviet science as not only a way of rewarding, and compromising with, scientists, but also a diplomatic channel to promote a pacific co-existence. Moreover, it was easier and safer to begin an international opening with an organized and already privileged group of Soviet Society.

In the period which became known as *The Thaw*, Nikita Khrushchev took a series of actions to signal to the world that Soviet Union was heading on a path different of that settled by Stalin. The landmarks of changes in foreign policies were drawn in the second half of the decade. In 1957, during the World Youth Festival, more than thirty thousand foreigners where received in Moscow in a peaceful and open atmosphere. The festival is regarded as a mark of the opening of the Soviet Union to the outside world, a redefinition of relationship between Soviet people and foreigners, and part of the revival of Soviet international cultural relations that climaxed in the signing of the "Lacy-Zarubin Agreement" on cultural exchange between the United States and the Soviet Union at the end of 1957.[18]

For science, some historians have argued, the Lacy-Zarubin agreement officially marked the beginning of scientific cooperation between the Soviet Union and the United States. [19] However, as we will see in this chapter, the actual exchange, began even before. In 1954 the Soviet leadership started lifting restrictions on scientists' international contacts and conference travel, and allowed Soviet scientists' nominations for Nobel prizes.[20] For physics 1955 was a landmark. That was the

[17] Taubman (as in n. 4), p. 285

[18] In a study about the festival Pia Koivunen argues that it aimed as well to amend the damaged in the international image of the Soviet Union caused by the impact of Khrushchev's Secret Speech, in which he denounced the cult of personality, and the subsequent crack down on Hungarian protests. Koivunen (as in n. 15).

[19] Graham (as in n. 8)

[20] Ivanov (as in n. 41)

year of the first International Conference on Peaceful Uses of Atomic Energy, held in Geneva, which counted with a large delegation of Soviet nuclear physicists. Many American and Soviet physicists met face-to-face for the first time and compared their impressions with the rumors and stereotypes they had brought from their homelands.

The Image of Soviet Science abroad in the 1950s

International conferences circa 1955 were settings of various first encounters between American and Soviet physicists, opportunities to get a first hand impression of scientists they had hear of, if they had heard at all, only in journal publications. An interesting account of impressions of American physicists after meeting their Soviet colleagues for the first time was given by John Krige in his analysis of the Geneva conference on peaceful uses of atomic energy, which happened in August 1955, four months after the meeting of the Faraday Society, in which Charles Townes first met Alexander Prokhorov: "American scientists", wrote Krige, "arrived in Geneva with a view of their Soviet colleagues as trapped in a closed, backward Communist society that had little respect for science. They went home chastened."[21] The US delegation was surprised by "the highly technical competence of Russian scientists and engineers generally, and the large numbers of students in training in universities and technical schools".[22] When they heard of the Soviet achievements in high-energy physics, they "were not just stunned, but panicked".[23]

To be sure, in 1955 Americans were not totally in the darkness about Soviet achievements in science and technology. They knew that the Soviets had made their own nuclear and thermonuclear weapons, and months before the conference, in November 1954, a front page of *The New York Times* alarmed that "Russia was Overtaking the U.S. in Training Technicians". The article covered the subject of a recent publication in Science magazine by the Soviet emigre Nicholas DeWitt on "Professional and Scientific Personnel in the USSR".[24] The Times article claimed

[21] John Krige, 'Atoms for peace, scientific internationalism, and scientific intelligence', *Osiris* (2006), p. 180.

[22] Quoted in Ibid., pp. 178

[23] Ibid., pp. 179

[24] Nicholas DeWitt, 'Professional and Scientific Personnel in the U.S.S.R', *Science*, (1954). The following year DeWitt's comprehensive book *Soviet Professional Manpower: Its Education, Training and Supply* (1955) was widely discussed in U.S. press. Working at Harvard's Russian Research Center, Dewitt was sponsored by the CIA, which since the early 1950s was very interested in matters of scientific training in Russia. David Kaiser, 'The physics of spin: Sputnik Politics and American physicists in the 1950s', *Social Research: An International*

that the Soviet Union was training "two to three times" more scientists and engineers than the United States. Few days later, president Dwight Eisenhower was demanded to comment at a press conference how the U.S. would overcome the "manpower gap".[25] Moreover, in 1954, when nearly 700 members of the American Institute of Physics, selected randomly, were asked whether it was worth translating major Soviet physics journals to English a large majority of them answered affirmatively, 76% answered so "because the of the technical value of the research now in progress in the USSR", and 71% "because of the national danger of underestimating the strength on the USSR, particularly as far as scientific advances are concerned".[26]

Thus, when Townes arrived in Cambridge, or when the Americans arrived in Geneva, they were already aware of the value of the scientific research and the impressive numbers of scientists and engineers being trained in the Soviet Union. This explains why the issue of training came up in the talks between the Americans and the Soviets during a conference on atomic energy. Those meetings were mostly confirmations of rumors circulating in American press and of the impression US physicists could make of their Soviet peers from journal publications.[27]

What really made American scientists and politicians panic was the launching of the Sputnik in 1957. For most people Sputnik became an object of fascination. It kindled dreams and astonished students, teachers, parents, and non-parents alike, across the globe. It became a symbol of Soviet cultural heritage. But for cold warriors (all those scientists, politicians, journalists, and military involved in the arms race), the beeping little ball was a nightmare. A rocket capable of sending a satellite to space could likewise deliver a nuclear warhead in the North-American continent.[28] Besides making science bigger, more democratic and diverse,[29], the Sputnik boosted the internationalization of Soviet science in this early stage of the Cold War, and reinforced the ideology of scientific internationalism. First, it made

Quarterly, (2006):73. For the creation of the Russian Research Center see Chapter 2, section 2.1.

[25] In the subsequent years the "two to three times" figure would be echoed endlessly by scientists, journalists, and politicians claiming for more government funding for scientific research and science education at almost every level of the educational system. For a critical discussion of the repercussion and interpretation of the manpower-gap scare see Kaiser (as in n. 24).

[26] Quoted in Ibid., p. 1241.

[27] The American Institute of Physics began translating Soviet journals in 1955. Another evidence of the high-regard American physicists had for Soviet physics in the 1950s is that by the end of the decade fully half of the physics departments with graduate programs in the US accepted Russian, alongside German and French, for language exams. Ibid..

[28] Siddiqi (as in n. 7); Kojevnikov, 'The Cultural Spaces of the Soviet Cosmos' (as in n. 7)

[29] Idem, 'Russian science: the little ball made science bigger.' *Nature*, 449 (2007):4 October, ISSN 0028–0836

the Soviet leadership realize the extant to which the scientific achievements could contribute to national prestige. The Sputnik elevated the international image of the Soviet Union from the status of backward superpower to that of technologically advanced nation which matched the United States not only in military power, but in science and technology as well. It made governments in both sides of the Iron Curtain adherents of the classical conception present in the ideology of scientific internationalism – propagandized by the scientists themselves since at least the 18th century – that the contribution of science to national prestige abroad is an automatic and inevitable byproduct of scientific achievement.[30] This made the space race a major front in the "battle for hearts and minds" of peoples across the globe, specially in third-world nations going through decolonization and battles for social justice. With the Sputnik, as it appeared to the outside world, Soviet socialism was launched as a viable alternative to capitalism. [31]

Second, making the USSR appear as a scientific and technologically advanced nation, the Sputnik made the United States more inclined to increase the scientific and cultural exchange between the two countries. Prior to the Sputnik, the respect for Russian science among US physicists was not widely shared by other sectors of US society. For instance, in a distasteful statement, General John B. Medaris remembered that until the Soviets launched Sputnik, "it was fashionable to think of the Russians as 'retarded folk who depended mainly on a few captured German scientists for their achievements, if any. And since the cream of the German planners had surrendered to the Americans, so the argument ran, there was nothing to worry about.'"[32] Perhaps not by chance, the first cultural agreement on cultural exchange mentioned above, was signed and effected a couple of months after the launching of the Sputnik. The result was a significant increase in the timid exchange which had began three years earlier.

The way Soviet Scientists were regarded by foreign peers after their first meetings and all the civilian and military scientific achievements obtained by Soviet scientists in the 1950s were reassuring for them. It was in this conjuncture that an intensification of personal and scientific exchange between Prokhorov, Basov and American

[30] Forman, 'Isis, No. 2, vol. 64, 1973' (as in n. 35)

[31] According to scholars of the Soviet-American cultural exchange, one of the major Soviet objectives in the exchange was to project to the outside world the image the the Soviet Union was the equal of the United States by engaging the United States in bilateral agreements and to demonstrate the achievements of Soviet people. Yale Richmond, *Cultural Exchange and the Cold War: Raising the Iron Curtain*, (University Park: Pennsylvania State University Press, 2003).

[32] Krige (as in n. 21), p. 178

physicists took place in mutual visits and scientific conferences. It was in this conjuncture that the tension between cooperation and competition typical of science explicitly merged with the Cold War tension.

4.1 The revealing encounter

On November 22, 1954. The foreign department of the Academy of Sciences forwarded to FIAN an invitation for Soviet scientists to participate in the discussions on "Microwave and radio-frequency spectroscopy" organized by the Faraday Society, to be held at Cambridge University from April 4 to 6, 1955 [33]. The goal was to gather scientists working in the field, experimentalists as well as theoreticians, to assess the new discoveries and works done in the last years. Leading scientists would open the discussions with introductory papers accounting the current state of given topics divided in four sessions: microwaves, nuclear magnetic resonance, paramagnetic resonance, and quadrupole spectroscopy. Proposals had to be sent as soon as possible to be included in the conference program, and a complete paper had to be submitted not later than January 15,1955 [34].

Subsequent correspondence concerning the preparations for the trip reveals Soviet authorities still cautious about the scientific exchange in microwave and radio-frequency spectroscopy. In November of 1954 the director of FIAN drafted a letter to the foreign department of the Academy of Sciences acknowledging the importance of the conference and recommending Alexander Prokhorov for the session on microwaves, and K. V. Vladimirsky for the session on nuclear magnetic resonance. The draft ended with the title of Prokhorov's presentation - "Theory of the molecular generator" [35]. However, in the letter actually sent to the foreign department the title of the presentation had been replaced by a short paragraph: "It's advisable to specify the subjects of their presentations only after all the questions concerning their trips are solved [36]." The next communication concerning Prokhorov's trip was in March 31, 1955, four days before the conference, with the paper to be presented. Hence, his presentation was not included in the program of the conference [37].

[33] L. M. Brekhovskikh and E. A. Koridalin, *to V. L. Levshin, Nov 22, 1954. ARAN, 532-1-241, p. 59*, 1954.

[34] F. Tompkins, *to M. M. Dubinin, Oct 29, 1954. ARAN, 532-1-241. pp. 60-61*, (Moscow, 1954).

[35] N. Dobrotin, *to L. M. Brekhovskikh (unsent draft), Nov, 1954. ARAN, 532-1-241. p.62*, (Moscow, 1954)

[36] Idem, *to Brekhovskikh (in Russian), Dec 2, 1954. ARAN, 532-1-241. p.62*, (Moscow, 1954)

[37] Idem, *to the Foreign Department of the Academy of Sciences, Mar 31, 1955. ARAN, 532-1-256. p.28*, 1955

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After so much time had passed since a Soviet delegation had taken part in a conference in England, that visit was viewed as an important diplomatic affair. Prokhorov was received by the workers of the Soviet embassy who were especially assigned to look after the scientific delegation during their stay in England. For some unknown reason, Vladimirsky, the physicist suggested in the first answer to the foreign department, was replaced by D. M. Kozyrev, a physicist from Kazan working on electron paramagnetic resonance. Besides Prokhorov and Kozyrev, the delegation was composed by more two "reliable comrades" assigned to watch over the scientists, a common practice in that period of gradual reestablishment of ties with foreign scientists [38].

"The presentation of Alexander Mikhailovich [Prokhorov] on the theory of the molecular generator caused a real sensation", wrote his wife in her recollections [39]. We have other reasons to believe so. After almost half century had passed, Charles Townes supposedly remembered his reaction and words after the "revealing presentation":

> What happened in Cambridge was an eye opener... When their turn came, they gave a discussion, to my amazement, on how an ammonia maser might work (though of course they did not call it a maser at that time). Their discussion was all theoretical, but they expected the device to work soon. After their presentation, I got up and said, "Well, that is very interesting, and we have one of these working [40]."

Given the importance scientists usually attribute to priority contends (many of the accounts written by scientists on history of lasers revolve around priority issues), it would probably be enough for the presentation and its follow-up to stir the audience. However, in this early stage of the Cold War, the aura of mystery around the work of Soviet physicists, intensified by the absence of the titles of their presentations in the program, certainly made things more exciting. It is likely that most of the audience was for the first time face-to-face with a Soviet physicist. Since the Soviet Union began closing itself in preparation for the upcoming war in mid1930s, the contacts between Soviet physicists and their peers abroad was mostly limited to journal publications.

[38]Prokhorova (as in n. 18)
[39]Ibid., pp. p. 66
[40]Townes, *How the Laser Happened* (as in n. 14), p. 77

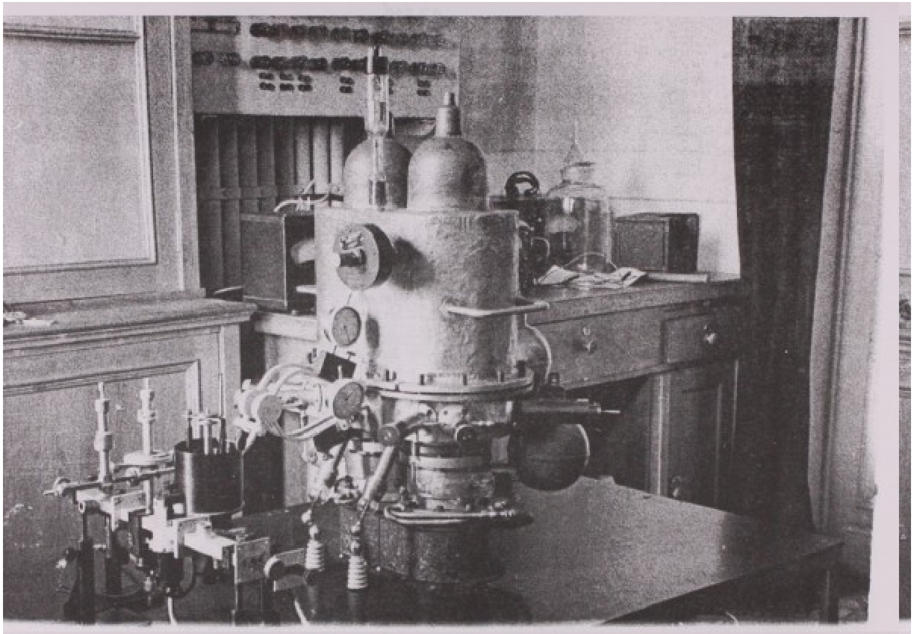

Figure 4.2: The first Soviet molecular-beam generator and amplifier launched at FIAN in September 1955.

Townes and Prokhorov, naturally, were eager to know more details about each other's works. After the conference they walked through the streets of Cambridge and compared their versions of the molecular generator. In Townes's account of the conversation:

> They [Prokhorov and Basov] had most of the same essentials, including a molecular beam and a resonant cavity. One thing that they had apparently missed was the quadrupole focuser, the scheme that I had picked up in 1951 from the German physicist Paul. Paul had published it in 1951, and so had we in 1954. Thus there was no compelling reason that the Russians would not have thought to use it, but they apparently had not. I told them everything I was doing, including the importance of the quadrupole focuser... Very soon, they had an ammonia maser working and even improved on the focuser, by putting in more than four poles. A number of later masers were to use their type of focuser, with many poles [41].

Basov and Prokhorov did consider using a quadrupole focuser, at least by October 1954. However, they opted for an original path. In the meeting of the All-Union

[41] Townes, *How the Laser Happened* (as in n. 14), pp. p. 77

Scientific Society of Radiotechnique and Radiocommunication Basov gave a talk where discussed the letter published by Gordon, Zeiger and Townes in Physical Review announcing the maser and made a detailed analysis of the quadrupole focuser [42]. Instead of using the same focuser, they opted for building their own version to obtain a molecular beam of higher intensity. When they finally launched the device in September 1955, they had devised a multichannel source of molecular beams and a more effective focuser that yielded a beam approximately 30 times higher than that of Townes's maser [43]. If there was an indirect competition for who would produce the first device, they had lost it anyway. However, besides having formulated the theory of the device, they could still put forward an improved version with their own design - which ended up being more powerful and compact.

Besides the generator using molecular beams, discussed in the previous chapter, they also had been developing an entirely new type of molecular generator, which would later become know as three-level maser. This new method for obtaining active molecules consisted of exciting the molecules by pre-irradiating them with a high-frequency auxiliary radiation, instead of separating according to their energy state, as occur with the molecular beam (as described in Figure 4.3).

They had come up with the scheme around the time James Gordon succeeded in obtaining weak amplification with their first maser. After they heard the news that a maser had been operating in the US, they rushed to write a two-page paper and submitted it to the Soviet Journal of Electronics and Theoretical Physics (Soviet *JETP)* on November 1, 1954, describing the three-level scheme and how the required energy-level distribution could be obtained [44]. This is the first of a series of short papers describing ideas that would be matured in later publications, what suggests that the announcement of the maser published by Gordon, Zeiger and Townes's in Physical Review Letters[45] was the "eye opener" for Basov and Prokhorov. As if once they discovered that the Americans were developing the same device, not to be scooped, they began publishing their ideas as soon as possible, even if the ideas were not well developed yet.

[42] Basov, 'Molekuliarnyi Generator (Tezisy doklada), 1954' (as in n. 140)

[43] Idem, *Molekuliarnyi Generator*, Avtoreferat dissertatsii (Fizicheskii institut imeni P. N. Lebedeva, 1956); idem, 'Interview with Dr. N. G. Basov by A. Guenther, on 14th September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.' (as in n. 4)

[44] N. G. Basov and A. M. Prokhorov, 'O vozmozhnykh metodakh polucheniia aktivnykh molekul', *Zh. Eksp. Teor. Fiz.* 28 (1955):2

[45] Gordon, Zeiger and Townes, 'Phys. Rev., No. 282, vol. 95, 1954' (as in n. 134)

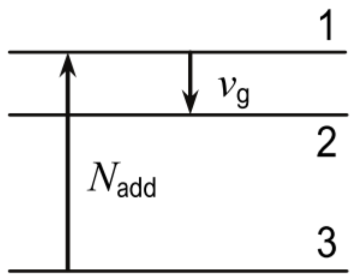

Figure 4.3: Three level maser. According to their scheme of the three-level system required transitions between three energy levels, two with a small energy gap separating them, and a third more spaced from the other two. The auxiliary high-frequency radiation (N_{add}) induces transition from level 3 to 1 (a process called radiation pumping), and after a relaxation time the molecules decay from 1 to 2, emitting a photon of a lower frequency ν_g (spontaneous emission).The transition from 2 to 3 is usually radiationless, with the energy being transferred to vibrational motion (heat) of the host material surrounding the atoms, without the generation of a photon. Reprinted in (Idem, 'Sov. Phys.-JETP., No. 3, vol. 1, 1955').

Time would proof they were right about the risk of being scooped by the Americans. As the news about the maser and its features spread, physicists in universities, research institutes, and research companies from the East to the West coast of the United States began to develop their masers. A few months after Basov and Prokhorov proposed the three-level scheme for obtaining population inversion, Harvard University physicist Nicolaas Bloembergen, an specialist in solid-state physics, got interested in masers and soon published his own version of a three-level maser. In a detailed analysis, Bloembergen showed that a three-level spin-system, pumped by electromagnetic radiation, was an effective method to create solid-state masers.[46]

Only publishing the new ideas in Soviet journals was not enough to receive full credit. They needed to publicize it beyond Soviet borders as well. Prokhorov very likely described the three-level scheme to Townes in their conversation in Cambridge, and upon returning to the Soviet Union he requested official permission to mail the theory of the molecular generator and the proposal of the three-level maser to Townes in the United States.[47] This deliberate effort to establish the priority over the proposal of the three level maser payed off, but Basov and Prokhorov had to share the credits with Bloembergen. While acknowledging that "Basov and

[46] Nicolaas Bloembergen, 'Proposal for a new type solid state maser', *Physical Review*, 104 (1956)

[47] B. M. Vul, *to the library of the Academy of Sciences, Jun 24, 1955. ARAN, 532-1-256. p. 49.* (Moscow, 1955)

Prokhorov proposed a maser involving three energy levels instead of two",[48] Jeff Hecht concluded that "The details were vague and the idea never quite worked exactly as Basov and Prokhorov proposed, but the idea opened a new door"[49]. He added, however, that Bloembergen's proposal "was entirely independent of Basov and Prokhorov's proposal, which he hadn't seen, and went well beyond it."[50] Hecht apparently missed the subsequent development of the three-level maser proposal published in 1955 by Prokhorov with his graduate student Alexander A. Manenkov analyzing the possibility of obtaining population inversion in chromium ions (Cr^{3+}) in ruby.[51] In the paper Manenkov and Prokhorov showed, as Bloembergen would show in a more general way a year later, that the three-level systems of Cr^{3+}in ruby could be used to make a maser using the technique of electron paramagnetic resonance with radiation pumping.[52] That work was the basis for the subsequent development of solid state masers, which the Soviets initially called by the clumsy name "sealed molecular generators"[53].

4.2 Not inferior, but smaller

Prokhorov returned from England reassured, certain that they were good competitors. His major finding at the conference was that the research he was leading at the laboratory of oscillations matched the research in microwave spectroscopy abroad in quality. They were falling behind only in pace of development. The Discussions of the Faraday Society gathered world leading researchers in microwave spectroscopy and this gave him an overview of the field of microwave spectroscopy that appeared in the next report of the activities of his laboratory, which begins as follows: "Since 1946 abroad (specially in the USA) a new division of physical radiospectroscopy of molecules began to develop intensively. Nowadays there are more than 400 scientists

[48] Hecht (as in n. 19), p. 28
[49] Ibid.
[50] Ibid., p. 30
[51] The chromium ions are responsible for the red color of ruby crystals; they can absorb light in the yellow-green region and re-emit as red luminescence. In 1955 ruby had been well studied by spectroscopists and for that reason was a natural choice to be used maser and laser medium both for Soviet and American physicists.Karlov, Krokhin and Lukishova (as in n. 80).
[52] A. A. Manenkov and A. M. Prokhorov, 'Tonkaia structura spektra paramagnitnogo rezonansa iona Cr3+ v khromovom konture', *Zh. Eksp. Teor. Fiz.* 28 (1955):6. More details about that work is given in A. A. Manenkov, 'EPR and development of quantum electronics', *Journal of Physics: Conference Series*, 324 (2011).
[53] Prokhorov, 'Otchet po sektoru radiospektroskopii laboratorii kolebanii FIAN za 1955 god. ARAN, 532-1-251. pp. 83-39' (as in n. 80)

working in radiospectroscopy abroad."[54] He concluded that the research conducted in his laboratory "is not inferior in quality to works conducted abroad, but in amount of work and pace of development is significantly behind. This is due to the small number of researchers (1 Doctor of Sciences, 5 Candidates of Sciences, and 5 engineers), lack of adequate number of working areas, insufficient provision of mechanical work, and weak provision of work in radiochemistry."

Riding on the Cold War rivalry, in the subsequent years Prokhorov, Basov, and their collaborators exploited the competition with the West to foster their research and build the new discipline of quantum radiophysics. The reports written in the second half of the 1950s reveal a conscious and successful strategy of using the new conjecture of competition with the West to advance research in radiospectroscopy. Once the small number of people working in the laboratory was pointed out as a major problem, they had to recruit young scholars and graduated students. If in the winter of 1955-56 the laboratory had a small staff of 11 researchers working in the sector of radiospectroscopy, in 1958 the sector went through a major reorganization and was divided in two independent groups of 15 to 20 people each. An increase of 3 to 4 times in little more than 2 years.[55]

Only increasing the the staff was not enough, though. To grow while maintaining the quality they needed more room and more funding. Perhaps it was around this time that Prokhorov discovered the "general effect" "military generals are very interested in physics".[56] To attract the attention of authorities, besides emphasizing the importance of the field abroad, specially in the USA, and that the USSR was falling short in numbers and quality of equipments, they emphasized the practical applications of the research, created ties with universities, military and industrial institutes, and translated the research into cutting-edge technology by themselves.

Between 1955 and 1956 they launched the molecular beam generator (Figure 4.2)and also studied its operation as an amplifier and spectrometer. As Charles Townes in the US, they paid special attention to the frequency stability of the device. First, they compared its frequency with the frequency of a quartz resonator, and after building a second molecular generator to obtain beats, they concluded that

[54] Group's annual report for 1955: Prokhorov, 'Otchet po sektoru radiospektroskopii laboratorii kolebanii FIAN za 1955 god. ARAN, 532-1-251. pp. 83-39' (as in n. 80), p. 83. In 1957 they would emphasize this once more: "all those works stand on the level of foreign work, although their scale is significantly smaller and some branches of radiospectroscopy are entirely absent."ibid., p. 84

[55] *Obyasnitelnaia zapiska k pismu FIAN v otdelenie fiziko-matematicheskikh nauk o strukture fizicheskogo instituta im. P. N. Lebedeva AN SSSR. ARAN, 532-1-300, pp. 27-31*, 1958

[56] Graham, *Lonely Ideas: Can Russia Compete?* (as in n. 16), p. 83

the device could be used as a highly precise frequency standard and molecular clock (what the Americans called atomic clock). For that work, still in 1955, they were awarded the prize of presidium of the Academy of Science. The tasks of building a frequency standard and a prototype of the molecular clock were carried out in 1957.[57]

In 1958, taking part in international conferences and sending copies of papers to foreign peers were becoming a common practice for researchers of the laboratory of oscillations. That year researchers of the laboratory took part in an international meeting promoted by the International Union of Radio Science (URSI) in which that the excitement about the potentialities of the maser dominated the atmosphere,[58] and "systematically exchanged printed works with foreign scientists".[59] That such apparently trivial actions as sending a preprint abroad was registered in annual reports suggests that at that time this was prestigious for the laboratory. Prokhorov and their pupils also emphasized, almost at any opportunity, that the maser was invented simultaneously at FIAN and Columbia University. In a time when to catch up with the West was the Kremlin's official policy, to share the credit with a top North American university for the invention of an extraordinary device was nothing short of prestigious.[60]

Stimulated by this initial success, several institutions were involved in the research on molecular generators and amplifiers in the Soviet Union. They included the Moscow State University, the Institute of Atomic Energy, the Kharkov Higher

[57] Prokhorov, 'Otchet po sektoru radiospektroskopii laboratorii kolebanii FIAN za 1955 god. ARAN, 532-1-251. pp. 83-39' (as in n. 80); *Laboratoria Kolebanii (Otchet). ARAN 532-1-295*, 1957.

[58] According to URSI's website, "In 1957... the meeting took place in an atmosphere of great scientific interest: the maser had been conceived a few years earlier and promised to be a device that could revolutionize measurements." *History of URSI*, ⟨URL: `http://www.ursi.org/en/ursi{\_}history.asp`⟩ – visited on 2015-08-14.

I have not found more details about the participation of researchers of the laboratory of oscillations in the meeting of the URSI. I found only the following mention in the report for 1957: "Researchers of the sector [of radiospectroscopy] took part in 5 conferences, one of them international (URSI), where two talks were presented". 'Laboratoria Kolebanii (Otchet). ARAN 532-1-295' (as in n. 57), p. 228. This is the period of creation of specific organs to deal with the bureaucracy related to scientific international relations and the documents available in the archive of FIAN regarding international affairs do not exhibit a defined pattern. For instance, I have found letters regarding the Discussions of the Faraday Society of 1955, but found no report; in contrast, I have found the report of the conference Quantum Electronics - Resonance phenomena held in 1959, but found no letters. I found no specific records of the conferences that happened between those two, only mentions in other documents.

[59] 'Laboratoria Kolebanii (Otchet). ARAN 532-1-295' (as in n. 57), p. 228

[60] Basov and Prokhorov, 'Doklady Akademii Nauk SSSR, No. 1, vol. 101, 1955' (as in n. 135); Prokhorov, Basov and Barchukov (as in n. 130).

Military School of Aviation and Engineering, the All-Union Research Institute of Physical-technical and Radio-technical Measurements (which provided the standard time-signal of the Soviet Union), and the Scientific Research Institute 695, a secret institute subordinated to the Ministry Radio-technical Industry. In 1957 alone, researchers of the sector of radiospectroscopy of the laboratory of oscillations gave more the 250 consultations on questions related to the molecular generators and amplifiers and their applications.[61]

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All that effort payed off. The reorganization of the sector of spectroscopy that took place in 1958 was a preparation to "greatly expand the work in the next year, after the completion of the special building for the work in radiospectroscopy."[62] The sector of spectroscopy was divided in two, with the task of developing molecular amplifiers being assigned to Prokhorov's group and the task of developing molecular generators assigned to Basov's group. Both groups would continue basic work in spectroscopy with studies on molecular structures, atomic nuclei and free radicals, but those works were increasingly becoming secondary in the reports.[63]

The central axis of the sector of molecular amplifier headed by Alexander Prokhorov was the effect Electron Paramagnetic Resonance (EPR). The effect had been discovered in 1944 by Evgeny Zavoisky in Kazan, a city on the Volga river, approximately 800km east of Moscow, to where factories and research institutes, including FIAN, were moved during the WWII. Basically, the effect is a transition between two energy levels created due to the alignment of the outer electron's spin in ferromagnetic elements with an external magnetic field. When atoms as Cr^{3+}, which have a single electron in the last orbital, are placed in a magnetic field the energy level corresponding to that last orbital is split in two. This split is know as Zeeman effect. It occurs because the spin can be either parallel or anti-parallel to the field, and each of this positions correspond to different energy-levels. The electron may switch from one state to another by absorbing or emitting energy. The difference between

[61] 'Laboratoria Kolebanii (Otchet). ARAN 532-1-295' (as in n. 57), p. 228.

[62] 'Obyasnitelnaia zapiska k pismu FIAN v otdelenie fiziko-matematicheskikh nauk o strukture fizicheskogo instituta im. P. N. Lebedeva AN SSSR. ARAN, 532-1-300, pp. 27-31' (as in n. 55), pp. 30–31.

[63] Ibid.; Prokhorov, 'Otchet po sektoru radiospektroskopii laboratorii kolebanii FIAN za 1955 god. ARAN, 532-1-251. pp. 83-39' (as in n. 80); (as in n. 57); *Otchet, sektor radiospektroscopii laboratorii kolebanii. ARAN, 532-1-303, pp. 19-113*, 1958; *Otchet laboratorii kolebanii po problemam 'Kvantovaia radiofizika' i 'radiospektroskopii' za 1959 g. ARAN, f. 532-1-324, pp. 19-97.* 1959.

energy levels can be increased or decreased by increasing or decreasing the external magnetic field. This made EPR a promising effect to create three-level systems that had been proposed by Basov and Prokhorov in 1954 (as in shown in figure 4.3).[64]

EPR thus can be used to make a high-resolution spectroscope and to make an ultra-low noise amplifier based on the maser/laser principle. In collaboration with the Institute of Inorganic Chemistry of the Academy of Sciences and the Institute of Atomic Energy, Prokhorov and his collaborators used EPR to devise the most sensitive spectroscope in the USSR, which was employed to study free radicals and to measure nuclear moments of radioactive isotopes, the latter a request by the Consul of Ministers of the USSR.[65] Besides, the team of the laboratory of oscillations helped to construct and launch EPR spectroscopes, which permitted to work with substances in the solid state, avoiding the dangers of working with gases of radioactive substances, in a series of institutions of the USSR. Among them were the Moscow State University, the Tomsk State University in Siberia, the Institute of Chemical Physics of the Academy of Sciences, the Moscow Engineering Physics Institute.[66]

An important step toward EPR masers for millimeter and sub-millimeter waves was given when Alexander Prokhorov proposed a new type of resonator - the open resonator - an open cavity formed by two parallel mirrors in which the size of the mirrors and distance between them are much larger than the wavelengths of the radiation to be amplified. That was the Fabry-Pérot resonator, well-known in optics, but not so in radiophysics. Theretofore the most common resonators in radiophysics were resonant cavities whose size had to be of the same order of magnitude of the wavelength. This requirement made the cavity resonators impracticable for millimeter and sub-millimeter waves because the available technology could not produce such tiny cavities with the quality required to sustain oscillations.[67]

[64] Basov and Prokhorov, 'Sov. Phys.-JETP., No. 3, vol. 1, 1955'.

[65] Prokhorov, 'Otchet po sektoru radiospektroskopii laboratorii kolebanii FIAN za 1955 god. ARAN, 532-1-251. pp. 83-39' (as in n. 80); (as in n. 57); 'Problemnyi Plan za 1958, Problema 'radiospektroskopiia'. ARAN, 532-1-301, pp. 17–68.' (1958); (as in n. 63). See Silva Neto and Kojevnikov (as in n. 2).

[66] Prokhorov, 'Otchet po sektoru radiospektroskopii laboratorii kolebanii FIAN za 1955 god. ARAN, 532-1-251. pp. 83-39' (as in n. 80); Prokhorov, Basov and Barchukov (as in n. 130). The list of institutions involved may be found in 'Laboratoria Kolebanii (Otchet). ARAN 532-1-295' (as in n. 57), p. 228.

[67] Prokhorov believed to be the first one suggesting to use Fabry-Pérot resonators to make a maser and submitted a paper to the Soviet *JETP* on April 1 1958, which was published in June that year. A. M. Prokhorov, 'O molekuliarnom usilitele i generator ha submillimetrovykh volnakh', *Zh. Eksp. Teor. Fiz.* 34 (1958):6.

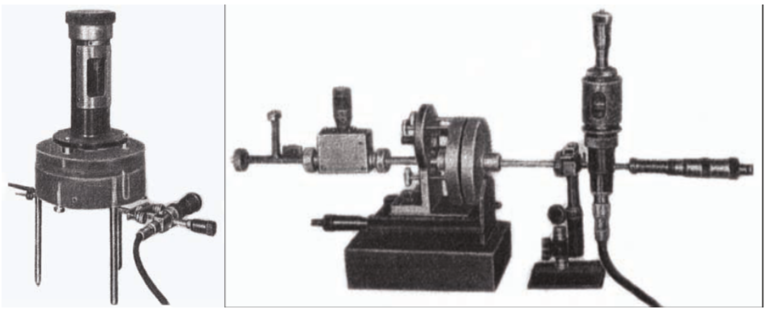

Figure 4.4: The disk resonator on left is for wavelength range of 11 to 13 mm. The one on the right is for wavelength range of 4 to 8mm.

As the Fabry-Pérot resonator was originally used in optics, it was natural to suggest that it could be used to push the maser toward the infrared and optical region, however this is not mentioned in Prokhorov's paper. Later in life he would offer the following reason for not mentioning this crucial detail:

> Sometimes I was asked the question: Why did I not mention in my paper that such resonators are also good for the optical spectral region? I did it intentionally, not to be prohibited from publishing this paper because new results for the optical range, and especially the IR, were under special control. It is obvious that if this paper had not been published by me, then certainly this resonator would be patented in the US. [68]

This passage was written in 2001, in the last year of his life, and might well be a rationalization *a posteriori*, one of those mechanism by which our memory reconstructs the past to render it coherent with or comprehensible in terms of our present,[69] but it makes total sense in a context in which compartmentalization of scientific research and competition with the Americans were the order of the day. In any case, it is meaningful that after so much time the dilemma stemming from the compartmentalization of research and the competition with American physicists were still very alive in Prokhorov's mind.

[68] Autobiographical note, published posthumously in Shcherbakov, Mikhailova and Prokhorov (as in n. 84), p. 418.

[69] An interesting reflexion on autobiographical accounts can be found in Carr (as in n. 57): "...we may think of autobiographical reflection as being conducted in the present and being directed entirely toward the past. More often, however, it is concerned with the past in order to render it coherent with or comprehensible in terms of a present and a future."ibid., p. 75.

Although Prokhorov is reputed to have suggested a ruby laser in 1957,[70] neither infrared nor optical masers is mentioned in publication or open laboratory report of the late 1950s. Sometimes, however, silence is as meaningful as an statement. This absence certainly does not mean that the Soviets were not trying to make a laser before 1960, when optical generators begin to be mentioned in print. It suggests, instead, that Prokhorov's claim that he did not mention that his open resonator could be used for the optical and infrared regions not to have his paper classified is likely to be the case. Townes and Schawlow published their proposal of the optical maser at the end of 1958. Had Basov, Prokhorov and their collaborators not started working on their own laser, that proposal would be enough to kindle their interest, as much as it did to many other physicists working on microwave masers.[71]

As it turned out, Prokhorov's strategy of publishing his proposal of the open resonator not to let an American patent the idea was not successful. The open resonator made from a pair of parallel plates had been already suggested by Princeton University physicist Robert Dicke in a patent application filled in 1956 and granted in 1958, a little after Prokhorov's publication. Besides, at the end of 1958 Charles Townes and his Bell labs collaborator Arthur L. Schawlow developed the idea further in their laser proposal, acknowledging in a footnote that Prokhorov and Dicke had independently proposed that kind of resonator.[72]

Leading a team of researchers at FIAN and at Moscow State University, where he had been teaching since 1954, and had founded a laboratory of radiospectroscopy in 1957, Prokhorov developed a broad study EPR in corundum crystals as ruby and sapphire[73]. They aimed to use the crystals as media to create solid state masers for various wavelengths. Identified as the most efficient, Ruby was used to devise

[70] Hey (as in n. 5).

[71] Basov also claims that they began wondering about lasers in 1957, what is very plausible. Basov, 'Interview with Dr. N. G. Basov by A. Guenther, on 14th September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.' (as in n. 4). The work toward the laser in the Soviet Union began at least in the first half of 1958 with the work on semiconductors described bellow.

[72] One of the major improvement made by Townes and Schawlow was the increase of the distances between the mirrors. Prokhorov, perhaps influenced by the idea that the dimensions of the resonator had to be close to that of the wavelength, had suggested 1 cm for a wavelength of 0,05 cm. Increasing the distance left more room for the medium and for the pumping radiation. Prokhorov, 'Zh. Eksp. Teor. Fiz., No. 6, vol. 34, 1958' (as in n. 67); Schawlow and Townes (as in n. 17).

[73] Ruby and sapphire are both varieties of the mineral corundum with traces of chromium and iron, respectively, that are responsible for the red color of ruby and the blue color of sapphire crystals.

various types of solid state masers that were much more practical and compact in comparison to molecular been masers.[74]

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After becoming a Candidate of Sciences, in 1954, and launching the first maser in 1955, Nikolai Basov began a careful study of the properties of the molecular-beam generator which would be the topic of his dissertation of Doctor of Sciences defended in 1957. During this time, Basov began attracting his own following of young and talented students, forming the group that in 1958 became the core of the sector of molecular generators of the laboratory of oscillations. The group focused on investigation and development of high-stability molecular generators, their employment as frequency standards, and new methods of generation of millimeter and sub-millimeter waves, and light. [75]

As the next steps after launching the ammonia-beam maser the group studied the dependency of the oscillator frequency on various parameters for a series of ammonia's spectral lines. Based on those results, they proposed a method of increasing frequency stability by slowing down the molecules. They also took upon themselves the task of translating that research into technologies as atomic clocks. In 1957 they designed and produced a frequency standard for the All-Union Research Institute of Physical-technical and Radio-technical Measurements that provided for years the time standard for the entire Soviet Union.[76] Besides, they designed three-level masers based on molecular beams of deuterated ammonias (NH2D, NHD2, ND3), which emitted radiation of wavelength 3.98 cm, 2.48 cm, and 1.02 cm, respectively. Once the principles and basic technology were mastered, the goal was to increase the stability of the oscillators and to find materials that could push the maser technology toward shorter wavelengths.[77]

At the beginning of 1958 Basov and collaborators took the first steps toward the optical region, electing semiconductors as the most promising material. The laboratory of semiconductor physics of FIAN headed by Bentsion M. Vul, created in

[74] One of the first ruby masers they produced was used by the group of radio astronomy of the laboratory of oscillations to make a radio telescope. Manenkov (as in n. 52); A. A. Manenkov and A. M. Prokhorov, 'The fine structure of the spectrum of the paramagnetic resonance of the ion Cr3+ in chromium corundum', *Sov. Phys.-JETP.* 28 (1955):6; G. M. Zverev et al., 'A chromium corundum paramagnetic amplifier and generator', *Sov. Phys.-JETP*, 7 (1958)

[75] 'Laboratoria Kolebanii (Otchet). ARAN 532-1-295' (as in n. 57); (as in n. 55).

[76] 'Laboratoria Kolebanii (Otchet). ARAN 532-1-295' (as in n. 57).

[77] Prokhorov, 'Problemnyi Plan za 1957. ARAN, 532-1-283, pp. 22-25' (as in n. 129); (as in n. 63); (as in n. 63); (as in n. 57); (as in n. 65).

1948, had been playing a key role on the development of the first Soviet semiconductor diodes, transistors, and photocells.[78] In 1958 Basov turned to the team of the laboratory of semiconductor physics asking about the possibility of obtaining population inversion in semiconductors. In July that year, Basov, Vul, and Yuri Popov, a graduate student pursuing his degree of Candidate of Sciences at FIAN, applied for a certificate of authorship (the Soviet equivalent of a patent), for a "quantum-mechanical semiconductor generators and amplifier of electromagnetic oscillations". Later the work was published in the Soviet *JETP* and presented at the first international conference in quantum electronics.[79] The application and the paper makes no mention to any specific frequency region. It only describes in general terms how semiconductors could in principle be used to create pulsed generators and amplifiers of radiation. They suggested using a high-intensity electric field to ionize the impurity, sending electrons to the conduction band. After a relaxation time, the electrons would fall to the lowest energy level liberating radiation. They claimed that the relaxation time could be controlled by changing the concentration of impurity. This was important because the system can be used as a generator or amplifier only during the relaxation time, namely while the system is still excited. The attractive feature of the method was that it used electric field instead of radiation pumping, what meant that a laser using that method would be powered directly by electricity; no high-power lamp or other radiation source was needed to pump electrons to the upper level.

As it turned out, obtaining efficient optical transitions was far more difficult than they had expected. The existing light emitting diode (LED), which had been first reported by the Soviet inventor Oleg Losev at the end of the 1920s,[80] had extremely low efficiency to find any practical use. They needed a novel way. When the first attempts to obtain light emission in germanium and silicon failed, Basov and his collaborators went in search for other materials. In the experimental plan they decided they needed a semiconductor with narrow forbidden bands and direct optical transitions. At that time the best known semiconductor with those qualities was the indium antimonide, grown at the Leningrad Physical-technical Institute, to where

[78] V. S. Vavilov, 'Bentsion Moiseevich Vul', *Uspekhi Fizicheskih Nauk*, 80 (1963):4; N. G. Basov, A. P. Shotov and Yu. N. Venevtsev, 'Obituary in memory of bentsion M. Vul', *Ferroelectrics*, 74 (1987):1.

[79] N. G. Basov, B. M. Vul and Y. M. Popov, 'Kvantovomekhnicheskie proluprovodnikovye generatory i usiliteli elektromagnetnykh kolebanii', *Zh. Eksp. Teor. Fiz.* 37 (1959):2; Karlov, Krokhin and Lukishova (as in n. 80).

[80] Nikolay Zheludev, 'Commentary. The life and times of the LED - a 100-year history', *Nature Photonics*, 1 (2007):4.

Basov and some of his close collaborators headed in the first opportunity. Soon FIAN had two groups trying to obtain efficient light emission in semiconductors, one at the laboratory of oscillations working on indium antimonide, and another at the laboratory of semiconductor physics insisting on germanium. Notwithstanding some interesting scientific results, both groups failed in their main goal - to obtain population inversion that could lead to light emission.[81]

Despite all the difficulties they persisted in semiconductor lasers. Later, joined by Oleg Krokhin, who would become Basov's life-long collaborator, specially in the development of high-energy lasers, Basov and Yuri Popov continued their search for practical schemes for obtaining population inversion in semiconductors that could result in infrared and optical emission. Only in 1961 they came up with a successful proposal. The idea was to obtain population inversion by injecting non-equilibrium carriers (electric charges) through a p-n junction. That was the first well-grounded proposal of the injection laser successfully constructed using gallium arsenide in the USA in 1962 and in the USSR in 1963.[82]

4.3 First time in America

Meanwhile a similar effort to create the same field, but with the name *quantum electronics*, began approximately at the same time in the United States. The landmark of the creation of the field is the conference "Quantum Electronics - Resonance Phenomena" organized by Charles Townes.[83] According to the report written by the Soviet delegation, about 185 researchers from 9 countries gathered at Shawanga Lodge, a resort 100 km away from New York, from September 14 to 16, 1959. Among them were "most of the outstanding researchers from various countries working in

[81] Y. M. Popov, 'Ob istorii sozdaniia inzhektsionogo lazera', *Physics-Uspekhi*, 174 (2004):10; idem, 'Istoriia sozdaniia inzhektsionnogo lazera', *Uspekhi Fizicheskih Nauk*, 181 (2011):1.

[82] N. G. Basov, O. N. Krokhin and Y. M. Popov, 'Vozmozhnost ispolzovaniia nepriamykh perekhodov dlia polucheniia otritsatelnoi temperatury v poluprovodnikakh', *Zh. Eksp. Teor. Fiz.* 39 (1960):5; idem, 'Generatsiia, usilenie i indikatsiia infrakrasnogo i opticheskogo izluchenii s pomoshchiu kvantobykh sistem', *Uspekhi Fizicheskih Nauk*, 72 (1960):2; idem, 'Poluchenie sostoianii s otritsatelnoi temperaturoi v p-n perekhodakh vyrozhdennykh poluprovodnikov', *Zh. Eksp. Teor. Fiz.* 40 (1961):6; Karlov, Krokhin and Lukishova (as in n. 80); Popov, 'Uspekhi Fizicheskih Nauk, No. 1, vol. 181, 2011' (as in n. 81). I would like to thank Yuri Popov and Oleg Krokhin for their hospitality, and for the sources they gave me, during my visits to FIAN.

[83] The Soviets later adopted the name quantum electronics as well. For a discussion on the meaning of those labels see conclusion of Silva Neto and Kojevnikov (as in n. 2).

Figure 4.5: The inventors of the maser. From left to right: Hebert Zeiger, Nikolai Basov, James Gordon, Alexander Prokhorov, and Charles Townes. USA 1959. Courtesy of Alexander K. Prokhorov.

microwave spectroscopy". "[T]he organizers", wrote Jeff Hecht, "invited almost every one who was anyone in maser research".[84] Scientists came also from Britain, France, Germany, Holland, Israel, Japan, and Switzerland. A total of 66 talks were delivered at the conference, squeezed in an overloading schedule which spanned from 9 a.m. to 10 p.m. Many of the presentations were not included in the program and had to be delivered over the intervals. Apparently the organizing committee did not expect such a scale. [85]

The Soviet delegation was composed of 4 members. Besides Alexander Prokhorov and Nikolai Basov, it included Alexander Barchukov, Prokhorov's former graduate student who had been working with him since he took up research in microwave spectroscopy and followed him into maser and laser physics, and Leonid Kornienko, at the time working under Prokhorov on his candidate dissertation on EPR masers. Both Barchukov and Kornienko were little known for the international community. Jeff Hecht has taken them for handlers sent by the Soviet bureaucracy to watch over Prokhorov and Basov. Here is how Hecht described the delegation:

> The organizers were particularly interested in the little delegation from the Soviet Union. Most of them had never met Basov and Prokhorov,

[84] Hecht (as in n. 19), p. 101.
[85] Prokhorov, Basov and Barchukov (as in n. 130); Hecht (as in n. 19).

> who showed up with a couple of scientific unknowns. It was a typical pattern in the Soviet era. Basov and Prokhorov were both well-connected members of the Communist Party, but the cautious Soviet bureaucracy always sent along a handler or two, typically quite, burly types who spoke little English.[86]

It was indeed a common pattern in the first years of the international opening. As we saw in a previous section, Prokhorov was accompanied by "reliable comrades" in his first international trip to take part in the meeting promoted by the Faraday Society. But by 1959 the pattern had changed and even scientists of questionable loyalty were allowed to travel abroad. Loyal, "well-connected members of the Communist Party" as Basov and Prokhorov were not only allowed to travel abroad without handlers, but became handlers themselves. As leader of a Soviet delegation Prokhorov was asked to watch over other scientists. An interesting episode was narrated by his wife when describing his first experiences abroad:

> Afterward [Prokhorov] was in England in various occasion, in various scientific conferences. One of those was very memorable for my husband, for a very different reason. In the composition of a delegation, of which my husband was assigned as scientific supervisor, was academician Vladimir Fock... Sasha knew him well and respected him very much. On the eve of the trip Alexander Mikhailovich was called to the competent organ where they "recommended" not to take the eyes out of the academician - he was supposedly not a very reliable comrade. The ambiguity of the situation became clear to Alexander Prokhorov when, in London, Fock expressed wish to travel to Cambridge. Sasha did not protest, only asked when he would be back. Fock promised to come back by next evening. And his promise, for the joy of my husband, he maintained...[87]

This episode most likely happened early in the 1960s, but by 1959, with major agreement on cultural exchange between the US and the USSR already in effect, Prokhorov and his collaborators were trusted to travel to the US with his collaborators with relatively little bureaucracy.[88]

[86] Hecht (as in n. 19), pp. 103-104.
[87] Prokhorova (as in n. 18), p. 67.
[88] After the Lacy-Zarubin agreement on cultural exchange was signed few Soviet scholars went to the United States for long exchange periods. The Soviet Union clearly preferred to send scientists from natural sciences rather than social sciences or humanities. In his book on the

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The barriers the Soviet delegation faced were set by the US Department of State (DoS), apparently in attempt to restrict the possibilities of scientific espionage in an strategic field. The Soviets were planing to arrive in New York on September 12 and stay until the end of the month, so that they could use the conference to open doors to leading US laboratories working on quantum radiophysics. For their surprise, the American embassy in Moscow granted them the visa from September 1 to 19, precluding the plan of visiting many laboratories after the conference. On the last moment they tried to anticipate the departure to make the visits prior to the conference. They requested the Foreign Office of the Academy of Science to anticipate their flight to September 8, but the next flight to New York would leave only on September 11.[89] In any case, the DoS found yet other means to preclude their plans:

> At our arrival in New York Prof. Townes showed us the answer of the Department of State to the question by American physicists as to whether we were allowed to visit their laboratories. In the answer they wrote that the authorization could be given if the competent Soviet organization warrant visit of American physicists to equivalent laboratories in the USSR... In this situation we went with the letter to the Soviet embassy in Washington in which we were told that it should be reminded to the Department of State that in 1959 the Soviet Union was visited by approximately 400 American scholars, against 40 Soviet scholars traveling to the United States, so that the demand for equivalence was untenable.[90]

Reassured by the asymmetry they had just discovered, the delegation not only reminded the DoS, which was prone to talks of fairness and equivalence,[91] of the numbers of the exchange for the current year, but at once requested to extend their

Soviet-American cultural exchange during the Cold War, Yale Richmond gives many examples of scholars who went to the United States in the last years of the 1950s without any KGB handler; although KGB agents were also sent as students in the exchange. Richmond (as in n. 31).

[89] Prokhorov, Basov and Barchukov (as in n. 130).

[90] Ibid., p. 3.

[91] For the emphasis on fairness and equivalence in the discourse of the DoS see Richmond (as in n. 31).

visa until the end of the month. As a result, "In the second answer of the Department of State... we were refused both the extension of the visa and the authorization to visit the laboratories, despite being invited by many American physicists".[92]

The Soviet delegation did use their few extra days after the conference to visit Columbia University and Columbia Radiation Laboratory (CRL).[93] Townes had visited the Soviet Union during his sabbatical year in 1956 and had visited FIAN and other laboratories, the same kind of laboratories Soviet physicists had been denied access to, with the typical Cold-War mix of open and classified research. Perhaps that was one of the reasons the Soviet delegation was allowed into CRL, which since the WWII had been one of the mainstay of the US radar development. Having been welcomed by Prokhorov and Basov in their institute, and playing the role of host, Townes found himself in no position to deny them a visit to his laboratory. But other physicists were not under the same obligation and preferred to act according to the decision of the Department of State.

That American physicists consulted the DoS as to whether they could receive Soviet Physicists in their laboratories reveals that they were ready to subordinate the interest of their discipline to that of their nation. Their scientific internationalism met its limit on the foreign policy of the Department of State. Besides, most of the US laboratories involved in maser research, if not all, were being funded by contracts with the military and therefore subjected to strict security regulations. At this stage of the Cold War, US physicists often found themselves helpless before a Kafkaesque, anti-communist bureaucracy.[94]

The case of Gordon Gould, directly concerning laser history, helps to understand why the Soviet delegation was not allowed in many laboratories, even if American physicists had invited them. Gould was one of the first physicist to come up with schemes to create lasers (he was the one who created the acronym) and to think

[92] Prokhorov, Basov and Barchukov (as in n. 130), p. 3.

[93] On their last days in NYC the Soviet delegation visited the Columbia Radiation Laboratory. In the report they described the structure of the laboratory, its personal, several of the lines of research being carried there. But they left CRL with a hunch that they were not shown everything. They were not shown any applied activity. And indeed applied research was a major share of CRL's research agenda.Forman, 'Osiris, No. Science after '40., vol. 7, 1992' (as in n. 14).

[94] In the first half of the 1950s, a period know in the United States as the Second Red Scare, many US intellectuals fell victim of McCarthyism, the anti-communist witch-hunt which subjected thousands of US citizens to aggressive investigations, many of whom lost their jobs and suffered others sanctions. For the impact of McCarthyism on American science see Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War*, (Chapel Hill: University of North Carolina Press, 1999).

Figure 4.6: Prokhorov, Townes and Basov at FIAN in 1965. Courtesy of Alexander K. Prokhorov.

of a number of visionary applications for lasers, including military ones, as laser weapon that could destroy Intercontinental Ballistic Missiles. Gould caught the attention of military agencies to lasers with help of the managers of TRG (Technical Research Group), the company he had joined to pursue his laser ideas, and they closed a millionaire deal. But when TRG's laser project went classified, having his security clearance denied for an youthful flirt with communism and his bohemian life-styles in anti-communist and puritan United States of the 1950s, Gould had his notebooks confiscated and was barred from the laboratories in which he was leading groups of researchers on the pursuit of several different ideas he had drafted in his notebook. Cutting Gordon Gould out of the TRG laser program seriously hampered its progress. Not only Gould was barred from the laboratories in which classified research was being conducted, but his colleagues who were left to materialize his ideas could not talk to him about what was going on within the lab.[95]

[95]While working in the Manhattan Project Gould got involved with a Marxist woman, a coworker in the Project, who drifted him into communist circles, and they married in 1947. Gould claims that his involvement with communism lasted until 1948, when the Soviet take over of Czechoslovakia drove him away from communism, his wife, and her communist friends. In 1954, when anti-Communist hysteria had downed over American universities, Gould was demanded and refused to name the people who were part of his former Marxist group and was fired from his position of physics instructor at City College of New York. Hecht makes a lengthy, interesting account of TRG's failed odyssey to get Gould a security clearance. HECHT (as in

Physicists involved in maser/laser projects in the US were probably aware of what was happening to Gould, certainly about the classification of laser projects. And if an American physicist was being submitted to such restrictions for his loose connections to the Communist Party during his youth, they would certainly be very careful before letting the Soviets into details of any military sponsored laser project.

We can only guess the reasoning behind the decision of the DoS, but denying visa was the best way of letting the Soviet delegation in the darkness about military exploitations of masers and lasers in the US. Were the Soviets told that they were not allowed to visit the laboratories for security reasons they would be sure that the Americans were pursuing military applications of lasers, but having the visa denied on the basis of "equivalence" they could only speculate, as they did in the report.

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At that final stage of the race to make the first laser, however, American physicists involved in laser projects were not only worrying about Soviet physicists. They were competing between themselves as well. Differently of high-energy physics, in which a single project may involve physicists of several nations, the competition to make the first laser involved several projects being carried on simultaneously, most of them with only a couple of people involved, and winning the race would bring wide recognition, quite possibly a Nobel prize.[96] Hence, caution and mistrust toward other scientists as competitors was blatant along the conference Quantum Electronics.

The conference addressed five main groups of questions: (1) Molecular beam generators and amplifiers; (2) Solid-state molecular generators and amplifiers; (3) Considerations on the possibilities of generators and amplifiers in the optical region; (4) Relaxation processes in solids; and (5) Considerations of general physical problems which may be solved in connection with the development of quantum electronics. As we can see by the questions addressed, quantum electronics, or quantum radiophysics, was mostly concerned with the development of existing masers and creating new types of masers, including the optical maser.

n. 19).

[96] In the end, Theodore Maiman, the winner of the race, was left out of the Nobel prize for the invention of the laser, but the decision of the committee was surprising. So much so that most of the authors when discussing that prize feel the need of discussing the rationale behind the decision. Maiman, aggrieved by the decision of the committee, claimed that they "did not do their home work well". Maiman (as in n. 76).

The Soviet delegation delivered five papers. Kornienko and Prokhorov discussed paramagnetic amplification using Fe^{3+} in sapphire crystal, Barchukov and Prokhorov presented their experimental study of disk resonator as a generator of millimeter waves, Manenkov and Prokhorov presented part of their work on ruby, a study of spin-lattice relaxation of chromium Cr^{3+} in ruby. Basov presented their proposal of semiconductor generator and amplifier published with Popov and Vul discussed above, and the application of slow molecules to increase the stability of molecular generators.[97]

Those presentations reflected well the main lines of work being pursued at FIAN, at least the side of their open research. However, they had been carefully thought not to reveal much more than what had already been published in Soviet journals. Soviet physicists participating in international conferences around this time were explicitly advised to give as little information as possible, and to get as much as they could. For example, three years earlier, when the physicist Roald Z. Sagdeev was preparing for the Geneva conference on peaceful application of nuclear physics, he received the following advice from a deputy minister: "The main focus of your activities at the conference should be to learn a dollar's worth of science and to give a kopeck's worth in return".[98]

However, along the conference the Soviets understood that everybody was there with the same intent. The report of the Soviet delegation "emphasized that the content of all talks presented concerned only physical problems, i.e. they showed only the general physical idea, without any concrete implementation in a device. Not even one design was presented ."[99] With only kopeck's worth of novel information being delivered during the presentations, scientists used the intervals to fill their piggy banks. The report continues:

> The conversations with the participants of the conference during the intervals of the presentations were specially meaningful for us. They were many, practically with all the outstanding physicists in the field. Most of the various information included in this report, specially concrete numbers, were gathered in those conversations.[100]

There were, however, more or less defined limits to what could be shared in those conversations. "The foreign researchers, with whom [they] talked, ran away from

[97] Prokhorov, Basov and Barchukov (as in n. 130), pp. 5-6.
[98] Quoted in Richmond (as in n. 31), p. 66. One kopeck is one hundredth of a ruble.
[99] Prokhorov, Basov and Barchukov (as in n. 130), p. 06.
[100] Ibid..

questions about the uses of molecular-beam generators for practical purposes". This seems to be the limits of the exchange between Soviet and American physicists. Cooperation and information exchange had to be limited fundamental problems and principles, applications only for strictly scientific purposes as radioastronomy.

The climax of the conference was the discussion "on the possibilities of molecular generators in optical region", namely, lasers. "To that question were devoted a big number of talks". The discussions versed about what would be the most promising laser media and methods to create population inversion. Semiconductor were discussed in few lectures, however, they emphasized once more, the discussions were kept to the basics:

> In the presentations were discussed only the many possibilities of using different effects to obtain population inversion in optical and infrared regions, but not even one experimental result was considered. However, from private conversations, it became absolutely clear to us that in many laboratories energetic research are being conducted in a practical way. [101]

The overall conclusion they took from those conversations was that the effort toward lasers in the Soviet Union fell short of efforts being carried on in the United States. As long as they knew, the effort to make the laser in the USSR were limited to the walls of FIAN. And they concluded the section with the following warning:

> The Soviet Union must pay the most serious attention at the development of the work on optical-, infrared-, and submillimeter-wave molecular generators because they may solve the problem of optical and infrared radiolocation, not to talk about the scientific value of those works.[102]

As general conclusions and suggestions ensued from the conference as a a whole, they claimed that the level of the work in quantum radiophysics executed in the Soviet Union does not differ from work in other countries in that field. However, there was no work in many branches of quantum radiophysics in the USSR, in special, they were not applying optical pumping and three-level systems to obtain population inversion in infrared region. They also claimed that the Soviet Union was not exploiting all practical application of the molecular generator and that the from process of practical implementation was slow.[103]

[101] Prokhorov, Basov and Barchukov (as in n. 130), p. 12.
[102] Ibid., pp. 11-12.
[103] Ibid., p. 14.

They concluded with a plea for scientific internationalism by emphasizing the value of the visit, which would be even more valuable had they been authorized to visit other firms and laboratories, and that they considered very important to invite two American physicists to the USSR. “That invitation would be one more opportunity to get into the details of the work conducted in America in that field”.

4.4 The Growth

As I have said above, no work toward a laser is mentioned in open publication or report until 1960. The early classification of the work towards lasers suggests that circa 1958 the Soviets already envisaged their potential military applications of lasers. But if on the one hand this prevented physicists from publishing openly and claiming the authorship of their ideas, on the other hand it helped to foster the growth of the laboratory of oscillations. Basov later recalled in an interview the impressive growth of his division of the laboratory, which in 1963 formed the laboratory of quantum radiophysics[104]:

> The Laboratory grew very rapidly: in 1959 there were only about 20 people there, by 1970 there were 500 people, and we attracted many youngsters. Very many degrees were awarded for this work: about 60 Doctors of Science in quantum electronics and 200–250 Ph.D.s. Some of our researchers have gone into industry, some to institutes that work on laser physics. The core has remained.[105]

Prokhorov’s group grew at a similar pace. In fact, both groups grew so fast that by the end of the 1960s, FIAN, one of the largest physical institutes in the Soviet Union, had become too small for them. In 1969 Prokhorov’s group moved to new building made for the laboratory of oscillations named “Omega”. That building would form the General Physics Institute (GPI), an institute independent of FIAN, which was directed by Prokhorov until 1998.[106]

[104] Bebikh, Goncharova and Zhukova (as in n. 88).

[105] Basov, ‘Interview with Dr. N. G. Basov by A. Guenther, on 14th September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.’ (as in n. 4).

[106] The predominance of the laser physics groups at FIAN was such that they polarized the institute and the split felt to contemporaries as if FIAN was being divided in two. Here is how Peter Zarubin recalled the occasion: “There is a Russian proverb that says that two bears cannot live in the same den... Prokhorov was the more outspoken, more humorous. Basov was a little more on the scientific side. But both had one strong part of their nature: they could form teams

Figure 4.7: Omega, building completed in 1969 to house Prokhorov's "share" of FIAN. Later the building was made an independent institute named General Physics Institute, and after Prokhorov's death in 2002, Prokhorov General Physics Institute.

The growth of quantum radiophysics, of course, was not limited to FIAN. In the early 1960s the number of institutions involve in laser research and development grew exponentially. According to Peter Zarubin, "An avalanche-like growth was observed in the number of institutes and industrial plants, as well as higher education institutes engaged in the research and development of lasers, and probably a hundred such organizations in the USSR were involved by the mid-1960s."[107]

Such growth, however, could not have happened without deliberate efforts by Basov, Prokhorov and their close collaborators to create and promote the field of quantum radiophysics. The first major effort on this direction was the organization of a large conference, the first conference of quantum radiophysics. Held in April 1959, the conference as attended by approximately 300 researchers from 60 institutions from all over the Soviet Union. Besides, they also organized open lectures on quantum radiophysics to attract "youngsters". Only in 1959 there were 20 of such lectures with an average public of 60 attendants. This initiatives, associated with the fast pace of forming graduate students at FIAN, helped to create a large community of laser physics, one of the most successful branches of Soviet science.[108]

around them. Each had a team. When Basov became director of Lebedev [Physical Institute], Prokhorov immediately said, 'I want my own institute', and they cut the Lebedev Institute in two parts - the Lebedev Institute and the Institute of General Physics - just because there were two exceptional men." Interview with Peter Zarubin by Neil Hey on 23 November 2003, quoted in Hey (as in n. 5), p. 41. Basov became director of FIAN in 1973 and the GPI was created in 1982. The story thus is not totally consistent, but it illustrates the tensions due to a power struggle between Basov and Prokhorov within FIAN, what indeed led to the creation of GPI.

107 Zarubin (as in n. 5), p. 1050.

108 'Otchet laboratorii kolebanii po problemam 'Kvantovaia radiofizika' i 'radiospektroskopii' za 1959

The secret for such growth seems to lie on a skillful promotion of the maser research that was presented in the previous sections. By emphasizing that the quality of the research done in their laboratory matched international standards, but was smaller and thus slower they appealed to scientists, politicians, and citizens concerned with the competition between capitalism and socialism. By emphasizing that the molecular generator could be used to make better radars, atomic clocks, and more precise measurements of nuclear moments of nuclei they appealed to all those concerned with, and fascinated by, the nuclear arms race or the potentialities of nuclear physics. In short, the report was appealing to virtually everyone with power of decision making in the Kremlin during the Cold War. Thus, it is not surprising that Basov and Prokhorov quickly found powerful patrons in the Kremlin. According to Peter Zarubin, a former director of high-energy laser programs of the Military Industrial Commission, "Basov and Prokhorov were high-level people. They communicated with people at a very high level of the Soviet leadership, and their opinions were considered to be very important... The leaders agreed to listen to what they said, even if they did not understand what they said." [109]

When news of Theodore Maiman's successful creation of a ruby laser arrived in Russia, Basov, Krokhin, Popov and other collaborators joined researchers of the laboratory of luminescence of FIAN, who were well familiar with the luminescence phenomena in ruby crystals, to replicate Maiman's laser, which had been described in a sentence: "To demonstrate the above effect a ruby crystal of 1 cm dimensions coated on two parallel faces with silver was irradiated by a high-power flash lamp" [110]. They succeeded in obtaining laser action on September 18, 1961. However, the first Soviet ruby laser, the first Soviet laser for that matter, was launched on

g. ARAN, f. 532-1-324, pp. 19-97.' (as in n. 63). Laser research and development is given by Evgeny Velikhov, Vice-President of the Academy of Sciences of the USSR (1978–1991), as an example of successful field of Soviet science, specially regarding the translation into technology. See E. P. Velikhov, 'Dostizheniia fiziko-tekhnicheskikh nauk i zadachi nauchno-tekhnicheskogo progressa v narodnom khoziaystve', *Vestnik RAN*, 5 (1984). Regarding comparison with the USA, Yuri Votintsev, commander in chief for Anti-Rocket and Space Defense, later claimed that "in the area of lasers, according to the data I had at the time, in the mid-1980s, the Soviet Union was ten years ahead of the Americans". Quoted in Hey (as in n. 5), p. 50.

[109] Quoted in Idem (as in n. 5), p. 41.

[110] Maiman (as in n. 18), p. 493; *50 let sozdaniia lazera*, ⟨URL: http://www.lebedev.ru/ru/history/laser50.html⟩ – visited on 2015-08-16. The work was described in the classified report N. G. Basov et al., 'Otchet po teme "Primenenie kvantovykh sistem dlia generatsii, usileniia i indikatsii opticheskogo izlucheniia", 1961.' in: *Zapiski Arkhivariusa T. 2, Byp. 1,* (1997), later published in Starodub (as in n. 82), pp. 284-420.

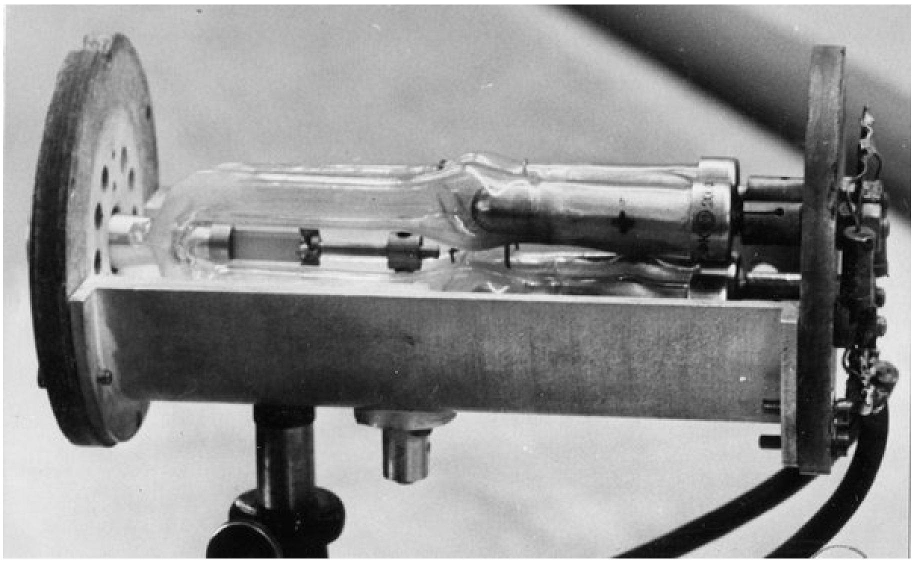

Figure 4.8: The first ruby laser made at FIAN, launched on September 18, 1961. On the left side of the picture we can see the small ruby rod inside the bulb of a flash lamp. This was the replication of Maiman's laser based on the published description. Source: ('50 let sozdaniia lazera' (as in n. 110)).

June 2, 1961, by Leonid Khazov and Inna Belousova at the Optical State Institute (GOI), which entered the competition in 1959, but its advanced optical technology and high-quality ruby crystals permitted Khazov and Belousova to scoop the teams of FIAN.[111]

Even though the first laser in the Soviet Union was not built at FIAN, that institute was the main hotbed of the Soviet laser physics. From there most of the first laser physicists went to work in industries, universities and research institutes all across the Soviet Union.[112] This was thanks to the consistent strategy carried out by Basov, Prokhorov, and their collaborators, to create and promote a new field of research. I have called this process the making of Soviet laser physics. Because in 1960, when the first laser was launched in the United States, the Soviet laser physics already had its basic structure, later, it was just scaled up.

4.5 Conclusion

It has become almost a truism that science and scientists are inevitably shaped by major events and trends in society. And what major events of the twentieth century

[111] Belousova (as in n. 81).

[112] An overview of the several research institutes involved in laser research in the Soviet Union is given in Bagayev, Krokhin and Manenkov (as in n. 79).

shaped science more than the wars and the enduring threat of war? Notwithstanding their removed position in the ivory tower, as their fellow citizens, scientists too were affected by the same apprehension, anxiety, and fears that disturbs the sleep of entire societies threatened by war. Scientists too were swayed by patriotic and nationalist mood characteristic of wartime. And Scientists too did whatever they could to be a good patriot when the times demanded so. Accounts of scientists who devoted their knowledge and skills toward solution of urgent practical problems during wartime are numerous.[113] In such situations the fulfillment of patriotic duty often conflicts with the scientist's conception of what science is, or should be. Naturally, with the strain or interruption of international relations between two scientific superpower, the idea of science as an unified supranational enterprise, or that of a republic of science that know no national borders, is the first to be put on hold.[114]

In peacetime, however, since at least the 18th century scientists had found a simple formula to merge the role of a good patriot with that of a good scientist. According to this formula the laurels bestowed upon a scientist for his scientific activity are automatically extended to his nation. "It does not require a choice on the scientist's part between serving the interests of science and serving the interests of his nation, between behaving like a good scientist and behaving like a good patriot".[115] In one of his classic papers on physics in Wiemar Germany, Paul Forman discussed how in the aftermath of the First World War, German scientists exploited the ideology of scientific internationalism to promote nationalist interests. As he puts it,

> The peculiarly interesting feature of the period following World War I is that while in contrast to the war years the political contribution of science was once again measured primarily in terms of prestige, with formal

[113] This does not mean, of course, that there were no scientists opposing the general trends, but the fate of those who did reveals not only that they were minority but the dangers of being a minority against the system in times of anxiety. For a rich account of the fate of outspoken opposition in the United States in early Cold War see Wang (as in n. 94). Outspoken opposition in the Soviet Union was rare, if there was any, until the Soviet Union obtained its first thermonuclear weapon, achieving a balance of power and deterrent based on the certainty of Mutually Assured Destruction (MAD). See: Ivanov (as in n. 41); Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (as in n. 71).

[114] Daniel J. Kevles, '"Into Hostile Political Camps": The Reorganization of International Science in World War I', *Isis*, 62 (1971):1 and Forman, 'Isis, No. 2, vol. 64, 1973' (as in n. 35). As Forman argued, the idea of science as an supranational enterprise is rarely questioned, though. Even in periods in which European physics community found itself divided "into hostile camps", German physicists did not question the basic tenants scientific internationalism because those tenants were the mainstay of German status as a scientific great power. A comparison or competition between nations is possible only if the rules and content of science are accepted as universally valid.

[115] Idem, 'Isis, No. 2, vol. 64, 1973' (as in n. 35), p. 155.

> allegiance to the ideology of scientific internationalism, the German scientists –and in some measure the Allied as well – no longer conceived of their political role in the classical passive terms. Rather, they regarded themselves as agents, or even as bearers, of the foreign policy interests of their nation and as such were often obliged to sacrifice the interests of German science, and their personal interests as scientists, for the sake of patriotic political posturing.

A similar pattern of political engagement of scientists can be found in the early Cold War. Circa 1955, after devoting all their knowledge and skills to solve the pressing problem of America's nuclear superiority, and having succeeded in their endeavor, Soviet physicists used their new prestige to promote changes in Scientific foreign policy of the Soviet Union. They once again emphasized the supranational character of scientific knowledge, and asserted the necessity of transnational social intercourse among scientists and of international collaboration in scientific work. In short, they rescued their commitment to the ideology of scientific internationalism, which had been downplayed through a long period of international isolation. However, they did so not in opposition the regime but as insiders who understood that they were facing the opportune moment, when the Soviet leadership was open to reform and international relations beyond the communist block. And more important, they became supporters and agents of the official foreign policy. As German and Allied scientists in the inter-war period, Soviet (as well as American) scientists in the late 1950s used the ideology of scientific internationalism to promote national interests.

A peculiarity of the post-Wold War II, when compared to the inter-war period, was that in that context, knowledge and military power were intertwined as never before. Access to information in fields as nuclear and high-energy physics, and laser physics beginning from 1958, for many could mean a shortcut to a major new weapon or at least a better position to assess the military capabilities of the other country. Thus, with both scientists and government concerned with national defense, as Krige concluded in his article on president Eisenhower's Atoms for Peace and the Geneva conference on peaceful uses of atomic energy,

> "in the context of Cold War rivalry scientific internationalism and scientific nationalism were two sides of the same coin. The first pushed back the frontiers of security restrictions and mutual distrusts, enabling scientists to build together a shared body of knowledge. The Second exploited that trust to learn what others were doing, to establish the

> limits of what they could speak about freely, and to access the dangers that may lurk behind what was left unsaid."[116]

As the report written by the Soviet delegation suggests, Soviet laser physicists learned quickly how to exploit the Iron Curtain, namely how to use the Cold War to promote their own agenda. Prokhorov, Basov were genuinely interested in the academic exchange and collaboration with American physicists. It brought them prestige at home and abroad. Foreign trips were allowed to none but a few privileged Soviet citizens and were included as distinctive accomplishments in professional biographies (at least until the end of the USSR, the first foreign trip is listed as a major event in the biography of Soviet physicists).[117] However, when they turned to their patrons in Moscow to justify or foster international exchange, their emphasis was mostly on the importance of the scientific exchange for information gathering, which, in turn, was viewed as crucial to national defense. As Krige also concluded studying the same period, "This double movement was indeed constitutive of scientists'behavior. One chord was struck when they spoke to their colleagues abroad, the other when they spoke to their patrons in Washington or Moscow."[118]

During the Cold War laser scientists would play simultaneously in multiple fronts that were already formed even before the first laser was launched. On one front, they would devote enormous effort to solve some "urgent practical problems" - devising anti-ballistic missiles, anti-aircraft systems and other weapons. On other front, they were part of efforts to cool down the heat generated by the military buildup, and to keep the Cold War cold, using science as diplomatic tool. For instance, "Basov was a member of academies and scientific societies of several countries. On a voluntary basis, he participated in the work of organizations such as the World Federation of Scientific Workers, the Soviet Peace Committee, the World Peace Council".[119] And yet, friendly scientific trips to conference or invitation for foreign scientists to visit the Soviet Union became valuable opportunities for information gathering. In a nutshell, in the Cold War scientists could be warriors, spies, as well as bearers of the white flag.

[116] Krige (as in n. 21), p. 167.
[117] Bebikh, Goncharova and Zhukova (as in n. 88); Pavel P. Pashinin, 'A brief sketch of the scientific, organizational, educational and social activities of Alexander M. Prokhorov', in: *Alexander M. Prokhorov - Academic Biobibliography* (2004).
[118] Krige (as in n. 21), p. 180.
[119] Vladimir Valentinovich Fortunatov, *Noveishaia istoriia Rossii v litsakh, 1917-2008*, (Sankt Peterburg: Izdatelskii Dom Piter, 2009), p. 126.

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5 Further research: Youth, Science, and Politics in Late-Stalinist Russia

Some historians, writing with the privilege of hindsight, after the Soviet Union is gone, have attributed the flow of young talents to science and technology to safety, arguing that young people chose those fields because the risks of political prosecution were few when compared to humanities. According to Yale Richmond, for example, "Talent in the Soviet era was attracted to science and technology, where opportunities were many and the risks were few".[1]

What the narrative presented in this dissertation and recent biographies of other Soviet physicists suggest is that the abundance of opportunities indeed played a major role in their choice, but considerations about risks played little or none. On the contrary, without taking in account natural inclination, many were driven to science because they believed in the Soviet Project and thought that as scientists they could give a greater contribution to build communism. Alexander Prokhorov and Nikolai Basov actively engaged with the official policies and were unabashed supporters of the regime throughout their lives. As argued in the chapters above, their initial careers were in tune with the needs and policies of Soviet Society. After returning

[1]Richmond (as in n. 31), p. 71. Richmond is part of the first generation of "Sovietologists" who took part in the Academic exchange between the United States and the Soviet Union in the 1960s, living for long periods in the Soviet Union. In recollections about their exchanges described in Richmond's book, scholars from that generation have described their experiences as something that gave them a "real feel for Russia" (52), or that "exposed [them] to the Russian 'psyche', to how Russians thought"(71). No doubt their experiences were invaluable, I must confess, I felt a bit envious, but I also felt scholarly disquieted. Those quotations, and the descriptions of the experiences, suggest a willingness to take the conclusions drawn from their experiences as something timeless, of seeing the "Russian psyche" and something immutable. They lived in Russia when the dissent movement began to gather momentum, specially among the intelligentsia, but the Soviet Union of the 1960s was significantly different of the Soviet Union of the 1930s and even 1940s (as discussed in chapter 4, the 1950s was a transition period in many regards).

I believe, Loren Graham has made a similar case in Loren R. Graham (as in n. 23) (and perhaps Richmond's assertion was based on Graham's studies). While writing this piece I did not have access to Graham's book to justify my belief.

from the war Prokhorov engaged in research whose appeal, according to his adviser, was "dictated by the social needs of that time". When afterwards, Prokhorov and his students Basov and Alexander Barchukov pioneered the new field of microwave spectroscopy "All [their] thoughts were about mastering the physics in full measure in order to develop [their] national economy",[2]. That research led to the maser and laser and indeed gave a remarkable contribution to the Soviet economy. In the context of the late 1950s it was used to develop new military technologies and to promote détente through scientific diplomacy. Scientific collaborations with American physicists were also used to assess whether the research in the field was being used in the US to develop military technologies. Their affiliation to the Communist Party and their close involvement with high-level officials are also indication that they were not afraid of getting involved in politics, but the politics they did was supportive to the regime.

That some young people were driven to science because they believed in the importance of science and technology to build communism is explicit in the biography of yet anther prominent Soviet laser physicist - Zhores Alferov. "Alferov became a communist in the same manner in which others become Catholics or Baptists: by way of his family and cultural upbringing."[3] He was one of those whom historian Juliane Fürst has called "communist perfectionists", faithful communists who criticized the Soviet leadership, if they did so, only for its inability of living up to the communist ideals they had been taught to believe from early childhood.[4] Zhores Alferov, named after the French socialist leader Jean Jaurès, and his elder brother Marks Alferov, named after Karl Marx, were educated to be exemplar communists. They grew up listening to his father's stories about the revolution "with sinking hearts", what perhaps helped them to embody early communist ideals such as internationalism and the Enlightenment dream of using science to perfect society[5].

In 1941 Marks Alferov was 17 when he enrolled at the Urals Industrial Institute "believing that energy was the key to the future."[6] However, upon turning 18,

[2]Basov, 'Interview with Dr. N. G. Basov by A. Guenther, on 14th September 1984. Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.' (as in n. 4).

[3]Kojevnikov, 'Physics World, No. March, 2011' (as in n. 64), p. 47.

[4]Fürst, *Stalin's Last Generation: Soviet Post-War Youth and the Emergence of Mature Socialism* (as in n. 73), pp. 327– 335.

[5]This biographical account is based mostly on Alferov's autobiography published in the Nobel prize page Alferov, 'Zhores I. Alferov - Biographical' (as in n. 65), but I have also drawn from his biography by Josephson, *Lenin's Laureate: Zhores Alferov's Life in Communist Science* (as in n. 65), and Alexei Kojevnikov's review of that biography, Kojevnikov, 'Physics World, No. March, 2011' (as in n. 64).

[6]Alferov, 'Zhores I. Alferov - Biographical' (as in n. 65).

Marks interrupted his studies to volunteer to defend his homeland against fascism. He would die the following year, but in the interlude he visited his family back home for a few days that would mark Zhores Alferov for life, and shape the image of Marks that became a life-long role model to Zhores. Half century later, Zhores Alferov recalled that last meeting with his brother: "I often look back and reflect on those three days, on his description of the war, his youthful enthusiasm and faith in the power of science, technology and human intelligence."[7]

Influenced by his brother, Zhores Alferov decided to become a scientist, beginning his career at the Leningrad Physico-Technical Institute in 1953, where he participated in the creation of the Soviet semiconductor electronics and in the invention of few semiconductor devices, including the first continuous semiconductor laser operating at room temperature. The first semiconductor lasers obtained in the early 1960s were inefficient and could not work in room temperature. Alferov's experience and knowledge of semiconductors permitted him to understand that those limitations could be overcome by using semiconductor heterostructures. With his students and some young colleagues, he embarked in an audacious, chancy project to develop the first laser based on heterostructure. Despite skepticism of most of his colleagues, including his former supervisor, Alferov and his small group continued the work and in 1967 managed to synthesize the first ideal pair of semiconductors gallium arsenide (GaAs) and aluminum gallium arsenide (AlGaAs) with the right properties for a laser medium. In 1969 Alferov presented their results in a conference in the USA in which his talk "produced an impression of an exploded bomb on American colleagues." That was the final sprint of the competition for being the first in getting the continuous wave operation of laser at the room temperature. In 1970 they "won the competition overtaking by a month"[8] a group working at Bell Telephone Laboratories. For that work Alferov was awarded the 2000 Nobel prize in physics.

Alferov's life and scientific career are chronicled by Paul Josephson in *Lenin's Laureate*. Josephson, however, downplays Alferov's communism in an attempt to render his narrative "sufficiently anti-communist and thus more familiar to its intended readers".[9] Building the narrative as the saga of a scientist fighting against the system, he was unable to explain satisfactorily key points of Alferov's career and

[7] Alferov, 'Zhores I. Alferov - Biographical' (as in n. 65).
[8] Ibid..
[9] Kojevnikov, 'Physics World, No. March, 2011' (as in n. 64), p. 47.

his commitment to communism. For example, as Alexei Kojevnikov has pointed out in his review of Josephson's book:

> Josephson often recites the mantra that the Soviet system did not respect the autonomy of researchers and pushed them too strongly towards applied results. But at least in Alferov's case, the blame is misdirected. "The system" trusted Alferov enough to support his research despite his supervisor's doubts. Meanwhile, in the US, Herbert Kroemer filed a patent for the double-heterostructure laser simultaneously with Alferov in 1963, but was refused support by his employer, Varian Associates in California's Silicon Valley, as "the device could not possibly have any practical applications". In the end, Kroemer shared half of the 2000 Nobel prize with Alferov.[10]

Kojevnikov believes that "a better strategy would have been not to downplay Alferov's communism, but to explain it as a modern variety, which has evolved about as far from its original version as modern global capitalism has from its origins in racist, slave-holding colonialism."[11] Zhores is part of a generation called 'children of the Twentieth Congress', after the 1956 congress of the Communist Party of the Soviet Union in which Nikita Khrushchev delivered his 'secret speech' denouncing Stalin's crimes and cult of personality, among which "there were many infected with messianic drive to reform ideology and society on true socialist principles lost under Stalin".[12] His faith in the power of science and technology as keys to that transformation is evident in Josephson's book in episodes as the one in which Alferov and other LETI students helped design a power station and worked in several brigades in competition with each other to push the project forward, an strategy typical of the times of building socialism in the 1930s (when whole cities were built from scratch). Then it was being employed to build communism[13].

How representative were Prokhorov, Basov, and Alferov of the youth of their generations (those formed in the first decade after to WWII)? Other historical studies on scientists and engineers of those generations point toward similar conclusions.

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[10] Kojevnikov, 'Physics World, No. March, 2011' (as in n. 64), p. 46.
[11] Ibid., p. 47.
[12] Melanie Ilic, 'Introduction', in: Idem, editor, *Soviet State and Society under Nikita Khrushchev*, (New York: Routledge, 2009), p. 20.
[13] Josephson, *Lenin's Laureate: Zhores Alferov's Life in Communist Science* (as in n. 65), p. 89. For a description of the enthusiastic building of socialism in the 1930s see Kotkin, *Magnetic Mountain: Stalinism as a Civilization* (as in n. 72).

Slava Gerovich tells us that "The engineers' belief in a technological utopia fitted well the Marxist view of scientific and technological progress as a foundation for building a better society."[14] According to Josephson, "the faith among Soviet scientists and citizens that they would build communism in their lifetimes would remain strong until the 1970s."[15] Even later dissidents as Andrei Sakharov, the most famous among scientists, lacked the critical view of Soviet society in their youth necessary to choose a career taking in account the risks of political prosecution. Many of them, and Sakharov seems to fit well this group, became dissident because of the growing chasm between the ideal image of a communist society they were taught to believe and the reality they were facing. For Sakharov that conflict surfaced when, after he had given his contribution to the hydrogen bomb, dizzy with success, he tried to intervene in important political affairs without knowing the rules and rituals of Soviet polity.[16]

Thus, it seems, young people were driven to science not to scape political prosecution, but because they learned to believe that science was the key to modernize the Soviet Union. However, to support this conclusion we still need studies on how youth interacted with the Soviet ideology after mid-1930s, after the excesses of the cultural revolution were reigned in, the revolutionary mood gave way to a more conservative patterns and the academic system, as society at large, became increasingly disciplined and militarized.

In the chapter Convergence of Cold War Science I have already began to approach that question when discussing militarization and secrecy in Soviet science, but it escaped the purpose of the paper to discuss in depth the process of becoming a scientist in the period. As I conclude this dissertation and look ahead to new research topics I see this as a promising one for its relevance both to history of Soviet science and to on going debates in cultural history of the Soviet Union about how the alternative modernity consolidated in the Soviet Union during the 1920s and early 1930s changed as the Soviet Union became increasingly industrialized and a new urban middle class, composed of several professional groups, began to emerge.

[14] Slava Gerovitch, 'Stalin's Rocket Designers' Leap into Space: The Technical Intelligentsia Faces the Thaw', *Osiris*, (2008):23, p. 208.

[15] Josephson, *Lenin's Laureate: Zhores Alferov's Life in Communist Science* (as in n. 65), p. 118. Josephson's statement is all the more significant, given that his narrative is overtly anti-communist.

[16] The ptoblem was less in Sakharov's attempt at influencing political affairs than in the way he did it, because in the 1950s the spheres of science and politics began to blend exchanging members. With politicians being elected members of the Academy of Sciences and Scientists, as Nikolai Basov, becoming politicians themselves. Kojevnikov, 'HSPS, No. 1, vol. 30, 1999' (as in n. 14).

My interest in the debate on Soviet modernity was kindled by a curious recollection by Galina Prokhorova which can be illustrated in the following episode:

In a cold night in the Moscow winter of 1940, three students of the Lebedev Institute of Physics, after eating onion, were preparing to leave for their nightly walk. The temperature was well bellow -15^oC. One of the students, nicknamed Pava, protested. It was late, too cold, and as usually, he had to wake up early morning next day. But the other two, Sasha and Lenia, "forced" him out anyway. That was part of the Iron Principle of Life (IPL), a special regime adopted by their group *(kompania)* which was described in the following terms: "Before going to bed it is obligatory to go for a walk, skating or skiing; do not eat sweets, instead of sweets eat onion". The principle involved also working six days a week at the Institute, Sunday was the library day, which they usually spent reading, to become more cultured, but they could as well go skiing in the woods. Besides, they also found time to take part in exalted discussions (*diskutsii*) happening at the time.[17]

The actors of the episode were three aspirants (graduate students) admitted by the Lebedev Institute of Physics (FIAN) in 1939. Two of them would later reach the top of the Soviet academic hierarchy. Sasha, Alexander Prokhorov, had just graduated from the Leningrad State University, and as shown in chapter 3 and 4, would become an influential academician and a Nobel laureate. Lenia, Leonid Maksimovich Brekhovskikh, had graduated from Perm University and would also become an academician. He was Prokhorov's companion for decades (later they lived in the same building and their families continued their traditional nightly walks, whenever possible, together).[18] Pava, Pavel Emmanuelovich Nemirovskii, had graduated from Odessa University; he was the only one who did not become an academician, but he did obtain a degree of doctor of physical and mathematical Sciences (doktor nauk).

The reader might be wondering what eating onion and nightly walks has to do with becoming a scientist. When I first came across that episode described by Galina Prokhorova, Alexander Prokhorov's wife, it struck me as "weird". But as I became more familiar with cultural history of Stalinism, specially of Stalinist subjectivity, which makes large use of memoirs, letters and other text for analysis of discourse, I began to make sense of it. Those actions may be meaningful in the Soviet cultural context of the late 1930s. They may be, for example, an exercise of discipline and self-improvement esteemed in a militarized society on the eve of the WWII and

[17] That specific episode did not actually happened. It is my creation, based on the description of the Iron Principle of Life by PROKHOROVA (as in n. 18)

[18] Ibid..

obsessed with the idea of making a superior kind of human being – "the new Soviet man".

The "new Soviet man" is ubiquitous in the literature on Soviet history. According to Richard Stites, "Arguments over the "new Soviet man" in the pedagogical world—a subject covered ad nauseam in hundreds of books—focused on raising a new generation in a spirit that would allow it or its successors to live virtuously under socialism and eventually, by shedding greed, egoism, and consumerism, under communism."[19] All this effort resonated in the population as a whole. Private documents from the Stalin era show that individuals took onto themselves an agenda of self-transformation and self-perfection. Historians have began to take seriously the role of that ideology on the formation of identity of several groups within Soviet society,[20] but the impact of this on the formation of Soviet scientists still awaits for historians of science.

Intending to make a "thick description"[21] of the episode to show what it reveals about the process of becoming a scientist in late-Stalinist Russia, I began to read more cultural histories of the Soviet Union and I realized that it might be even more meaningful than I thought at first. In the end I decided to leave that endeavor for future works because a proper thick description of the episode demands a reading hard to fit within the time schedule of my doctorate and relies on an embryonic debate among cultural historians began last year (2014) with an essay by Duke University's historian Anna Krylova.[22]

As discussed in the chapter *The Cold War and Western Perspectives on Soviet Science*, the work which marked the arrival of the cultural turn to Soviet history, and set a broad agenda for the new generation of historians growing up in the post-Cold War, Stephen Kotkin's Magnetic Mountain portrayed "Stalinism as a civilization". For him "Stalinism was not just a political system, let alone the rule of an individual. It was a set of values, a social identity, a way of life."[23] He saw the Soviet Union as an alternative, anti-liberal and anti-individualist modernity, formed in opposition to the European and North American modernity. Kotkin's conception of Bolshevik

[19] R Stites, *Revolutionary dreams: Utopian vision and experimental life in the Russian revolution*, (New York: Oxford University Press, 1989), p. 119.

[20] For a call to take the ideology and the agenda of self-transformation and self-perfection in Soviet culture see Halfin and Hellbeck (as in n. 8).

[21] Thick description of a human behavior explains not just the behavior, but also the cultural context in which the behavior becomes meaningful to an outsider. Geertz (as in n. 52).

[22] Anna Krylova, 'Soviet Modernity: Stephen Kotkin and the Bolshevik Predicament', *Contemporary European History*, 23 (2014):2.

[23] Kotkin, *Magnetic Mountain: Stalinism as a Civilization* (as in n. 72), p. 24

modernity was one of those powerful ideas that burst upon the intellectual landscape with a tremendous force resolving so many fundamental problems at once that they seem to promise that they will resolve all fundamental problems.[24] It shaped a decade an a half of cultural studies on Soviet Union, and ended up being extended to later periods of Soviet history by Kotkin himself and by the following generation of cultural historians.[25] Last year, however, Anna Krylova, herself one of the historians who had been guided by Kotkin's works, questioned the underlying assumption of Kotkin's concept of Bolshevik modernity and its uncritical application by cultural historian to periods after mid 1930s. Krylova contends that from mid 1930s a post-Bolshevik modernity began to be shaped by new social and professional groups that appeared as a result of the modernization of Russia and would over a period of half century promote a "gradual and never complete 'de-centring' of the Bolshevik cultural formation with the post-Bolshevik one."[26]

Scientists became one of the most powerful interest groups of Soviet Society in the postwar and they were a professional group with a cultural formation of its own, and therefore, had a significant potential to promote the "gradual and never complete de-centring" of the Bolshevik cultural formation. Becoming a scientist was a process of leaving a school guided by the Bolshevik ideal of making a New Man to become part a professional group with its own ideology. The young student was a point in which the Bolshevik ideology blended with the ideology of scientists, which, as discussed in the section on Mandelstam's school in chapter 3, was strongly influenced by the world view of leading scientists. Thus an inquiry on the process of becoming a scientist in late-Stalinist Russia may help to understand not only Soviet science, but the face of Soviet modernity and how scientists helped to reshape it after the World War II.

That agenda does not diverge with the research presented in this dissertation. On the contrary, the case studies on laser history which in various points of the dissertation I remarked that would be left to be explored in future works all may

[24] Geertz (as in n. 52), p. 3.

[25] Kotkin, *Armageddon Averted: The Soviet Collapse, 1970-2000* (as in n. 60); Krylova (as in n. 22).

[26] Idem (as in n. 22), p. 191. Krylova correctly points out that "An integration of the complex and variegated history of the twentieth-century liberal and economic thought that was preoccupied with the question of the individual and the social, and with encompassed positions varying between welfare liberalism and neo-liberalism seems to be paramount for producing a rethinking of Soviet socialism." ibid., p. 192. Many of the points made by Krylova on the role of professional groups are approached in Galbraith's reflection on the role of the techonostructure on the new industrial states. See: John Kenneth Galbraith; Andrea D. Williams, editor, *The Essential Galbraith*, (Boston, New York: Houghton Mifflin Company, 2001).

offer a different, complementary perspective on the training of Soviet scientists in late-Stalinist Russia. The cases of Valentin Fabrikant, Evgeny Zavoisky, Alexander Prokhorov, Nikolai Basov, and Zhores Alferov, each move slightly forward in time and represent a diverse spectra of initial backgrounds and trajectories. Together those biographies may yield not only a comprehensive account of Soviet laser history, but a fresh perspective on Soviet history as well; they are a promising research agenda for the next 5 years or so.

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Appendix: Selected Archival Material

This appendix contain a collection of historical sources for maser and laser history in the Soviet Union collected during two visits to the Archives of the Russian Academy of Sciences, commonly refereed to by its Russian acronym ARAN, in the summer of 2013 and winter of 2013-2014.[1] The documents are separated by type, and within each type, organized in chronological order. They consist of laboratory plan of problems (a kind of research proposal), laboratory reports, the verbatim of the defense of A. M. Prokhorov to obtain the title of Candidate of Sciences, and letters regarding the participation of Soviet physicists in the meeting of the Faraday Society held in England in 1955. A brief description is given before each document and a table of contents is found at the end of the appendix.

[1] I am thankful to my adviser Olival Freire Júnior and my co-adviser Alexei Kojevnikov for sponsoring those trips with funding from their research projects, to the archivist Irina Tarakanova for her patience and help, and to my wife Daria Chusovitina, who helped me in every single stage of this research.

Selected Plans and reports of the Laboratory of Oscillation of FIAN (1944 - 1961)

The Physical Institute of the Academy of Sciences of the USSR maintained the practice of filing yearly plans of problems and laboratory reports for each of its laboratory, usually written by the head of the laboratory. My archival search focused on the Laboratory of Oscillations and the main criteria used to copy the documents were their relevance as source for maser and laser history or for history of Mandelstam's school of oscillations.

Prospective plan for the work on the theory of oscillations and radiophysics (1944)

This prospective plan, written by Sergei Vavilov on Aug 15,1944, describes the main problems being addressed in the Laboratory of Oscillations of FIAN, emphasizing their relevance in that context.

Reference:

Vavilov, S. I. 1944. "Perspektivnyi Plan Rabot Po Teorii Kolebanii I Radiofizike. Archives of the Russian Academy of Science (ARAN), f.532; op.1, d. 90, p. 10."

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ПЕРСПЕКТИВНЫЙ ПЛАН РАБОТ

по теории колебаний и радиофизике.

В настоящее время и на ближайшие годы в области электромагнитных колебаний и радиофизики основное внимание несомненно будут занимать следующие проблемы:

В области колебаний центральной проблемой являются вопросы колебаний в системах с "большой" нелинейностью и большой "глубиной модуляции" параметра, а также вопросы нелинейных колебаний в системах со многими степенями свободы. Существующая в настоящее время теория не позволяет разрешить эти вопросы в общем виде, поэтому представляется необходимым развитие теории нелинейных колебаний в этих направлениях.

Тесно связанными с этой проблемой являются чрезвычайно актуальные в настоящее время вопросы генерации, модуляции и приема колебаний ультравысоких частот, которые приобрели чрезвычайно важное значение не только собственно для радиосвязи, но и для ряда других новых применений радиоволн (радионавигация, радиолокация, телеавтоматика и др.). Поэтому чрезвычайно актуальным является создание теории генерации колебаний сверхвысоких частот, связанное с разработкой новых нелинейных методов трактовки, а также проведение обширных экспериментальных исследований в этой области.

Кроме того, в области радиофизики остаются по прежнему актуальными вопросы уверенной радиосвязи, особенно на большие расстояния, требующие дальнейших исследований, в области распространения радиоволн всех диапазонов частот и связанного с этим дальнейшего изучения явления прохождения радиоволн в ионосфере и тропосфере (а, следовательно, и свойств ионосферы), а также дальнейшей разработки методов борьбы с радиопомехами (разработка новых методов модуляции и приема).

Директор Физического Института
Академик

15.VIII-44г.

(С.И.Вавилов)

Report for 1946

Report on the research on nonlinear oscillation supervised by Alexander Andronov, Mikhail Leontovich, Sergei Rytov, and Semion Khaikin. It presents several examples of applications of the theory oscillations to solve practical problems.

Reference: "Otchet za 1946. ARAN, f.532, op.1 d.112, pp. 11-14." 1946.

ведено исследование потерь и диэлектрических постоянных в полях высокой частоты (λ = 20 см) различных материалов: титанат бария, рутил (канд.физ.мат.наук Маш Д.И. и лаборант Высоцкая М.А.) и некоторых специальных веществ (канд.физ. мат.наук Маш Д.И. и Бунимович В.М.). Статья Д.И.Маша о титанате бария и рутиле находится в печати (Ж.Т.Ф.).

б) Доктором физ.мат.наук С.М.Рытовым проведено теоретическое исследование применимости термометрического метода для абсолютных измерений в полях сверхвысокой частоты и показана возможность его усовершенствования для области сантиметровых волн, где размеры термометра не малы по сравнению с длиной волны. Работа доложена на Совещании Всесоюзного Радиосовета в Горьком в декабре 1946 года. Начата разработка специальных типов термометров (кандид. физ.матем.наук Д.И.Маш).

в) Проведены теоретические работы, касающиеся теории щелевых антенн (докторант Фельд Я.Н.), а также вопросов связи между об'емными резонаторами (докторант Бродский В.Б.)

г) Проведена большая работа по оборудованию лаборатории современной измерительной аппаратурой в области микрорадиоволн (канд.физ.мат.наук Д.И.Маш, А.Б.Меликьян и старший научный сотрудник В.М.Бунимович).

УI. Нелинейные колебания

Руководители: акад.А.А.Андронов, акад.М.А.Леонтович, доктор физ.матем.наук С.М.Рытов, доктор физ.мат.наук С.Э.Хайкин.

В 1946 г. по этой проблеме выполнены следующие работы:

А. Закончена монография акад.А.А.Андронова и доктора физ.мат.наук Г.С.Горелика по общей динамике машин и теории

Б. Разработаны доктором С.М.Рытовым ~~общая~~ некоторые вопросы теории синхротрона ~~[illegible]~~.

В. а) Развита теория нелинейных систем, близких к линейным системам Штурм-Лиувиллевского типа. Экспериментальная проверка выводов теории произведена с помощью специально сконструированного и осуществленного в лаборатории генератора сантиметровых волн. (Доктор физ.мат.наук С.М. Рытов, аспирант Жаботинский М.Е.).

б) Развита теория принудительной синхронизации самовозбужденного генератора с лехеровой системой. Статья послана в печать (ЖТФ). (канд.физ.мат.наук Прохоров А.М.).

в) по предложению члена-корр.АН СССР Люстерника Л.А. разработана и осуществлена модель электрического устройства для определения собственных значений и функций некоторых операторов (Штурм-Лиувилля и друг.) с помощью электрической схемы $R-C$. Статья послана в ДАН (канд.физ.мат.наук Прохоров А.М.).

г) Теоретически и экспериментально исследован вопрос о взаимном захватывании на расстоянии. Работа послана в печать (Изв.АН СССР) и доложена на Всесоюзном Совещании Радиосовета в Горьком в декабре 1946 г.(кандидат физ.мат.наук Горожанкин Б.Н.).

Г. По теме "Исследование электрических флуктуаций в нелинейных системах" проводилось изучение вопроса о распределении флуктуационных процессов в некоторых сопротивлениях и в токе электронных ламп с помощью ранее разработанного метода. Предварительное сообщение о наблюденных отступлениях от Гауссова распределения было напечатано в ДАН ———— № I, (канд.физ.мат.наук Пумпер Е.Я.). Теоретическое обоснование метода опубликовано в ДАН, ~~, № 1~~ (акад.М.А.Леонтович, кандидат физ.матем.наук В.И.Бунимович).

Д. По теме "Исследование свойств механического и электрического контакта при малых смещениях соприкасающихся поверхностей " - разработана методика исследования сопротивления контакта между соприкасающимися поверхностями при весьма малых смещениях поверхностей - порядка $1 \cdot 10^{-7}$см (динамический метод).

Обнаружен новый эффект - изменение электрического сопротивления контакта при малых тангенциальных смещениях соприкасающихся поверхностей. Сейчас изучается механизм этого явления и выясняется его связь с характером изменения механических сил и изменением сопротивления контакта при нормальных смещениях. Разработан интерференционный метод измерения амплитуд колебаний пьезокварцевых резонаторов, применяемых для получения малых смещений. (Доктор физ.мат. наук С.Э.Хайкин и аспирант А.Е.Соломонович).

б) По теме "Изучение поведения жидкостей при быстрых изменениях температуры" разработана методика наблюдения изменений проводимости электролита при быстрых изменениях температуры, получающихся вследствие адиабатического сжатия жидкости при распространении ультра-звука. ~~Установлено, что при частоте изменений температуры (частоте ультра-звука) порядка $5 \cdot 10^5$ герц проводимость электролита не успевает изменяться при изменениях температуры, т.е. обнаружен новый релаксационный эффект в жидкостях.~~

~~Сейчас начаты опыты на более низких частотах (звуковых), на которых, можно думать, изменения проводимости должны успевать за изменениями температуры. Эти~~ методика ~~опыты~~ позволяет определить времена релаксации проводимости, т.е. времена установления нового значения проводимостей при быстрых изменениях температуры. (Доктор физ.мат.наук С.Э.Хайкин, аспирант Я.И.Лихтер).

В истекшем году продолжалась работа по изданию трудов акад.Л.И.Мандельштама. Сдан в Издательство АН СССР том II. Подготовлен к сдаче том I.

VI. Исследование механизма свечения кристаллофосфоров.

Руководитель акад.С.И.Вавилов. и д.ф.-м.н. проф. В.Л.Левшин

В истекшем году исследование свечения кристаллофосфоров производилось в следующих направлениях:

1. Были обработаны и переданы в печать многочисленные материалы, полученные в 1941-1945 г.г. группой сотрудников лаборатории (В.Л.Левшин, В.В.Антонов-Романовский, З.Л.Моргенштерн, З.А.Трапезникова) по щелочноземельным фосфорам, обладающим высокой чувствительностью к инфракрасным лучам. В этих исследованиях был разработан новый тип светящихся составов, обладающих высокой способностью аккумулировать энергию возбуждения и отдавать ее в виде видимого излучения при действии на фосфор инфракрасных лучей. Были исследованы спектры, световые суммы, законы затухания свечения и другие оптические характеристики фосфоров и установлена роль отдельных составных частей фосфора: основного вещества и взаимодействующих активаторов - редких земель. В текущем году продолжались исследования новых фосфоров. Из новых результатов, опубликованных в текущем году следует отметить произведенное З.Л.Моргенштерн исследование соотно-

Notes on the five-years plan for the laboratory of oscillation of FIAN

The prospective five-years plan for the laboratory of oscillations focused on three basic problems that then were considered central in the field of oscillations:

1. Propagation of radiowaves
2. Scientific and technical mastering of ultra-high frequency waves
3. Further development of the theory of nonlinear oscillations

This document, signed by Nikolai Papaleksi on December 1, 1945, discusses the importance of each of those problems and the main issues to be addressed. It also lists how many people were then working on those problems and projects how many will be involved by the end of that five-years period.

Reference:

Papaleksi, Nikolai. 1946. “OBYASNITELNAIA ZAPISKA K PERSPEKTIVNOMU PYATILETNEMU PLANU RABOT LABORATORII KOLEBANII FIAN. ARAN f.532, op.1, d.111, pp. 14-18.”

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ОБ"ЯСНИТЕЛЬНАЯ ЗАПИСКА
К ПЕРСПЕКТИВНОМУ ПЯТИЛЕТНЕМУ ПЛАНУ РАБОТ ЛАБОРАТОРИИ КОЛЕБАНИЙ ФИАН

Перспективный пятилетний план работ лаборатории колебаний ФИАН включает в себе исследования по следующим основным проблемам, которые в настоящее время несомненно можно считать центральными в области колебаний.

I. Проблема распространения радиоволн.

2. Проблема научного и технического освоения волн сверхвысокочастотного диапазона.

3. Дальнейшее развитие теории нелинейных колебаний.

I. Проблема распространения радиоволн.

Несмотря на то, что в последние годы были достигнуты весьма значительные результаты, как теоретические, так и экспериментальные, в изучении проблемы распространения электромагнитных волн - радиоволн - и были выяснены многие, существенные для практических применений, стороны ее, что привело к созданию новой отрасли радиотехники - радиогеодезии и радиолокации, имеющих своей целью точное измерение расстояний и определения положения с помощью радиоволн, однако настоятельная необходимость в углублении и развитии работ в этом направлении далеко еще не отпала. Необходимость повышения точности определения положения с помощью радиоволн, будь-то радиоинтерференционными или импульсными методами, требует более точного знания величины скорости распространения радиоволн в действительных условиях (чем это имеет место в настоящее время (порядка 2-3.10⁻⁵)) и детального исследования различных природных факторов для того, чтобы их можно было точно учитывать. Таким образом первоочередной задачей ~~Лаборатории Колебаний~~ является возможно более точное измерение скорости распространения радиоволн в различных действительных условиях, что представляет и значительный самостоятельный научный интерес. Ввиду того, что природные факторы, влияющие на скорость распространения радиоволн различно проявляют себя в различных диапазонах волн как при распространении вдоль поверхности земли, так и при распространении вверх от земли, необходимо произвести экспериментальные исследования в самом широком диапазоне волн вплоть до миллиметровых и притом в самых различных условиях местности. Для выполнения этих работ необходимо усовершенствовать методику, создать ряд вариантов специальной измерительной аппаратуры, а также организовать базы в соответствующих местностях и экспедиции.

Ввиду того, что эти измерения должны быть обеспечены геодезическими определениями высокого класса, необходимо участие в этих работах геодезистов /ЦНИГАИК/, а также гидрографов /Главн. Гидр. Упр. ВМФ и ГУГУСМП)

Наряду с исследованиями распространения радиоволн вдоль земной поверхности на сравнительно небольшие расстояния, при которых влияние ионосферы сказывается лишь в виде поправок, необходимо также продолжить исследование условий распространения радиоволн в самой ионосфере, существенным образом определяющих уверенность дальней радиосвязи.

Для выполнения этих работ также потребуется организация баз и экспедиций, которые в значительной степени могут быть совмещены с базами и экспедициями для указанных выше работ.

В связи с работами по ионосфере намечается производство наблюдений во время солнечного затмения в 1947г. Число руководящих работников по ~~первой~~ этой проблеме должно быть доведено до 4 /сейчас 2/,а технических до 10 /сейчас 4/.

2. Проблема освоения волн сверхвысокочастотного диапазона.

Не подлежит сомнению,что очередным этапом в развитии радиотехники и вместе с тем и электротехники в широком смысле слова является дальнейшее развитие и освоение техники электрических колебаний сверхвысоких частот / деци-,санти и милли-метрового диапазона/.

Вопросы специальной связи,телевидения,радиолокации и радиогеодезии,различных применений в технике и медицине, а также вопросы ядерной физики делают теоретическую и экспериментальную разработку этой области одной из наиболее актульных проблем. Но помимо своего важного практического значения исследования в этой области представляют очень большой научный интерес,т.к. выдвигают ряд новых проблем для учения о колебаниях и в частности для теории нелинейных колебаний в системах с распределенными постоянными.

В первую очередь ~~Лаборатория Колебаний предполагает заниматься~~ намечается разработка методов измерений на сверх высоких частотах /вопросы метрики/. Как известно, с укорочением длины волн вопросы измерений все больше осложняются, а существующие в настоящее время методы дают лишь грубые,скорее качественные результаты.

Первостепенной важности являются также вопросы ста сущности говоря, определяют возможности их широкого практического использования.

Кроме них предполагается также исследование вопросов канализации и излучения волн сверхвысокочастотного диапазона.

Для выполнения всех этих работ потребуется оборудование вакуумной лаборатории, а также обеспечение тонкими механическими работами. Работа будет проводиться в контакте с НИИ 108 и другими институтами НКЭП. Число руководящих сотрудников 4 /теперь 2/, лаборантов и технических сотрудников 8 /теперь I/, в том числе I точный механик и I стеклодув.

3. Теория нелинейных колебаний.

Универсальное значение, которые приобрели вопросы нелинейных колебаний в различных областях физики и техники, требуют дальнейшего развития этой области.

~~Лаборатория Колебаний~~ Предполагается проводить работы по следующим проблемам:

а/ Вопросы общей динамики машин, как электрических так и паровых и др.

б/ Применение теории колебаний к движению релятивистских частиц, что имеет, в частности, значение для теории приборов, применяющихся в ядерной физике.

Далее, освоение сверхвысоких частот выдвигает ряд нелинейных проблем как в области стабилизации, так и генерации, трансформации частоты и приема сверхвысоких частот.

Кроме того ~~в лаборатории~~ будут производиться теоретические и экспериментальные исследования вопросов флуктуаций в нелинейных системах, важных для радиоприема, а также изучение с помощью динамических колебательных методов характера и природы сил, действующих на весьма малых расстояниях /порядка 10^{-7} см/ от свободной поверхности тел твердых или жидких. Эти последние вопросы имеют большое значение для выяснения явлений, имеющих место в электрическом контакте, при механическом трении и т.д.

Число руководящих сотрудников 6 /теперь 5/, лаборантов и технических сотрудников 6 /теперь 0/. Необходима также организация Вычислительного Бюро.

xx) → верно

ЗАВЕДУЮЩИЙ ЛАБОРАТОРИЕЙ
КОЛЕБАНИЙ
Академик Н. Папалекси /Н.Д.Папалекси/

" 1 " декабря 1945г.

Parts of the Report of FIAN for 1947 concerning the Laboratory of Oscillations

Three parts of FIAN's report for 1947 concerned works performed in the Laboratory of Oscillations:

The first is a summary of the study of nonlinear oscillations supervised by Alexander Andronov, Sergei Rytov, and Semion Khaikin. The second part, signed by Sergei Rytov, diagnoses that the Soviet Union fell short in translating the radio-interference methods developed by Mandelstam and Papaleksi into technology, and given the pace of development of radio devices in the West, the Soviet Union was catastrophically legging behind. A plan of action to overcame that problem is presented, suggesting to strengthen the ties between the Laboratory of Oscillations and radio industries and to further develop the research on propagation of radiowaves for various wave ranges and natural conditions. The third part of the report is a general assessment of works on nonlinear oscillations in the Soviet Union. It also discuss the repercussion of the theory of oscillations abroad and mentions a request to translate and publish Andronov and Khaikin's book in the USA.

Reference:

"Otchet za 1947. ARAN f.532, op.1,d.126, pp.8-70." 1947.

У ПРОБЛЕМА ПЛАНА РАБОТ ОТДЕЛЕНИЯ ФИЗ.МАТ.НАУК АН СССР.

НЕЛИНЕЙНЫЕ КОЛЕБАНИЯ.

Руководители: академик А.А.Андронов, доктора наук С.М.Рытов, и С.Э.Хайкин.

198 В плане 1947 года было предусмотрено дальнейшее исследование ускорителей заряженных частиц. Проведена дальнейшая разработка теории синхротрона. Разработан новый общий метод малых и медленных возмущений, который имеет значение для ряда задач теории нелинейных колебаний. Общность метода открывает широкие возможности для последовательного учета осложняющих факторов, имеющих место в реальных устройствах. Кроме предусмотренных планом работ был проведен ряд дополнительных исследований: исследование в области распределенных
200 автоколебательных систем; исследования модели высокочастот-
201 ного асинхронного мотора. Исследование теплового эффекта в электрических проводниках /для повышенных частот флюктуаци-
201 онного спектра/. Теоретические исследования кинематики флюк-
202 туационных процессов. Исследование механического и электрического контакта между поверхностями твердых тел при их малых смещениях /характер изменения электрического контакта обнаруживается по детекторному эффекту, возникающему при пропускании через контакт переменного тока синхронного с
203 частотой колебания пьезо-кварца/. Исследование влияния быстрых изменений температуры и давления на электропроводность электролита /разработана методика исследования весьма малых периодических изменений электросопротивления электролита, вызванных колебаниями давления и температуры, соответствующими распространению ультразвука в жидкости, позволившая исследовать температурный коэффициент и пьезо-коэффициент

слабого раствора азотнокислого серебра; исследование условий самовозбуждения механических автоколебаний /доказана
1.20 ошибочность теории скачков силы трения, разработанной Боуденом и Лебеном, а также теории Ишлинского и Крагельского; в результате работы установлено, что самовозбуждение колебаний с сухим трением возможно только при наличии падающего участка на характеристике силы трения, как функции скорости, и подтверждена справедливость теории автомеханических колебаний, развитой ранее сотрудниками Института./

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РАБОТА, подлежащая внедрению, по ЛАБОРАТОРИИ КОЛЕБАНИЙ ФИАН

Многолетние (с 1932 г.) исследования академиков Л.И.Мандельштама и Н.Д.Папалекси по распространению радиоволн привели помимо выдающихся научных результатов к созданию новой области практического применения радио- интерференционной дальнометрии и координации. Интерференционные методы отличаются от импульсных -(локационных) чрезвычайной простотой практического осуществления и высокой точностью (более чем I/I0000 дистанции). Они, конечно, не заменяют локации, но дополняют ее и решают те задачи, которые для локации непосильны.

В течение более I0 лет разработка радиоинтерференционных приборов (для целей навигации (морской и воздушной), гидрографии, аэрофото-съемки и геодезии) влачила жалкое существование, не имея промышленной производственно-технической базы. Тем не менее почти-что самодельные аппараты уже много лет и с исключительным успехом применяются Главсевморпутем, ЦНИИГА и К'ам, Гидрографическим Управлением ВМФ и ФИАНом. Лишь в 1944 году удалось поставить техническую разработку и изготовление первого промышленного образца радиоинтерференционного "координатора", в НИИ № I0 Мин.Судостр.Промышленности. Сейчас он проходит испытания и на этом дальнейшая разработка прекращается, так как Комитет по радиолокации исключил навигационную тематику из профиля НИИ-I0. Следует отметить, что в НИИ-I0 в процессе работы были созданы квалифицированные и обладающие опытом в этом деле кадры радиоинженеров.

В итоге, несмотря на то, что развитие радинтерференционных методов заграницей, начавшееся лишь во время войны, быстро идет вперед и привлекает сейчас не меньшее внимание, чем развитие радиолокации, у нас в Союзе работа практически остановлена и через год-два наша техническая мысль в этой области катастрофически отстанет. Актуальнейшими задачами являются поэтому интерференционных устройств в отраслевом институте типа НИИ-10 (лучше всего в самом НИИ-10), в виду наличия в нем опытных кадров и установившейся связи с ФИАН) при консультации ФИАН.

2) Развертывание исследовательской работы в области смешанных фазово-импульсных систем, также предполагающее тесную кооперацию с промышленностью, изготовляющей аппаратуру.

3) Развертывание исследований по распространению радиоволн различных диапазонов и в различных природных условиях, осуществимое только при условии разработки и изготовления совершенных промышленных образцов интерференционной аппаратуры на эти диапазоны.

Зам.зав.лабораторий Колебаний ФИАН,
Профессор

/С.М.Рытов/

14.IУ.47г.

РАБОТЫ ПО НЕЛИНЕЙНЫМ КОЛЕБАНИЯМ В СОВЕТСКОМ СОЮЗЕ

Среди разнообразных колебательных процессов, которые наблюдаются в природе или осуществляются в специальных устройствах очень важное место занимают, так называемые, „нелинейные колебания". Такие нелинейные колебания происходят, например, в обычных часах, во многих музыкальных инструментах (смычковые и духовые инструменты), в ламповом генераторе радиопередатчика и во многих более сложных устройствах (параллельно работающих электрических машинах, некоторых приборах для автоматического регулирования процессов, автоматического поддержания курса самолета и т.д.).

Несмотря на бо́льшую важность и широкое распространение нелинейных колебаний, в течение долгого времени не существовало единой теории этих явлений. Каждый отдельный случай нелинейных колебаний рассматривался отдельно специально для него подбирались какие-либо частные приемы теоретического рассмотрения.

Советские ученые (ныне покойные) академики Л.И.Мандельштам и Н.Д.Папалекси впервые поставили во всей широте вопрос о необходимости создания единого учения о нелинейных колебаний. Один из учеников Л.И.Мандельштама ныне академик А.А.Андронов нашел нужные для этого математические методы и с этого времени (1928 г.) учение о нелинейных колебаниях начало быстро развиваться. Не только были созданы теории многих важных для техники устройств (например, теория лампового генератора), но был открыт ряд новых явлений (различные новые типы резонанса, новые методы преобразования электрических колебаний и т.д.), нашедших себе важное применение на практике, например, при измерении расстояний с помощью радиоволн (радиодальномеры Л.И.Мандельштама и Н.Д.Папалекси).

В настоящее время исследования по нелинейным колебаниям продолжаются в Физическом Институте им.П.Н.Лебедева АН СССР (в лаборатории им.Л.И.Мандельштама), в Горьковском Государственном Университете и в Московском Государственном Университете. В Горьковском Государственном Университете академик А.А.Андронов со своими сотрудниками, применяя методы теории нелинейных колебаний, разрешил целый ряд важных для практики задач из области автоматического регулирования и создал новое направление в теории регулирования.

Благодаря работам академиков Л.И.Мандельштама и Н.Д. Папалекси и их учеников, а также работам советских математиков академика Н.М.Крылова и члена-корреспондента АН СССР Н.Н.Боголюбова, центр исследований по нелинейным колебаниям примерно с 1930 года переместился в СССР.

Работы советских ученых по нелинейным колебаниям широко известны заграницей. По просьбе руководителей международного научного радиотехнического союза (URSI) советские ученые дважды представляли этому союзу доклады о своих работах по нелинейным колебаниям. Издательство Принстонского Университета (США) обратилось к авторам вышедшей в СССР в 1937 году монографии по теории колебаний (А.А.Андронову и С.Э.Хайкину) с просьбой разрешить издание этой монографии в США.

Plan Problems for 1953

This plan of problems to be studied laboratory of oscillations in 1953 lists Prokhorov as the leader of the group working on the determination of nuclear moments using microwave spectroscopy. They were working toward measuring the quadrupole moments of radioactive isotopes of Iodine and Selenium and improving the resolution of a stark spectroscope which could be used to determine moments of short-living nuclei (~8 days). The plan stresses that those measurements were important to develop theories of nuclear physics.

Reference:

Prokhorov, A. M. 1953. "Problemny Plan za 1953. ARAN, F. 532, Op. 1, d.216, pp. 1, 28."

1

№№ пп	Наименование темы, руководитель, сроки начала и окончания.	Объем работы в 1953 г.	Выполнение плана и сущность полученных результатов	Применение результатов
I	2	3	4	5

Проблема III: "МАГНИТНЫЕ МОМЕНТЫ ЯДЕР".

	Определение моментов ядер методом микроволновой спектроскопии. Руководитель - доктор физ.-мат. наук А.М.Прохоров. Работа выполн. [illegible] ке. Тема переходит на 1954 г.	Измерение спинов и квадрупольных моментов радиоактивного изотопа J^{131} или Sc^{75}.	Построен и испытан радиоспектроскоп со штарковской модуляцией и необходимой защитой для работы с радиоактивными веществами. Были измерены спин и квадрупольный момент J^{127}. Моменты радиоактивного изотопа J^{131} или Sc^{75} измерены не были из-за отсутствия препаратов этих веществ. Вместо этого была проведена работа по увеличению чувствительности радиоспектроскопа примерно в 10 раз.	Созданная аппаратура может быть применена для определения моментов короткоживущих ядер (~8 дней), что представляет интерес для теории ядра.

Экз.№___

Тема: ОПРЕДЕЛЕНИЕ МОМЕНТОВ ЯДЕР МЕТОДОМ МИКРОВОЛНОВОЙ СПЕКТРОСКОПИИ.

Исполнитель – Физический Институт им. П.Н.Лебедева АН СССР

Научный руководитель – доктор физико-математ.наук А.М.Прохоров

В 1953 г. должны быть измерены спины радиоактивного ядра иода или селена. Однако указанная работа не выполняется ввиду отсутствия необходимых химических соединений (CH_3J или COS_e) с нужной концентрацией радиоактивного изотопа. Следует отметить, что благодаря усовершенствованию радиоспектроскопической аппаратуры удалось сильно повысить ее чувствительность, так что необходимая для работы концентрация радиоактивного элемента иода снижена в соединении CH_3J с 30% до 0,5%, а радиоактивного элемента селена – с 1% до 0,05% в соединении COS_e . Однако и такие малые концентрации пока еще не могут быть получены соответствующими институтами, что указывает на слабость развития радиохимии в Советском Союзе.

В 1-ом полугодии, вместо исследований радиоактивных изотопов, проведена теоретическая работа, разработана методика и построена аппаратура для определения моментов ядер, спины которых предполагаются равными нулю или половине.

Ввиду отсутствия необходимых радиоактивных соединений, во-втором полугодии будут продолжены исследования по определению моментов стабильных изотопов, равными нулю или половине. Предполагается определить спин O^{18}.

Зам.зав.лабораторией ФИАН – А.М.Прохоров
доктор физико-математ.наук

[signature]

Report for 1955

Report of the sector of radiospectroscopy of the Laboratory of Oscillations. In 1955 radiospectroscopy was already the main sector sector of the laboratory. Having returned from a trip to England, Prokhorov writes an introduction comparing the research on microwave spectroscopy at FIAN against the development of the field abroad and summarizes the activities of his group since 1952, when they began working in the field.

In 1955 the research in the laboratory was divided in two lines, namely application of molecular beams to radio spectroscopy, which included the development of the maser (molecular generator) and determination of nuclear moments of nuclei, the latter by request of the Council of Ministers of the USSR.

Reference:

Prokhorov, A. M. 1955. Otchet po Sektoru Radiospektroskopii Laboratorii Kolebanii FIAN za 1955 God. ARAN, F. 532, Op. 1, D. 251. pp. 83-39.

О Т Ч Е Т

по сектору радиоспектроскопии лаборатории колебаний ФИАН за 1955 год.

В в е д е н и е

Начиная с 1946 года за границей (особенно в США) начал бурно развиваться новый раздел физики-радиоспектроскопия молекул. В настоящее время работами по радиоспектроскопии за границей занято свыше 400 научных сотрудников. Работы по радиоспектроскопии газов в ФИАН"е начали развиваться с 1952 года. В настоящее время в ФИАН"е создано 4 вполне современных радиоспектроскопа, на которых ведется исследование структур молекул. Разработана методика определения абсолютной интенсивности спектральных линий газа, которая может быть применена для целей химического анализа. Проведена работа по исследованию возможностей определения моментов радиоактивных ядер. С 1954 года в ФИАН"е начали развиваться работы по электронному парамагнитному резонансу, в настоящее время создан лучший в Союзе спектроскоп, на котором проведено исследование ряда веществ. В частности были определены отношение и относительные знаки стабильных магнитных моментов двух изотопов Eu.

С 1952 года в ФИАН"е ведутся работы по применению молекулярных пучков в радиоспектроскопии. Заканчивается создание радиоспектроскопа с применением молекулярных пучков. В 1955 году был построен и запущен принципиально новый тип генератора сантиметровых волн, так называемый молекулярный генератор, В настоящее время в секторе разрабатываются три темы.

I) Применение молекулярным пучков в радиоспектроскопии

2) Определение ядерных моментов

3) Исследование структур молекул

Работы, выпускаемые сектором не уступают по качеству работали, проводимым за границей, однако количество работ и темп развития значительно ниже, чем за рубежом. Это объясняется малочисленностью коллектива научных сотрудников (в секторе I доктор наук, 5 кандидатов наук и 5 инженеров), а отсутствие необходимого количества рабочих площадей, недостаточным обеспечением механических работ, а также слабым обеспечением работ по радиохимии, которое возложено на институты Геохимии и неорганической химии.

II. Общая характеристика и оценки работ, выполненным в 1955 году

I) По теме применения молекулярных пучков в радиоспектроскопии в 1955 году.

а) Создана теория молекулярного генератора и молекулярного усилителя; которая показала, что молекулярный генератор может быть использован в качестве абсолютного эталона частоты с точностью 10^{-9} ~~10.9~~.

б) Разработан, построен и запущен первый макет молекулярного генератора.

в) Было проведено сравнение частоты молекулярного генератора с частотой гармоник кварцевого генератора, которое показало, что молекулярный генератор дает синусоидальное колебание. Разработанная схема может быть использована для создания молекулярных часов с точностью $\sim 10^{-9}$.

г) Предложены новые методы получения активных молекул для молекулярного генератора.

д) Предложены 2 варианта молекулярного генератора без использования молекулярных пучков (так называемого "отпаянного молекулярного генератора").

е) Построен макет спектроскопа с использованием молекулярных пучков для исследования твердых соединений.

Результаты работы опубликованы в открытой печати.

Работы по молекулярному генератору получили премию Президиума АН СССР.

2) По теме определение ядерных моментов, которая проводится по Постановлению Совета Министров СССР ~~от~~

В 1955 году ~~была~~ закончена разработка и постройка радиоспектроскопа для исследования электронного парамагнитного резонанса. Этот радиоспектроскоп имеет высокую чувствительность и позволяет производить измерения моментов радиоактивных ядер. Была исследована тонкая и сверхтонкая структура спектров парамагнитного резонанса в соединениях $Al_2O_3 - Cr_2O_3$, $SrS \cdot Eu$, $CaF_2 \cdot Eu$. Из сверхтонкой структуры этих спектров были подтверждены спины Cr, Eu.

Измерены константы сверхтонкой структуры в названных соединениях, а также определено отношение и относительные знаки

8b

ядерных магнитных моментов Eu^{151} и Eu^{153}. Результаты работы опубликованы в открытой печати.

3) По теме Исследование структур молекул в 1955 году" был снят и расшифрован спектр молекул C_2H_5Cl и определены вращательные постоянные этой молекулы, а так же дипольный момент молекулы. Результаты работы опубликованы в открытой печати.

б) Был снят спектр молекулы CH_2ClSiH_3

в настоящее время ведется работа по расшифровке спектра.

в) На основании экспериментальных и теоретических исследований показано, что точность, даваемое "атомными" часами порядка 10^{-7}-10^{-8}, а не 10^{-10}, как это предполагалось иностранными авторами.

31/XII-55

87

ОТЧЕТ

по работе "Определение моментов атомных ядер"

I.

В Физическом Институте им.П.Н.Лебедева Академии Наук СССР уже в 1954 г. была закончена аппаратура для определения моментов ядер методом исследования сверхтонкой структуры вращательных спектров газов. Поэтому в 1955 году было обращено внимание на подбор необходимых объектов. Совместно с институтом ГЕОХИ /В.И.Кузнецов/ производилась работа под подбору, исследованию теллуристых соединений с целью их использования для определения моментов ядер Te /теллура/. Были синтезированы соединения $Te(CH_3)_2$ TeC_4H_8 и TeC_2H_4. Спектр этих соединений исследовался, однако не было получено положительных результатов.

Институтом Физхимии /Скуратова/ разрабатывался метод получения соединения CH_3J из небольшого количества вещества KJ /около 100 $\mu г$/. В настоящее время выход CH_3J достигает в некоторых случаях до 30%. Выход CH_3J проверялся радиоспектроскопическими методами. Институт Физхимии продолжает усовершенствовать методику с тем, чтобы иметь выход не менее 40% при каждом синтезе. Это дает уверенность, что синтез соединения CH_3J

с радиоактивным иодом J¹³¹ получится сразу с хорошим выходом. Трудность дальнейшей работы заключается в том, что необходимо будет работать с активностью I-2 кюри с газообразным соединением радиоактивного иода J¹³¹. В виду того, что ФИАН не имеет специального помещения для проведения такого рода работ, а вопрос о возможности проведения этой работы в институте Физхимии также не решен, возникает угроза срыва измерений с радиоактивным иодом из-за отсутствия специализированного помещения.

II.

Как показала практика, синтез газообразных соединений из небольших количеств исходных материалов встречает большие трудности. Кроме того, работа с газообразными радиоактивными соединениями представляет значительную опасность. Поэтому было решено провести исследование возможности использования метода электронного парамагнитного резонанса для определения моментов радиоактивных ядер. Достоинства этого метода заключаются, в частности, в том, что здесь можно пользоваться твердыми нелетучими соединениями.

Работа, проведенная в 1955 г. по выяснению возможности использования парамагнитного резонанса для целей измерения моментов ядер, дала следующие результаты.

I. Аппаратура, разработанная и построенная в ФИАН'е, дает возможность работать с очень малыми количествами исследуемых ядер, что позволяет использовать этот метод для определения моментов радиоактивных ядер.

2. Подтверждены спины ядер Mn $(I=5/2)$ Cr^{53} $(I=3/2)$, Eu^{151} $(I=5/2)$ Eu^{153} $(I=5/2)$

3. Определено отношение магнитных моментов ядер Eu^{151} и Eu^{153}, а также их относительные знаки.

4. Имеется полная возможность определения моментов радиоактивных ядер Eu^{152}, Eu^{154}, Eu^{155}, Cr^{51} Mn^{5} в случае, если указанные радиоактивные ядра будут получены без сильных примесей изотопов /не более 50% / в количестве около 10÷100 μг.

Заведующий Лабораторией
Колебаний ФИАН

/А.М.ПРОХОРОВ/

Plan of problems for 1957

In this report the research of sector of radiospectroscopy is divide between measurements of atomic nuclei, radiospectroscopy of molecules, and the development of new radio-spectroscopic methods. They were using masers to study rotational spectra of molecules and expected to produce the first prototype of a maser-based atomic clock. Besides, they were pursuing new types of molecular generators based on Electron Paramagnetic Resonance and studying resonance in semiconductors.

Reference:

Prokhorov, A. M. 1957. “Problemnyi Plan za 1957. ARAN, f.532; op.1, D. 283, pp. 22-25.”

57

Проблема - "МОМЕНТЫ АТОМНЫХ ЯДЕР"

Исполнитель - ФИАН.

Руководители - доктор физ.мат.наук А.М.Прохоров
- канд.физ.мат.наук К.В.Владимирский.

Моменты атомных ядер являются одной из важных физический характеристик вещества. По этой проблеме работа будет проводиться в следующих направлениях:

1. Предполагается проводить систематические поисковые измерения для изотопов, для которых ядерный магнитный резонанс не наблюдался. Проводить детальное изучение формы линии ядерного магнитного резонанса, позволяющее определять спины и оценивать квадрупольные моменты ядер.

Руководитель канд.физ.мат.наук К.В.Владимирский.

2. Определение моментов ядер стабильных нечетных изотопов гадолиния.

3. Исследование возможности определения электрического 2^4 - польного момента ядра иода J^{127} (переходи[т] на 1958 год).

Руководитель доктор физ.мат.наук А.М.Прохоров.

К Владимирский

Проблема - "РАДИОСПЕКТРОСКОПИЯ"

Исполнитель - Физический институт им.П.Н.Лебедева и частично МГУ им.М.В.Ломоносова и институтом неорганической химии АН СССР.

Руководитель - доктор физ.мат.наук А.М.Прохоров.

В настоящее время радиоспектроскопические методы исследования находят все более широкое применение в различных разделах физики, в частности ядерной физики и химии. Получение и изучение спектров поглощения и излучения веществ в радиочастотном диапазоне позволяет получить сведения о строении вещества недоступные другим путем.

Это направление уже привело к созданию в 1955-56 гг высокочастотного генератора чрезвычайно высокой стабильности так.наз. молекулярного генератора. В 1957 году будут проводиться работы по созданию молекулярных генераторов, пригодных для использования их в установках, называемых "молекулярными часами". Эта работа имеет не только научное, но и практическое значение.

Будут продолжены работы по исследованию вращательных спектров молекул с целью выяснения их свойств и структуры.

Получат дальнейшее развитие работы по парамагнитному резонансу. Методом электронного парамагнитного резонанса будет проводиться определение состояний активаторов в фосфорах и изучение свободных радикалов, возникающих в результате различных реакций.

Будет проводиться также работа по изучению ~~[illegible]~~ "циклотронного" резонанса в полупроводниках (совместно с МГУ).

Основные направления работы

1. Создание молекулярных генераторов, пригодных для использования их в "молекулярных часах". Работа переходит на 1958 год.

Руководитель канд.физ.мат.наук Басов Н.Г.

2. Исследование вращательных спектров молекул $TlBr$, $HDSe$, $HDTe$ и определение структур этих молекул.

3. Определение состояний активаторов в фосфорах ($SrS\ Eu$; $SrS\ Gd$; $SrS\ Tb$) совместно с лабораторией люминесценции ФИАН).

4. Изучение радикалов OH и HO_2 (совместно с институтом неорганической химии АН СССР).

5. Определение эффективных масс носителей в полупроводниках (германий или кремний) методом циклотронного резонанса" (совместно с МГУ). Работа переходит на 1958 г.

Руководитель - канд.физ.мат.наук Лазукин В.Н.(МГУ)

25

Проблема - "ИССЛЕДОВАНИЕ ФЛУКТУАЦИОННЫХ ПРОЦЕССОВ"

Исполнитель - Физический институт им.П.Н.Лебедева

Руководитель - доктор физ.мат.наук С.М.Рытов

В 1957 году предполагается применить теорию тепловых электрических флуктуаций к некоторым вопросам, интересующим радиоастрономию. Будет проводиться разработка спектральной теории релеевского рассеяния в жидкостях, а также исследование некоторых вопросов, связанных с распространением волн в статистически неоднородных средах.

Report for 1957

In 1957 the Laboratory of Oscillations studied the rotational spectra of several molecules, employed electron paramagnetic resonance to study paramagnetic ions, free radicals, and designed and developed molecular generators and amplifiers. They also built a radio telescope and measured the stability of the molecular generator (10^{12}). This report discusses the results of research in all those topics. It's important to notice that all the work of the laboratory was already focused on what two years later they would call quantum radiophysics (quantum electronics). By then, however, they considered that research as radiospectroscopy.

The report includes a section titled "scientific-organizational work" in which they reported the efforts of the staff of the laboratory to transfer the technology of the molecular generator to other Soviet institutions. They helped to build and launch molecular generators and microwave spectroscopes in several institutions. Only that year physicists of the Laboratory of Oscillations paid more than 250 consultation visits to universities and research institutes around the country.

The report also mentions that in 1957 they "systematically exchanged preprints with foreign scholars".

Reference:

"Laboratoria Kolebanii (Otchet). ARAN f. 532, Op. 1, D. 295." 1957. Archives of the Russian Academy of Science, Fond 532, Opis 1, Delo 295.

Лаборатория колебаний.

В секторе радиоспектроскопии лаборатории колебаний в течение 1957 года велись работы по исследованию вращательных спектров молекул, по созданию молекулярных генераторов и усилителей, а также по электронному парамагнитному резонансу, парамагнитных ионов и свободных радикалов.

С помощью аппаратуры, ранее созданной в лаборатории были определены вращательные спектры молекул C_2H_5-Cl, $HDSe$, CH_2ClCH_2F, CH_3GeCl_3, CH_3GeH_3, причем были определены вращательные постоянные этих молекул, межатомные расстояния, квадрупольные связи, установлены изомерные конфигурации и др., а также найдены спины и магнитные моменты ядер атомов двух изотопов гадолиния (Gd^{155} и Gd^{157}).

В работе с молекулярными генераторами спроектирован, изготовлен и налажен блок из трех молекулярных генераторов для молекулярных часов. Показано, что стабильность молекулярных генераторов за короткое время не хуже 10^{-12}, рассмотрен вопрос о влиянии внешней силы на частоту колебаний молекулярного генератора, проведены расчеты новых типов молекулярных генераторов и др.

В связи с работой по молекулярным усилителям проведено изучение спин-решоточной релаксации парамагнитного иона Cr^{3+} в решетке Al_2O_3 при комнатной температуре и температуре жидкого азота. При этом оказалось, что изучаемая релаксация при всех температурах связана с процессами 2-го порядка.

В секторе радиоастрономии помимо большой работы по созданию новых больших приборов (радио-телескоп с диаметром 31 и 22 метра) крестообразный радио-телескоп размером в 1 квадратный километр) и разработки соответствующей аппаратуры (в частности, для изучения поляризации и спектрального состава радиоизлучения) были получены новые интересные результаты:

Открыта линейная поляризация радиоизлучения Крабовидной Туманности на волне 10 см. Этот факт является веским доводом в пользу представления об испускании радиоизлучения релятивистскими электронами.

Впервые на волне 3 см получено "радиоизображение" Солнца, т.е. двумерная картина распределения яркости в данный момент времени. Выяснено, что области, ответственные за радиоизлучение отдельных образований, хотя и связаны с пятнами, но имеют размеры, значительно превышающие их.

При изучении ионосферы радиоастрономическими методами показано, что неоднородности ионосферы размером порядка 200-300 км расположены на высотах 300-400км.

Продолжение исследований сверхкороны Солнца привело к обнаружению корональных лучей, распространяющихся до расстояний в 15 радиусов солнца.

Показано, что электронные неоднородности сверхкороны имеются практически в любое время.

В теоретическом секторе лаборатории продолжалась разработка теории флуктуационных процессов.

Ранее была развита математическая теория тпеловых флуктуаций в системах с распределенными параметрами, примененная затем к упруговязкой изотропной среде. На этой основе в 1957 г. была построена спектральная теория релеевского рассеяния света, охватывающая общий случай наличия у рассеивающей среды частотной дисперсии параметров.

Проведено исследование методами корреляционной теории рассеяния электронов при прохождении через жидкость и на ее поверхности.

Закончена разработка теории корреляции амплитуд и фаз сигналов при зондировании или просвечивании ионосферы одновременно на двух частотах.

В группе сверхвысоких частот разработана и построена аппаратура, предназначенная для измерения компонент тензора магнитной проницаемости ферритов в диапазоне миллиметровых волн.

226 55

Сектор радиоспектроскопии.

Радиоспектроскопические методы в настоящее время играют весьма большую роль в различных областях науки. Не менее важную роль эти методы играют и в практике. Только благодаря радиоспектроскопии стало возможным создание систем, генерирующие радиоколебания сверхвысокой стабильности, а также подойти к решению актуальной задачи по повышению чувствительности приемных устройств на сверхвысоких частотах.

В секторе ведутся работы по исследованию вращательных спектров молекул, по созданию молекулярных генераторов и усилителей, а также по исследованию электронного парамагнитного резонанса парамагнитных ионов и свободных радикалов. Все эти работы стоят на уровне зарубежных работ, хотя (см.н/о) размах этих работ значительно ниже и некоторые разделы радиоспектроскопии полностью отсутствуют.

Результаты исследований по важнейшим законченным в 1957 году работам.

За 1957 г. в секторе радиоспектроскопии были закончены следующие основные работы:

1. Спроектирован, изготовлен и налажен блок из трех молекулярных генераторов для молекулярных часов. Показано, что стабильность молекулярных генераторов за короткое время не хуже 10^{12} (Басов Н.Г., Петров А.Л.).

2. Исследованы вращательные спектры ряда молекул ~~(и др.).~~

Определены вращательные постоянные этих молекул, определены межатомные расстояния, дипольные молекулы, квадрупольные связи, а также изомерные конфигурации ~~, мо-лекулы CH_2FCH_2Cl~~ (Барчуков А.И., Веселаго В, Ирисова Н.А., Мурина Т.М., Мухтаров И.А., Прохоров А.М).

3. Определены спины и магнитные моменты ядер двух нечетных изотопов гадолиния ~~(и)~~

(Маненков А.А., Прохоров А.М)

4. Экспериментально показано, что исследование вспомогательного излучения на более высокой частоте позволяет увеличить интенсивность переходов на более низкой частоте на несколько порядков. (Басов Н.Г., Осипов Б.Д.).

5. Проведено измерение спин-решеточной релаксации парамагнитного иона Cr^{3+} в рещетке Al_2O_3 при комнатной и азотной температурах. Показано, что спин-решеточная релаксация при всех температурах связана с процессами 2-го порядка. Эти результаты важны для создания молекулярных усилителей. (Пашинин П.П., Прохоров А.М.).

6. Теоретически рассмотрен вопрос о влиянии внешней силы на частоту колебаний молекулярного генератора.

Приведены расчеты новых типов молекулярных генераторов. (Басов Н.Г., Ораевский А., Свидзинский К.).

Научно-организационная работа.

Результаты работы сектора по молекулярным генераторам были переданы и внедрены в ряд институтов страны - (ХВАИВУ, ВНИФТРИ, Хар.ин-т мер и измер.приборов, МГУ, НИРФИ, ИР и ЭАН, НИИ 695 МРТП).

Сотрудниками сектора радиоспектроскопии за 1957 год было дано свыше 250 консультаций работникам различных учреждений по вопросам, связанным с молекулярными генераторами и усилителям, использованию явления парамагнитного резонанса и газовой спектроскопии для различных целей.

Сотрудники сектора в 1957 году принимали участие в 5 конференциях, их них одна является международной (УРСИ), ~~где было сделано два доклада~~.

Систематически происходит обмен оттисками работ с зарубежными учеными.

Семинар сектора радиоспектроскопии нерегулярно посещается большинством сотрудников, работающих в области радиоспектроскопии в г.Москве

По чертежам ФИАН с помощью сектора радиоспектроскопии в ряде институтов были построены и запущены радиоспектроскопы для исследования спектров парамагнитного резонанса (ТГУ, МИФИ, Институт химфизики АН СССР, МГУ, ин-т биофизики)

Plan of problems for 1958

Plan of problems in Spectroscopy to be studied in the Laboratory of Oscillations. Besides continuing the development of research began in the previous years (development of atomic clocks and new types of molecular generators), in 1958 they planed to study the possibility of storing free radicals for long periods, rotational spectra of isomer molecules and molecules with internal rotation, and to devise low noise amplifiers based on electron paramagnetic resonance.

Reference:

"Problemnyi Plan za 1958, Problema 'Radiospektroskopiia'. ARAN, F. 532, Op. 1 D. 301, pp. 17-18 and 67-68." 1958.

58 17

Проблема "Радиоспектроскопия"

Исполнитель - ФИАН, Институт неорганической химии АН СССР, Институт атомной энергии АН СССР.

Руководитель - доктор физ.мат.наук А.М.Прохоров.

В настоящее время радиоспектроскопические методы исследования широко применяются для решения важных вопросов физики и химии.

В ФИАНе работа по радиоспектроскопии будет проводиться главным образом в развитие начатых в прошлые годы исследований.

На основе разработанного в ФИАНе молекулярного генератора будет создан макет молекулярных часов. Наряду с этим будут разрабатываться новые типы молекулярных генераторов.

Будут продолжены и развиты работы по определению моментов атомных ядер.

Метод электронного парамагнитного резонанса будет применен для изучения свободных радикалов и изучения состояний активаторов в фосфорах.

Будет также выясняться возможность хранения свободных радикалов очень длительное время.

Новым направлением является создание малошумных усилителей с использованием явления электронного парамагнитного резонанса.

Новым также является изучение вращательных спектров изомерных молекул, а также молекул имеющих внутреннее вращение. Это в частности позволит пополнить данные о величинах потенциальных барьеров.

Основные направления работы.

1. Создание молекулярных весов и проведение измерений с ними. Переходит на 1959 год.

   Руководители: Н.Г.Басов, А.М.Прохоров и И.В.Штраних.

2. Создание новых типов молекулярных генераторов (на пучке молекул ND_3). Переходит на 1959 год.

   Руководитель Н.Г.Басов.

3. Определение моментов ядра Tc 99

   (совместно с ИАЗАН)

4. Создание малошумных усилителей с использованием явления электронного парамагнитного резонанса

   ~~Переходит~~ на 1959 год.

   Руководители: канд.ф.м.н. А.А.Маненков и д.ф.м.н. А.М.Прохоров.

5. Изучение радикалов OH и HO_2 при водородных и гелиевых температурах.

6. Изучение вращательных спектров молекул CH_2FCH_2Cl, $(CH_3)_3GeCl$ и CH_3GeCl_3 .

7. Изучение состояний активаторов в фосфорах ZnS или CdS

Проблема: "РАДИОСПЕКТРОСКОПИЯ"

Исполнители: Физический институт им.П.Н.Лебедева АН СССР, Институт общей и неорганической химии им.Н.С.Курнакова АН СССР, Физико-технический институт Казанского филиала АН СССР, НИИ ядерной физики МГУ им.М.В.Ломоносова

Руководитель - доктор физ.мат.наук А.М.Прохоров
/Постан. см. секр. № 1576-291 от 13/IX-1955./

Радиоспектроскопические методы исследования в настоящее время широко применяются для решения важных вопросов физики и химии. Изучение спектров поглощения и излучения веществ в микроволновом диапазоне позволяют получить сведения о строении вещества, которые не могут быть получены другим путем.

В ФИАНе работа по радиоспектроскопии в 1958 году будет проводиться, главным образом, в развитие начатых в прошлые годы исследований.

На основе разработанного в 1955-56 гг. молекулярного генератора будет создан макет молекулярных часов, что имеет большое научное и практическое значение. Будут разрабатываться также новые типы молекулярных генераторов.

Получат дальнейшее развитие работы по парамагнитному резонансу. Метод электронного парамагнитного резонанса будет применен для изучения свободных радикалов при водородных и гелиевых температурах и изучения состояний активаторов в фосфорах. Будет также выясняться возможность хранения свободных радикалов очень длительное время. Новым явится изучение вращательных спектров изомерных молекул, а также молекул, имеющих внутреннее вращение. Это, в частности, позволит пополнить данные о величинах потенциальных барьеров.

Новым направлением является создание малошумных усилителей с использованием явления электронного парамагнитного резонанса.

В ФТИ Казанского филиала АН СССР методом парамагнитного резонанса будет продолжаться изучение структуры и свойств жидких растворов с целью получения новых сведений о химическом строении комплексных ионов в растворах, а также изучение спектров электронного /а частично и ядерного/ парамагнитного резонанса в твердых телах с целью выяснения деталей структуры этих тел. Кроме того, будут поставлены опыты по изучению влияния изменения температуры в широком диапазоне на явления парамагнитного поглощения в различных типах солей.

В НИИ ядерной физики МГУ при комнатных и гелиевых температурах будут продолжаться исследования спектра электронного парамагнитного резонанса различных элементов группы железа и трансурановых элементов с целью определения состояний ионов в исследуемых соединениях и уточнения их констант. С помощью разработанной ранее методики будет исследоваться циклотронный резонанс в полупроводниках типа InSb и в так называемых стекловидных полупроводниках.

Целью работы является установление формы энергетических поверхностей указанных кристаллов и выяснение возможностей создания циклотронного радиочастотного масспектрометра высокой разрешающей силы.

По данной проблеме исследования проводятся также и в научно-исследовательском Радиофизическом институте при Горьковском Гос.университете. Направление работ этого института – радиоспектроскопические исследования строения молекул некоторых химических соединений типа каучука.

проблемный план 1958

Report for 1958

Report mains results of the scientific research, listed in the plan of problem above, conducted in the sector of Spectroscopy of the Laboratory of Oscillations.

Reference:

"Otchet, Sektor Radiospektroscopii Laboratorii Kolebanii. ARAN, F. 532, Op. 1 D. 303, pp. 111-112." 1958.

111 125/16

О Т Ч Е Т

сектора радиоспектроскопии лаборатории колебаний
за 1958 год.

I. Отчет по проблемам плана научно-исследовательских работ.

Созданы парамагнитные усилители на ряд длин волн (А.А.Маненков и А.М.Прохоров). (Работа проводилась совместно с НИИЯФ МГУ (Зверев Г.М., и Корниенко Л.С.).) Закончена разработка принципиальной схемы и проведены предварительные испытания молекулярного стандарта частоты (времени) с абсолютной стабильностью 10^{-9} (Басов Н.Г., ~~Шраних И.В., Петров А.П.~~ ~~и~~ Мурин И.Д. Петров А.П., Прохоров А.М., Шраних И.В.)

Путем применения в газовой спектроскопии метода двойного резонанса удалось повысить чувствительность и разрешающую силу радиоспектроскопа при наблюдении чисто квадрупольных переходов в соединении CH_3J (Басов Н.Г. и Осипов Б.Д.

Методами радиоспектроскопии был исследован вращательный спектр молекул $HDSe$, CH_3GeH_3 и CH_3GeCl_3. Были определены структуры этих молекул с большой точностью, а также определены дипольные моменты, потенциальный барьер внутреннего вращения молекулы CH_3GeH_3 и другие параметры этих молекул. (Барчуков А.И., Веселаго В.Г., Ирисова Н.А., Прохоров А.М.). Был изучен вращательный спектр молекул CHF_2CHF_2 и CH_2FCHF_2. Некоторые линии вращательного перехода расщеплялись на дублеты из-за наличия двух эквивалентных изомерных состояний в этих молекулах. Это позволяет определить высоту потенциального барьера внутреннего вращения в этих молекулах (Мухтаров И.А.).

На различных частотах был изучен спектр парамагнитного резонанса радикала OH, полученного путем облучения ультрафиолетом замороженной перикиси водорода. Было показано, что сверхтонкое расщепление из-за взаимодействия с магнитным моментом ядра водорода равно I2 эс, а $g_{\perp} = 2,00$ и $g_{\parallel} = 2,03$. Было также показано, что радикалы, получаемые путем замораживания продуктов разряда в парах H_2O и H_2O_2, являются в основном радикалы OH, а не HO_2, как это принималось ранее (Кайтмазов С.Д. и Прохоров А.М.). Развита методика расчета сверхтонкой структуры молекул, имеющих несколько одинаковых ядер. Эта методика значительно упрощает громоздкие расчеты сверхтонкой структуры (Басов Н.Г.и Свидзинский К.К.)

Был проведен синтез ряда химических соединений, в частности германоорганических, часть которых была исследована методами радиоспектроскопии (Взенкова Г.Я.).

Построена и налажена установка для наблюдения чисто квадрупольных переходов в жидкостях и твердых телах (Прохоров А.М. и Шупуло Г.П.).

2. Отчет о научно-организационной деятельности сектора радиоспектроскопии.

Explanatory notes to the letter from FIAN to the Department of Physical and Mathematical Sciences [of the Academy of Sciences]

In 1958 the academic council of FIAN suggested making the following changes in the organization of the Institute:

a) Transform the sector optics of flames of the Laboratory of Luminescence into optics of low-temperature plasmas.

b) Pick out from the staff of the Laboratory of Accelerators and photo-nuclear reactions 3 new sectors.

c) Pick out from the staff of the Laboratory of Oscillations a new sector of Molecular Generator

d) Form a new scientific auxiliary department of computing devices.

These notes justify and explain those changes. The file bellow presents the notes on the changes a to c.

Reference:

"Obyasnitelnaia Zapiska K Pismu FIAN v Otdelenie Fiziko-Matematicheskikh Nauk O Strukture Fizicheskogo Instituta Im. P. N. Lebedeva AN SSSR. ARAN, F. 532, Op. 1, d.300, pp. 27-31." 1958.

52 27

ОБЪЯСНИТЕЛЬНАЯ ЗАПИСКА

К ПИСЬМУ ФИАН В ОТДЕЛЕНИЕ ФИЗИКО-МАТЕМАТИЧЕСКИХ НАУК

О СТРУКТУРЕ ФИЗИЧЕСКОГО ИНСТИТУТА им.П.Н.ЛЕБЕДЕВА АН СССР

Как указывается в основном тексте письма, Ученый совет ФИАН предлагает внести следующие изменения в структуру лабораторий Института:

а/ Преобразовать сектор оптики пламен лаборатории люминесценции в лабораторию Оптики низкотемпературной плазмы.

б/ Выделить в составе лаборатории ускорителей и фотоядерных реакций 3 новых сектора.

в/ Выделить в составе лаборатории колебаний новый сектор молекулярных генераторов.

г/ Образовать новый научно-вспомогательный отдел Счетно-решающих устройств.

Предлагаемые изменения в структуре Института являются минимальными и отражают фактически существующее положение.

Ниже приводится обоснование предлагаемых изменений.

I. В старой структуре Института в состав лаборатории люминесценции входил сектор Оптики пламен, который по тематике не связан с лабораторией люминесценции и фактически уже давно в научном и организационном отношении представляет собой совершенно самостоятельную группу. В сектор Оптики пламен, в настоящее время, входит 20 человек.

В течение ряда лет основной задачей этого сектора было спектроскопическое исследование физических процессов в пламенах и разработка оптических методов измерения температуры пламен. В последние два года сектор переключился на исследование плазмы. Сектор работает успешно и ряд организаций, заинтересованных в его работе, отмечают большое значение исследований, проведенных и проводимых в секторе.

В новой структуре этот сектор выделяется в лаборатории Оптики низкотемпературной плазмы, основной целью которой будет исследование спектроскопическими методами плазмы состоящей из частиц с сравнительно малыми энергиями /2000-20000°К/. В настоящее время в секторе исследуется состояние воздуха, азота и благородных газов за ударной волной, а также плазма благородных газов в дугах постоянного тока. Актуальность этой тематики несомненна как с чисто научной точки зрения, так и с практической. В частности, с практической точки зрения, эти исследования важны для современных газодинамических задач.

II. По старой структуре Института, все сотрудники лаборатории Ускорителей и фотоядерных реакций, работающие на синхротронах С-3 и С-25, формально входят в сектор руководимый профессором П.А.Черенковым и созданный еще в первый период существования лаборатории.

В последующие годы - после начала нормальной работы этих ускорителей - на них были развернуты широкие исследования фотоядерного взаимодействия до 30 Мэв /на С-3/ и до 265 Мэв /на С-25/ составляющие ныне основное содержание работы коллектива научных, технических сотрудников и вспомогательного персонала, насчитывающего около 200 человек.

Соответственно развитию этого направления, возникшего как естественное следствие создания в ФИАН"е синхротронов, в новой структуре предлагается оформить фактически осуществленное разукрупнение сектора проф.П.А.Черенкова с выделением двух новых ядерно-физических экспериментальных секторов, базирующихся на синхротроне С-3 и С-25, а именно:

I. Сектор Фото-мезонных процессов. В составе этого сектора уже около двух лет фактически работает около тридцати научных и технических сотрудников, исследующих вопросы структуры нуклонов по изучению упругого рассеяния γ -квантов высокой энергии протонами и дейтонами, а также проблемы фоторождения π -мезонов на водороде и ядрах, фоторасщепления ядер, выхода и угловых распределений фотонуклонов.

В настоящее время эксперименты ставятся на синхротроне С-25. Одновременно проводится создание аппаратуры для постановки аналогичных фотоядерных и фотомезонных исследований с γ -квантами с большей энергией, которые будут проводиться в Институте после запуска нового синхротрона.

2. Сектор Фотоядерных реакций при малых энергиях. Этот сектор проводит работы на малом синхротроне /С-3/ - в области гигантского резонанса при энергии γ -квантов до 30 Мэв и охватывает вполне самостоятельный как в научном, так и в организационном отношении коллектив, состоящий более чем из 30 человек, обеспечивающий эксплуатацию ускорителя и все проводимые на нем физические исследования. В настоящее время проводятся исследования выхода и угловых распределений фотонуклонов, неупругое рассеяние γ -квантов, излучение фотоделения ядер.

3. Сектор Циклических ускорителей. Наряду с проведением работ на синхротронах, лаборатория Ускорителей и фотоядерных реакций проводит разработку и исследования новых методов ускорения ядерных частиц. Для разработки новых идей в области создания

циклических ускорителей в лаборатории уже более года фактически действует сектор, состоящий из почти 20-ти сотрудников-теоретиков, физиков-экспериментаторов и инженеров. Этот сектор осуществляет разработку и создание первого образца кольцевого фазотрона-одного из новых типов циклических ускорителей.

Ш. В старой структуре Института, в составе лаборатории Колебаний, имелся сектор Радиоспектроскопии. За последние годы в секторе Радиоспектроскопии получили свое развитие два новых направления:

I. Разработка и исследование молекулярных генераторов.

2. Разработка и исследование молекулярных усилителей.

В этих направлениях работают две группы сектора Радиоспектроскопии, каждая из которых по существу уже давно является отдельным сектором с числом сотрудников I5-20 человек и с обособленной актуальной тематикой.

В новой структуре Института предусмотрено разделение сектора Радиоспектроскопии на 2 сектора: сектор Молекулярных генераторов и сектор Молекулярных усилителей.

В секторе Молекулярных генераторов будут вестись исследования и разработка высокостабильных молекулярных генераторов и работа по использованию их в качестве стандартов частоты и времени и по разработке новых методов генерирования миллиметровых и субмиллиметровых волн. Кроме того, в этом секторе будут вестись исследования структур молекул различных веществ методом газовой радиоспектроскопии.

В секторе Молекулярных усилителей будет вестись разработка высокочувствительных квантомеханических усилителей дециметровых волн; в этом секторе будут также вестись исследования свойств различных веществ методами парамагнитного резонанса, а также изучаться возможности усиления миллиметровых и субмиллиметровых волн.

В перспективных планах предусмотрено весьма значительное развитие работ по радиоспектроскопии. Ведущим институтом по этому направлению должен быть ФИАН /лаборатории Колебаний/. Планы работ, предусматривают значительное расширение масштаба исследований, проводимых в секторе Молекулярных генераторов и в секторе Молекулярных усилителей. Особенно существенно расширятся эти работы в 1959 году, после окончания строительства специального здания, для проведения работ по радиоспектроскопии.

IУ. В старой структуре Института вычислительная работа не была выделена в специальный отдел. В настоящее время в Институте имеется электронная вычислительная машина "Урал", представляющая собой сложный комплекс электронной аппаратуры, состоящей из целого ряда самостоятельных устройств /всего семь устройств/. Более чем полугодовой опыт эксплуатации машины показал, что для ее удовлетворительного обслуживания необходима группа инженеров и техников, хорошо знающих электронику, математическую логику и программирование. Существование такой группы вызвано и тем, что для увеличения эффективной отдачи машины необходима ее круглосуточная работа, которая обеспечивает постоянный тепловой режим и в значительной степени повышает надежность работы устройств машины.

Имеющаяся в Институте машина "Урал" относится к самым первым экземплярам машин этого типа. В настоящее время промышленность выпускает эти машины с различными усовершенствованиями. Институт же вынужден производить их своими силами. Поэтому груп-

Report on International connections of the Physical Institute of the Academy of Sciences of the USSR with capitalist countries (1958)

Report on international relations divided in two sections: "Scientific Connections with Capitalist Countries" and "Visits of foreign guests to FIAN in 1958".

The first section emphasizes the importance of establishing close contacts with physicists from capitalist countries not only in conferences, but also through consistent preprint exchange and inviting well-known foreign scholars to lectures and work in Soviet institutions for short periods.

The second section reveals that in 1958 FIAN received almost 200 foreign guests. Among them, more than 80 from the peoples republics. The institute also received visitors from Egypt and India (Chandrasekhara Raman visited the institute on occasion of his visit to Moscow to receive the international Lenin Prize "for strengthening the peace between peoples"), what suggests an interest in diplomatic relations with third world countries as well. In addition, FIAN received scientists from many capitalist countries (USA, France, England, Canada, Austria, Sweden, Finland, Holland). Most of the guests were from USA, France, and England. Between them were Frédéric Joliot-Curie, John Cockroft, Patrick Blackett, Philip Anderson and other well-known physicists. The report also mentions foreign writers and journalists, such as Wilfred Burchett, a communist Australian journalist who would became famous for his reporting on Vietnam War, who visited FIAN that year.

Reference:

Basov, N. G. 1958. “Nauchnye Sviazi S Kapitalicheskimi Stranami. ARAN, F. 532, Op. 1 D. 303, pp. 42-46.”

НАУЧНЫЕ СВЯЗИ С КАПИТАЛИСТИЧЕСКИМИ СТРАНАМИ

Отдельные сотрудники Института были командированы в капиталистические страны на срок в несколько месяцев. Однако основной формой общения с учеными капиталистических стран являлись встречи на конференциях и совещаниях.

Значение конференций для развития советской науки весьма важно. В основном оно обусловлено пользой личных контактов ученых, при которых выясняются вопросы актуальности того или иного направления в науке, достаточно широко выясняется состояние важнейших проблем и отдельных работ, выясняется актуальность работ советских физиков.

Однако, несмотря на очевидную пользу конференций, значительно важнее было бы приглашать для прочтения курса лекций или непродолжительной работы наиболее квалифицированных физиков. Опыт показывает, что во время конференций, ввиду большой напряженности программы, бывает очень трудно обсудить все интересующие вопросы.

Следует отметить одно важное обстоятельство, тормозящее научное творчество. Культура научной информации в СССР стоит значительно ниже, чем на Западе.

В настоящее время отдельные сотрудники Института получают "препринты" (предварительные оттиски) из ЦЕРН'а, Рочестерского университета (США), Брукхейвенской лаборатории, Илинойского университета, ряда университетов Англии, Италии, Венгрии, ГДР. Однако, необходимо отметить, что налаженные связи находятся под угрозой, ибо посылка оттисков производится на началах взаимности. Посылка же ответных предварительных оттисков не налажена в Институтах АН СССР.

Следует отметить, что за границей "препринты" посылаются не отдельным лицам, а институтам и лабораториям. Они хранятся в библиотеках в специальных папках. В Советском Союзе только в ОИЯИ налажена работа по обмену "препринтов". ФИАН"у в связи с этим необходимо иметь издательский отдел и ротатор.

Имеют место также недостатки в организации приглашения иностранных ученых на совещания, проводимые в СССР. Например, на УП Всесоюзное совещание по люминесценции в Москве были приглашены иностранные ученые, но из 20 приглашенных участвовали в совещании только трое (Ортман из ГДР, Яшин из Польской Народной Республики и Надь из Венгерской Народной Республики). Это объясняется в основном опозданием (не по вине Оргкомитета) с посылкой приглашений. Участие же приглашенных ученых было бы весьма полезным.

ПОСЕЩЕНИЕ ФИАН"а ИНОСТРАННЫМИ ГОСТЯМИ в 1958 ГОДУ

За 1958 год ФИАН посетило около двухсот иностранных гостей. Среди них более 80 – гости из стран народной демократии (из Венгрии, Польши, ГДР, Чехословакии, КНР, КНДР, Румынии, Болгарии, Югославии). Из гостей из стран народной демократии можно назвать таких крупных ученых как акад.Яноши, проф.Сигетти и других, которые не только осматривали институт, но и выступали с лекциями. ФИАН посетила делегация руководящих деятелей науки КНДР.

ФИАН посещали ученые из Египта и Индии. С работой лабораторий нашего института знакомился проф.Раман, приезжавший в Москву для получения международной Ленинской премии "За укрепление мира между народами".

ФИАН посещали ученые из многих капиталистических стран (из США, Франции, Англии, Канады, Австрии, Швеции, Финляндии, Голландии). Большее число гостей было из таких стран как США, Франция, Англия. Среди них были такие крупные ученые как Ф.Жолио-Кюри, Кокрофт,Броуд, Блеккет, Андерсон и другие. Некоторые иностранные ученые делали доклады на семинарах лабораторий (Андерсон делал сообщение на семинаре лаборатории колебаний, Ван Суивер (США) рассказывал о своей работе на семинаре лаборатории люминесценции).

Иностранные ученые принимали участие в работе Всесоюзной конференции по физике диэлектриков (всего 18 делегатов от Польши (I), Чехословакии (3), ГДР (4), США (6) и Франции (4), где они выступали с докладами и сообщениями.

Физический институт посещали не только ученые зарубежных стран. В нашем институте побывали американский журналист Берчет Уилферд, который беседовал с И.Е.Таммом о его поездке на Памир, известный американский [illegible] Уилсон, итальянский журналист Андреа Коэн. Несколько раз посетил ФИАН известный писатель Стефан Гейм, который собирается писать роман о советских учёных.

За 1958 г. в ФИАН'у было прикомандировано 18 научных сотрудников из КНР, ВНДР, Чехословакии, Польши. В ФИАН'е учатся три аспиранта из КНР.

Заместитель директора ФИАН
доктор физ.мат.наук.- /Н.Г.Басов/

9.1.59.

уч. секр. ФИАН.

Plan of Problems for 1959

Problem Plan of the Laboratory of Oscillations for the Sector Radiospectroscopy for 1959. This plan is more laconic than the previous ones. It lists the following research directions to be pursued :

1 - Molecular amplifier

2 - Molecular generator

3 - Paramagnetic resonance

4 - Gas spectroscopy

5 - Double resonance

It also enumerates the researches to be conducted within those directions. Its significances lies more on what it does not mention. At this late stage of the race to make the first laser in the world, there is no mention to optical molecular generator (laser), although both groups that formed the sector were working hard towards it. At that point, it was arguably their main goal.

Reference:

"Problemnyi Plan Laboratorii Kolebanii FIAN po Sektoru 'Radiospektrokopiia'. ARAN F. 532, Op. 1, D. 320, pp.14-15." 1959.

ПРОБЛЕМНЫЙ ПЛАН ЛАБОРАТОРИИ КОЛЕБАНИЙ ФИАН ПО СЕКТОРУ "РАДИОСПЕКТРОСКОПИЯ" НА 1959 ГОД

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Руководитель: доктор физико-математических наук А.М.Прохоров

В течение 1959 года радиоспектроскопические исследования лаборатории будут вестись по следующим направлениям:

1. Молекулярные усилители
2. Молекулярные генераторы
3. Парамагнитный резонанс.
4. Газовая спектроскопия
5. Двойной резонанс

Все эти направления являются продолжением ранее проводимых лабораторией работ. В течение 1959 года будут проводиться следующие работы:

1. Исследование характеристик ранее созданных парамагнитных усилителей и молекулярных генераторов.

2. Исследование релаксационных явлений в парамагнитных кристаллах в широком диапазоне температур.

3. Применение созданных парамагнитных усилителей в радиоастрономии.

4. Исследование молекулярных генераторов с целью дальнейшего повышения стабильности частоты, а также использование созданных молекулярных генераторов в стандартах частоты и в службе времени.

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5. Теоретические и экспериментальные исследования по созданию новых типов молекулярных генераторов.

6. Исследование спектров парамагнитных кристаллов и свободных радикалов.

7. Исследование вращательных спектров молекул, в частности молекул, имеющих внутреннее вращение.

8. Исследование тонких взаимодействий в спектрах молекул методом двойного резонанса.

Лаборатория будет продолжать консультацию сотрудников различных учреждений, внедряющих созданные в лаборатории новые приборы.

9/X-58

Report for 1959

Report of the laboratory of oscillation for problems in "Quantum Radiophysics" and Spectroscopy. Besides results of the research listed in the plan of problems, this report explains what is Quantum Radiophysics, a field born few years earlier, closely linked to radiospectroscopy, and compares the work done in the laboratory to works conducted abroad. It argues that although some researches began earlier in the USSR, they were completed later. It also emphasizes that in the USA that new field is being developed in many scientific institutions. The term "Quantum Radiophysics" is used for the first time in a laboratory report. It was also used in the report of the trip to the conference in the USA. Perhaps they coined that term because the Americans had created Quantum Electronics.

The report also reveals that they were working "In order to accelerate the development of the new field" in the Soviet Union. In April 1959 they organized a meeting called Discussions on Quantum Radiophysics with about 300 physicists from 60 institutions. Along the year they delivered a series of 20 lectures on quantum radiophysics attended by an average of 60 people. Besides, an academic council, formed by members of academic and industrial institutes, "worked systematically on the problem of 'application of quantum systems for generation of and amplification of radio oscillations'", meeting 7 times in 1959.

Reference:

"Otchet Laboratorii Kolebanii Po Problemam 'Kvantovaia Radiofizika' I 'Radiospektroskopii' Za 1959 G. ARAN, F. 532, Op. 1 d.324, pp. 19-97." 1959.

Проблемы: "КВАНТОВАЯ РАДИОФИЗИКА" И "РАДИОСПЕКТРОСКОПИЯ"

Лаборатория колебаний

Научное направление "Квантовая радиофизика" зародилось всего несколько лет назад. Это направление тесно связано с радиоспектроскопическими исследованиями.

В настоящее время лаборатория начала проводить цикл работ по развитию квантовой радиофизики и радиоспектроскопических исследований в области миллиметровых, субмиллиметровых и более коротких волн. Это новое направление также начинают развивать и в США, причем во многих научно-исследовательских учреждениях.

Басову Н.Г., Никитину В.В. и Ораевскому А.Н. удалось повысить абсолютную стабильность молекулярных генераторов на один порядок.

В области молекулярных усилителей удалось расширить полосу усилителей на один порядок, при неизмененной величине усиления (Карлов Н.В., Пименов Ю.П.,Прохоров А.М.).

Создан молекулярный генератор на ND_3 (Басов Н.Г., Зуев В.С., Свидзинский К.К.). Заканчивается работа по фазовой стабилизации частоты клистрона с помощью двух молекулярных генераторов (Басов Н.Г., Осипов Б.Д., Никитин В.В.).

Создан и испытывается первый макет, развернуты работы по созданию молекулярного усилителя бегущей волны (Карлов Н.В., Карлова Е.К., Прохоров А.М.).

Исследован новый тип резонатора для субмиллиметровых волн (Барчуков А.И., Прохоров А.М.).

Методом двойного резонанса были определены константы сверхтонного взаимодействия в CH_3J (Б.Д.Осипов).

Разработан и создан молекулярный генератор с абсорбционным насосом (Н.Г.Басов, А.В.Дуденкова).

Исследованы времена релаксации спинов Cr^{3+} и Al_2O_3 при различных концентрациях и температурах (Маненков А.А., Прохоров А.М.).

Разработана установка для выращивания монокристаллов веществ, затвердевающих при низких температурах (Прохоров А.М., Шипуло Г.П.).

Совместно с лабораторией люминесценции проведено исследование фосфора Sr S. Sm Eu как оптическими методами, так и методом парамагнитного резонанса. (Со стороны лаб. колебаний Прохоров А.М.).

2. Конференция по квантовой радиофизике.
(Нью-Йорк, Колумбийский университет)

Первая международная конференция по проблемам, связанным с новыми методами усиления и генерации электромагнитной энергии вплоть до оптического диапазона.

На конференции рассматривались только общие физические идеи без их конкретного приложения.

Конструкции не рассматривались вообще.

Путем бесед с американскими учеными членам делегации (Прохоров, Басов, Барчуков) удалось достаточно ясно установить тенденцию развития данной отрасли физики, а также получить ряд конкретных характеристик работы парамагнитных усилителей и молекулярных генераторов; в частности, сведения о применении материала TiO_2 с присадками хрома и железа для парамагнитных усилителей, сведения об использовании изотопа N^{15} в аммиачном стандарте частоты, а также объяснение явления "перекрестной релаксации", правильный учет которого весьма важен при расчете парамагнитых усилителей.

Прохоров, Басов и Барчуков посетили и осмотрели лабораторию излучений и лабораторию химии Колумбийского университета и лабораторию излучений фирмы IBM.

О Т Ч Е Т

лаборатории колебаний по проблемам "Квантовая радиофизика" и "Радиоспектроскопии" за 1959 г.

1. Научное направление "Квантовая радиофизика" зародилось всего несколько лет назад. Это направление тесно связано с радиоспектроскопическими исследованиями.

Работы, проводимые в лаборатории, по уровню не уступают зарубежным работам, однако фронт работ за рубежом значительно шире. Выполнение ряда работ затягивается из-за недостаточного материально-технического обеспечения. Это приводит к тому, что хотя некоторые работы в лаборатории начинаются раньше, чем за рубежом, они заканчиваются позже. В настоящее время лаборатория начала проводить цикл работ по развитию квантовой радио-физики и радиоспектроскопических исследований в области миллиметровых, субмиллиметровых и более коротких волн. Это новое направление также начинают развивать и в США, причем во многих научно-исследовательских учреждениях.

П. В 1959 г. удалось повысить абсолютную стабильность молекулярных генераторов на один порядок /Басов Н,Г, Никитин В.В., Ораевский А.Н./. В области молекулярных усилителей удалось расширить полосу усилителей на один порядок, при неизменной величине усиления /Карлов Н.В., Пименов Ю.П., Прохоров А.М./.

96 ~~98~~

Создан молекулярный генератор на ND_3 /Басов Н.Г., Зуев В.С., Свидзинский К.К./. Заканчивается работа по фазовой стабилизации частоты клистрона с помощью двух молекулярных генераторов /Басов Н.Г., Осипов Б.Д., Никитин В.В./.

Создан и испытывается первый макет молекулярного усилителя бегущей волны на волну $\lambda = 3,2$ см./работа совместно с НИИ/./Карлов Н.В., Карлова Е.К., Прохоров А.М./.

Исследован новый тип резонатора для субмиллиметровых волн./Барчуков А.И., Прохоров А.М./.

Методом двойного резонанса были определены константы сверхтонкого взаимодействия в CH_3J/Б.Д.Осипов/.

Разработан и создан молекулярный генератор с абсорбционным насосом /Н.Г.Басов, А.В.Дуденкова/.

Исследованы времена релаксации спинов Cr^{3+} в Al_2O_3 при различных концентрациях и температурах /Маненков А.А., Прохоров А.М./.

Разработана установка для выращивания монокристаллов веществ, затвердевающих при низких температурах. /Прохоров А.М., Шипуло Г.П./.

Совместно с лабораторией люминисценцией проведено исследование фосфора $SrS \cdot Sm \cdot Eu$ как оптическими методами, так и методом парамагнитного резонанса./Со стороны лаб. колебаний-Прохоров А.М./.

В апреле 1959 г. проведено /совместно ФИАН и радиосоветом АН СССР/ совещание по квантовой радиофизике с общим числом участников около 300 чел. от 60 учреждений.

Ш. В целях быстрейшего освоения новой области - квантовой радиофизики- для работников отраслевых научно-исследовательских институтов было организовано чтение лекций. Было прочитано 20 лекций, на которых присутствовало в среднем около 60 человек.

Систематически работал Ученый Совет по проблеме "Применение квантовых систем для генерации и усиления радиоколебаний". Членами Ученого Совета являются не только представители академических учреждений, но также и представители отраслевых институтов. В 1959 г. проведено 7 заседаний.

ЗАВ.ЛАБОРАТОРИЕЙ КОЛЕБАНИЙ
профессор

/А.М.ПРОХОРОВ/

" 3 " декабря 1959 г.

Plan of problems for 1960

After returning from the conference in the US they increased significantly the breadth of the research on molecular oscillators and amplifiers pursued in the laboratory. Besides devising a few types of molecular generators and amplifiers they planned to install a molecular generator in a satellite or rocket, to detect gravitational effects predicted by general relativity, and an amplifier in a telescope. Up to this point there is no explicit mention to optical molecular generator (laser).

Reference:

"Problemnyi Plan. ARAN, F. 532, Op. 1 D. 337, pp. 31-37." 1960.

I	2	3	4	5	6
65.	Методические работы	переходит с 1959г. переходит на 1961г.	Проведение работ по созданию μ- мезонного тракта на фазотроне ОИЯИ (совместно с ОИЯИ).		Руководитель - канд.ф.м.н. В.Г.Кириллов-Угрюмов, отв.исполнитель - А.Самойлов.
		Проведение работ по созданию новых пузырьковых камер в магнитном поле (совместно с МИФИ).			
		Продолжение работ по изучению искровых счетчиков.			

Лаборатория колебаний

Проблемы: "РАДИОСПЕКТРОСКОПИЯ" и "КВАНТОВАЯ РАДИОФИЗИКА"

А. Сектор молекулярных генераторов

I	2	3	4	5	6
66.	Исследование возможностей установки молекулярных генераторов на спутнике или космической ракете	Начало 1960г. переходит на 1961г.	I. Разработка двух лабораторных макетов генераторов, имеющих высокую относительную стабильность частоты 10-11 (в течение длительного времени порядка нескольких суток).	Отчет или статьи	Руководители: доктор физ.м.н. Н.Г.Басов, канд.ф.м.н. Б.М.Чихачев. Исполнители - В.В.Никитин, канд.ф.мн. Б.Д,Осипов

1	2	3	4	5	6
			2. Теоретическое рассмотрение различных схем измерения гравитационных эффектов общей теории относительности.		
67.	Исследование сверхтонкой структуры спектра молекулы ND_3	Начало 1960г. переходит на 1961г.	С помощью радиоспектроскопа на пучке молекул будет исследована сверхтонкая структура этой молекулы. Будет интерпретирован спектр на основе ранее созданной теории	Отчет или статья	Руководитель - доктор ф.мн. Н.Г.Басов, Исполнители - мл.н.сотр. К.К.Свидзинский, инж.В.С.Зуев.
68.	Молекулярные стандарты частоты (времени)	Начало 1960г. переходит на 1961г.	1. Создание молекулярных часов и молекулярного стандарта частоты с точностью $\sim 10^{-10}$		Руководители - доктор ф.м.н. Н.Г.Басов, канд.т.н. Г.М.Страховский. Исполнители - инж.В.В.Никитин, инж.А.Н.Ораевский, инж.И.В.Черемискин
			2. Исследование возможности использования медленных молекул для повышения стабильности частоты молекулярного генератора (до 10^{-11}) (экспериментальное исследование)		
			3. Исследование зависимости частоты молекулярного генератора на пучке молекулы NH_3 от различных параметров (линия 3,2)	Статья или отчет	

I	2	3	4	5	6
69.	Исследование возможности создания новых типов молекулярных генераторов	Начало I960г. переходит на I961г.	I. Исследование возможности получения отрицательных температур при возбуждении молекул газов электронными ударами	Статья или отчет	Руководители: доктор ф.м.н. Н.Г.Басов, канд.ф.м.н. Д.Ш.Маш. Исполнители - О.И.Крохин.

Б. <u>Сектор молекулярных усилителей</u>

I	2	3	4	5	6
70.	Создание парамагнитных усилителей бегущей волны	Начало I960г. Конец I960г.	Разработка замедляющей системы для волны λ - 3,2см. Создание макета усилителя. Измерение параметров усилителя.	Статьи или отчет	Работа проводится совместно с НИИ. Руководитель доктор ф.м.н. проф.А.М.Прохоров. Ответств.исполнит канд.ф.м.н. Н.В.Карлов, инж.Ю.П.Пименов, инж.Е.К.Карлова.
7I.	Некоторые теоретические вопросы квантовой радиофизики	Переходит с I959г. на I96Iг.	Будут разработаны некоторые теоретические вопросы, ~~ини~~ связанные с квантовой радиофизикой	Статья или отчет	Руководитель и исполнитель канд.ф.м.н. Ф.В.Бункин

1	2	3	4	5	6
2.	Создание парамагнитных усилителей миллиметрового диапазона волн	Начало I960г. переходит на I96Iг.	Радиоспектроскопическое исследование веществ, пригодных для усилителей миллиметрового диапазона волн. Расчет различных вариантов усилителей. Проведение экспериментальных исследований для выбора окончательного варианта усилителей	Статья или отчет	Руководитель - доктор ф.м.н. проф.А.М.Прохоров Отв.исполнитель канд.ф.м.н. Н.В.Карлов
.	Исследование физических процессов в парамагнитных усилителях	Переходит с I959г. переходит на I96Iг.	Исследование релаксационных процессов в парамагнитных усилителях	Статья или отчет	Руководители: канд.ф.м.н. А.А.Маненков, доктор ф.м.н. А.М.Прохоров, Отв.исполнитель канд.ф.м.н. Н.В.Карлов, канд.ф.м.н. А.А.Маненков, инж.Пименов.
.	Создание парамагнитного усилителя дециметрового диапазона волн	Начало I960г. переходит на I96Iг.	Будет разработана замедляющая система для усилителя дециметрового диапазона волн		Руководитель - доктор ф.м.н. проф.А.М.Прохоров. Отв.исполнитель канд.ф.м.н. Н.В.Карлов, инж.Ю.П.Пименов, инж.Е.К.Карлова. Работа будет вестись совместно с НИИ.

I	2	3	4	5	6
75.	Использование парамагнитного усилителя в радиотелескопе	Начало 1960г. конец 1961г.	Будет установлен парамагнитный усилитель на радиотелескопе	Отчет или статья	Руководители - канд.ф.м.н. Н.В.Карлов, доктор ф.м.н. проф.А.М.Прохоров. Работа будет вестись совместно с сектором радиоастрономии
76.	Исследование спин-решеточной релаксации в ионных кристаллах и в свободных радикалах при сильном обменном взаимодействии	Переходит с 1959г. переходит на 1961г.	Будут исследованы релаксационные явления при различных температурах и на двух частотах с целью выяснения механизма релаксации при очень низких температурах	Статья	Руководители - канд.ф.м.н. А.А.Маненков, доктор ф.м.н. проф.А.М.Прохоров, отв.исполнитель - канд.ф.м.н. А.А.Маненков, аспирант П.Пашинин
77.	Создание молекулярных генераторов и усилителей в миллиметровом и субмиллиметровом диапазоне волн с использованием молекулярных пучков.	Начало 1960 г. Переходит на 1961г.	Будут построены радиоспектроскопы высокой разрешающей силы в миллиметровом диапазоне волн с использованием молекулярных пучков	Статья или отчет	Руководители - доктор ф.м.н. проф.А.М.Прохоров, канд.ф.м.н. А.И.Барчуков, отв.исполнитель канд.ф.м.н. А.И.Барчуков, инж.Т.М.Мурина, инж.А.В.Прохиндеев

I	2	3	4	5	6
78.	Исследование вращательных спектров	Переходит с I959г. переходит на I96Iг.	Будут изучены вращательные спектры веществ с целью определения структуры, дипольных моментов и потенциальных барьеров молекул	Статья или отчет	Руководители - канд.ф.м.н.Н.А.Ирисова, канд.ф.м.н.И.А.Мухтаров, д-р ф.м.н.профессор А.М.Прохоров. Отв.исполнители - канд.ф.м.н.Н.А.Ирисова, инж.Т.М.Мурина, канд.ф.м.н.И.Мухтаров, канд.ф.м.н.А.Емельянов, мл.н.с.Г.П.Шипуло
79.	Исследование спектров парамагнитного резонанса и электронно-ядерного резонанаса	Переходит с I959г. переходит на I96Iг.	Будут изучаться спектры парамагнитного резонанса ионных кристаллов и свободных радикалов	Статья или отчет	Руководители - к.ф.м.н.А.А.Маненков, д-р ф.м.н.проф.А.М.Прохоров, Отв.исполнители - мл.н.с.Л.Н.Бородовская, мл.н.с.С.Д.Кайтмазов, к.ф.м.н.А.А.Маненков.
80.	Исследование спектров квадрупольного резонанса	Начало I959г. переходит на I96Iг.	Будут изучены спектры квадрупольного резонанса ряда веществ	Статья	Руководитель - доктор ф.м.н. проф.А.М.Прохоров, отв.исполнитель - Г.П.Шипуло
8I.	Исследование спектров ядерного резонанса	Начало I960г. переходит на I96Iг.	Будет построена установка для изучения химических сдвигов методом ядерного резонанса	Статья или отчет	Руководитель - канд.ф.м.н.В.Веселаго, д-р ф.м.н.А.Прохоров отв.исполнитель - канд.ф.м.н.В.Веселаго

Прохоров

I	2	3	4	5	6

Проблема: "РАДИОАСТРОНОМИЯ"

Сектор радиоастрономии

83.	Изучение явлений, связанных со вспышками радиоизлучения на Солнце.	переходит с 1959 г. переходит на 1961 г.	1. Изучение условий ускорения заряженных частиц на Солнце до релятивистских энергий (возникновение солнечных космических лучей). 2. Исследование физических условий в солнечной короне применительно к объяснению спокойной и слабо возмущенной компоненты радиоизлучения Солнца. 3. Исследование причин и условий возбуждения плазменных колебаний применительно к объяснению радиоизлучения всплесков.	Статьи	Руководитель: А.А.Корчак. Ответ.исполнит. Б.Н.Пановкин М.В.Конюков
84.	Исследование радиоизлучения слабовозмущенного Солнца.	переходит с 1959г. переходит на 1961г.	1. Исследование поляризации радиоизлучения на волне 1,5 м. 2. Исследование динамических спектров вспышек радиоизлучения. Изучение механизма радиоизлучения. 3. Исследование структуры неоднородностей сверхкороны Солнца.	Статьи или отчеты	Руководитель: к.ф.м.н. В.В.Виткевич Ответ.исполнит. Ю.И.Алексеев Л.И.Матвеенко В.И.Бабий М.В.Горелова

Report for 1960

This report for 1960 presents the main results of the research conducted in the Laboratory of oscillations, the General Assembly of International Union of Radio Science (URSI) held in London in September 1960, and the main conclusions of trips of Soviet physicists to US and Japan. The conclusion of the trips to the US concerned particle physics and application of masers in radio astronomy, the conclusion of the trip to Japan concerned theoretical physics.

Reference:

"Otchet za 1960 ARAN, F. 532, Op. 1 D. 338, pp. 26-51." 1960.

Изучались случаи упругого рассеяния π^- - мезонов с энергией 2,8 Bev на ядрах углерода в пропановой пузырьковой камере (углы рассеяния I°-5°). Показано, что экспериментальное распределение углов рассеяния хорошо описывается кривой дифракционного рассеяния на углероде, вычисленной по оптической модели, исходя из полного сечения $\pi - N$ взаимодействия равного 30 mb . Кривые, рассчитанные с учетом реальной части амплитуды рассеяния на нуклоне, соответствуют потенциалу + 30 и -30 Мэв, не согласуются с экспериментальной гистограммой. Работа ведется совместно с ОИЯИ, где эксперимент поставлен для другой области энергий π^- - мезонов - 6,8 Бэв. Из сравнения данных можно будет сделать заключение о зависимости "прозрачности" нуклона от энергии налетающей частицы.

(инженер-физик Котенко Л.П., инженер-физик Кузнецов Е.П., инженер-физик Мерзон Г.И.).

В лаборатории колебаний продолжались работы по проблемам "КВАНТОВАЯ РАДИОФИЗИКА" и "РАДИОСПЕКТРОСКОПИЯ".

В 1960 году получены следующие наиболее интересные результаты.

Были исследованы спектры и релаксационные явления в различных парамагнитных кристаллах:

$SrS.Mn$, $SrS.Cd$, $K_3Co(CN)_6 . K_3Cr(CN)_6$

(ст.н.с. А.А.Маненков, мл.н.с. П.П.Пашинин, чл.-корр. АН СССР А.М.Прохоров).

Предложенный в лаборатории колебаний новый тип резонаторов уже используются для проведения радиоспектроскопических измерений. Были определены дипольные моменты молекул CH_2O и CH_3GeH_3. Показано, что дисковые резонаторы пригодны для точного определения дипольных моментов, а также для исследования спектров в миллиметровом диапазоне волн (ст.н.с. А.И.Барчуков, к.ф.м.н.Т.М.Мурина, чл.-корр. АН СССР А.М.Прохоров). Исследован вращательный спектр молекулы H_2NCN (мл.н.с. Г.П.Шипуло).

Впервые осуществлен спиновой генератор с использованием линий ядерного резонанса (к.ф.м.н. В.Г.Веселаго).

Исследованы характеристики молекулярного генератора на ND_3 (д.ф.м.н. Н.Г.Басов, мл.н.с. В.С.Зуев). Исследована зависимость ширины спектральной линии пучка аммиака в зависимости от типа источника молекулярного пучка, а также от давления.

(Н.Г.Басов, ст.н.с. Г.М.Страховский, к.ф.м.н. И.В.Черемискин)

Создана новая конструкция молекулярного генератора для специальных исследований (д.ф.м.н. Н.Г.Басов, ст.н.с. Г.М.Страховский, ст.н.с. Б.М.Чихачев).

Предложен новый метод получения отрицательных температур в полупроводниках с использованием непрямых переходов (д.ф.м.н. Н.Г.Басов, мл.н.с. О.Н.Крохин, ст.н.с. Ю.М.Попов).

Среди других вопросов на конференции рассматривались вопросы имеющие непосредственное отношение к работам, проводимым в ФИАНе. Это вопросы исследования поверхностных явлений в полупроводниках, вопросы радиационной физики, туннельные диоды, сверхвысокий вакуум. Конференция позволила ознакомиться с новейшими достижениями в этих областях физики.

По окончании работы конференции члены советской делегации посетили в общей сложности 12 различных лабораторий и ознакомились с проводимыми в них работами. Данная поездка в США оказалась весьма эффективной.

XШ конгресс *URSI*

От ФИАНа принимали участие: А.М.Прохоров и В.В.Виткевич.

Для ФИАНа представила интерес, главным образом, работа секций радиоастрономии и квантовой радиофизики. Из докладов и частных бесед удалось установить, что квантовые усилители уже практически используются в радиотелескопах США, за рубежом достигнуты большие успехи в создании параметрических усилителей с электронным пучком, обладающие малым уровнем собственных шумов.

Ряд лабораторий западных стран развертывают работы, связанные с использованием сверхсильных магнитных полей, в частности, для монохроматической генерации микронных радиоволн. Учитывая важность постановки таких работ дирекцией ФИАНа принято решение о создании аналогичных установок. Следует вообще заметить, что, как правило, работы доложенные на конференциях появляются в печати спустя примерно год. Например, на конгрессе особенно привлекло внимание сообщение об открытии радиационных поясов вокруг Юпитера, аналогичных тем, которые недавно обнаружены вокруг Земли. Однако, до сих пор в печати подробных сообщений об этом нет.

теоретической работой, используя экспериментальные данные, полученные в лаборатории колебаний. Работы Д.Рея полезны и для лаб.колебаний, так как они позволяют правильно интерпретировать экспериментальные результаты. В ближайшее время выйдет в свет ряд совместных работ д-ра Рея и сотрудников лаборатории.

В течение года в ФИАНе организовывались чтения лекций зарубежных ученых. Всего было прочитано около десяти лекций, в основном, американскими физиками. В большинстве случаев содержание лекций повторяло сведения, имеющиеся в литературе или известные из материалов международных конференций.

Большое место в международных связях ФИАНа занимал прием иностранных специалистов с целью ознакомления с лабораториями Института. С указанной целью всего было принято в 1960 г. 89 ученых из 14 западных стран, из них 39 человек из США.

Основные выводы.

В 1960 г. ФИАН впервые получил возможность подробно ознакомиться с ускорительной техникой США (поездка А.Коломенского) с работами по радиоастрономии и радиотелескопам США (поездка В.Виткевича) и с состоянием развития теоретической физики в Японии (поездка М.Маркова). Из анализа этих поездок можно сделать следующие основные выводы.

1. США успешно осуществляя в настоящее время большую программу строительства ускорителей на сверхвысокие энергии, большие, чем это имеется в Советском Союзе, очевидно в ближайшие годы будут сохранять и инициативу в получении важных экспериментальных результатов, в частности, по элементарным частицам.

2. Общий объем строительства радиотелескопов в СССР заметно отстает от такового в США. Для радиоастрономических наблюдений используются молекулярные усилители, широко применяется автоматическое управление радиотелескопами, вычислительные электронные машины. В США широко развита радиолокационная радиоастрономия, радиолокация Луны, планет, Солнца.

3. Поездка М.А.Маркова в Японию показала, что следует уделять самое пристальное внимание развития физики в Японии. Необходимо запланировать на ближайшие годы ряд ознакомительных поездок советских ученых по различным разделам науки в Японию.

Report for 1961

The first time optical molecular generators (lasers) are mentioned in an open laboratory report is in this surprisingly short report for 1961. Yet that was the year they built their first laser (the first American lasers were launched at the end of 1960). Most of the work was described in classified reports. See the bulky report for 1961 in Starodub, A. N. (Ed.). (1997). Zapiski Arkhivariusa (Notes of Archivist). FIAN (P. N. Lebedev Institute), Moscow. pp. 248–415.

Reference:

"Otchet Za 1961. ARAN, F. 532, Op. 1 D. 353, P. 98." 1961.

98 ~~99~~

О т ч е т
лаборатории колебаний ФИАН за период 1961г.

Лаборатория провела широкие физические исследования различных парамагнитных кристаллов, некоторые из которых, как было показано, обладают новыми ценными свойствами, улучшающими характеристики квантовых парамагнитных усилителей, предназначенных для работы в радиодиапазоне.

На основе этих новых материалов созданы новые макеты квантовых усилителей.

Создан усилитель бегущей волны на $\lambda \doteq 3{,}2$ см, предназначенный для работы на радиотелескопе.

Проведены физические исследования различных полупроводников с целью выяснения условий получения инверсной заселенности уровней в них. Впервые показано, что при пробое полупроводника InSb, благодаря магнитному сжатию, образуется шнур тока. При этом плотность электронов достигает концентрации $10^{16} \div 10^{17}$ $см^{-3}$.

Был проведен ряд теоретических исследований в области квантовой радиофизики. В частности, было предложено использовать ~~тонкие~~ е диэлектрические нити для квантовых усилителей и генераторов оптического диапазона. Эти системы обладают рядом преимуществ.

Для получения высокой плотности излучения в оптическом диапазоне волн предложено использовать накопления частиц на верхнем уровне с последующим резким увеличением добротности, что создает условия для мощной импульсной генерации.

"27" ноября 1961г.

Verbatim report of the session of A.M. Prokhorov's defense (Kandidat Nauk).

The session of the academic council of the Physical Institute of the Academy of Sciences USSR on January 10, 1946, was presided over by Sergei Vavilov. On the agenda of the day was the defense of Alexander Prokhorov's dissertation, titled "Frequency Stabilization in the Theory of Small Parameters". Present were leading Soviet physicists such as Sergei Rytov, Semion Khaikin, Boris Vvedensky, Nikolai Papaleksi and Pavel Cherenkov.

Reference:

"Stenograma Zasedaniia Uchenogo Soveta ot 10 Ianvaria 1946. ARAN, F. 532, Op. 1, D. 122. pp. 1-10." 1946.

1 43

С т е н о г р а м м а

заседания Ученого совета Института ФИЗИКИ АН СССР

от 10 января 1946 г.

Председатель - акад.С.И.Вавилов

Акад.ВАВИЛОВ

Товарищи, позвольте открыть заседание Ученого совета Физического института. Сегодня у нас на повестке защита диссертации т.Прохоровым А.М. на соискание ученой степени кандидата физико-математических наук. Тема "Стабилизация частоты в теории малого параметра". Официальные оппоненты д-р физ.мат.наук Теодорчик и д-р физ.-мат.наук Хайкин.

Попрошу огласить биографические данные.

тов.ЧЕРЕНКОВ

/оглашает данные личного дела/.

акад.ВАВИЛОВ

Будут вопросы по поводу оглашенных документов? Нет, переходим к диспуту.

тов.ПРОХОРОВ

Теоретическое рассмотрение вопроса о стабилизации частоты с помощью стабилизаторов предпринималось многими авторами, причем все они трактовали этот вопрос с точки зрения линейной и квази-линейной теории. В прошлом году С.М.Рытов указал, как может быть поставлена задача в строгой нелинейной теории и в нашей совместной работе мы дали строгую теорию этого явления. Было получено полное описание явления стабилизации, а также возможность указать пределы применимости квази-линейной те

Как показано в совместной работе, задача стабилизации частоты не сводится просто к задаче об автоколебании генер тора с двумя слабо связанными контурами. Уравнение имеет такой общий вид:

/формула/

$$\left.\begin{aligned} \ddot{x}+x &= \mu f(x,\dot{x},y,\dot{y}) \\ \ddot{y}+y &= \mu g(x,\dot{x},y,\dot{y}) \end{aligned}\right\} \qquad /1/$$

В задаче о стабилизации частоты нужно учесть особеннос стабилизатора, например, в случае кварца - малость его дек мента затухания и большое значение его волнового сопротивл ния. Учет особенностей кварца и всякого другого стабилизат ра приводит к тому, что вместо этих уравнений /1/ нужно рас-сматривать уравнение /2./

/формула/

$$\left.\begin{aligned} \ddot{x}+x &= \mu f(x,\dot{x},y,\dot{y}) \\ \ddot{y}+y &= \mu^2 g(x,\dot{x},y,\dot{y}) \end{aligned}\right\} \qquad /2/$$

Это уравнение отличается от уравнения /1/ тем, что прав части имеют ~~другой~~ разный порядок малости относительно параметра

В случае схемы затягивания это имеет простое наглядное толкование в том смысле, что стабилизация частоты представ ляет собой захват генератора кварцем. Об этом упомянуто в совместной работе и подробно я на этом сейчас останавливат не буду.

Из этих уравнений получается, что частота колебания бу изменяться лишь в порядке μ^2, при настройке анодного контура - в порядке μ.

Когда работа была закончена, я обнаружил, что решение уравнения /2/ может быть ~~решено~~ получено из уравнения /1/ следующим способом. Нужно в решении уравнения /1/ повысить порядок малости коэфициентов таким образом, чтобы при этом само уравнение /1/ переходило в уравнение /2/. Мы можем исходить прямо из решения уравнения /1/.

Может показаться на первый взгляд, что такое повышение порядка малости в окончательных решениях является тривиальным. Однако, это не так. Возьмем напр. случай автоколебаний при сильно связанных контурах. Если мы хотим рассмотреть случай слабой связи, мы должны решить задачу заново, а нельзя переходить к слабой связи в окончательных решениях, так как мы в этом случае получим неверные результаты. Но в задаче о стабилизации есть одна особенность, благодаря которой такой переход дает то, что нужно. Особенность та, что у нас правая часть одного из уравнений целиком меняет свой порядок малости, т.е. вместо $g(x,\dot{x},y,\dot{y})$ у нас будет $\mu g(x,\dot{x},y,\dot{y})$. Эта особенность дает законное основание повышения порядка малости в окончательных формулах.

Спрашивается, какими преимуществами обладает этот способ решения перед непосредственным решением уравнения 2? По этому поводу можно сказать следующее.

Решение уравнения 1 нужно производить до порядка μ, в время как решение уравнения 2 нужно производить до по- рядка μ^2. Больше того, решения уравнения 1 являются бо общими, чем решения уравнения 2. Следовательно, мы все можем сказать, когда решение уравнения 2 перестает дав правильные результаты, исходя из решения уравнения 1. Кроме того, напр. в случае схемы затягивания для урав ния 2 необходимо решать задачу три раза. У нас получае нестабилизационный режим и переходный режим, связываю стабилизационный и нестабилизационный режимы. Для урав ния 1 мы имеем всего одно решение, которое содержит тр решения уравнения 2. Таким образом, предлагаемый путь является в некотором отношении полезным.

Возникает вопрос об однозначности перехода из урав нения 1 к уравнению 2. Вообще говоря, вопрос сводится тому, какие величины мы будем считать нулевого порядка какие [illegible] - величи первого порядка относительно μ и т.д. Всегда имеется ряд выборов, формально эквивалентных друг другу, но не все они являются равноценными в смысле соответствия на физическим представлениям. Об этом говорилось в работе С.М.Рытова и на этом я останавливаться не буду.

Для того, чтобы ограничить выбор порядка малост мы сделаем следующее естественное предположение, что все особенности при таком переходе мы будем относить счет стабилизатора, в частности, кварца, оставляя порядок величин генератора теми, которые мы выбрали, еслибы стабилизатора не было, при этом считая, что декремент затухания стабилизатора будет порядка μ^2. Если сделать такое ограничение, то оказывается, что мы будем иметь всего два варианта перехода от уравнения 1 к уравнению 2 для обоих основных схем стабилизации - осциллаторной и затягивания.

Эти два варианта перехода сводятся к тому, что мы можем иметь два типа стабилизаторов, а именно, в одном случае у нас может быть стабилизатор, где малость декремента затухания, которая может быть записана в таком виде $\frac{R_2}{\sqrt{\frac{L_2}{C_2}}}$) обусловлена большим волновым сопротивлением. Наоборот, может быть второй тип стабилизатора, у которого волновое сопротивление такого же порядка, как у контура, но благодаря тому, что сопротивление будет очень мало, у него будет малый декремент затухания.

Первый случай - случай большого волнового сопротивления характерен для кварца. Второй случай характерен для эндовибратора.

Для осцилляторной схемы получается несколько отличное уравнение. В чем заключается различие? Для краткости я буду называть стабилизатор, в котором малость декремента обусловлена большим волновым сопротивлением, кварцем, а второй тип буду называть эндовибратором. Тогда получается, что если мы

имеем кварц, то воздействие на контур в порядке μ буд
происходить не только через лампу, но также непосред
но через емкость С. Это обусловлено тем, что энергия
стабилизаторе будет гораздо больше, чем энергия, соср
точенная в контуре.

Если же мы имеем стабилизатор типа эндовибратора, т
действие эндовибратора на контур будет происходить в
рядке μ только через управляющую сетку лампы. Отсюда
роль лампы для получения ~~этих~~ хороших стабилизаций. Чем бол
ассиметрия, тем лучше будет стабилизация.

Для схемы затягивания, которая здесь зарисована,
два выбора приводят к одним и тем же уравнениям. Это
чается из-за того, что энергия стабилизатора в обоих
ях будет очень большая по сравнению с энергией конту
В одном случае, в случае большого волнового сопротивл
энергия стабилизатора /формула/ $\frac{1}{2} L_2 J_2^2$
будет больше, потому что L_2 велика, а во втором
возбуждается такой большой ток, что энергия становит
очень большой.

Итак, первым вопросом моей диссертации являлось
чение всех выведенных нами формул для период
го решения, устойчивости таким переходом окончательны
решений к уравнению 1. Это было получено, причем для
мы затягивания из одного решения получились все 3 ре

для случая стабилизации, и ничего удивительного здесь нет.

Я хочу проиллюстрировать на частном примере, что происходит с кривой амплитуды стабилизатора для осциллографической схемы в случае такого перехода.

/чертеж/

Если у нас есть два обыкновенных контура, то амплитуда колебания сеточного контура будет иметь такой вид. Колебание у нас будет по обе стороны точки резонанса. Если же мы вместо контура включим сюда стабилизатор, то оказывается, что тангенс этого угла одной из асимптот становится бесконечным, т.е. эта асимптота сольется с осью ординат. Тогда квадрат колебания в зависимости от расстройки будет иметь такой вид.

/чертеж/

То есть, в этом случае у нас колебание будет существовать по одну сторону от точки резонанса.

~~Я хотел бы привести некоторые экспериментальные данные~~ по осциллографической схеме и схеме затягивания.

/демонстрация диапозитивов/

Я хочу остановиться еще на схеме затягивания вот на чем/чертеж/.

Было указано, что для схемы затягивания наилучшая стабилизация на границе срыва, иными словами, что изменение частоты при расстройке на границе срыва будет меньше, чем на других частях устойчивой кривой. Чем определяется эта

величина? Она будет равняться удвоенному произведению з
хания стабилизатора на величину регенерации /формула/
т.е. стабилизация будет тем лучше, чем меньше затухание
стабилизатора и чем больше регенерация. Как мы видим, о
не зависит от величины связи между стабилизатором и кон
Увеличение связи дает увеличение петли затягивания, но
не улучшает стабилизации.

Второй вопрос, на котором я хотел остановиться, э
какие еще причины, какие факторы влияют на частоту коле
а именно, характеристика лампы, анодной реакции и сето
тока.

Мы пользовались характеристикой лампы в виде кубиче
полинома. Тогда было получено, что частота колебаний в
порядке m^2 не зависит от характеристики лампы, а с дру
стороны известно, что в простом ламповом генераторе ча
колебаний зависит от этого. Таким образом, стабилизато
устраняет влияние параметров лампы.

Может показаться, что это справедливо только для
случаев характеристики лампы. Я показал, что это спра
ливо для любой характеристики. Но в каком порядке ска
гармоники на частоту стабилизированных колебаний? Для
затягивания они будет зависеть в порядке m^3, а для ос
ллографической схемы в порядке m^4. Это объясняется тем,
в схеме затягивания напряжение на сетку подается с ко

в то время как для осцилляторной схемы напряжение на сетку лампы подается со стабилизатора.

Второй вопрос – это влияние анодной реакции. У нас анодный ток будет являться функцией анодного напряжения.

/формула/

Для пентродов Д практически равно нулю, для триодов оно достигает больших величин. Как оно будет влиять на частоту колебания?

В случае осцилляторной схемы влияние будет в порядке μ^2, но это влияние будет зависеть только от самой схемы, т.е. и здесь в осцилляторной схеме параметры лампы не войдут в частоту стабилизированных колебаний.

Что касается схемы затягивания, то влияние Д в порядке μ^2 не скажется, а скажется только в порядке μ^3. В области хорошей стабилизации влияние Д сильно ослабляется.

Последний фактор – это влияние сеточного тока. Учет сеточного тока для схемы затягивания приводит к тому, что у нас изменяется вид характеристики лампы. Как я уже сказал, характеристика лампы влияет в случае схемы затягивания на частоту колебания только в порядке μ^3, следовательно наличие сеточного тока влияет на частоту только в порядке μ^3, т.е. относительно мало.

Что касается осцилляторной схемы, тот тут вопрос обстоит хуже, ибо сеточный ток будет влиять или в порядке μ или в порядке μ^2 в зависимости от типа применяемого стабилизатора.

Если стабилизатор типа эндовибратора, то частота к
ний будет уже зависеть от сеточного тока в порядке μ,
практически никакой стабилизации не будет. Если же у н
имеется стабилизатор типа кварца, то влияние сеточного
сказывается в порядке μ^2, это уже более или менее хор

Таким образом, отсюда видно, что желательно иметь
билизатор, у которого малость декремента обусловлена е
большим волновым сопротивлением, причем при ~~включении~~ под
стабилизатора к какой либо схеме, всегда будут иметь м
потери. В случае эндовибратора потери будут итти за сч
отверстий.

Я хочу сейчас рассмотреть пример, как можно подбира
стабилизатор. /чертеж/

Если мы имеем эндовибратора прямоугольный с квадратны
ванием, если мы выберем такое колебание, электрически
которого направлен параллельно высоте Н, то длина вол
основного колебания будет определяться по формуле /фо
т.е. она не будет зависеть от высоты Н.

Посмотрим, как будет зависеть волновое сопротивлен
от высоты эндовибратора /чертеж/

У нас эта функция будет иметь следующий вид. Мы видим
декремент затухания велик при малых колебаниях высоты
высота больше, чем λ, то величина Н мало умень
мент затухания. С точки зрения декремента затухания
смысла брать очень большую высоту Н.

Но если мы посмотрим, как меняется волновое сопротивление, то волновое сопротивление растет линейно с высотой Н.

Таким образом, как я уже указывал, нам выгодно иметь стабилизатор, у которого велико волновое сопротивление. Поэтому имеет смысл брать высоту много больше, чем λ.

Последнее, на чем я хочу кратко остановиться, это схема с ~~1 1/2~~ 1,5 ступенями свободы. Эта схема вот какая /чертеж/ Здесь можно также ввести порядок m. Мы можем легко найти решение в виде ряда m, так как при $m=0$ решение находится легко. Эта схема напоминает по своему поведению осцилляторкую.

Я приведу некоторые экспериментальные результаты по этой схеме.

/Демонстрация диапозитивов/.

Я здесь, конечно, не приводил все мои эксперименты, с одной стороны, а с другой стороны, не останавливался на экспериментальной установке за недостатком времени.

<u>акад. ВАВИЛОВ</u>

Будут ли вопросы к т. Прохорову?

<u>проф. ~~ПРОХОРОВ~~ ХАЙКИН</u>

В таком общем виде Ваша установка правильна. Вы ищете условий наилучшей стабилизации. Рассматривается вопрос о том, где частота колебаний не зависит ни от чего, но практически это не всегда интересно. Это обычно делается этими

связями. Вы не попробовали с этой точки зрения ра
ривать задачу, рассматривать, не следует ли подби
эти связи для того, чтобы достичь удовлетворитель
стабилизации. Это компромиссное решение.

ПРОХОРОВ

Я не рассматривал этого, но это можно сделать.

проф.ХАЙКИН

Этот вопрос важен для практики.

Вопрос о перескакивании частоты. Тогда кварц ст
2 системами, Вы можете при изменении настройки ра
либо при одной стабилизации, либо при другой. Это
вполне укладывается в вашу схему расчетов. Вы не
бовали по этому поводу ничего сделать?

тов.ПРОХОРОВ

Я не пробовал.

акад.ВВЕДЕНСКИЙ

Вы только что сказали, что затухание такой сис
определяется главным образом дырками связи. В ка
то, что Вы говорили, увязывается с действием дыр

тов.ПРОХОРОВ

Я более конкретно не рассматривал. Если дырки
велики, для того, чтобы избавиться от них, нужно
большую высоту Н.

акад.ПАПАЛЕКСИ

Ал.Мих., Вы нам Вашу экспериментальную прове
рии показали. Было бы интересно узнать принципы

методики и проводили ли Вы оценку ~~[illegible]~~ точности Ваших экспериментальных результатов?

тов.ПРОХОРОВ

На методике я могу кратко остановиться. Методика была у меня такая.

/чертеж/

Исследуемый кварцовый генератор, колебание которого передается на лампу 6/7. ~~Это вещь передается затем на~~ осциллограф. С другой стороны, от другого стабилизированного генератора передается на вторую сетку ~~откатку~~. Фиксирование разности частоты этих частот производится по звуку генератора, это можно сделать совершенно точно. ~~Если манипулирую там~~, Маленькие изменения частоты легко заметить на осциллографе. Такую точность я не могу соблюдать в опытах, потому что отсчет по шкале генератора был 3-5%, с другой стороны, влияние других параметров, скажем, характеристики лампы такого же порядка 3-5%, если я буду менять смещение ~~или крутизну лампы, то у меня частота на несколько периодов изменится.~~

акад.ПАПАЛЕКСИ

Вы проводили эти расчеты, основываясь на простейших формах линейности или Вы пробовали более высокие степени?

тов.ПРОХОРОВ

Я поступал следующим образом. Я измерял величины, которые не зависят от характеристики лампы, поэтому эти вещи меня уже не интересовали.

Зависимость от анодной нагрузки или граница устойчив стабилизационного режима - все это не входит в характ стику лампы. ~~Поэтому естественно у меня так получилос~~

акад.ПАПАЛЕКСИ

Расчеты Вы не производили?

тов.ПРОХОРОВ

Нет, не производил.

ВОПРОС

Не могли ли бы Вы кратко перечислить, что являлось том экспериментального исследования.

тов.ПРОХОРОВ

Общий ответ - это проверка теории. Для осцилляторно схемы производилось выяснение основной зависимости ча ты колебания от настройки, во вторых, определение дек та затухания кварца. Далее для схемы затягивания тоже ~~проводилась зависимость частоты колебаний от настройк~~ проводилась зависимость границы срыва колебаний от св между кварцем и контуром.

/чертеж/

У меня получается в теории совершенно линейная за Это подтверждение экспериментом в очень широких преде Область хорошей стабилизации не зависит от величины связей. Для схемы с 1 1/2 ступенями свободы производ зависимость частоты от емкости анода и катода и зави

частоты колебаний от анодной нагрузки. Для схемы затягивания был произведен эксперимент зависимости амплитуды от настройки в ~~остальных~~ относительных величинах по отношению к не-стабилизационным.

Далее был произведен ~~следующий~~ эксперимент /чертеж/. Когда существует изолированный стабилизированный режим. Изменением ~~Значение параметра в схеме затягивание, мы идем на стабилизационный режим,~~ настройки ~~но~~ на стабилизационный режим нельзя попасть. Попасть на него можно следующим образом: сомкнув эти 2 ветви и изменив какой либо параметр так, чтобы делать перемычку. Тогда мы получим срыв слева и справа ~~сюда~~. Это экспериментально было проведено. Вот собственно все.

тов.ЛАНСБЕРГ

Сколь слабый декремент доступен измерению этой схемой и насколько велика точность?

тов.ПРОХОРОВ

Я не рекомендовал бы это для измерения декремента. У вас всегда есть величина сопротивления .... еслибы декремент затухания был равен нулю, то из-за наличия вредных сопротивлений вы, конечно, имели конечный декремент затухания ~~не могли бы точно промерить это~~

акад.ВАВИЛОВ

Есть еще вопросы ? Нет, тогда переходим к заслушанию отзывов официальных оппонентов.

Проф.ТЕОДОРЧУК

/отзыв прилагается/

тов.ПРОХОРОВ

Первое замечание терминологическое. Нормальная частота -

это термин, который применяется к линейным системам. применении к нелинейным системам нужно принимать те частоты, которые ~~неравны~~ соответствующим равным нулю. Если я беру слабую св то у нас автоматически связь равна нулю, (при $\mu=0$) поэтому час нормальными ~~с нормальными~~ будут совпадать. Я беру решение в виде нулевого реше Для 1-го и для 2-го случая ~~одинаково.~~ колебаний

Второй вопрос – это о пределах допустимости ~~шири~~ с меньшим знаком параметров. Это вопрос сложный, ког можем переходить и когда нет. Взяв достаточно больше личество членов, мы всегда можем добиться лучшего сс дения. Этот вопрос в общем виде нигде не исследова: В случае стабилизации эксперимент здесь идет навстре одной стороны, с другой стороны, когда у нас два од параметра, мы можем сравнивать.

Третий вопрос относительно общего условия стаби. ции. Можно видеть, что это условие является ~~не~~доста условием для получения хорошей стабилизации.

акад.ВАВИЛОВ

Будут ли какие либо замечания по поводу отзыва чика? Нет, тогда разрешите перейти к заслушанию мне официального оппонента проф.Хайкина.

проф.ХАЙКИН

/отзыв прилагается/.

тов.ПРОХОРОВ

Первое замечание, которое было сделано, это в того, что недостаточно ясен вопрос, какие величины будем считать малыми и какие очень малыми. Я этот вопрос в своей диссертации нее ставил, но я преследовал более узкую цель - показать, что решение уравнений типа 2 можно получить из более общих уравнений и каким образом переходить от этих уравнений к уравнениям стабилизированным. Этот вопрос может быть следовало бы ~~упомянуть~~ рассмотреть, но вопрос этот сложный и пока нет путей для его разрешения.

Что касается второго замечания, то оно справедливо.

акад. ВАВИЛОВ

Желает ли еще кто либо высказаться по диссертации?

проф. РЫТОВ

Я хотел бы отметить здесь некоторые черты научного склада Ал.Мих., которые я имел возможность констатировать, руководя его работой и которые я считаю чрезвычайно положительными.

Прежде всего я хотел бы отметить сочетание способностей экспериментатора у А.М. с хорошими теоретическими способностями. Экспериментальное искусство А.М. само по себе, как мне кажется, должно быть оценено достаточно высоко. Он работает тщательно, работает точно и вместе с тем работает легко и быстро, работает без натуги. Эта легкость в экспериментальной работе, я бы даже сказал, непринужденность в эксперименте связана с имеющимся у него довольно большим опытом в области работы. Но в значительной степени она является именно результатом его

умения сочетать свою работу руками с хорошим теоретич
продумыванием вопроса.

Что касается теоретической работы А.М., то и здес
он работает умело и быстро. Он легко ориентируется в
теоретических вопросах, легко хватает суть дела, легк
может войти в новый круг вопросов и, самое главное, ч
мне кажется больше всего следует отметить, это его сп
ность заранее предвидеть, чего можно ожидать на том и
ином пути, т.е. наличие у него того чутья, которое дл
научного работника столь же необходимо, как зрение и
для каждого человека.

А.М. обладает научной инициативой. У нас была прок
очень большая работа по вопросам стабилизации, котора
нашла свое выражение в совместной публикации. В диссе
цию А.М. вошли только те вопросы, которые были постав
им самим и им самим были приведены в ясность.

Я хотел бы прежде всего отметить с теоретической с
ны замеченный и проведенный им способ прихода к пред
в окончательных результатах. Затем очень ясно в дисс
А.М. дано деление стабилизаторов на две группы; ран
в таком подразделении ясности не было.

Надо указать, что весь эксперимент был поставле
проделан им самим.

Также нельзя не отметить чрезвычайно большую ра способность А.М., он работает много и упорно.

Я думаю, что можно полностью согласиться с заключением официальных оппонентов, что ему может быть присуждена степень кандидата физико-математических наук.

акад.ВАВИЛОВ

Желает еще кто либо высказаться? Нет. Тогда переходим к баллотировке. В состав счетной комиссии предлагаются т.т.Черенков, Божулин и Коваленко.

/Протокол счетной комиссии прилагается/

Letters

Correspondence regarding the Meeting of the Faraday Society held in Cambridge from April 4-6, 1955. The correspondence spans from October 1954 to June 1955. The following letters are included:

Tompkins, F. 1954. “to M. M. Dubinin, Oct 29, 1954. ARAN, F. 532; Op. 1, D. 241. pp. 60-61.” Moscow.

Brekhovskikh, L. M., and E. A. Koridalin. 1954. “to V. L. Levshin, Nov 22, 1954. ARAN, f.532; Op. 1, D. 241. P. 59.” ARAN. f.532; Opis 1, delo 241 p.59.

Dobrotin, N. 1954. “to L. M. Brekhovskikh (unsent Draft), Nov, 1954. ARAN, F. 532; Op. 1, D. 241. p.62.” Moscow.

Dobrotin, N. 1955. “To the Foreign Department of the Academy of Sciences, Mar 31, 1955. ARAN, f.532; Op. 1, D. 256. p.28.”

Vul, B. M. 1955. “To the Library of the Academy of Sciences, Jun 24, 1955. ARAN, F. 532; Op. 1, D. 256. p. 49.” Moscow.

…ор, 8
Лондон, В.С.I.
29 октября I954 г.

Академику Дубинину М.М.
Отделение химических наук
Академия наук СССР
Б.Калужская ул., д.3I
Москва

Общая дискуссия на тему:
"Микроволна и радио-частотная спектроскопия"
Кембридж, 4-6 апреля I955 г.

Уважаемый академик Дубинин,

Имею честь обратить Ваше внимание на общую дискуссию Фарадеевского общества, организуемую 4-6 апреля I955 г. на тему: "Микроволна и радио-частотная спектроскопия".

Основная цель Общества в организации этой дискуссии сводится к тому, чтобы собрать научных работников в этой области как экспериментаторов, так и теоретико в и оценить новые открытия и работы, которые получили свое развитие за последние годы. Надеются, что ведущий ученый в этой области откроет дискуссию вступительной речью, в которой изложит настоящее положение указанной проблемы.

Предполагают, что дискуссия будет проходить по следующим четырем основным секциям:

I. Микроволны.

П. Ядерный магнитный резонанс (Nuclear magnetic resonance)

Ш. Парамагнитный резонанс.

IУ. Четырехпольная спектроскопия.

Надеются, что ведущие ученые из различных стран мира примут участие в дискуссии.

Совет Фарадеевского общества высоко оценил бы присутствие предтавителей СССР на этой дискуссии.

Если бы в дополнение к дискуссии кто-либо из Ваших ученых мог бы представить отчет иди дискуссию по своей работе, мы были бы рады получить резюме (в 300 слов) в ближайшее вреия для того, чтобы Организационный комитет смог составить подробную программу.

Программа почти готова и мы не сможем принять более 2-х докладов. Полный текст каждого доклада, который не должен превышать 3.000 слов, необходимо представить к 15 января 1955 г. или не позже конца января.

Мы будем весьма удовлетворены получить от Вас ответ как можно быстрее.

Искренне Ваш

Секретарь и издатель

Ф.Томпкинс

Верно: [подпись]

АКАДЕМИЯ НАУК СОЮЗА СОВЕТСКИХ СОЦИАЛИСТИЧЕСКИХ РЕСПУБЛИК
ОТДЕЛЕНИЕ ФИЗИКО-МАТЕМАТИЧЕСКИХ НАУК

Москва
Б. Грузинская ул., д. 10
тел. Д 2-23-07
ком. ДО-35-00 доб.

59

„22" ноября 1954 г. № 101-11

И. А. Богородицкий
В. Л.

ЗАМ.ДИРЕКТОРА ФИЗИЧЕСКОГО ИНСТИТУТА
им. П.Н.ЛЕБЕДЕВА
д.ф.м.н. В.Л.ЛЕВШИНУ

364/б 22/XI 54

А.М. Прохорову
23/XI 54
Прошу поговорить с Вами по этому вопросу
24/XI-54

Отделение физико-математических наук направляет Вам копию приглашения секретаря английского Фарадеевского общества Томпкинса принять участие в дискуссии на тему: "Микроволновая и радиочастотная спектроскопия", организуемой обществом в Кэмбридже 4-6 апреля 1955 г.

В связи с изложенным Отделение физико-математических наук просит сообщить точку зрения Института по данному вопросу и в случае положительного решения выделить по одному ученому по каждой секции для включения в состав делегации, а также сообщить их темы докладов, с которыми они выступят на дискуссии.

Ответ необходимо прислать к I декабря 1954 г.

Приложение на 2 листах.

Зам.Академика-секретаря
член-корр. АН СССР (Л.М.Бреховский)

Ученый секретарь (Е.А.Коридалин)

АКАДЕМИЯ НАУК СССР
ФИЗИЧЕСКИЙ ИНСТИТУТ имени П. Н. ЛЕБЕДЕВА
[Мо]сква, 134, п/я № 1580.
Телефон В 2-05-50, доб. 2-78

62

„___"________ ноября 1954 г.

ОТДЕЛЕНИЕ ФИЗИКО-МАТЕМАТИЧЕСКИХ НАУК АН СССР

Заместителю академика секретаря
члену-корреспонденту Л.М.БРЕХОВСКИХ

Физический институт им.П.Н.Лебедева АН СССР считает возможным послать на конференцию по микроволновой радиочастотной спектроскопии следующих сотрудников ФИАН:

I. ВЛАДИМИРСКОГО К.В. - канд.ф.м.наук (секция II).
2. ПРОХОРОВА А.М. - доктора ф.м.наук (секция I).

Темы докладов: Прохоров А.М. "Теория молекулярного генератора".

ЗАМЕСТИТЕЛЬ ДИРЕКТОРА,
профессор /Н.ДОБРОТИН/

1112-6-3288

63

2 декабря 54

ЗАМЕСТИТЕЛЮ- АКАДЕМИКА-СЕКРЕТАРЯ ОФМН
члену-корреспонденту АН СССР
Л.М.БРЕХОВСКИХ

В ответ на Ваш запрос относительно командирования специалистов на совещание "Микроволновая радиочастотная спектроскопия" созываемое Фарадеевским обществом (Англия) сообщаю, что ФИАН считает целесообразным, чтобы Советские ученые приняли бы участие в этом совещании. Из сотрудников ФИАН"а в нем могли бы принять участие доктор физико-математических наук А.М.Прохоров (1-ая секция совещания) и кандидат физико-математических наук К.В.Владимирский (П-ая секция совещания).

Темы их докладов на совещании целесообразно уточнить после решения вопроса об их командировании.

Зам.Директора
Физического института АН СССР
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В ИНОСТРАННЫЙ ОТДЕЛ АКАДЕМИИ НАУК
С С С Р

Физический институт имени П.Н.Лебедева АН СССР направляет статью Н.Г.Басова и А.М.Прохорова "Теория молекулярного генератора и молекулярного усилителя мощности" для прочтения на заседании Фарадеевского общества в Кембридже / Англия /.

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В ФУНДАМЕНТАЛЬНУЮ БИБЛИОТЕКУ АКАДЕМИИ НАУК

СССР

Посылаю для пересылки в Америку в Колумбийский Университет проф.Таунсу (C. H. Townes) следующие статьи:

I. Н.Г.Басов и А.М.Прохоров

" Применение молекулярных пучков для радиоспектроскопического изучения вращательных спектров молекул" ЖЭТФ, 27, 431-438 (1954)

2. Н.Г.Басов и А.М.Прохоров

" О возможных методах получения активных молекул для молекулярного генератора" ЖЭТФ 28, 249-250 (1955)

3. А.М.Прохоров и А.И.Барчуков

" Метод измерения коэффициентов поглощения в микроволновой радиоспектроскопии" ЖЭТФ, 26, 761-763, (1954)

Указанные выше статьи не содержат сведений, препятствующих их посылке за границу.

И.о. Директора ФИАН
чл.-корр. АН СССР /Б.М.Вул /

Report of the First Conference on Quantum Electronics and Resonance Phenomena in the United States (1959)

"Report on the working trip to the USA for the 1ª International Conference on Quantum Radiophysics, organized by the physics department of Columbia University with support of the Office of Naval Research of the US Navy.

Period of the trip: September 12-19, 1959."

This 20-pages report describes the most relevant events around the trip, including their struggle with the Department of State to obtain visas, the structure and public of the conference, and how difficult it was to squeeze more detailed information from other scientists during the conference. It also describes a visit to the Physics Department of Columbia University and reflects upon the strategic importance of close collaboration with American Scientists.

Reference:

Prokhorov, A. M., N. G. Basov, and Barchukov. 1959. "Otchet O Komandirovke v SSHA Na 1-iu Mezhdunarodnuiu Konferentsiiu Po Kvantovoi Radiofizike. ARAN F. 471, Op. 5, Delo 34. pp. 1–23."

Не для печати.

А.М.ПРОХОРОВ, Н.Г.БАСОВ, А.И.БАРЧУКОВ, Л.С.КОРНИЕНКО

О Т Ч Е Т

О командировке в США на 1-ю международную конференцию по квантовой радифизике, организованной физическим департаментом Колумбийского Университета при поддержке Морской исследовательской лаборатории армии США.

Срок командировки 12-19 сентября 1959 г.

ОБЩИЕ СВЕДЕНИЯ О КОМАНДИРОВКЕ

В соответствии с директивным заданием мы должны были принять участие в конференции по квантовой радиофизике, а также ознакомиться с ведущимися в США работами по радиоспектроскопии путем посещения ряда университетов и лабораторий фирм. Предполагалось выехать в США 12-го сентября и пробыть там до конца месяца. В конце августа на имя главного организатора конференции проф. Таунса нами было послано письмо в котором выражалась просьба оказать содействие для посещения ряда научных учреждений США, в которых ведутся работы по квантовой радиофизике, известные нам из литературы.

Американское посольство в Москве в ответ на ноту МИД выдало нам визы с 1-го по 19 сентября 1959 г. Это обстоятельство сразу поставило под угрозу возможност посещения научных центров США, т.к. конференция заканчивалась 16-го вечером. По договоренности с ин.отделом АН СССР мы решили выехать 8-го сентября с тем, чтобы осуществить программу посещения до начала конференции, однако ввиду отсутствия билетов на рейсы самолетов в Нью-Йорк, мы смогли выехать только 11-го сентября.

По прибытии в Нью-Йорк проф. Таунс показал нам ответ Госдепартамента США на просьбу американских физик разрешить нам посещение их лабораторий. В этом ответе писалось, что посещение может состояться в том случае, если компетентные советские власти гарантируют посещение американскими физиками эквивалентных лабораторий в Советском Союзе.

В создавшейся ситуации мы обратились с письмом в Советское посольство в Вашингтоне в котором было сказано, что следует напомнить Госдепартаменту о том что в 1959 г. Советский Союз посетило около 400 американских ученых против 40 выезжавших в США, так что требования эквивалентности несостоятельны. Одновременно мы просили содействия в продлении виз до 25-го сентября.

Во втором ответе Госдепартамента, которое прилагается к отчету, нам было отказано как в продлении виз так и в разрешении посещения лабораторий, хотя мы и имели многочисленные приглашения американских ученых.

Хронологически наше пребывания в США проходило следующим образом.

11-го сентября вечером - прибытие в Нью-Йорк.

12-го сентября - посещение Колумбийского Университета, общее знакомство с Университетом.

13-го сентября - с утра осмотр Нью-Йорка, вечером отъезд в местечко Шаванга Лодж в 100 км от Нью-Йорка, где происходила конференция.

14-го - 16 сентября - конференция.

17-го сентября - посещение лаборатории излучений физического департамента - Колумбийский Университет.

18-го сентября - посещение лаборатории проф. Дейли в химическом департаменте Колумбийского Университета.

19-го сентября - посещение лаборатории фирмы IBM, в Нью-Йорке, ведущей работы по квантовой радиофизике.

ОБЩИЕ СВЕДЕНИЯ О КОНФЕРЕНЦИИ.

Конференция состоялась с 14-го по 16-е сентября с.г. в местечке Шаванга-Лодж в 100 км от Нью-Йорка.
Это первая международная конференция по квантовой радиофизике. Организована конференция по инициативе Колумбийского Университета, при поддержке Военно-Морской исследовательской лаборатории. На конференцию были приглашены все видные ученые из различных стран, работающие в области радиоспектроскопии. Всего на конференцию приехадо 185 ученых, согласно официального списка. Большинство ученых было из США, так как Америка занимает в настоящее время ведущее положение в области квантовой радиофизики.

Состав ученых, по типу исследовательских центров, которые они представляли следующий:

из Университетов - 72 чел.

из лабораторий фирм - 67 чел.

из исследовательских институтов - 32 чел.

из научных центров армии - 14 чел.

Иностранных ученых было около 20 чел. /из 8 стран/.

На конференции были рассмотрены следующие группы вопросов:

1. Пучковые молекулярные генераторы и усилители; молекулярные и атомные часы.

2. Молекулярные усилители и генераторы на твердом теле.

3. Рассмотрение всевозможных молекулярных усилителей и генераторов в оптической, инфракрасной и субмиллиметровой областях. Методы получения отрицательных температур.

4. Релаксационные процессы в твердых телах.

5. Рассмотрение общих физических проблем, которые могут быть решены в связи с развитием квантовой радиофизики /общая теория относительности, космогония, теория коммуникаций и.т.п./.

Заседания проходили пленарно и по секциям с 9 час. утра до 10 час. вечера с небольшими перерывами.
Всего было прочитано 57 докладов. Из-за перегрузки программы заседаний много докладов не было включено в программу, хотя они и представляли несомненный интерес.
Наша делегация представила 5 докладов, а именно:

"Парамагнитный усилитель с использованием Fe^{+++} в Al_2O_3". Л. Корниенко, А. Прохоров.

"Экспериментальное исследование дисковых резонаторов в миллиметровом диапазоне длин волн". А. Барчуков, А. Прохоров.

"Спин-решеточная релаксация в хромовом корунде". А. Маненков, А. Прохоров.

"Квантовомеханические полупроводниковые генераторы и усилители электромагнитных волн". Н. Басов, Б. Вул, Ю. Попов.

"Применение медленных молекул в молекулярных генераторах". Н. Басов, А. Ораевский.

Все доклады были заслушаны на заседаниях и вызвали оживленную дискуссию.

Следует особо подчеркнуть, что содержание всех представленных на конференции докладов касалось только физических проблем, т.е. рассматривались лишь общие идеи без их конкретного применения в приборах. Никакие конструкции не рассматривались.

Ввиду этого особое значение приобрели для нас частные беседы с участниками конференции в перерывах между заседаниями. Этих бесед было очень много, практически со всеми видными физиками в этой области. Наибольшее количество различных сведений, особенно конкретные цифры, которые будут перечисляться в отчете, получены именно из этих бесед.

Ниже мы приводим изложение состояния различных разделов квантовой радиофизики за рубежом в настоящее время в той степени в которой позволила это сделать конференция, частные беседы и посещение одного из главных научных центров этой области физики - Колумбийского Университета.

1. Пучковые молекулярные генераторы и усилители. Атомные часы и стандарты частоты.

До сих пор для целей построения стандартов частоты и времени используются спектральные линии аммиака и цезия. На конференции на эту тему было сделано два обзорных доклада и 13 докладов, посвященных специальным вопросам.

Можно считать, что пучковым молекулярным генераторам и их использованию для стандартов частоты /времени/ уделяется самое серьезное внимание, так как они обеспечивают максимально достижимую в настоящее время точность. Участники конференции пришли к общему согласию, что пучковые генераторы на аммиаке и цезии имеют сравнимую точность порядка 10^{-10}. В аммиачном генераторе предпочтительнее использовать изотоп азота N^{15}, так как в этом случае отсутствует сверхтонкая структура /спин ядра N^{15} равен 1/2 /. Швейцарский физик Бонономи для этой цели использует обогащение аммиака до 80%. Ведутся работы по созданию аммиачных генераторов с использованием геттеров для поддержания вакуума /продолжительность работы до одного месяца/. Генераторы с геттерами весьма удобны для прикладных целей, особенно в передвижных установках. Работы по использованию геттеров в молекулярных генераторах ведутся и в Советском Союзе. Использование геттеров значительно сокращает вес установки. Она делается более легкой чем генератор с использованием цезия.

Американский физик Lyons и Национальное Бюро Стандартов / США / / P. Bender / ведут переговоры о правительственном контракте на создание молекулярного генератора для спутника с целью проверки общей теории относительности.

P. Bender заявил, что его группа работает над использованием относительной стабильности молекулярного генератора для абсолютных измерений.

Наши работы по использованию медленных молекул вызвали живой интерес. Было признано, что это перспективный метод повышения точности молекулярных генераторов. Ряд ученых работает над использованием молекулярных генераторов для контроля государственных эталонов времени /станций, передающих радиочастоты с высокой точностью/ Они выразили удивление по поводу сравнительно невысокой точности наших станций, излучающих стандартные частоты.

Иностранные ученые, с которыми нам приходилось беседовать, избегали ответов на вопросы об использовании пучковых генераторов для прикладных целей.

Ведутся работы по созданию пучковых генераторов на других молекулах для миллиметровой и субмиллиметровой областей.

Из новых молекул на которых уже работают генераторы, были приведены молекулы: HDO, HDS и CH_2O .

Следует заметить, что не было сделано каких-либо предложений по радикальному улучшению стабильности молекулярных генераторов, скажем до $10^{-13} \div 10^{-12}$.

П. Молекулярные усилители и генераторы на твердом теле.

На конференции разбирались главным образом вопросы, касающихся релаксационных процессов в материалах, в частности, используемых в генераторах и усилителях.

Чрезвычайно важным процессом для понимания работы парамагнитных усилителей является так называемая перекрестная релаксация. Учет времени перекресной релаксации может резко улучшить или сделать невозможной работу усилителя. Это явление было обнаружено в лаборатории Белл и объяснено Бломбергеном. Советская делегация узнала об этом на конференции, так как до нее никаких публикаций на эту тему не было.

Нами было выяснено, что в настоящее время в Америке работают усилители с использованием рубина на 3 см, 5 см, 10 см и 21 см. Произведение усиления на полосу по словам некоторых ученых составляет от 200 до 1000 МГГЦ. Эти данные некоторые американские ученые ставят под сомнение. Используются резонаторы волноводного типа, небольшого сечения с рубином прямоугольной формы, что вполне возможно так как ε рубина = 10.

Усилители 10 см. диапазона используют рубин концентрации 0,1% при ориентации оси кристалла перпендикулярно к постоянному полю. Для расширения полосы используется неоднородное магнитное поле.

Начинают применять постоянные магниты. Усилители при этом получаются малогабаритными.

В настоящее время успешно ведутся работы по созданию парамагнитных усилителей типа бегущей волны /на 3, 10 см и 21 см/. Прикладное значение парамагнитных усилителей обсуждалось только в разрезе их применения к радиоастрономии.

Парамгнитный усилитель на радиотелескопе имеет Белл-лаборатория. Общая температура шумов антены, тракта и усилителя порядка 70°К. Ведутся работы по уменьшению этих шумов до 10°К.

Из новых материалов, пригодных для парамагнитных усилителей назывались окись титана /TiO_2 / с присадками Cr^{+++} и Fe^{+++}. Fe^{+++} в TiO_2 имеет большое начальное расширение /$D \approx$ 20.000 МГГЦ/. Это обстоятельство делает возможным создание парамагнитного усилителя для мм диапазона длин волн.

Нам были сообщены следующие данные об этом материале: при концентрации железа 0,04% /вес Fe по отношению к TiO_2 / ширина линии составляла 25÷ 84 МГГЦ, T_1 около 5.10^{-3} сек при $T = 1,5°$К. Это время релаксации значительно короче, чем время релаксации Fe^{3+}, Cr^{3+} в Al_2O_3, что имеет

важное значение для прикладных целей.
Американцам удалось получить концентрацию 0,5%, что представляет большой интерес.

Рубин, используемый в усилителях, американцы выращивают в основном методом Вернеля, а также гидротермальным методом.

Имеется мнение, что парамагнитные усилители можно успешно использовать на частотах свыше 1.000 МГГЦ. Вообще же общее мнение таково, что пока нет основания при сравнении парамагнитных усилителей с параметрическими отдавать предпочтение одному из них.

Было сделано сообщение об усилителях и генераторах с использованием 2-х уровней. Удалось получить генерацию на 70.000 МГГЦ.

Ш. Рассмотрение молекулярных усилителей и генераторов в оптической,инфракрасной и субмиллиметровой областях.

Этому вопросу было посвящено большое число докладов. Рассматривались различные идеи. Интерес к оптическим и инфракрасным молекулярным генераторам обусловлен прикладными и научными потребностями в источниках излучения с большой спектральной плотностью в этой области, а также в высокочувствительных приемниках.

Рассматривался молекулярный генератор с использованием спектра паров натрия, калия, рубидия. Подсветка оптическая.

В одном из докладов предлагалось использовать электронную бомбардировку для возбуждения атомов с целью получения отрицательных температур, применительно к первой группе элементов периодической системы.

Были также рассмотрены возможности генерации с использованием полупроводников. В качестве методов предлагались циклотронный резонанс, использование отрицательной массы, переходы между различными уровнями в полупроводниках. В одном из докладов предлагалось использовать для этих целей фосфоры, однако конкретных схем предложено не было.

В ряде докладов были доложены результаты по исследованию свойств твердых тел на сверхвысоких частотах при их возбуждении ультразвуком.

В перечисленных докладах рассматривались только принципиальные возможности использования различных эффектов для получения отрицательных температур в оптической и инфракрасной областях и не было рассмотрено ни одной какой-либо схемы, какие-либо экспериментальные результаты. Однако, из частных бесед, нам было совершенно ясно, что во многих лабораториях ведутся энергичные исследования в практической плоскости.

Следует заметить, насколько нам это известно, что кроме нескольких работ, ведущихся в ФИАН"е в Советском Союзе подобных работ не ведется.

На развитие работ по оптическим, инфракрасным и субмиллиметровым молекулярным генераторам и приемникам в Советском Союзе следует обратить самое серьезное внимание, так как они практически помогут решить проблему оптической и инфракрасной радиолокации, не говоря уже о большой научной ценности этих работ.

1У. Релаксационные процессы в твердых телах.

Этим процессам на конференции было уделено самое серьезное внимание.

Наиболее интересные работы ведутся в Гарвардском университете.
Наибольший интерес вызвали работы по перекрестной релаксации, возникающей в результате взаимодействия системы спинов, имеющих примерно одинаковый энергетический интервал. Это в явление в ряде случаев может сильно влиять на работу парамагнитных усилителей. Явление перекресной релаксации может создать в системе спинов отрицательные температуры на более высоких частотах, чем частота вспомогательного излучения. Так как публикации по этому вопросу практически отсутствуют, конференция оказалась для нас весьма полезной и позволила более глубоко понять это важное явление в парамагнитных материалах.

Ряд докладов был посвящен рассмотрению обычных релаксационных процессов. Это рассмотрение поможет улучшить характеристики парамагнитных усилителей.

У. Рассмотрение общих физических проблем, которые могут быть решены в связи с развитием квантовой радиофизики.

Был рассмотрен ряд методов для проверки общей теории относительности. Все они требуют высокостабильных молекулярных генераторов и высокочувствительных приемников для установления связи с удаленными объектами. В ряде докладов рассматривались варианты использования таких систем.

В одном из докладов рассматривалась задача о передаче сигнала без искажения на большие расстояния, используя молекулярные генераторы и усилители.

Были рассмотрены различные процессы которые могут привести к изменению скорости течения времени.

Общие выводы и предложения, вытекающие из итогов конференции.

1. Конференция показала, что уровень работ по квантовой радиофизике, выполняемых в Советском Союзе не отличается от работ в этой области в других странах.

2. По многим разделам квантовой радиофизики в СССР работ не ведется, а именно:
по применению оптической подсветки для получения

отрицательных температур в различных веществах, по получению отрицательных температур в различных веществах, по получению отрицательных температур в инфракрасной области, по использованию для этих целей двухуровневых систем. Чрезвычайно мало ведется работ по релаксационным процессам при низких температурах.

До сих пор в Советском Союзе нет стандарта частоты с использованием пучка атомов цезия.

Не внедрены практически в практику молекулярные и атомные стандарты частоты для службы времени.

Медленно внедряются молекулярные генераторы и усилители в практику.

Лаборатория излучений Колумбийского Университета.

Общие сведения.

В лаборатории излучений Колумбийского Университета, руководимая проф. Таунсом было положено начало /одновременно в ФИАН"е АН СССР/ квантовой радиофизике. Эта лаборатория и в настоящее время является одним из крупнейших научных центров в этой области. Насколько нам известно наша делегация впервые из советских ученых приезжавшим в Америку получила возможность провести подробное ознакомление с ведущимися в ней работами а также организационной структурой.

Лаборатория излучений проф. Таунса входит в состав Физического департамента Колумбийского Университета. Ее годовой бюджет, включая оплату сотрудников составляет в среднем 300.000 долларов. Общее количество научных сотрудников составляет 50-60 человек. Около 30 человек вспомогательного состава. Лаборатория занимает 15-20 больших комнат. Характерно что лаборатория не имеет в своем составе специально физиков-теоретиков.

Характеристика работ, ведущихся в лаборатории.

В лаборатории в настоящее время ведутся следующие работы.

1. Работы по пучковым молекулярным генераторам. Имеется несколько установок. В установке с двумя пучками на линии $J=3$, $K=2$ исследуется зависимость частоты от различных параметров. Ведется сравнение частоты этого генератора с однопучковым. Давление в камере поддерживается автоматически.

   Квадрупольные конденсаторы изготовляются из 8 параллельно расположенных по кругу, диаметром 10 мм, вольфрамовых проволочек /d = 2-3 мм/. Эти проволочки-конденсаторы окружены цилиндром, который охлаждается жидким азотом.

Система получается компактной и экономичной.

В лаборатории фирмы IBM, в которой ведутся работы по генераторам, расположенна недалеко от Колумбийского университета, была поставлена работа по проверке теории относительности - опыт Майкельсона на радиочастотах. Характерно, что детали молекулярных генераторов, используемых в этой установке изготовлены не кустарным образом, а имеют вполне "индустриальный" вид.

2. Ведутся работы по использованию других молекул /кроме аммиака/ для молекулярных генераторов. Нам был показан генератор работающий на молекулах HDO, HDS . Направление этой работы преследует двуякую цель. С одной стороны исследуется квадрупольная связь дейтерия в этих соединениях, а также магнитное взаимодействие ядер с магнитным полем молекулы. С другой стороны эти вещества позволяют получить различные стандартные частоты.

3. В лаборатории имеются парамагнитные усилители на 3 см, 10 см и 21 см. Усилитель на 21 см предназначен для наблюдения галактического водорода. Используется циркулятор у которого затухание в прямом направлении составляет 0,3 дб. Работа ведется по совершенствованию усилителей, т.е. получение более широкой полосы, максимальной

перестройке и т.п. Резонаторы, используемые в усилителях волноводного типа, полностью заполненные веществом.
Работа на радиотелескопе с 3 см. усилителем в настоящее время прекращена ввиду того, что сотрудники, которые им занимались ушли в промышленность. Усилители типа бегущей волны, не строятся, так как это связано с большой инженерной работой.
Любопытно отметить, что подводящие фидера к резонатору делаются из тонкостенных /0,25 мм/ латунных трубок, достаточно длинных. Это дает меньшее затухание, чем стальны трубок при умеренном испарении гелия.

4. Ведутся работы по исследованию релаксационных явлений в парамагнитных кристаллах по поведению линий сверхтонкой структуры, когда одна из них насыщается.
Собираются ставить работы по исследованию релаксации в 2-х уровневых системах, когда нет сверхтонкого расщепления.

5. Делается установка для радиоастрономии на 63.000 МГГЦ с использованием усилителя на твердом теле для наблюдения эмиссионных линий от Солнца. В качестве антены используется зеркало обычного прожектора. Установка смонтирована на крыше одного из зданий университета. Детали установки нам не были продемонтрированы.

6. В лаборатории имеется установка, находящаяся в стадии отладки, для генерации миллиметровых волн, используя эффект Черенкова. Раскачка производится на волне в 3 см. В качестве диэлектрика используется титанат бария. Излучения на других волнах /кроме основной в 3 см/ пока не наблюдалось. Для получения излучения на волне в 1 мм. предполагается использовать для раскачки более мощный 3 см генератор.

7. Ведется экспериментальная работа по созданию молекулярного генератора на оптические частоты с использованием паров калия. Нам был продемонстрирован резонатор, сделанный по типу эталлона Фабри-Перо. По внешнему виду это стеклянная трубочка длиной примерно 100 мм с зеркалом Ø =20-25 мм. В качестве детектора предполагается использовать вакуумные термопары.

8. Большое число работ лаборатории посвящено исследованию молекулярных пучков с целью исследования различных тонких ядерных взаимодействий. Работы ведутся под руководством проф. Куша, который этим вопросом занимается довольно давно. Как известно, им был открыт аномальный момент электрона. Работы проф. Куша прямого отношения к квантовой радиофизике не имеют.

9. Газовой радиоспектроскопией лаборатория кончила заниматься примерно 5 лет тому назад. Однако ведутся работы по изучению микроволновых спектров свободных радикалов в газовой фазе. Нам была показана установка, которая представляет из себя обычный радиоспектроскоп. Радикалы непосредственно получаются здесь же и вводятся в волновод с помощью стеклянных трубок.

Метод ввода радикалов в волновод находится в стадии разработки.

10. В лаборатории имеется помещение в котором установлен кварцевый стандарт частоты. Из этого помещения при помощи шлангов в.ч. передаются частоты 1, 10 и 100 мггц с точностью 10^{-8}. Надо заметить, что такая система очень удобна.

Мы не можем утверждать, что нам были показаны все работы ведущиеся в лаборатории излучений Колумбийского Университета.

Лаборатория проф. Дейли в химическом департаменте Колумбийского Университета.

Лаборатория широко использует методы радиоспектроскопии для чисто химических целей.

Имеются следующие работающие установки:

1. Установка по наблюдению спектра парамагнитных веществ.
2. Установка по исследованию ядерного резонанса.
3. Газовый радиоспектроскоп.

1. Исследуются в основном свободные радикалы, /в основном из бензола/ которые получаются непосредственно в об"емном резонаторе. Установок имеется несколько. Конкретных результатов пока нет.

2. Установка для исследования ядерного резонанса а также парамагнитного изготовлена фирмой Varian /стоимость 40 т. долларов/. Магнит обладает однородностью поля порядка 10^{-7} в 0,5 $см^2$/. Использование вращающегося образца позволяет достигнуть разрешающей силы до 10^{-8}. Рабочая частота установки 30 и 60 МГГЦ. При переходе с одной рабочей частоты на другую производится юстировка магнита. Магнит охлаждается водой, которая термостатирована.

3. Был показан радиоспектроскоп 1,5 см диапазона. Характерно, что используемые клистроны в диапазоне 1-3 см имеют мощность порядка 200 милливатт.

ЗАКЛЮЧИТЕЛЬНЫЕ ЗАМЕЧАНИЯ

1. Следует признать, что посылка советской делегации на конференцию по квантовой радиофизике была весьма полезной. Она была бы более полезна, если удалось продлить визы и получить разрешение госдепартамента на посещение ряда лабораторий фирм, ведущих работы в этой области.

Посещение нами Колумбийского Университета - одного из центров квантовой радиофизики дало довольно ясное представление об уровне работ, но картина оказалась не полной, так как с прикладными задачами мы ознакомлены практически не были.

2. Мы считаем, что весьма целесообразно в 1960 году пригласить одного, двух видных американских физиков, например проф. Стрэндберга и проф. Таунса.

Это приглашение предоставило бы еще одну возможность подробнее войти в курс работ ведущихся в в Америке в этой области.

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Contents