The Schooling of Lev Landau:
The European Context of Postrevolutionary Soviet Theoretical Physics

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ABSTRACT

Theoretical physicist Lev Landau is internationally renowned for his school, whose social and intellectual coherence rests heavily on his personal agency: his brilliance, the scope of his research, his pedagogical program (textbooks and Theoretical Minimum). This essay considers the broader historical factors that made the Landau school possible and sustainable. The collective values associated with it were already in circulation in revolutionary Russia. Landau's own training took place in an atmosphere of unstable social relations that rewarded behaviors that were neither previously nor subsequently tolerated. Yet the Landau school was not sui generis Soviet. It was also the product of Russia's long-standing ties with German physics and of Landau's own intellectual development in interaction with crucial figures such as Niels Bohr. Although Landau famously claimed Bohr as his only teacher, he adopted the Bohr "style" in only a very restricted sense and actively discouraged his students from embracing Bohr's epistemological concerns. His school was a "purely practical" version of a long intelligentsia tradition.

INTRODUCTION

> Let me remind the reader that although, at the beginning of the twentieth century, the large experimental school of Lebedev in Moscow and the small groups of Joffe and Rozhdestvenskii in Petersburg began to form, theoretical physics as a branch of science did not, in fact, exist anywhere in Russia. It is true that, just before the revolution, theoretical research conducted by individual scientists of high caliber started appearing more frequently. However, they did not have their own schools.
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> —I. E. Tamm, "Theoretical Physics" (1967)[1]

With its move from Leningrad to Moscow in August 1934, the Soviet Academy of Sciences found that it had to become ever more practiced at painting itself as a pro-

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[1] From an essay for *Oktiabr' i nauchnyi progress*, in B. M. Bolotovskii and V. Ya. Frenkel, eds., *I. E. Tamm: Selected Papers* (Berlin, 1991), 284–90, on 285.

ductive smithy for the tools of socialist construction, while retaining a role for "the basic fundamental questions of modern physics."[2] This task was not eased the following year by the revised academy statute that elevated the Technology Group to division status, ensuring a growing role for engineers—many with party ties—in the upper tiers of the academy.[3] Having approved the new academy charter, the Council of People's Commissars (Sovnarkom) further proposed to hold a special session of the academy devoted to critical reports from its leading figures in all the disciplines: the physiologist I. P. Pavlov (1849–1936), the geologist A. E. Fersman (1889–1972), the physicist A. F. Joffe (1880–1960), and others.[4] It became apparent rather quickly that this exercise in public accountability threatened to expand into a bureaucratic babel of superficial analyses. This situation and the death of Pavlov in February 1936 helped resolve the government to focus on one discipline: physics. Even then the many competing research agendas threatened to make the session superficial and disjointed, and Joffe argued that he alone should shoulder the responsibility for conveying a "complete picture of a unified grand enterprise."[5] The government partly relented, narrowing the exercise to three institutions: the Leningrad Physico-Technical Institute (LFTI), the State Optical Institute (GOI) in Leningrad, and the Lebedev Physics Institute (FIAN) in Moscow. The choice was peculiar as only FIAN was an academy institute at the time and the focus of the session would be on LFTI and GOI. In Soviet practice, however, it made perfect sense, for scientific discipline and social leadership were strongly linked, and each institute was tied to a senior figure in the academy: Joffe (LFTI), D. S. Rozhdestvenskii (GOI), and L. I. Mandelstam (FIAN).[6] The academy vice president emphasized the connection when he opened the session by announcing that the assessments of Soviet physics that the assembled worthies were about to hear were directed at "the most important schools in physics."[7]

Standing in the wings were three younger corresponding members of the academy, each invited to give supplemental reports on recent developments in theoretical physics. As none of the three theorists would attend to applied themes nor would any of their talks generate much debate, the motives for having them share the stage at the March session were mixed.Yet there had to be some acknowledgment of FIAN and of Mandelstam (1879–1944), who was respected equally for his experimental and theoretical work, while his nervous constitution left him in abhorrence of these Soviet rituals. The head of FIAN's small Theory Department, Igor Tamm (1895–1971) spoke publicly for his older colleague and friend, a role he had played for more than a

[2] See the academy's report to the Seventh Congress of Soviets, *VII Vsesoiuznomu s"ezdu sovetov—Akademiia nauk SSSR* (Moscow, 1935), 33–6. On the move, see V. D. Esakov, "Puteshestvie iz Peterburga v Moskvu: Pereezd Akademii nauk," *Priroda*, 1997, no. 9:131–8; "Shtab sovetskoi nauki meniaet adres," *Vestnik rossiiskoi akademii nauk* 67 (1997): 840–8.

[3] See *Ustavy Akademii nauk SSSR* (Moscow, 1974), 142–50.

[4] Vladimir Vizgin has written the authoritative study of the "March session" in his "Martovskaia (1936 g.) sessiia AN SSSR: Sovetskaia fizika v fokuse," *Voprosy istorii estestvoznaniia i tekhniki*, 1990, no. 1:63–84, and 1991, no. 3:36–55. Further accounts may be found in V. Ia. Frenkel', "K 50-letiiu martovskoi sessii Akademii nauk SSSR (1936 g.)," *Chteniia pamiati A. F. Joffe 1985* (1987): 63–86; Paul Josephson, *Physics and Politics in Revolutionary Russia* (Berkeley, Calif., 1991), 295–305; David Holloway, *Stalin and the Bomb* (New Haven, Conn., 1994), 16–24.

[5] Joffe quoted in Vizgin, "Martovskaia sessiia" (cit. n. 4), 38.

[6] One observer pointedly remarked that it was not the work of the *academy* that was discussed, as much as the work of the *academicians*. S. Krum, "Fizika na sessii Akademii nauk," *SORENA,* 1936, no. 5:105–16, on 106. Three years later, LFTI was transferred to the academy's jurisdiction.

[7] *Izvestiia Akademii nauk SSSR*, seriia fizicheskaia, 1936, nos. 1–2:5–409, on 5.

decade at the university.[8] That Tamm himself was in the process of building a school had been one of the reasons cited for his election to the academy in 1933.[9] Tamm's immediate superior, FIAN director and optics specialist S. I. Vavilov, was reporting on GOI (his second post), so Tamm's report on the state of nuclear theory would have to stand in for a more comprehensive assessment from Mandelstam. V. A. Fock (GOI) would also address the many-body problem in quantum mechanics, while Ia. I. Frenkel (1894–1952) (LFTI) would share his latest work on the solid-liquid phase transition.[10] Yet despite Joffe's (and Vavilov's) careful plans, Soviet physicists did not present a united front to party and state dignitaries nor to their fellow scientists. When *Pravda* applauded "the fresh gust of self-criticism that has burst its way into the walls of the academy," it was reporting more than the enactment of an ideological rite whose outcome was predetermined.[11] Joffe's younger colleagues, it turned out, took the exercise entirely in earnest—especially twenty-eight-year-old theorist Lev Landau (1908–68), who had begun his career at LFTI.[12]

The theorists made trouble because they felt their institutional status remained insecure. Despite the calculated deference shown to them by Vavilov, they had good reason to be concerned. Tamm's group would be formally dispersed to the various experimental laboratories in April 1938, albeit with continued permission to work on "general topics."[13] This was partly because Tamm's engineer brother had been arrested during the purges and partly because Tamm could not come up with a tenable theory of nuclear forces.[14] But a complaint about insufficient attention to school building also played a part. Only as Tamm began attending more closely to the demands of cosmic ray physicists, and as international praise began to accrue for his interpretation of Cherenkov radiation, did the decision begin to look premature.[15] For his part, Landau was already building a school of his own in Kharkov, eventually succeeding brilliantly—to this day, the designation "Landau school" requires no explanation among theoretical physicists.[16] But founding schools in *theoretical* physics relied upon a rather different understanding of the past of Russian physics and of the

[8] In a December 14, 1922, letter to his wife, Tamm noted that Mandelstam got a migraine if so much as forced to share a dinner table with even a polite Communist. L. I. Vernskii, "I. E. Tamm v dnevnikakh i pis'makh k Natalii Vasil'evne," in *Kapitsa, Tamm, Semenov v ocherkakh i pis'makh* (Moscow, 1998), 273–4.

[9] "Vybory chlenov-korrespondentov i pochetnykh chlenov," f. 2, op. 2 (1933), d. 9, l. 178, Russian Academy of Sciences (Arkhiv Rossiiskoi Akademii Nauk, hereafter cited as ARAN), Moscow.

[10] For Tamm's talk, see *Izvestiia AN SSSR*, seriia fizicheskaia, 1936, nos. 1–2:300–23; for Fock's, 351–62; for Frenkel's, 371–93.

[11] Quoted in Vizgin, "Martovskaia sessiia" (cit. n. 4), 71. For a close analysis of the rituals of criticism and self-criticism, see Alexei B. Kojevnikov, *Stalin's Great Science: The Times and Adventures of Soviet Physicists* (London, 2004), 186–216; Oleg Kharkhordin, *The Collective and the Individual in Russia* (Berkeley, Calif., 1999), 123–63.

[12] On the attitudes of the younger participants, see S. E. Frish, *Skvoz' prizmu vremeni* (Moscow, 1992), 217–8.

[13] I. E. Tamm, "Otchet laboratorii teoreticheskoi fiziki," 7 May 1938, f. 532 op. 1 d. 31 ll. 89–93 (including discussion), ARAN.

[14] The doomed Leonid Tamm's testimony is found in *Report of Court Proceedings in the Case of the Anti-Soviet Trotskyite Centre*, 23–30 Jan. 1937 (Moscow, 1937), 439–42.

[15] I. M. Frank and Ig. Tamm, "Coherent Visible Radiation of Fast Electrons Passing through Matter," *Doklady AN SSSR* 14, no. 3 (1937): 109–14; Ig. Tamm and S. Belenkii, "On the Soft Component of Cosmic Rays at Sea Level," *Journal of Physics USSR* 1, no. 3 (1939): 177–98. Tamm, Frank, and Cherenkov were awarded the Nobel Prize in 1958.

[16] E.g., Frank Wilczek, "Nobel Lecture: Asymptotic Freedom: From Paradox to Paradigm," *Reviews of Modern Physics* 77 (2005): 859. Cf. John Ziman, "Landau and His School," in *Of One Mind: The Collectivization of Science* (Woodbury, N.Y., 1995), 17–22.

relation of the Soviet physicist to his European counterparts. The March session was merely a marker of discontents and ambitions that stemmed from the early days of the Russian Revolution, whose generational effect had been especially strong among theorists born after 1890. It is also a point of entry for understanding how the unstable professional roles of Soviet theorists contributed to the renewal of intelligentsia sensibilities that, in part for reasons of disciplinary self-conception and probably in part from sheer historical ignorance, the theorists had largely suppressed during the formative years of their training.

In an era when abstract "theory" could be an ambiguous and occasionally dangerous social marker of one's area of expertise, theoretical physicists uniformly made theory credibly *Soviet* by endorsing a genetic fallacy among their colleagues: it had had no real place in tsarist Russia, it was "practically the same age as the revolution," and it had only gotten started "in the new conditions of scientific work created by the Soviet government."[17] Prepared by 1938 to treat Frenkel, Tamm, and Landau as nascent heads of Soviet schools, Vavilov deliberately cast theory as the labor of Soviet youth, for "it received almost no heritage, no tradition from the prerevolutionary past," when there had been "no theoretical school."[18] Since Soviet theory had blossomed in the 1920s and 1930s, the so-called golden age of modern physics, its vigorous international engagement with other practitioners of the new quantum mechanics made domestic institutionalization possible. The alacrity with which the first Soviet theorists had gained international regard during their travels abroad became central to their collective identity. The European moment was indeed constitutive because "this participation enabled the development of theoretical physics in the Soviet Union, [and] the founding of the prominent schools of L. D. Landau, I. E. Tamm, V. A. Fock, [and] Ia. I. Frenkel."[19]

Schools, in other words, were the form of life that let Soviet theorists to build and sustain informal and exclusive modes of social interaction amid recurrent cycles of egalitarian proletarianization and party intervention in the Soviet sciences during the Stalin era—processes in which the collective homogeneity of the physics community vis-à-vis interlopers could not be taken for granted.[20] In the smaller confines of Landau's elite seminar, wrote a postwar participant, "everyone felt as if they belonged to one faith," and that sense of social and intellectual unity was immensely valuable to its members.[21] At the same time, schools also came to provide a respectable outlet for the official reward mechanisms for individual distinction that won out

[17] E. V. Shpol'skii, "Fizika v SSSR 1917–1937," *Uspekhi fizicheskikh nauk* (hereafter cited as *UFN*) 18 (1937): 295–322, on 322; Shpol'skii, "Piat'desiat let sovetskoi fiziki," *UFN* 93 (1967): 197–276, on 213. (Shpol'skii was a classmate of Vavilov's in Moscow, and longtime editor of *UFN*.) Cf. S. Khaikin, "Dostizheniia v oblasti fiziki za 15 let," *Pod znamenem marksizma*, 1932, nos. 9–10:221–9; D. I. Blochinzew, "The Advance of Theoretical Physics in the Soviet Union in Twenty Years," *Physikalische Zeitschrift der Sowjetunion* 12 (1937): 542–3; Blokhintsev [Blochinzew], "Puti razvitiia teoreticheskoi fiziki v SSSR," *UFN* 33 (1947): 285–93, on 286; P. S. Kudriavtsev, "Iz istorii stanovleniia sovetskoi teoreticheskoi fiziki," in *Razvitie fiziki v Rossii*, vol. 2 (Moscow, 1970), 6–21, on 7.

[18] S. I. Vavilov, "Sovetskaia fizika—nauka molodezhi," *Priroda*, 1938, no. 9:37–8; Vavilov, "Fizika v Rossii i v SSSR," *Priroda*, 1932, nos. 11–12:989–1012, on 995.

[19] M. A. El'iashevich, "Ot vozniknoveniia kvantovykh predstavlenii do stanovleniia kvantovoi mekhaniki," *UFN* 122 (1977): 673–717, on 688–9.

[20] The agonistic milieu of early Soviet theory is detailed in Karl Hall, "Purely Practical Revolutionaries: A History of Stalinist Theoretical Physics, 1917–1941" (PhD diss., Harvard Univ., 1999).

[21] M. I. Kagan, "Shkola Landau: Chto ia o nei dumaiu. . ." *Priroda,* 1995, no. 3:76–90, on 85. Cf. B. L. Ioffe, "Landau's Theoretical Minimum, Landau's Seminar, ITEP in the Beginning of the 1950's" (preprint arXiv:hep-ph/0204295v1, April 2002), http://www.arXiv.org.

over the long term. It was an arena of professional individualization (*otlichie*) safely surrounded by strong norms of collective endeavor and thus well suited to mature Soviet society.[22]

Schools offered a legitimate institutional identity to this most cosmopolitan of sciences even when Soviet political autarky meant that the achievements of physicists were constantly being evaluated in patriotic terms. In the case of Landau, scorn for such provincialisms by no means meant that his research school lacked a local distinctiveness. But to understand why the disciplinary practices fostered by the Landau school were in a certain sense "Soviet," we must appeal to more than the reactive dynamic of state-sponsored rituals such as the March session. Recovering a more immediate, anthropological account of Soviet cultures of theory will thus be central to this essay.[23] To do this, we must also follow Landau to Zurich, Cambridge, and Copenhagen, the locales where he consolidated a truly Soviet identity and developed proactive agendas for *teoretika* (his neologism for theoretical physics played down the resonances with "theory" in the broader sense) back home. If Landau held out Niels Bohr as his model leader of a school, he did so not to emulate Bohr's theoretical practices in toto but to marginalize Soviet competitors to his own school. And in the mouths of Landau and his peers, Bohr's language of crisis and renunciation at the heart of quantum theory took on distinctive resonances in the context of massive Soviet industrialization circa 1930. In the failed revolution of quantum electrodynamics that followed, Soviet theorists encountered conceptual dilemmas that only served to exacerbate their dissatisfactions with local institutions. This made schools all the more important in developing "purely practical" procedures for Soviet theory, in which "practical" did not mean "applied" but rather "not overly concerned with the physics of principles," with all its attendant epistemological problems.[24] It would be matter of fact and pragmatic in technique without reducing to mere phenomenology, even if its abstruse subject matter looked at times to Soviet laypeople like the "abstract cult" of science once thought to have been characteristic of the prerevolutionary Russian scientist-*intelligent*.[25] The historical analogy was actually quite weak, yet it was their early Soviet conflation of revolutions—political and quantum—that had led the Soviets to treat the institutions of theory as a novel Soviet phenomenon. The collapse of the revolutionary analogy brought them back into history, and in finding ways to survive and thrive in the Stalinist order, they also became sometimes reluctant heirs of intelligentsia traditions that demanded an independent spirit "in matters large and small, in life and in science."[26] If rigorous disciplinary practices were always at the heart of the school, they were reinforced and rendered socially stable

[22] Kharkhordin, *Collective* (cit. n. 11), 337–40.

[23] Peter Galison and Andrew Warwick, eds., "Cultures of Theory," *Studies in History and Philosophy of Modern Physics* 29B (1998): 287–434.

[24] Landau once characterized his seminar method brusquely as "chisto delovoi" in response to a tendentious query. See "Protokol No. 9 zasedaniia Uchenogo Soveta Instituta fizicheskikh problem," 9 June 1951, f. 1, op. 1, d. 106, l. 20, Arkhiv Instituta Fizicheskikh Problem, Moscow.

[25] Boris Sokolov, *Nauka v sovetskoi rossii* (Berlin, 1921), 6. On mathematical abstraction as a purported political danger, see E. Kol'man, "Vreditel'stvo v nauke," *Bol'shevik,* 31 Jan. 1931, no. 2:75–6.

[26] See E. L. Feinberg's remarks on Tamm's relation to the prerevolutionary intelligentsia in "Epokha i lichnost'," in *Vospominaniia o I. E. Tamme*, 2nd ed., ed. E. L. Feinberg (Moscow, 1986), 225; echoed in Andrei Sakharov, *Memoirs* (New York, 1990), 128.

by a larger system of group values that Landau liked to call the "theoretical physics approach to life."[27]

BACK TO SCHOOL: POLEMICS AND PRECURSORS

March 1936 was less than two decades after the founding of LFTI, but in the foreshortened horizon of socialist construction, this was already more than a generation since Joffe himself had become the first post-October academician among the physicists. His report ranged from atomic physics to industrial laboratories and proudly noted the founding of daughter institutes in Kharkov, Tomsk, and Dnepropetrovsk. While the topical scope of the report does not concern us, the scope of Joffe's claims about LFTI's historical trajectory does, for although he did hearken back rather dismissively to the fin de siècle in Russian physics, he had good reason to paint LFTI as the engine that had propelled Soviet physics onto the world stage. However, Joffe's readiness to cast Soviet physics as nearly on par with the top two or three international counterparts invited Landau's derision, and the theorist took the floor at his most combative. (Only a very modest fraction of the hundreds of people in attendance could claim experience abroad comparable to Landau's.) Landau sweetly credited to Joffe much of the very existence of Soviet physics, for "in tsarist Russia up to the moment of the revolution physics practically didn't exist. Those little fragments of the Lebedev school that existed in Moscow could not pretend at the time to the title of real physics." So Joffe's subsequent contribution had been great indeed, but his rosy assessments were unsustainable, said Landau, declaring the actual situation "almost catastrophic" in comparison with the tasks at hand. He counted no more than a hundred independent researchers in Soviet physics, not all of whom evidenced much concern for training the next generation. Landau then launched into a pointed critique of the less-than-careful habits of research he saw prevailing in the profession, habits that led to a tremendous waste of resources. Such waste could have been prevented if senior colleagues such as Joffe, rather than designing entire experimental research agendas based on "extremely primitive" calculations, had consulted with tough-minded theorists in a timely way.[28]

The forty-year-old Tamm—also well traveled—was equally fervent, if generally more diplomatic, when his turn came to speak. He mounted a strong defense of basic research, maintaining that a proper "scientific base" would be the only guarantee of physicists' participation in socialist construction. Like Landau, however, he was not at all sanguine about the Russian past. He referred in passing to Lebedev and another contemporary as "isolated researchers who had not left behind schools, who had not founded a common scientific culture, who had not founded any scientific cadres, no opportunity to work as members of a strong collective, the fundamental requirement for any successful work."[29] These were harsh words, but the mixed audience took them as a healthy part of the exercise. Neither theorist hesitated to dispense with professional comity on this larger stage if he thought that displays of unity came at the cost of internal disciplinary reform. Landau even received some applause when he

[27] A. A. Abrikosov, "O L. D. Landau," in *Vospominaniia o L. D. Landau*, ed. I. M. Khalatnikov (Moscow, 1988), 33.
[28] L. D. Landau, "Vystuplenie," *Izv. AN*, nos. 1–2 (1936): 83–6.
[29] I. E. Tamm, ibid., 87–91.

condemned Joffe's high estimates of Soviet physics. It was commonly understood, even by those who did not entirely care for the "ugly" tone of Landau's criticism, that the debate was supposed to be "passionate" and not heed academic conventions.[30] Yet I single out these remarks because they did not accord well with the nascent patriotic trend—accelerated in the postwar era—to reclaim the past of Russian physics as a worthy predecessor to a vibrant Soviet present.[31] To do so meant, first and foremost, that Lebedev had to be retrieved, and in subsequent years he was—by one of the not-so-little "fragments," who dubbed him the "first organizer of collective scientific work" in Russian physics.[32] Especially among nontheorists, Landau and Tamm's tendentious dismissal of prerevolutionary history would rankle for a long time indeed.[33]

The mere fact that many Soviet contemporaries might have been inclined to treat the October Revolution as year zero scarcely comes as a surprise. But what were the disciplinary ramifications? As early as 1921, one can find Russian talk of a "new era" in atomic physics with reference to Rozhdestvenskii's spectroscopic work, and as we will see in the next section, quantum theory became a rich resource for talking about revolutions.[34] Geochemist and liberal V. I. Vernadskii perceptively surmised that the accelerating revival of Russian science already evident to him in 1922 would unavoidably come to be associated with the revolution, notwithstanding the diverse political views of the twenty scholars he named as instrumental in this process.[35] The eagerness of the Bolsheviks to transform the state's role as patron for science largely ensured this confluence of political and disciplinary temporalities. Yet we need to examine more closely the theorists' historical reasons for differentiating themselves in this fashion in the years after 1917, for the discrepancies can tell us much about the function of research schools in the Russian context. My aim is not to test the well-developed historiography on European research schools in the Russian case.[36] In any event, Russian historians of science have had an interest in research schools since the growth of science studies (*naukovedenie*) in the 1960s and 1970s, when it was hoped that better understanding of school formation could aid in the planning of science. In their work, these historians are often admirably sensitive to analytic distinctions, and they frequently invoke examples of Russian schools, yet seldom pause to ask whether

[30] Krum, "Fizika na sessii" (cit. n. 6), 110.

[31] Epitomized by *Razvitie fiziki v Rossii*, vol. 1 (Moscow, 1970).

[32] S. I. Vavilov, "Petr Nikolaevich Lebedev," *Liudi russkoi nauki* (Moscow, 1961), 277–84. Cf. P. P. Lazarev, *A. G. Stoletov, N. A. Umov, P. N. Lebedev, B. B. Golitsyn* (Leningrad, 1927); Shpol'skii, "Fizika v SSSR," 296, and Shpol'skii, "Piat'desiat let," 200 (both cit. n. 17); Iu. A. Khramov, *Nauchnye shkoly v fizike* (Kiev, 1987), 42–61; A. E. Ivanov, "Nauka," in A. N. Sakharov et al., ed., *Rossiia v nachale XX veka* (Moscow, 2002), 676. In a table on p. 59 of his book, Khramov identifies no fewer than twenty-three schools that purportedly evolved from the Lebedev school, which suggests that dilution of the concept may be as much a danger as fragmentation of the initial shared practices.

[33] L. V. Levshin, *Sergei Ivanovich Vavilov,* 2nd rev. ed. (Moscow, 2003), 60–1, berates the two theorists and restores Lebedev to his rightful place in the school pantheon.

[34] Sokolov, *Nauka v sovetskoi Rossii* (cit. n. 25), 59. On Rozhdestvenskii, see A. N. Osinovskii and A. F. Kononkov, *D. S. Rozhdestvenskii* (Moscow, 1974); S. E. Frish and A. I. Stozharov, eds., *Vospominaniia ob akademike D. S. Rozhdestvenskom: k 100-letiiu so dnia rozhdeniia* (Leningrad, 1976).

[35] See the entry of July 4, 1922, in his *Dnevniki: Mart 1921–avgust 1925*, ed. V. P. Volkov (Moscow, 1998), 67.

[36] Gerald L. Geison and Frederic L. Holmes, eds., *Research Schools: Historical Reappraisals*, *Osiris* 8 (1993); Lutz Danneberg, Wolfgang Höppner, and Ralf Klausnitzer, eds., *Stil, Schule, Disziplin: Analyse und Erprobung von Konzepten wissenschaftsgeschichtlicher Rekonstruktion* (Frankfurt am Main, 2005).

there was anything historically unusual about the Russian context.[37] In the present case, although Landau's students have reflected constructively on the admission criteria and sociology of this most durable of the Soviet schools, they take for granted the narrative of its origins and valuation.[38] Valuable published reflections on the less cohesive schools of Mandelstam and Tamm also betray no interest in historicizing the category.[39] But how did research schools become naturalized so thoroughly in twentieth-century Russia?

To understand the historical appeal of the "school" as institution, we must go back to revolutionary Russia and reflect further on the challenges facing an ambitious young physicist seeking to advance a discipline that had enjoyed only limited state support in the nineteenth century. How would a Russian physicist go about acquiring professional credibility internationally? What social groupings would be most effective professionally in a volatile political setting? How should textbooks be structured so that the mind of the student would be trained (*dressiruet'sia*) more reliably (and by implication, uniformly)? How might national identity enter into this most cosmopolitan of sciences? How would the physicist regard the legacy of imperial Russia? In important respects, the answers to these questions contribute to the evolving collective conception of the "school" in Russia.

In answer to the first question, the biographies of Frenkel, Tamm, Fock, Landau, and George Gamow (1904–68) make it clear that a period of study in central Europe was crucial for a Russian physicist in training. To use contemporary phrasing, these scholars were not possessed by that false sense of national pride that would treat as unnecessary the acquisition of experience in the West. To visit a German-speaking institution and to demonstrate the ease with which one could address problems set by a master physicist, thereby assuming a leadership role among his students—this was a crucial rite of passage for a truly talented Russian. Thus Landau's ability to feed *der Theorienfresser*, Wolfgang Pauli, set him apart in Zurich during extended visits in 1930 and 1931.[40] "Now there is some life in the institute," wrote Pauli's assistant, Rudolf Peierls, and "ardent discussions about everything and if you would come near my room you would hear a dreadful noise, because at any given time of the day there will be some people of different opinion trying to convince each other. All this, naturally, is due to Landau."[41]

What should the talented young physicist do upon returning home? Again drawing on contemporary formulations, it was important that small and fluid groups of students linked by common intellectual interests should have the opportunity to gather outside the usual constraints of official institutions—in a social unit that Russians call the circle, or *kruzhok*. The informal kruzhok could in turn serve as the kernel of

[37] S. R. Mikulinskii et al., eds., *Shkoly v nauke* (Moscow, 1977) (cf. partial exception in the essay of B. A. Starostin, which treats the eighteenth and early nineteenth centuries); Khramov, *Nauchnye shkoly* (cit. n. 32); D. Gouzévitch, "Nauchnaia shkola kak forma deiatel'nosti," *Voprosy istorii estestvoznaniia i tekhniki*, 2003, no. 1:64–93.

[38] Kagan, "Shkola Landau" (cit. n. 21); I. M. Khalatnikov, "Kak sozdavalos' shkola Landau" (1980), in Khalatnikov, *Vospomaniia o L. D. Landau* (cit. n. 27), 267–74. Much the same holds true for Khramov (*Nauchnye shkoly*, cit. n. 32), who treats Mandelstam, Landau, and Tamm among the theorists.

[39] B. M. Bolotovskii, "Shkola Tamma," in Feinberg, *Vospomaniia o I. E. Tamme* (cit. n. 26), 42–64; G. E. Gorelik, "Leonid Mandel'shtam i ego shkola," *Vestnik RAN* 74 (2004): 932–9.

[40] Rudolf Peierls to Evgeniia Kanegisser, 28 Jan. 1931, Rudolf Peierls Papers (hereafter cited as RPP), Department of Western Manuscripts, Bodleian Library, Oxford University, Oxford.

[41] Peierls to Genia, 6 Dec. 1930, ibid.

future research schools, insofar as it provided a fluid setting within which to form intellectual alliances and establish hierarchies of taste and talent. Convene a kruzhok in one's own apartment, if necessary, and when the opportunity arises, formalize it institutionally. At these meetings of the kruzhok, "participants exchange thoughts about topical problems of science, read reviews on the latest achievements of physics, [and] acquaint colleagues with the results of their researches." The subjects discussed ranged widely: "The kruzhok participants do not just discuss science at their meetings," writes one biographer, for "burning social questions also agitate them."[42] Although Leningrad may have dominated the academic scene in Landau's student days, Moscow later offered many opportunities for the scientific prodigy to transform his initial circle of students into "the center of an entire school of Russian physicists who have occupied chairs in universities and other higher educational institutions," as a colleague put it.[43]

The youthful reformer would demonstrate a flair for balancing the stubborn empiricism of many of his experimentalist colleagues and the historical dominance of Russian mathematics, achieving a "harmonious blending" of the two in his theoretical work. Undaunted by his own lack of teaching experience, the junior physicist could pursue an interest in textbook writing, with the spotty Russian publishing landscape permitting someone who was "very nearly a debutante" to intervene productively in university pedagogy—working from first principles, as it were. Deductive rather than inductive methods were to be preferred (as any reader of Landau's *Course of Theoretical Physics* would later confirm).

> We think that concision and precision of expression, unity of terminology, the strictest possible system in the assignment of material—these are the qualities needed for a textbook, especially in the exact sciences. These qualities are hardly reconcilable with a "historical" exposition with the aid of excerpts from books of different eras and of a different nature. We propose that the mind of the student is more reliably trained by strictly consistent and precise exposition of an elaborated series of ideas, than by a spotty and superficial historical survey.[44]

Authorship would be fluid, for the teacher's students could help assemble the materials for these textbooks under his strong editorial direction. When introducing the texts into the university curriculum, he would also enforce a strong hierarchy of examination standards by establishing a formal "minimum," a term now firmly associated with Landau.

Finally, how was the reform-minded physicist to sustain the coherence of the kruzhok amid the volatility of contemporary educational politics, given the state's recurrent suspicion of intellectuals? Learn to avoid becoming the creature of any particular patron, and do not treat rectors or institute directors as all-purpose fixers in daily affairs. Let the kruzhok be partisan only in its collective pride at being able to avoid such external entanglements. Treat the members of the kruzhok as an extended family, for whom there is no greater devotion than the life of science. In this combination

[42] See Anna Livanova, *Landau*, 2nd. ed. (Moscow, 1983), 23.
[43] See V. B. Berestetskii, "Lev Davidovich Landau," *UFN* 64 (1958): 615–23, on 616.
[44] See the introduction to the English edition in L. D. Landau and E. Lifshitz, *Mechanics*, trans. J. B. Sykes and J. S. Bell (Oxford, 1960), vii.

of imperatives, perhaps, we can see the social and intellectual roots of the Landau school.

If I have employed somewhat stilted characterizations of the historical elements of a nascent Landau school, it is because all of the factors described above rely on close paraphrasing and quotation of sentiments that predate Landau by decades. Although I could make the case that each of these challenges facing the young Russian physicist has direct analogies in the formation and identity of the Landau school, everything I have just recited was originally used to describe the career of the physicist Aleksandr Grigorievich Stoletov (1839–96).[45] And I am not referring simply to Soviet-era characterizations of a heroic Russian precursor molded to contemporary needs, for most of the crucial identifying markers—including the distinctive "minimum"—may be found in the obituary of Stoletov written by his biologist colleague Kliment Timiriazev.[46] Indeed, it is striking how many of Stoletov's colleagues agreed on the adoption of the kruzhok and then the school as crucial markers of his ability to bear European and Russian identities simultaneously.[47] Just as important, in the Soviet period all of these elements became standard tropes in Stoletov biographies.[48]

The point here is not to dilute the distinctiveness of the Landau school but rather to emphasize that the kruzhok provides a great deal of historical continuity in the dynamics of identity formation for the Russian scientist.[49] Most concretely in the case of Stoletov, but in many other manifestations as well, the kruzhok-to-school dynamic of the tsarist era provides many of the historical conditions for legitimation of subsequent Soviet institutions of theoretical physics. During the early Soviet period, the school was widely celebrated and increasingly emulated, and the figure of Pavlov

[45] Thus the previous three footnotes point to a later parallel, rather than the original source of the quotation (given below).

[46] K. A. Timiriazev, "Aleksandr Grigor'evich Stoletov," *Russkaia mysl'*, 1896, no. 11:262–80, on 265 (leader among students of master physicist, in this case Gustav Kirchhoff); 266 ("center of an entire school"); 267 (no false sense of national pride); 272 ("harmonious blending"); 273 (textbook authorship); 277 ("minimum").

[47] P. M. Pokrovskii, "Aleksandr Grigor'evich Stoletov," *Universitetskiia izvestiia* (Kiev) 36, 1896, no. 11:1–9; N. N. Shiller, "Kharakteristika lichnosti i nauchnykh trudov pokoinago professora," *Universitetskiia izvestiia* (Kiev) 36, no. 12 (1896): 1–10; A. Sokolov, "Aleksandr Grigr'evich Stoletov," *Zhurnal russkago fiziko-khimicheskago obshchestva* (hereafter cited as *ZhRFKhO*) 29, no. 2 (1897): 25–74, especially, 35–9.

[48] Lazarev, *A. G. Stoletov* (cit. n. 32), 12–3; M. Funder, "Rol' A. G. Stoletova v istorii russkoi fiziki," *Pod znamenem marksizma*, 1940, no. 2:165–80, on 167; V. Bolkhovitinov, *Aleksandr Grigor'evich Stoletov, 1839–1896* (Moscow, 1951), 210–45; A. I. Kompaneets, *Mirovozzrenie A. G. Stoletova* (Moscow, 1956), 34–5; A. A. Glagoleva-Arkad'eva, "Aleksandr Grigor'evich Stoletov," in *Liudi russkoi nauki* (cit. n. 32), 152–9; M. S. Sominskii, *Aleksandr Grigor'evich Stoletov* (Leningrad, 1970), 69–74, 163–6 (all on kruzhok and its institutionalization); Bolkhovitinov, *Aleksandr Grigor'evich Stoletov*, 148–9 ("burning social questions"), 151–2 ("exchange thoughts"), 153 (scientists as family); cf. V. M. Dukov, "Razvitie teorii elektromagnitnogo polia v trudakh russkikh fizikov do opytov Gertsa," *UFN* 49 (1953): 579. Stoletov's statement against historical exposition is quoted in Kompaneets, *Mirovozzrenie A. G. Stoletova*, 47. On the "scientists as family" trope, see also P. P. Lazarev, "O mezhdunarodnykh nauchnykh snosheniiakh," *Nauchnyi rabotnik*, 1926, no. 3:3–10. The kruzhok autonomy trope is found in I. M. Sechenov, *Avtobiograficheskie zapiski* (1904; repr. Moscow, 1952), 226, and is repeated by Lazarev in *A. G. Stoletov*, 26 (cit. n. 32).

[49] Daniel A. Alexandrov, "The Politics of Scientific '*Kruzhok*': Study Circles in Russian Science and Their Transformation in the 1920s," in E. I. Kolchinskii, ed., *Na perelome: Sovetskaia biologiia v 20-30-kh godakh* (St. Petersburg, 1997), 255–67; and Michael Gordin's essay in this volume, "The Heidelberg Circle: German Inflections on the Professionalization of Russian Chemistry in the 1860s." Cf. Barbara Walker, *Maximilian Voloshin and the Russian Literary Circle: Culture and Survival in Revolutionary Times* (Bloomington, Ind., 2005), 1–23.

likely provides the strongest element of continuity in this process.[50] One of his students emphasized that a common topic and a leader were not sufficient to constitute a school. "The term refers also to a definite body of views and ideas, to a whole complex of special methods of investigation. The latter are transmitted as a sort of scientific tradition, being sometimes handed down through a number of generations of scientific workers."[51] The Soviet milieu in general and the physics community in particular thus offered a fertile environment linking the virtues of the kruzhok to school formation in historical terms.[52]

Not so long ago, captive schools were still arrayed against free science, where alone "no dictum of authority oppresses."[53] As John Servos describes it, for much of the nineteenth and early twentieth century, European scientists most often used the term "school" in a derogatory fashion. It could signal dogma, unthinking emulation, even the stifling of genius. Dominance of a given research school could even impede the progress of a discipline. "Schools might train, but could they educate or liberate?"[54] Before 1900, the answer in Europe was usually negative. Had one asked Aleksandr Herzen at midcentury—recalling that he had earlier been trained in astronomy at Moscow University—his usage of "school" would likewise have been with this constricting definition.[55] Yet available evidence strongly suggests that Russian scientists nonetheless began to embrace the positive valence of the term comparatively early, and that this was subsequently taken up wholesale in Soviet usage. If Dmitrii Mendeleev reported from the famous 1860 Karlsruhe Congress about disagreements "among followers of different chemical schools" in the conventional agonistic sense, within a decade he was nominating A. M. Butlerov to a university chair by stressing the link between school formation and original research.[56] In contrast to German and English students of Gustav Kirchhoff, Stoletov praised his teacher's creation of a school in the 1870s, while by the early 1880s, physiologist I. M. Sechenov was already celebrating school formation in chemistry and zoology.[57] According to an early Soviet biographer, Sechenov's great service in the last quarter of the nineteenth century had been not so much introducing students to the latest developments in the West

[50] L. A. Andreyev, "The Great Teacher and Master of Science," *Scientific Monthly* 45, no. 2 (1937): 158–71; Y. P. Frolov, *Pavlov and His School: The Theory of Conditioned Reflexes* (London, 1938). On the workforce trained by Pavlov in the first phase of his career, see, especially, Daniel P. Todes, "Pavlov's Physiology Factory," *Isis* 88 (1997): 205–46.

[51] Frolov, *Pavlov and His School* (cit. n. 50), 255.

[52] A. K. Timiriazev, "Aleksandr Grigor'evich Stoletov—Osnovatel' russkoi fiziki," *UFN* 22 (1939): 369–83, on 370–1. This was the physicist son of K. A. Timiriazev and a thorn in Tamm's side at Moscow University.

[53] Emil du Bois-Reymond, *Kulturgeschichte und Naturwissenschaft* (Berlin, 1878).

[54] John Servos, "Research Schools and Their Histories," *Osiris* 8 (1993): 2–15, on 6.

[55] A. I. Gertsen, "Diletanty-romantiki," *Sobranie sochinenii*, vol. 3 (Moscow, 1954), 25, 32.

[56] D. I. Mendeleev, "Khimicheskii kongress v Karlsrue (Pis'mo k A. A. Voskresenskomu)," in *Sankt-Peterburgskie Vedomosti*, no. 238, 2 Nov. 1860; Mendeleev, "Predstavlenie v sovet universiteta" (1868), both in *Sochineniia*, vol. 15 (Moscow, 1949), 165, 295. For Soviet usage, cf. I. A. Kablukov, "Aleksandr Mikhailovich Butlerov—Osnovatel' russkoi khimicheskoi shkoly," *Zhurnal khimicheskoi promyshlennosti* 5 (1928): 903–9, drawing directly on reminiscences of Butlerov's students written in 1887; A. D. Petrov, "Peterburgskaia shkola A. M. Butlerova," in *Istoriia khimicheskikh nauk*, ed. Iu. I. Solov'ev (Moscow, 1961), 197–211; Iu. I. Solov'ev, *Istoriia khimii v Rossii: Nauchnye tsentry i osnovnye napravleniia issledovanii* (Moscow, 1985), 188–201. See also Gordin, "Heidelberg Circle" (cit. n. 49).

[57] A. G. Stoletov, "G. R. Kirkhgoff," *Priroda*, 1873, no. 2:174–99, on 175; I. M. Sechenov, "The Scientific Activity of the Russian Universities during the Last Twenty-Five Years," *Science* 3 (1883): 756–9, a translation of an article in *Vestnik evropy*.

but his ability to coax them toward independent approaches to scientific problems. This constituted the foundation of the "Russian physiological school." In Russian usage, school formation thus became a sign of scientific progress vis-à-vis western Europe, rather than a marker of restricted outlook.[58] It was a highly functional form of local cosmopolitanism.

In Sechenov's day, one increasingly saw distinctive research methods (*priemy issledovaniia*) characteristic of "scientists of Russian nationality," and there were many domains that had recently acquired their own "Russian schools" (meaning both ethnic Russian scientists and other scientists working on Russian soil). Already before the fin de siècle, schools were becoming constitutive of Russian science.[59] After the 1905 revolution, the kruzhok also enjoyed renewed popularity in a slightly more tolerant university setting.[60] V. A. Steklov (1864–1926), who after 1906 became the dominant mathematician in St. Petersburg with an interest in mathematical physics, is known to have been an active supporter of science kruzhki.[61] While this did nothing for Russian theoretical physics per se, it did ensure comparative neutrality when young physicists bent on theorization sought to avoid the gaze of the senior physics professors by convening their own kruzhok.

By far the most important school precursor for Soviet physicists was Lebedev, best known for his experimental measurement of light pressure in the 1890s. The son of a Moscow merchant, he effectively became Stoletov's successor in Moscow and was among the key faculty presiding at the opening of Moscow University's Physics Institute in 1904. Initially staffed by three people, his laboratory grew to nearly thirty members by the end of the decade. Although Lebedev was known as a rather indifferent lecturer, he showed great skill at reconciling laboratory research and training without compromising the former. He frequently wrote articles in German and encouraged his students to publish abroad. The expansion of his research agendas, however, came along comparatively narrow experimental lines: beyond visible light, he wanted to develop techniques for measuring any ponderomotive forces exerted on solids or gases by other wave phenomena. In that respect, his enlistment of protégés in an increasingly complex division of labor is mainly a story of professionalization, and evidence of a Lebedev school should simply be taken as a symptom of that diverse Pan-European process.[62]

Russian society drew Lebedev into a larger role, however. When the government overreacted to student demonstrations in 1911, the rectors at Moscow University resigned their administrative positions in protest at this heavy-handed disruption of university life. The Ministry of Education then unexpectedly fired them as professors;

[58] M. N. Shaternikov, "Biograficheskii ocherk I. M. Sechenova," in I. M. Sechenov, *Izbrannye trudy* (Moscow, 1935), xv, quoted in M. G. Iaroshevskii, "Logika razvitiia nauki i nauchnaia shkola," in Mikulinskii et al., *Shkoly v nauke* (cit. n. 37), 29.

[59] V. I. Modestov, *Russkaia nauka v poslednie dvadtsat' piat' let* (Odessa, 1890), 10. My thanks to Tatiana Khripachenko for help with this source.

[60] Samuel D. Kassow, *Students, Professors, and the State in Tsarist Russia* (Berkeley, Calif., 1989), 302–3.

[61] I. I. Markush, "K voprosu o sozdanii peterburgskoi–leningradskoi shkoly matematicheskoi fiziki V. A. Steklova," *Istoriia i metodologiia estestvennykh nauk* 16 (1974): 141–53, on 144.

[62] P. Forman, J. L. Heilbron, and S. Weart, "Physics around 1900: Personnel, Funding, and Productivity of the Academic Establishments," *Historical Studies in the Physical Sciences* 5 (1975): 1–185; A. M. Korzukhina, *Ot prosveshcheniia k nauke: Fizika v Moskovskom i S.-Peterburgskom universitetakh vo vtoroi polovine XIX v.–nachale XX v.* (Dubna, 2006), 63–86. St. Petersburg University's physics institute also opened around this time.

Lebedev and many other colleagues demanded that they be reinstated. After the government refused to relent, he and about a third of the faculty resigned, crippling the university. This was not an act of political engagement as such, for Lebedev and most of his colleagues had little sympathy for student agendas and were scarcely eager to acknowledge the failure of the liberal tactic of compromise since 1905. They simply wanted the university to be "above politics."[63] That is why Lebedev found it rhetorically convenient to invoke the temporally distant Mikhail Lomonosov as the proxy for his contemporary dilemmas some months after his resignation. The physicist, by now ailing with the heart disease that would soon end his life, published a memorial essay in *Russkie vedomosti* marking the 200th anniversary of Lomonosov's birth.[64] According to Lebedev, Lomonosov had never been dissuaded from his main aim, "educating Russian society and forcing it to reflect upon new ideas that were alien to her." Lebedev believed that those few Russian scientists who had achieved something worthy of note had done so more despite the conditions offered by Russia than because of them. What he called the "Russian scientific school" had continually been forced to struggle for its very existence.[65] Via the school, a political problem could be recast as a cultural one, and divisive state objectives could be displaced on to unifying national ones. Cohort solidarity could benefit science, thus benefiting the common good. But that was a strategy often better suited to the canny "nonparty" Vavilov than to the splenetic "Trotskyite" Landau under high Stalinism. Lebedev's students—several of whom were among the politically (and disciplinarily) cautious colleagues the former Menshevik Tamm found himself despising when he returned to Moscow to teach in 1922—not only celebrated Lebedev's genuine achievements but also benefited indirectly from the canonization of his school during the early Soviet period.[66] Their living witness to "enduring scientific schools" was further proof that Soviet Russia was no longer a "barbarous country with low culture."[67]

Not only did the absence of a school signal the absence of a prerevolutionary history for Soviet theorists. It also gave them license to reinvent the kruzhok in Soviet life.

THE JAZZ BAND PLAYS AT THE REVOLUTION

There actually was a prerevolutionary theoretical physicist in Russia who offered a small measure of continuity between pre- and postwar experience, fostering the

[63] Kassow, *Students, Professors* (cit. n. 60), 353–66. An inordinate number of those resigning came from the natural science faculty.

[64] P. N. Lebedev, "Pamiati pervogo russkogo uchenogo 1711–1911 gg.," *Sobranie sochinenii*, ed. T. P. Kravets (Moscow, 1963), 350–8.

[65] Ibid., 357. This was in contrast to the school formed by his revered teacher, August Kundt. Lebedev, "August Kundt" (1894), in ibid., 49–67, on 61–6.

[66] T. P. Kravets, "P. N. Lebedev i sozdannaia im fizicheskaia shkola" (1913), in *Ot N'iutona do Vavilova: Ocherki i vospominania* (Leningrad, 1967), 321–7; Lazarev, *A. G. Stoletov* (cit. n. 32), 38; Vavilov, "Fizika v Rossii" (cit. n. 18), 995; Shpol'skii, "Fizika v SSSR" (cit. n. 17), 296; O. A. Lezhneva, "Issledovaniia po istorii fiziki," in *Razvitie fiziki v SSSR*, vol. 2 (Moscow, 1967), 345; Khramov, *Nauchnye shkoly* (cit. n. 32), 46–61.

[67] Lazarev, "O mezhdunarodnykh nauchnykh snosheniiakh" (cit. n. 48), 4, likewise writing about Lomonosov and the dilemmas of Russia's lone geniuses under the tsars—and the latter-day benefits of the Soviets.

school ideal in his own quirky way: Paul Ehrenfest (1880–1933)—Pavel Sigismundovich to his Russian colleagues.[68] Although he eventually became the successor to H. A. Lorentz's chair at Leiden in 1912, as a Viennese Jew his initial professional prospects had been rather poor. He and his Russian wife, Tatiana Afanasieva, had decided to move to St. Petersburg in 1907 for family reasons, and Ehrenfest scraped by on editorial assignments, winning only one short-term teaching contract at the Polytechnic Institute. It was in Russia that he and Joffe, also Jewish and also facing obstacles to obtaining a permanent position in the capital, became fast friends. Although both men were trained at German-speaking universities, each took his Russian experience as constitutive in forming a scientific identity vis-à-vis an initially ambivalent professional world.[69]

The two men had first met briefly in Munich, where Joffe was studying with Wilhelm Röntgen (1902–6). A major factor in the later tension between Joffe and Landau regarding the professionalization of theory was Joffe's own youthful confidence as an experimentalist who could keep up with the latest results of atomic theory and productively query colleagues such as Ehrenfest. After assuming the theory chair in Munich, Arnold Sommerfeld had been eager to get up to speed with experimental developments in Röntgen's institute, and he determined to spend an hour or so in "training" each day. He latched on to Joffe as his interlocutor, and Joffe proposed not long afterward that they gather at a local café and hold the discussions there in a less formal atmosphere: a Bavarian kruzhok. Joffe and Ernst Wagner then took the initiative, inviting a mixture of theoretical and experimental physicists, crystallographers, and physical chemists to the kruzhok, with Sommerfeld student Peter Debye eventually taking the lead in Sommerfeld's (and later Joffe's) absence.[70]

In St. Petersburg, Joffe and Ehrenfest joined with Rozhdestvenskii to start their own kruzhok on much the same model, having been urged to do so by Tatiana's aunt, who simply assumed that the kruzhok was the natural social milieu for restless young scientific intelligentsia.[71] Mathematicians were always welcome, assuming they were ready to engage Ehrenfest's interests in statistical mechanics, for instance. The kruzhok was formed largely to bypass disapproving university superiors and to discuss the latest developments in atomic physics.[72] When word of the mass resignations in Moscow came to the capital, it was many of these same figures who used their control of the council of the physics division of the Russian Physico-Chemical Society to call for their St. Petersburg colleagues not to become involved. Members of the kruzhok were the most vocal in protesting this minority position, forcing the society to pass a resolution that included the following: "Anyone who holds dear

[68] Martin J. Klein, *Paul Ehrenfest: The Making of a Theoretical Physicist* (Amsterdam, 1970); V. Ia. Frenkel', *Paul' Erenfest* (Moscow, 1971).

[69] Ehrenfest to Joffe, 20 Aug. 1910; Joffe to Ehrenfest, 25 Aug. 1910, in *Erenfest-Ioffe nauchnaia perepiska 1907-1933gg*., 2nd ed., ed. V. Ia. Frenkel' (Leningrad, 1990), 66–7, 270–1.

[70] A. F. Joffe, *Vstrechi s fizikami* (Leningrad, 1983), 34; Wagner to Joffe, 4 Jan. 1908, ibid., 138.

[71] Klein, *Paul Ehrenfest* (cit. n. 68), 85.

[72] By this time, even senior physicist I. I. Borgman (1849–1914), former rector and object of their disdain, had his own formal physics kruzhok for the advanced undergraduates to report on current topics, so there was no special social frisson to the Ehrenfest kruzhok. They were in direct competition to advance more ambitious research agendas for their young peers. Cf. "Fizicheskii kruzhok pri un-te za 1911," *Otchet o sostoianii i deiatel'nosti imperatorskogo S.-Peterburgskogo universiteta za 1911 god* (St. Petersburg, 1912), 217–9.

the development of physics in Russia has followed with a feeling of deep satisfaction as a model school of physicists has grown and developed in Moscow . . . The Moscow school represents a viable developing organism" engaged in all the activities necessary to train independent researchers. "We cannot reconcile ourselves to the thought that the oldest Russian university is depriving itself of a school of physicists so exceptional in its significance."[73]

Although this availed the members of the kruzhok nothing, it did seal Joffe's determination to build a research school of his own. Having defended his Russian doctoral dissertation in 1915 (with Steklov as one of the opponents), Joffe lost a bid for a university professorship but finally won a chair at the Polytechnic later that year. The following year, the kruzhok was institutionalized as an interdisciplinary seminar led by Joffe, and an inordinately high percentage of physicists who gained positions of influence in the Soviet era counted this as a rite of passage—Frenkel included. The kruzhok also gave Ehrenfest a chance to further the training of a university student named Yuri Krutkov (1890–1952), who then followed him to Leiden and later published the earliest survey of quantum theory in Russian.[74] Krutkov is generally reckoned the first Russian theoretical physicist, albeit without benefit of the "school" moniker, as the epigraph by Tamm implies.[75] Daniel Alexandrov has rightly observed that this kruzhok served in turn as a model for the proliferation of similar informal institutions during the early Soviet era.[76] Indeed, when Joffe subsequently felt the bite of young Soviet theorists eager to put him in his place, Ehrenfest calmed him with reminders of their own youthful behavior toward senior professors while running the St. Petersburg kruzhok.[77]

War and revolution cut Ehrenfest off from Russia for the better part of six years, but he was drawn back by the chance to interact with young Soviet physicists. After renewing correspondence in 1920, he became the sole foreigner to attend the 1924 meeting of the Russian Association of Physicists.[78] As he told Joffe afterward, "Both in Germany and in Holland I am coming closer and closer to the conclusion that it's infinitely easier for me to make intellectual contact with Russians than with non-Russians."[79] Soviet students took note, swarming the diminutive theorist after his presentation on the correspondence principle and the quantum theory of light, at which he melodramatically declared in his stilted Russian that the battle between wave and corpuscular theories "takes place in the heart of every physicist."[80] That the battle had famously driven Bohr, Hendrik Kramers, and John Slater to speculate that both energy conservation and causality might only be preserved at the statistical level did not discourage Ehrenfest's audience. "In these young people we

[73] M. S. Sominskii, *Abram Fedorovich Ioffe* (Moscow, 1964), 176–8.
[74] Iu. Krutkov, "O teorii kvantov," *ZhRFKhO* 48 (1916): 43–76.
[75] V. Ia. Frenkel, "Iurii Aleksandrovich Krutkov," *UFN* 102 (1970): 639–54.
[76] Alexandrov, "Politics of Scientific '*Kruzhok*' " (cit. n. 49).
[77] Joffe to Ehrenfest, 27 Dec. 1932, *Erenfest-Ioffe nauchnaia perepiska* (cit. n. 69), 298–301; Ehrenfest to Joffe, 21 Dec. 1932, Ehrenfest Scientific Correspondence (hereafter cited as ESC), Museum Boerhaave, Leiden.
[78] Joffe to Ehrenfest, 18 June 1920, and Ehrenfest to Joffe, 6 Sept. 1920, *Erenfest-Ioffe nauchnaia perepiska* (cit. n. 69), 274–5, 138–42; Joffe to NTO VSNKh, before 17 July 1924, in *Organizatsiia nauki v pervye gody Sovetskoi vlasti (1917–1925): Sbornik dokumentov* (Leningrad, 1968), 392; Sominskii, *Abram Fedorovich Ioffe* (cit. n. 73), 398–9.
[79] Ehrenfest to Joffe, 19 Oct. 1924, *Erenfest-Ioffe nauchnaia perepiska* (cit. n. 69), 174.
[80] See the brief resumé in P. S. Ehrenfest, "Teoriia kvantov," *ZhRFKhO* 56 (1924): 449–50.

have the future scientific workers," proclaimed a junior physicist at the scene, "and their attitude toward the purely theoretical problems touched upon at the congress clearly demonstrates that they are interested not only in practice but in genuine *living physical science*." The dialectical appeal of Bohr's correspondence principle offered even more, however, for it suggested to the young Russian that "there is nothing to be concerned about by the contradiction of modern physics, which is undergoing at present in quantum theory and relativity theory a revolution equal in its significance to the world revolution [sic] that took place seven years ago."[81] Some months later, writing about magnetism just before the appearance of Pauli's seminal papers formulating the exclusion principle, Tamm likewise painted modern physics as characterized by "the accumulation of deep internal contradictions in new fruitful theories."[82]

Soviet enthusiasm for the contradictions of modern physics in the early 1920s could not be taken for granted—resources were too scarce. While in the German-speaking universities in 1921 "the interest of the greatest portion of the younger physicists [was] concentrated on either atomic theory or quantum theory," the same could not be said for their Soviet counterparts.[83] The dominance of the mathematicians not only limited the ranks of the physicists in Petrograd and Moscow but also made it more likely that mathematically adept students with a bent for applied problems would choose mechanical engineering over physics.[84] In Moscow, specialization in the exact sciences was regarded as "without prospects" early in the decade, even though the physicists were generally credited by militant students with being "closer to industry, to workers, to the organs of Soviet power, [and] joined us a lot earlier than the mathematicians living in their calculations or the flower-loving botanists."[85] Those few who did opt to study physics lagged considerably behind their counterparts in Petrograd when it came to relativity and quantum theory.[86]

Moscow University appointed its first professor of theoretical physics, S. A. Boguslavskii, in the spring of 1919, although the civil war kept him from assuming his post until 1921. Trained under the Göttingen crystallographer Woldemar Voigt, Boguslavskii was best known for his attempt to give pyroelectricity a basis in quantum

[81] D. Galanin, "Osnovnye voprosy sovremennoi fiziki," *Krasnaia molodezh'*, Oct. 1924, no. 3:146–54, on 147, 154 (emphasis in original). D. D. Galanin worked with Tamm on a modest research project in the spring of 1923 at Sverdlov Communist University, f. 5221, op. 4, d. 68, l. 93, State Archive of the Russian Federation (Gosudarstvennyi Arkhiv Rossiiskoi Federatsii, hereafter cited as GARF), Moscow. This is likely the father of an accomplished postwar student of quantum electrodynamics, A. D. Galanin.

[82] I. E. Tamm, "Magnetizm i stroenie atomov," *UFN* 5, nos. 1–2 (1925): 105–37, translated as "Magnetism and the Structure of Atoms," *Soviet Physics Uspekhi* 36 (April 1993): 246.

[83] Hans Thirring, "Ziele und Methoden der theoretischen Physik," *Die Naturwissenschaften* 9 (1921): 1023–8, on 1026.

[84] Although faculty numbers at Moscow University were nearly the same, four times as many students pursued advanced degrees in pure and applied mathematics as in physics. See *Otchet o sostoianii i deistviiakh imperatorskago Moskovskago Universiteta za 1914 god* (Moscow, 1915); *Otchet o sostoianii i deistviiakh Moskovskago Universiteta za 1916 god* (Moscow, 1917). If anything, Petrograd favored mathematics even more.

[85] N. M. Beskin, "Vospominaniia o Moskovskom fizmate nachala 20-kh godov," *Istoriko-matematicheskie issledovaniia* 34 (1993): 164; Semen Rin, "Revoliutsiia na fizmate," *Krasnoe studenchestvo*, 7 Nov. 1927, nos. 4–5:148.

[86] P. S. Kudriavtsev, "Prepodavanie fiziki v moskovskom universitete v 1922–1926 gg.," *IMEN* 17 (1975): 126.

theory.[87] Although Boguslavskii took an active interest in atomic theory,[88] both Tamm and Mandelstam held him in low regard.[89] As it turned out, they were the primary beneficiaries when the unfortunate Boguslavskii took ill and died of tuberculosis in the fall of 1923, leaving the modest "office of theoretical physics with laboratory" in disarray.[90] Ehrenfest was invited to take the chair but declined in deference to Lorentz.[91] Mandelstam was finally lured back to Moscow with an appeal to form a kruzhok, bringing together "people who desire and who are able to work, to put an end to the countless intrigues that have permeated the grounds of the [university's physics] institute."[92] Tamm, who had been surviving since 1922 on modest teaching assignments at Sverdlov Communist University and the second Moscow University (run directly by the Commissariat of Education), as well as consulting for the state electrical power supplier, promptly accepted Mandelstam's invitation to join the staff.[93] With this foothold, he became the leading advocate of modern physics as he remained more politically engaged than the reticent Mandelstam. For Tamm, political conservatism and disciplinary conservatism went hand in hand, which is why he told his wife in 1922 that he was becoming more radical and would keep doing so ("leveiu i budu levet'"), because the "struggle for the sovietization of higher education" demanded no less.[94] Even further to the left, the new party monopolists had their reasons for defending the "theoretical science" of the physicists, indirectly benefiting the modernizers. For it was here that "one can specialize in those problems which have cardinal significance for our worldview and which find application in party propagandistic work (structure of matter, theory of relativity, astronomy)."[95] Theoretical physicists happily exploited this conflation of tasks. Despite dropping any party affiliation soon after October 1917, Tamm retained a reputation as a Bolshevik among the faculty until the late 1920s, when party watchdogs began detecting signs of "wavering."[96]

This level of apparent politicization was not necessarily at odds with professionalization for someone such as Tamm in the early years of Soviet power. Like Frenkel just old enough to have wrestled with the "accursed questions" of the Russian intelligentsia during war and revolution, Tamm cast them aside and turned to what he likewise called the "accursed questions" of fundamental physical theory, generally

[87] S. Boguslawski [Boguslavskii], "Pyroelektrizität auf Grund der Quantentheorie," *Physikalische Zeitschrift* 15 (1914): 569–72.

[88] See S. Boguslavskii, "K voprosu o stroenii atomnogo iadra," *ZhRFKhO* 52 (1922): 73–84; Boguslavskii, "O vnutrennem iavlenii Zeemana," ibid., 89–97; Boguslavskii, "K istolkovaniiu uravneniia, opredeliaiushchego chastotu sveta izluchaemogo model'iu atoma Bora," ibid., 85–7; Boguslavskii, "On the Problem of Two Moving Charges in Its Connexions with the Atomic Theory," *Philosophical Magazine* 45 (1923): 145–60.

[89] I. E. Tamm to N. V. Tamm, 11 Nov. 1922, in *Priroda*, July 1995, no. 7:150.

[90] *Otchet I-go Moskovskogo Gosudarstvennogo Universiteta za 1923 g.* (Moscow, 1924), 134–6; D. D. Gulo, A. F. Kononkov, and B. I. Spasskii, "Nauchnye trudy S. A. Boguslavskogo," in *Voprosy istorii fiziko-matematicheskikh nauk*, ed. K. A. Rybnikov et al. (Moscow, 1963), 332–45.

[91] Ehrenfest to Joffe, 6 July 1924, *Erenfest-Ioffe nauchnaia perepiska* (cit. n. 69), 164–5.

[92] G. S. Landsberg to L. I. Mandelstam, 18 June 1924, f. 1622, op. 1, d. 75, l. 1, ARAN.

[93] On Tamm at Sverdlov, see f. 5221, op. 4, d. 83, l. 46, GARF. Tamm and Mandelstam had worked together in Odessa from 1920 to 1922.

[94] Tamm to his wife, 6 Nov. 1922, in Vernskii, "I. E. Tamm v dnevnikakh i pis'makh" (cit. n. 8), 269.

[95] A. Vyshinskii, A. Timiriazev, and V. Egorshin, "Chto takoe fizmat?" *Izvestiia*, 5 June 1927, 7.

[96] I. M. Frank, "Otryvki vospominanii raznykh let," in Feinberg, *Vospominaniia o I. E. Tamme* (cit. n. 26), 259; A. A. Maksimov, "Kharakteristika sostava osnovnykh professorskikh grupp fizicheskogo razdela fizmata," f. 225, op. 1, d. 23, Moscow State University Archive, Moscow.

working hard to ensure that the latter would *not* have to speak to extradisciplinary issues at every turn.[97] Theirs was not the classic October resolution of "internal conflict" for the tsarist-era scientist-intelligent finally given "the opportunity to infuse his scientific work into the construction of a new socialist society."[98] Or rather, there was little perceived conflict to begin with, because the second term of the equation was largely taken for granted among those who reached maturity after 1917. Tamm and his younger peers, while supremely confident in the efficacy of the new quantum theories they came to master in the course of the decade, did not subscribe to the boundary-dissolving cultural logic of the Bolsheviks. To be sure, that they were increasingly drawn into work "on the cultural front" was testimony to the gradual sovietization of intellectual life, but it was more than that.[99] Although suspicious in the early 1920s of the few Communist physicists as "new monopolists" on the left, and contemptuous of the more opportunistic Old Regime physicists on the right, they came to see the politically insecure and often professionally undistinguished "gray" middle as equally worrisome for theory.[100] Especially in the hands of its quantum theoretic flag bearers, the disciplinary question of the place of atomic physics became entangled at times in the recurrent political question of who was to dominate Soviet culture—more ideologically militant "proletarian" intellectuals, or "fellow travelers," intellectuals who were unquestionably "Soviet" but invested in professional values. No other options were available. As fellow travelers, these theorists' challenge was at once intellectual and social in nature: to make quantum physics central to the profession, and to secure an institutional role for theory in the process.

The rejection of generational prerogatives that F. Scott Fitzgerald would identify with the Jazz Age found analogous expression among these young Soviets as they entered Leningrad University in the early 1920s. Several of them formed the so-called Jazz Band as the (decidedly nonmusical) vehicle for this iconoclastic social dynamic. The atmosphere of "militant informality" that they cultivated in this kruzhok has been colorfully described by Matvei Bronshtein's biographers.[101] Centered around "Jonny" (Gamow), "Dimus" (Dmitrii Ivanenko, 1904–94), and "Dau" (Landau), membership in this cohort seems to have been based in equal measure on talent for modern physics and capacity for scathing irony, with no subject exempt from heated debate, summary scorn, or rollicking amusement.[102] As a biographer of Landau depicts it, "[T]hey spared no one, including one another. Salvos of criticism were aimed in all directions, and the more gladly, the more important the 'cause.' "[103]

It was in this spirit that the Jazz Band regularly issued an informal manuscript titled *Physikalische Dummheiten*, devoted to uncovering mistaken calculations and

[97] See the youthful reflections reproduced by Tamm's grandson in L. I. Vernskii, "V kabinete i vne ego," Feinberg, *Vospominaniia o I. E. Tamme* (cit. n. 26), 82, 86. Cf. V. Ya. Frenkel, *Yakov Ilich Frenkel: His Work, Life and Letters*, trans. A. S. Silbergleit (Basel, 1996), 20–1.

[98] The reference is to biochemist, tsarist-era radical, and Bolshevik favorite A. N. Bakh (b. 1857), "Kak i kogda ia stal marksistom," *Front nauki i tekhniki*, 1933, no. 2:123–4.

[99] Katerina Clark, "The 'Quiet Revolution' in Soviet Intellectual Life," in *Russia in the Era of NEP: Explorations in Soviet Society and Culture*, ed. S. Fitzpatrick, A. Rabinowitch, and R. Stites (Bloomington, Ind., 1991), 210–30.

[100] I. E. Tamm to N. V. Tamm, [Fall] 1922 and 6 Nov. 1922, reproduced in *Priroda*, July 1995, no. 7:148–9. Cf. Sokolov, *Nauka v sovetskoi Rossii* (cit. n. 25) , 19–23.

[101] G. E. Gorelik and V. Ia. Frenkel, *Matvei Petrovich Bronshtein* (Moscow, 1990), 25–9.

[102] Compare Landau's remarks to a Danish student group in March 1931, as related by Hendrik Casimir, *Haphazard Reality: Half a Century of Science* (New York, 1983), 113.

[103] Livanova, *Landau* (cit. n. 42), 23.

misguided assertions in the leading physics journals of the day. Its contents were often announced without warning at the university physics seminar, to the occasional discomfiture of senior physicists in attendance. When Joffe sniffed about "half-mad Schrödingerites who don't understand why the rest of physics and letters other than Ψ still exist," it was members of the Jazz Band he had in mind.[104] There is perhaps no better testimony to the unease this prevalent kruzhok dynamic engendered among cultural authorities than the bookish desire to condemn "kruzhok cliquishness" by definition, so to speak, as "a tendency toward the formation of tight and closed groups within the greater collective or organization, leading to splitting, disorganization, and to isolation from the masses."[105] The cliquishness of these new adepts of quantum mechanics would only be increased by the opportunity to obtain further training abroad.

The international successes of quantum mechanics in the mid-1920s fired revolutionary expectations for the future of theory, which Soviet theorists saw as threatened by the indifference of mediocre fellow physicists who neither properly appreciated its potential nor even acknowledged a viable role for theory.[106] I say "revolutionary" advisedly, because the term carried so much cultural baggage into these disciplinary disputes, yet "revolutionary" was still embraced knowingly by these same theorists. Without intellectual ratification (and occasional resistance) from renowned outsiders such as Einstein and Bohr, however, they would not have made much headway in staking their sometimes convoluted claims to cultural authority at home. The language of crisis and revolution that they invoked was partly an echo of Bohr's institute and the anomalies of the old quantum theory circa 1923, taken over to the problems of relativistic quantum mechanics in the late 1920s. Following Suman Seth, we can regard this as a rhetorical moment in the continual reconfiguration of the international theoretical physics community, with portions seeing the "physics of principles" (e.g., Copenhagen), while others saw the "physics of problems" (e.g., Arnold Sommerfeld's school), with little sense of accompanying crisis.[107] If histories of modern theory in Europe have frequently adopted the crisis language of the former to characterize the processes of conceptual change involved, the background of recurrent political crises in Soviet Russia also hints at how the physics of problems could gain traction in the wake of failed "revolutions" in the physics of principles—not just between groups but within them.[108]

Those few Soviet physicists with precious experience abroad were often in a stronger position to advocate science as culture within the Bolshevik enlightenment

[104] Joffe to Ehrenfest, 26 May 1928, *Erenfest-Ioffe nauchnaia perepiska* (cit. n. 69), 281.

[105] S. v. "kruzhkovshchina," in *Malaia sovetskaia entsiklopediia* (1929), as noted in B. F. Egorov, "Russkie kruzhki," *Iz istorii russkoi kul'tury*, vol. 5 (Moscow, 1996), 504–17, on 513.

[106] This was not a uniquely Soviet problem as there was no one "natural" trajectory for the institutionalization of (quantum) theory. In the closest parallel, French physics was also slow to create chairs for theoretical physics, as described by Dominique Pestre, *Physique et physiciens en France, 1918–1940* (Montreux, 1984), chap. 4.

[107] Suman Seth, "Crisis and the Construction of Modern Theoretical Physics," *British Journal for the History of Science* 40 (2007): 25–51.

[108] Much this sort of piecewise resolution may be seen in quantum electrodynamics in the late 1940s, as described in S. S. Schweber, *QED and the Men Who Made It* (Princeton, N.J. 1994). For an explicit focus on problem solving as collective process, see also David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago, 2005); David Kaiser, Kenji Ito, and Karl Hall, "Spreading the Tools of Theory: Feynman Diagrams in the United States, Japan, and the Soviet Union," *Social Studies of Science* 34 (2004): 879–922.

project, countering or reappropriating the stronger utilitarian strain in Bolshevik political economy. They could do so in part because so many of them shared with party leaders a picture of Russia's "backwardness" that made the approach of socialism hinge upon wresting the fruits of (Western) science from the hands of "civilized barbarians" and placing science "on the mantel of cultural rebirth."[109] Once the Soviet state had begun investing considerable sums in these domains, there was no more treasured endorsement among the Bolsheviks than, for instance, a German scholar's comment that "all competent critics testify that the new Russia stands behind no other civilized land [*Kulturland*]" in the natural sciences and medicine.[110] In such an environment, the rare young physicist such as Gamow who "bore the banner of Soviet science abroad in honorable fashion" had every reason to expect that he would attract "the attention of the cultural world" at home.[111] As the Soviet Union undertook a massive industrialization effort in the late 1920s and early 1930s, even so abstract an endeavor as nuclear physics could become the focus of Soviet physicists' bids for public esteem and state support—in the militaristic language of the day, they proclaimed the "storming of the atom."[112]

Although Soviet theorists made no crucial contributions to early quantum mechanics (1925–27), once Gamow made the first successful application of the theory to the atomic nucleus in 1928, it did not take long for his achievement to become a Soviet virtue.[113] For in his work, theory had "decisively broken with stubborn conservatism and classicism no matter what."[114] Gamow was even heralded in *Pravda* for raising the profile of Soviet science.[115] He wasted no time capitalizing on his newfound celebrity. Just as he was preparing to submit his paper, he appealed to Bohr (whom he had not met) for an invitation to Copenhagen, an invitation soon forthcoming.[116] Glad to come in closer contact "with young Russian science," Bohr enthused to Ehrenfest that Gamow "has seized on the problem of nuclear structure with the highest

[109] L. B. Kamenev, "Rabochaia revoliutsiia i nauka," *Narodnoe Prosveshchenie*, 1925, no. 9:23–5 ("civilized barbarians" was adopted from an unidentified Russian scientist); A. I. Rykov is quoted in I. Brusilovskii, "Nauchnyi front," *Krasnaia gazeta*, 22 March 1928, 2. For Lenin's obsession with this topic, see, e.g., the compilation *V. I. Lenin o kul'ture* (Moscow, 1980). On American debates about Russian backwardness, see David C. Engerman, *Modernization from the Other Shore: American Intellectuals and the Romance of Russian Development* (Cambridge, Mass., 2003).

[110] Eduard Meyer, "Eindrücke von der Jubiläumsfeier der russischen Akademie der Wissenschaften," *Deutsche Rundschau* 52, no. 205 (1925): 101–18, on 112.

[111] V. E. L'vov, "Mirovoe otkrytie sovetskogo uchenogo," *Krasnaia gazeta*, 17 May 1929, 4.

[112] Fizik [Physicist], "Shturm atomov," *Krasnaia gazeta*, 28 Feb. 1928, 5. Certain details suggest the story was planted by Joffe. Frenkel later repeated the trope: "We are now on the eve of the last decisive storming of the problem of matter, and in all likelihood we will soon be able to celebrate final victory." Ia. I. Frenkel, "Proiskhozhdenie i razvitie volnovoi mekhaniki," *Priroda*, 1930, no. 1:3–32, on 32.

[113] George Gamow, "Zur Quantentheorie des Atomkernes," *Zeitschrift für Physik* 51 (1928): 204–12; Ronald W. Gurney and Edward U. Condon, "Wave Mechanics and Radioactive Disintegration," *Nature* 122 (1928): 439; Roger H. Steuwer, "Gamow's Theory of Alpha-Decay," in *The Kaleidoscope of Science*, ed. Edna Ullmann-Margalit (Dordrecht, 1986), 147–86.

[114] Vavilov, "Fizika v Rossii" (cit. n.18), 1004. The official history of Soviet theory prepared for the fiftieth anniversary of the revolution—whose editorial board included Tamm, M. A. Leontovich, and Landau student A. A. Abrikosov—simply bypassed the modest first decade of "revolutionary" theory by starting with quantum field theory, at which point V. A. Fock's contributions became indispensable. See *Razvitie fiziki v SSSR*, vol. 1 (Moscow, 1967).

[115] Demian Bednyi, "Do atomov dobralis'," *Pravda*, 28 Nov. 1928; cf. George Gamow, *My World Line: An Informal Autobiography* (New York, 1970), 74–5.

[116] Gamow to Bohr, 28 July 1928, Bohr Scientific Correspondence (hereafter cited as BSC), Niels Bohr Archive, Copenhagen.

imaginative capacity."[117] Within a short time, Gamow found his way into Bohr's inner circle.[118]

Gamow's time in Copenhagen and Cambridge opened the way for other visitors, including Landau. On Frenkel's recommendation, the Commissariat of Education dispatched Landau to Germany in the fall of 1929. By the following spring, Landau had obtained funding from the Rockefeller Foundation to extend his stay in Europe for another year, thus providing him with the opportunity to search out all that was new and noteworthy in Western physics.[119] In short order, he visited Berlin and Leipzig but finally settled in Zurich under the watchful eye of Wolfgang Pauli. In certain respects, Pauli, at an earlier age, had been no less brash toward his elders than Landau had toward his and thus was unlikely to be cowed or offended by his visitor's antics. The cozy confines of the Eidgenössische Technische Hochschule indeed did little to dampen the revolutionary sensibilities of a twenty-one-year-old scholar given to "very extreme views about everything, not only physics."[120] Mildly flattered by the Swiss government's unwillingness to extend his visa for more than a brief stay, Landau noted with some amusement that a generation earlier his compatriot Lenin had been judged less potentially troublesome.[121]

Landau sought notoriety of another sort, however. The frisson of student nonconformity, combined with professional approbation for his first physics papers on quantum mechanics while still at the university, instilled in him a fierce determination to fashion a distinct persona for himself among the "bourgeois" European physicists whom he simultaneously revered and despised.[122] His new friend Peierls (Pauli's assistant) came to see in Landau a helpful "litmus test" for drawing out the true sentiments of everyone he encountered.[123] The twenty-three-year-old Berliner and son of an Allgemeine Elektrizitäts-Gesellschaft managing director marveled at Landau's capacity to shock bourgeois people. It was not simply that he spouted revolutionary views—that was not uncommon among European intellectuals.

> But he discusses not at all with reasons; he simply puts against them a system which is as closed and "obvious" as theirs. He does not try to be objective but is as subjective as they are, only with different prejudices and this amazes them awfully much. It is really an interesting experiment to notice the effect he makes on different people . . . If somebody is not shocked, he is surely in a very safe [psychological] position.[124]

[117] Bohr to Ehrenfest, 28 Oct. 1928, BSC.

[118] See the contemporary letter of Nevill Mott to his mother, reproduced in Nevill Mott, *A Life in Science* (London, 1986), 28.

[119] Landau to Peierls, 17 May 1930, RPP; V. I. Frenkel' and P. Josephson, "Sovetskie fiziki–stipendiaty Rokfellerovsko go fonda," UFN 160, no. 11 (1990): 103–43, on 130.

[120] Rudolf Peierls interview by J. L. Heilbron, 18 June 1963, AHQP, Niels Bohr Library, American Institute of Physics, College Park, Maryland.

[121] Rudolf Peierls, *Bird of Passage* (Princeton, N.J. 1984), 49.

[122] In Copenhagen, Landau's dogmatic pronouncements on all topics were soon lampooned; see Heisenberg to Pauli, 12 March 1931, in Wolfgang Pauli, *Wissenschaftlicher Briefwechsel*, vol. 2, ed. K. von Meyenn (Berlin, 1985), 66. The unusually informal social atmosphere of Bohr's institute made it perhaps the only place in central European academia where scientists such as Landau and Gamow could give free rein to these unconventional behaviors. See Finn Aaserud, *Redirecting Science: Niels Bohr, Philanthropy, and the Rise of Nuclear Physics* (Cambridge, UK, 1990), 6–15, for a colorful evocation of the Copenhagen setting.

[123] Peierls to Genia, 6 Jan. 1931, RPP.

[124] Peierls to Genia, 21 Dec. 1930, RPP.

These antics extended to the physics colloquium, where Landau summarily condemned Edward Milne's cosmological "kinematic relativity" as "horrific nonsense," injecting his favorite epithet "pathological" in every phrase, again discomfiting the "bigwigs" in attendance.[125]

Convinced that proponents of the existing theory had not yet provided the answer, Landau kept asking, "[*W*]*ho* is going to make quantum electrodynamics?"[126] During a fruitful year of intellectual and geographic peregrination, Landau finally seized upon a conceptual conflict that seemed to suit his purposes. The end result was a contentious paper with Peierls that proclaimed limits upon the applicable domain of quantum mechanics and, while not actually ushering in the new quantum electrodynamics, still celebrated the failure of the "old" theory.[127] Landau and Peierls's hasty assertions that there would be "no physical quantities and no measurements in the sense of wave mechanics" in the future relativistic theory provoked Bohr and Léon Rosenfeld to produce in response a seminal paper clarifying the problem of measurement in quantum electrodynamics.[128] Although it did sow the seeds for many future anecdotes about him in the physics community, this dispute did not initiate the conceptual rupture Landau had sought. On the contrary, it helped put measurement on an even firmer footing within the prevailing formalism. It is not my purpose here to give a proper account of the "triumph" of Bohr's views on field measurement, and the Bohr-Rosenfeld paper will consequently get short shrift.[129] Yet there were profound temperamental differences between Bohr and Landau in their approaches to physics, differences whose consequences for the practice of theoretical physics are largely obscured in standard accounts of how Bohr and Rosenfeld disposed of the criticisms of Landau and Peierls.[130]

The restless Landau had already left for Leipzig by the time the final draft was officially submitted for publication, but he found the city's denizens too "boring" to justify lingering there for long.[131] An element of social insecurity may have been

[125] Peierls to Genia, 11 Feb. 1931, RPP.

[126] H. Casimir interview by Thomas S. Kuhn, 5 July 1963, AHQP. Léon Rosenfeld was in attendance and injected this recollection.

[127] L. D. Landau and R. Peierls, "Erweiterung des Unbestimmtheitsprinzips für die relativistische Quantentheorie," *Zeitschrift für Physik* 69 (1931): 56–69.

[128] Niels Bohr and Léon Rosenfeld, "Zur Frage der Messbarkeit der elektromagnetischen Feldgrössen," *Konigelige danske videnskabernes selskab, mathematisk-fysiske meddelelser* 12, no. 8 (1933); English translation by Aage Petersen in R. S. Cohen and J. J. Stachel, *Selected Papers of Léon Rosenfeld* (Boston, 1979), 357–400.

[129] For authoritative treatments of the Bohr-Rosenfeld paper, see Olivier Darrigol, "Cohérence et complétude de la mécanique quantique: L'exemple de 'Bohr-Rosenfeld,'" *Revue d'histoire des sciences* 44, no. 2 (1991): 137–79; Jørgen Kalckar, introduction to Niels Bohr, *Collected Works*, vol. 7 (Amsterdam, 1996), 3–40; Theodore R. Talbot, "Bohr and Rosenfeld's Foundations for Quantum Electrodynamics" (PhD diss., Columbia Univ., 1978).

[130] Léon Rosenfeld, "On Quantum Electrodynamics," in *Niels Bohr and the Development of Physics*, ed. Wolfgang Pauli (New York, 1955), 70–95; Rosenfeld, "Niels Bohr in the Thirties," in *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues*, ed. S. Rozental (Amsterdam, 1967), 125–7; Jørgen Kalckar, "Measurability Problems in the Quantum Theory of Fields," in *Foundations of Quantum Mechanics* (Proceedings of the International School of Physics Enrico Fermi), ed. B. d'Espagnat (New York, 1971), 127–69; Max Jammer, *The Philosophy of Quantum Mechanics* (New York, 1974), 142–5; Arthur I. Miller, "Measurement Problems in Quantum Field Theory in the 1930's," in *Sixty-Two Years of Uncertainty*, ed. Arthur I. Miller (New York, 1990), 139–52; Abraham Pais, *Niels Bohr's Times, in Physics, Philosophy, and Polity* (Oxford, 1991), 358–64.

[131] Landau to Peierls, 6 Feb. 1930, RPP.

involved. When Peierls visited Leipzig not long afterward, he soon picked up on Heisenberg's drive to cultivate a school in the Sommerfeld mold, noting his ability to farm out topics in productive fashion, impressed at his competitiveness in sports—perhaps the off-putting aspect for the unathletic Landau—and quite conscious that Heisenberg "does all these things not like a professor but like a boy [*mal'chik*]."[132] What eluded the peripatetic Soviet, however, was a sense of where next to focus his energies, despite repeated epistolary prompting from Peierls. With the commencement of his Rockefeller fellowship, Landau at last made his way to Cambridge, hoping for inspiration from Paul Dirac. At Cambridge, however, he was disturbed to find the Englishman deeply immersed in an attempt to formulate a unified theory of particles based on proton-electron annihilation, a theory then hampered by puzzling negative-energy electron states.[133] "Not that he is the only one who seems to me to hope to reach quantum electrodynamics along old pathways," Landau informed Peierls, "but something is amiss with him. I have the very strong feeling that it has to do with serious symptoms of contamination (perhaps better named Einstein Sickness)."[134] The young Soviet clearly entertained no desire to pursue the "dream of philosophers" that Dirac invoked in his search for aesthetic unity in physical theory.

It was Peierls, well versed in observability problems from his student days in Munich, who at last persuaded Landau to join the search for conceptual breakdown in earnest.[135] "With your light quantum clarifications I believe I am now fully in agreement," Landau informed Peierls at summer's end. "I would only formulate them somewhat differently." Actually reaching accord on these formulations would occupy the better part of six months once their colleagues got wind of the project, but Landau was finally convinced that publication was necessary to correct previous mistaken notions concerning the interaction of photons with the electromagnetic field. As he told Peierls, "These matters also seem to me independent in a certain sense from the tragedies of contemporary quantum electrodynamics, and must for example come out of any future electrodynamics where $m \to \infty$."[136]

It was upon his return to Copenhagen for a lengthier stay beginning in September 1930 that Landau determined to make his way to center stage at the Institut for Teoretisk Fysik, immersing himself in study of the Danish language and quickly picking up the equivalents of "inferior" and "philistine," the better to express his contempt for the bourgeois establishment.[137] Bohr's young assistant, Hendrik Casimir, concisely conveyed the impression made by the visiting Soviet: "Landau, a young Russian, is terribly clever. Enormously disagreeable but not unamusing. Bohr is also very fond of him."[138] In this last, Landau was fortunate as he had shown very little regard for Bohr's own current research concerns, while Bohr in turn was not much taken with

[132] Peierls to Genia, 26 July 1931, RPP. In the German-speaking world, *Knabenphysik* had become a common designation for the early days of quantum mechanics in Göttingen.

[133] Helge Kragh, *Dirac: A Scientific Biography* (Cambridge, UK, 1990), 94–105; Schweber, *QED* (cit. n. 108), 56–69.

[134] Landau to Peierls, 9 Aug. 1930, RPP.

[135] E.g., Peierls gave a report on "Neuere Arbeiten zur physikalischen Deutung der Quantenmechanik (prinzipielle Unschärfe der Beobachtung)" on February 3, 1928, as cited in Pauli, *Wissenschaftlicher Briefwechsel* (cit. n. 122), 11n9.

[136] Landau to Peierls, 9 Aug. 1930, RPP.

[137] Casimir, *Haphazard Reality* (cit. n. 102), 105. On Landau's aggressive behavior, see also the interview of Christian Møller by Thomas S. Kuhn, 29 July 1963, AHQP.

[138] Casimir to S. Goudsmit, 27 Sept. 1930, Goudsmit Correspondence, AHQP.

"narrow" applications of quantum mechanics such as diamagnetism (where Landau made his first breakthrough). Upon arriving in Copenhagen Landau immediately asked, "What's Bohr doing? What's Bohr doing?" When told that Bohr was discussing examples of complementarity, Landau promptly exclaimed, "Oh, yes, but that's not physics."[139] What did count as physics for both of them could be found in the formal content of the uncertainty relations. The interpretation of their proper application to the electromagnetic field struck both men as central to determining how much of the quantum mechanical formalism would survive in a future theory incorporating relativistic processes.

Landau aimed to call the entire formalism into question, to the consternation of Bohr and the edifying distraction of his peers. Casimir, a delighted witness to many of these protracted exchanges, exclaimed that "a Bohr-Landau discussion on any subject in or outside physics is always a splendid spectacle."[140]

According to Bohr and Rosenfeld, it turned out that Landau and Peierls had neglected to explain the connection between their uncertainty relations and the commutation relations for the electromagnetic fields. And quite apart from the woes of quantum theory, they had not taken into account the statistical fluctuations in a classical field measured over finite distances. Bohr and Rosenfeld went back and carefully defined all the field quantities in operational terms, demonstrating that the errors in momentum measurement could be just balanced out by a combination of extended test charges and field fluctuations. They concluded that the classical field of the macroscopic test body does not impair the microscopic field measurements. It is, instead, "an essential feature of the ultimate adaptation of quantum field theory to the measurability problem."

The irony is that the close attention to *Gedanken* procedures that enabled Bohr and Rosenfeld to make the field-theoretic concepts operationally meaningful also took them further from experimentally realizable configurations. Their extended test charges occupied the entire measurement cell, and consistency notwithstanding, there was little in their formulation to guide the working experimentalist. Years later, Peierls, while deferring to the fundamental correctness of the Bohr-Rosenfeld results, still claimed that when one looked closely at the demands they were making upon the actual experimental configuration, "the resulting operations look quite unlike any kind of measurement that an experimentalist would design."[141] It is safe to say that Landau shared this view and had every reason to foster relations with experimentalists premised on the utility of theoretical insights for realizable experiments in the laboratory.

It is certainly not the case that Landau was ever inclined to repudiate his and Peierls's work after the appearance of the Bohr-Rosenfeld paper. On the contrary, at an international conference in Kiev nearly three decades later, he still alluded to it proudly as a groundbreaking work.[142] When Bohr visited the Soviet Union for the

[139] Casimir interview (cit. n. 126). The characterization of Bohr's concerns at the time is Casimir's, while Landau's response is recalled by Rosenfeld, who sat in on the interview.

[140] Casimir to Ehrenfest, 15 Dec. 1930, ESC.

[141] R. E. Peierls, "Field Theory Since Maxwell," in C. Domb, ed., *Clerk Maxwell and Modern Science* (London, 1963), 36.

[142] L. D. Landau, "On Analytical Properties of Vertex Parts in Quantum Field Theory," *Ninth International Annual Conference on High Energy Physics*, vol. 2 (Moscow, 1960), 97. Cf. Landau, "Fundamental Problems," *Collected Papers of L. D. Landau* (New York, 1965), 801.

last time in 1961, Landau modestly confessed before flocks of admiring students that it was Bohr himself who had taught him to understand the uncertainty principle in quantum mechanics.[143] Whatever the truth of that assessment, there is no evidence Landau ever acknowledged the Bohr-Rosenfeld paper publicly, and even his students were occasionally puzzled to note that he never broached the subject of measurement.[144] Throughout his career he consistently maintained a position first articulated in the official report he gave on his travels in June 1931:

> My scientific work developed in several different directions during this period. A crucial problem of modern theoretical physics occupied an essential locus in this . . . the problem of the unification into a single whole of the two most general modern theories: the principle of relativity and quantum theory. This problem has led to immense complications, rendering its precise resolution an issue only for the extremely distant future.[145]

The pragmatic solutions to the dilemmas of quantum electrodynamics achieved in the 1940s never entirely satisfied Landau, for whom the "extremely distant future" turned out to extend well beyond his own career. There would be no revolution.

MATTERS OF STATE, AND SOME POPULAR INTEREST

Among Soviet theoretical physicists in the interwar period, it was the Dane Niels Bohr who served as the most important reference point for these cross-cultural encounters, and his influence reverberated more broadly in Soviet intellectual culture. As with Ehrenfest, it was of no small importance that Bohr did not hail from France, Germany, or Great Britain as that made it a good deal more difficult for philosopher-courtiers to project Soviet political rivalries on to Denmark's most famous scientist. Although he was at the center of numerous Soviet disputes about the Copenhagen interpretation of quantum mechanics, Bohr was exceedingly difficult to cast as a puppet of larger foreign powers, and that lent a unique value to his presence in Russia.[146] I would suggest that the ways in which he was seen to be seeking understanding of the Soviet Union in the context of interwar European politics were linked with Soviet theoretical physicists' attempts to stabilize their own roles within the Soviet system of science.

When a world-renowned physicist such as Bohr joined "the best minds of humanity" by bearing witness to the Soviet experiment, it was more than a mere propaganda coup for party-state officials.[147] It was also an important contribution to the efforts of Soviet theoretical physicists to advance concrete research agendas, both within a heterogeneous physics community and within this broader Soviet discourse of civilization and backwardness.

When Tamm first met Bohr at a conference (in Leiden), the Dane proved to be an enigmatic exception to the younger Russian's growing peer confidence; this per-

[143] Livanova, *Landau* (cit. n. 42), 31.

[144] A student who knew Landau from 1951 onward testifies that he never heard his teacher discuss the foundations of quantum mechanics. I. E. Dzialoshinskii, "Landau glazami uchenika," in Khalatnikov, *Vospominaniia o L. D. Landau* (cit. n. 27), 119–20.

[145] V. I. Frenkel' and P. Josephson, "Sovetskie fiziki–stipendiaty Rokfellerovskogo fonda," *UFN* 160, no. 11 (1911): 103-43, on 131.

[146] On Soviet philosophical disputes about quantum mechanics, see Loren R. Graham, *Science, Philosophy, and Human Behavior in the Soviet Union* (New York, 1987).

[147] A. F. Joffe, "K priezdu Nil'sa Bora v Sovetskii Soiuz," *Izvestiia*, 4 May 1934, 6.

ception of Bohr was almost as visceral as it was intellectual. Tamm confided to his wife that Bohr "makes a completely overwhelming impression—the heavy face, as if hewn from a rock, the deep-set, I would say, not entirely normal eyes." For the Soviet theorist still looking to make his mark, and not much given to philosophical reflection about modern physics at a moment when Soviet philosophers would happily have celebrated Bohr's own predilections in a Soviet physicist, Bohr provided a sometimes problematic but also invaluable mediating role for Tamm and his colleagues at home. In Tamm's eyes, the earnest Bohr fostered the impression that "it's not easy bearing the burden of genius."[148] This was not the institution builder described by Finn Aaserud,[149] but rather the sage presiding over the Copenhagen "Mecca" of theoretical physics (Tamm's term).[150] Quite apart from the actual intellectual stimulus that Bohr's obsession with the foundational concepts of physics provided to his colleagues, I would suggest that the public image of the barely comprehensible genius provided a useful foil for Soviet theorists bent on portraying themselves as tough-minded pragmatists by comparison. This is despite the fact that their "pragmatism" made sense only if construed in fairly narrow disciplinary terms and that in the near term their pragmatism was making little direct contribution to the economic aspects of socialist construction.

Bohr had been a foreign member of the Soviet Academy of Sciences since 1924 and the object of repeated invitations to visit.[151] In 1934, Bohr finally made his first trip to the Soviet Union in the company of his assistant, Léon Rosenfeld. Joffe provided fulsome greetings in the pages of *Izvestiia*, proclaiming the arrival of the top theorist a joyful occasion for Bohr to witness the enthusiasm and heroic efforts of the proletariat to build a happy life on earth.[152] At his initial destination, in Leningrad, Bohr witnessed the May Day celebrations and pronounced himself impressed by the martial splendor. As tanks rumbled past, a local reporter wrote in rapture that "the orchestra can no longer be heard. The square is filled with the music of metal in motion."[153] Such panegyrics to technological accomplishment were nonetheless rather incidental to the Danish physicist, who proceeded to Moscow and then on to Kharkov, where he was slated to attend a conference hosted by Landau.[154] While the substantive lure of the visit for Bohr may have been the chance to argue with the combative Landau, the urban milieu of the Kharkov physics institute made no less an impression on foreign physicists.[155]

The British science journalist J. G. Crowther reported that the public aspects of the conference were a success, including a lecture by Bohr on causality, which was

[148] Tamm to his wife, 11 March 1928, *Priroda,* July 1995, no. 7:158.

[149] Aaserud, *Redirecting Science* (cit. n. 122).

[150] I. E. Tamm to N. V. Tamm, 12 June 1931, papers held by the Tamm Family, Moscow.

[151] P. P. Lazarev et al., "Zapiska ob uchenykh trudakh Nil'sa Bora (Niels Bohr)," *Izvestiia rossiisskoi akademii nauk* (1924): 458–9.

[152] Joffe, "K priezdu Nil'sa Bora" (cit. n. 147).

[153] *Leningradskaia Pravda*, 4 May 1934, quoted in V. Ia. Frenkel, "Nil's Bor i sovetskie fiziki," in *Nil's Bor i nauka XX veka: Sbornik nauchnykh trudov* (Kiev, 1988), 20–5, on 21.

[154] On Bohr's visit and the impression it made on his hosts, see also J. G. Crowther, *Soviet Science* (London, 1936), 112–25. Further sources may be found in the exhaustive study by Peder J. E. Kragh, "Niels Bohr and the Soviet Union between the Two World Wars: Resources at the Niels Bohr Archive" (master's thesis, Copenhagen Univ., 2003).

[155] In an impromptu *Baedeker* guide for the trip, the orthographically challenged George Gamow noted, "If you are not expetially interested in facturies nothing to beee seen in this place except of *Landau*." Gamow to the Bohrs, 14 April 1934, Niels Bohr Archive, Copenhagen.

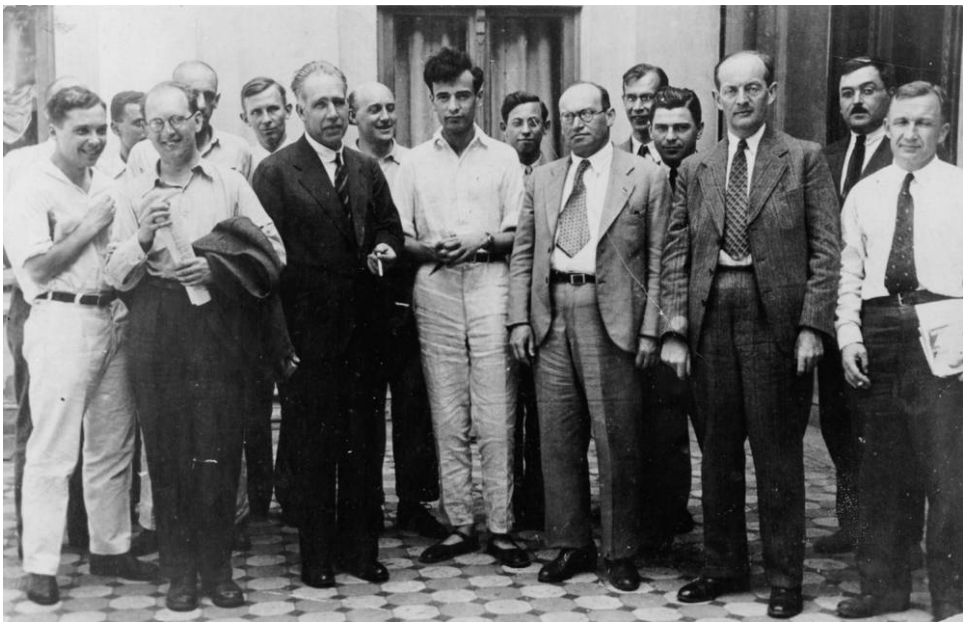

Figure 1. *Conference on theoretical physics in Kharkov, May 1934. From left to right: D. D. Ivanenko, L. Tisza (partially obscured), L. Rosenfeld, (figure obscured), Iu. B. Rumer, N. Bohr, J. G. Crowther, L. D. Landau, M. Plesset, Ia. I. Frenkel, I. Waller, E. J. Williams, W. Gordon, V. A. Fock, I. E. Tamm. Landau and Ivanenko purposely wore more casual attire to establish their disdain for conventional social mores and to emphasize generational distinctions. (Photo courtesy of the Niels Bohr Archive, Copenhagen.)*

attended by top functionaries such as the Ukrainian commissar of enlightenment (a chemist by training). This "was not regarded merely as a meeting of persons engaged in researches of no interest to others," claimed Crowther. "The arrangements showed that theoretical physics, however recondite, was considered to be a matter of State, and some popular interest."[156] One of the participants obligingly reported on the proceedings in the pages of *Nature*. Although he dwelt primarily on the technical substance of the conference, he also remarked on the "youthful enthusiasm" of the staff and expressed admiration for the Dzerzhinskii School for Orphans (run by the notorious State Political Directorate, or GPU), which (among other things) provided its charges with vocational training for the manufacture of Leica lenses.[157]

Bohr's experience also points to the need to incorporate the Soviet side of these interactions. Tamm, a passionate socialist, spent much time cloistered with Bohr during the 1934 journey, and they by no means limited their conversations to physics. For Tamm, this encounter marked a retreat from the factionalist instincts honed in the politics of his youth, since he had been inclined to follow the Soviet government's position that all the states of Europe were effectively arrayed in opposition to Soviet

[156] Crowther, *Soviet Science*, 119 (cit. n. 154).

[157] "International Congress on Theoretical Physics at Kharkov," *Nature* 134 (21 July 1934): 109–10. The author may well have been Crowther, but judging from the technical content, Welsh physicist E. J. Williams seems the more likely candidate.

Communism, even if some of them proclaimed socialist platforms. But as Tamm reported to Paul Dirac in his slightly stilted English, "I mentioned to Bohr occasionally the discussion we have had with you last autumn about Bohr's theory of the absence of any intellectual and psychological difference between nations; he argued on the point for about an hour and, as you did rightly foretell, totally convinced me."[158] Bohr was arguing for alliances in opposition to Fascism, but it is interesting that the Danish scientist had to work so hard to persuade his Soviet counterpart to assess the political subjects of the Soviet experiment in universal terms. If it was the case that "under the unifying bonds of science [diverse European scholars] found themselves in complete harmony," there were those among Tamm's contemporaries who realized that this commonplace could not be taken for granted.[159]

Bohr visited the Soviet Union again in 1937 and in 1961. This is not the occasion to delve into the personal relations between Landau and Bohr, but I would argue that their considerable mutual affection and respect had very little to do with compatible temperaments or intellectual styles and a great deal to do with Bohr's peculiar status as an approving foreigner. For the young Landau, these encounters were crucial in the formation of his personal and professional identity as a Soviet. For public purposes, Landau adopted Bohr (and to a lesser extent, Wolfgang Pauli) as his teachers but not from any great urge to emulate Bohr's concern for foundational questions. Landau wanted instead to disassociate himself conspicuously from his onetime Soviet mentors, especially Frenkel. Conveniently, neither the Dane nor the Swiss stemmed from one of the great powers of European politics in the 1930s, which meant that Landau could identify himself as a disciple without readily falling prey to increasingly patriotic talk at home about "national" schools in the sciences. And at the same time, he could work assiduously to build his own distinctive school, very much in keeping with Soviet expectations of cultural autarky.[160] Landau's close friend Iurii Rumer painted the Soviets as "inheritors of the culture of the dying classes [i.e., of Western capitalism]," obliged to proclaim the "tremendous revolutionary teaching" of twentieth-century physics.[161] As the prospect of conceptual revolution receded in the 1930s, however, "purely practical" theory came to thrive in the nascent Landau school. Schools provided the social mechanism to sustain "local" disciplinary affinities with theorists like Bohr in a heterogeneous global physics community. At the same time they fostered specific collective relations among Soviet compatriots that kept theory credible as Landau retreated from Bohr-style crisis talk to develop a training regimen more in keeping with the Sommerfeld-style physics of problems.

Bohr's own desire to explore the philosophical implications of modern physics well beyond the realm of quantum mechanics also had domestic utility for Landau, who had social as well as epistemological reasons for maintaining firmer disciplinary boundaries. That is why he found it expedient at a moment of institutional crisis in 1935 to include a brief attack on Bohr in a polemic for *Izvestiia*, chiding him

[158] Tamm to Dirac, 13 May 1934 (with minor spelling corrections), Dirac Scientific Correspondence, Florida State University, Tallahassee, or Churchill College, Cambridge, UK. Tamm repeated the anecdote in his obituary of Bohr, "Nil's Bor—Velikii fizik XX veka," 80 *UFN* (1963): 191–5.

[159] F. A. Paneth, "The Mendeléeff Centenary and Scientific Progress in the U.S.S.R.," *Nature* 134 (24 Nov. 1934): 799–801.

[160] On Landau's pedagogical efforts, see Karl Hall, "'Think Less about Foundations': A Short Course on the *Course of Theoretical Physics* of Landau and Lifshitz," in *Training Scientists, Crafting Science*, ed. David Kaiser (Cambridge, Mass., 2005), 253–86.

[161] Iu. Rumer, "Teoriia otnositel'nosti," *Izvestiia*, 22 Oct. 1935, 3.

for the August 1932 lecture on "Light and Life," in which the Danish physicist had attempted to extend some of the notions of complementarity to the life sciences.[162] Landau expressed amazement that a man who understood physics so profoundly could say such foolish things about biology. But again, this wasn't so much a matter of Landau's engaging Bohr in a scientific debate as it was an attempt to hold up a particular image of a Western scientist whose stature was already acknowledged by Landau's domestic critics and thus could not easily be dismissed by them. Yet it was also a useful means of establishing differences that pointed to peculiarly Soviet disciplinary virtues without acceding to know-nothing demands for a sui generis "proletarian science." When philosopher-courtiers later started faulting Landau, Frenkel, Fock, and Tamm as disciples of the "idealist" Bohr, they failed to realize that these missionaries had no need of the Copenhagen theology as it was popularly understood.[163] Fervency and technique would suffice—only Fock would later develop a taste for exegesis, and Bohr found him heterodox.[164]

CONCLUSION

After Landau moved to Kharkov in 1932 he began to concentrate increasingly on the physics of the solid state and on the creation of a pedagogical framework for a distinctive school. What I call the "principled phenomenology" that became the basis for Landau's research program with students achieved its coherence within the school framework as a technique-oriented dialectic of global principles versus local models. The famous Theoretical Minimum quickly became the standard for membership in the Landau school, with early students such as A. S. Kompaneets, E. M. Lifschitz, A. I. Akhiezer, I. Ia. Pomeranchuk, and I. M. Khalatnikov among the successful aspirants.

With his 1941 theory of superfluid helium—neither a fundamental microscopic theory nor an ad hoc model—Landau sealed his reputation among his peers. As his first students began to come into their own as professional theorists, he was finally selected to the Academy of Sciences in 1946, with his growing "scientific school" as an important factor in his favor. As successive volumes of the *Course of Theoretical Physics* made their way into print at home and abroad, textbooks also became approved markers of school building. By the time Landau was awarded the Nobel Prize in 1962, kruzhok sensibilities had little to do with the officially sanctioned role of the school in Soviet science. The Landau school was most concretely institutionalized in 1965 in a theory institute founded outside Moscow by his students. Though small by Soviet standards, it remained one of the most productive institutes in the academy on a per capita basis until the demise of the Soviet Union.

Both Landau and his associates recognized some of the latent tensions between the school as a marker of unofficial science (i.e., as kruzhok legacy) and as an epitome of official science. Through the next two generations the moral qualities of this social cohort remained very much alive to its members, who saw in scientific schools "one

[162] L. D. Landau, "Burzhuaziia i sovremennaia fizika," *Izvestiia*, 23 Nov. 1935; Niels Bohr, "Light and Life," *Nature* 131 (1933): 421–3, 457–9.

[163] J. L. Heilbron, "The Earliest Missionaries of the Copenhagen Spirit," *Rev. Hist. Sci.* 38, nos. 3–4 (1985): 195–230.

[164] Graham, *Science* (cit. n. 146), 337–43.

of the highest humanistic achievements of civilization."[165] In the 1990s, after many key members of the Landau school had dispersed to take positions outside Russia, Khalatnikov depicted the school as "something typical of this country. Indeed, this is a unique phenomenon which cannot be found anywhere else in the world."[166] In his eyes, its peculiar esprit de corps constituted a tradition foreign to the individualism of physicists in the West.

What for Khalatnikov was a historically peculiar set of professional virtues, however, was also less obviously a product of deep ambivalence toward Soviet power among Landau and his students in the years after his arrest in 1938. The experience of the young A. I. Larkin (1932–2005) is revealing in this regard. His youthful determination in the early post-Stalin era to "be obligated to no one, belong to nothing" was driven by disgust at the pervasive role of party institutions in daily life, and the tremendous prestige and comparative autonomy of Soviet physics in the 1950s made it an appealing career outlet. Larkin was somewhat shaken upon witnessing his teacher A. B. Migdal's readiness to shrug off a French colleague's membership in the Communist Party. (Migdal had taken his doctorate with Landau and was perhaps the most successful disciple in fostering a school of his own.) Neither entirely facetious nor fully in earnest, Migdal explained to Larkin that in this world one had to have friends, and the French Communists were very friendly people. As Larkin relates it, he thenceforth resolved that his party would be the Migdal school and the Landau school.[167] The resilience of this kind of self-identification was both intellectual and political. The social dynamic that made the Landau school a productive institution for criticizing, testing, and extending contemporary theoretical practices was in no small part the outcome of similar longstanding intelligentsia dilemmas of collective identity.

[165] V. F. Klepikov, "Shkola fiziki I zhizni," *A. i. Akhiezer: Ocherki i vospominaniia*, ed. V. G. Bar'iakhtar (Kharkiv, 2003), 177.

[166] I. M. Khalatnikov, "Our History," Landau Institute for Theoretical Physics (1966), http://www.itp.ac.ru.

[167] A. I. Larkin, "A. B. Migdal v moei zhizni," *Vospominaniia ob akademike A. B. Migdale,* ed. N. O. Agasian (Moscow, 2003), 40–8.