PUBLIC HEALTH IN THE SOVIET PERIPHERY:
THE CASE OF TICK-BORNE ENCEPHALITIS (TBE),
1932-1937

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Executive Summary

In the early 1930s, as Stalin’s transformation of the Soviet economy entered its stride, laborers in the Soviet Far East taiga were felled by a mysterious disease that initially defied treatment. After five years of struggle, an expedition of Moscow scientists was deployed and quickly provided a resolution. The accepted historical narrative of the events surrounding TBE does not match the documentary record. This paper offers new analysis and explores some of the distinctive features of public health in the Soviet periphery in the 1930s.

Three moments in the initial investigations of the emerging infection are highlighted. The paper concludes that physicians and scientists based in the Soviet regions were hampered by two significant regional handicaps: a dearth of specialized medical expertise, and poor institutions and institutional relationships. A cultural gap and considerable difference in experience divided public health activity in the Soviet regions from biomedical research that was conducted in the center.
Introduction

In 1932, as the Soviet Union’s first Five-Year Plan for industrial and agricultural development entered its stride, men and women laboring in the taiga forest lands of the Soviet Far East were struck down by a mysterious disease. Some of its early symptoms resembled influenza, while others indicated a more serious infection of the brain. The illness defied treatment, infected people in a way that doctors were unable to understand, and left about a third of those it infected dead. Many of those robust workers who survived the infection were left with paralysis and brain damage when they recovered.

Historians of these episodes recall that the disease threatened to derail efforts to master this far-flung region and utilize its natural resources in the development of the rest of the country. A deadly disease also had the potential to wreck Soviet defense of its Pacific territory, at an uncertain moment when Japan was maneuvering aggressively just over the Soviet border in northeast China. A plague with such deadly effects could make it even more difficult to fortify the perennially sparse population in the Russian Far East, which had made the territory appear ripe for the taking to other powers in the region, and left it dangling even more precariously in light of both Japanese and Chinese attempts at power-grabbing in Manchuria in the 1930s.1

After physicians in the Far East had struggled with the disease – apparently a form of encephalitis – for five years to no effect, a team of scientists from Moscow and Leningrad was called in by the Ministry of Health. In the single summer of 1937, as historians have recounted, under the direction of a brilliant microbiologist named Lev Zilber, these investigators made

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remarkable headway in understanding the frightening disease. And despite Zilber’s sudden arrest after the members of the expedition returned to Moscow that August, many of the same investigators made the seven-thousand-kilometer journey again over the following four summers, working to establish all of the basic reference points about what they called “spring-summer tick-borne encephalitis” and its causal agent, and making important strides toward treating and preventing the disease. In the process, these biomedical scientists contributed to the development of both the fledgling field of virology and the Soviet Union itself, by helping to overcome the disease that had threatened to undo the most vital work being done to strengthen and defend the new socialist state.

This is a classic narrative of hardship and heroism, and one that allowed scientists to shed their lab coats and participate in a canonical experience in Soviet public life. Elements of several Soviet heroic tropes are at play here: Zilber’s group could draw upon the myth of the unexplored frontier and Soviet devotion to the rugged explorer marching off to claim distant territories for the young socialist state. They could hope to share in the adoration of hardy, youthful comrades who built and strengthened new Soviet cities and regions, including specifically the Far East. With their quick resolution of the key questions about the mysterious taiga disease, they could even lay claim to Soviet society’s respect and emulation of the shock worker, who labored intensely and completed almost superhuman feats in a short burst of productive activity.² Telling the story in this way also lent their contributions a significance that was perhaps more

comprehensible and meaningful to the broad lay population than a virological expedition might otherwise have automatically had.

Unfortunately it is not a narrative that is supported completely by the evidence found in archives and libraries of the Far East. While it certainly felled a great proportion of its victims, the mysterious disease struck far fewer people in the Far East at the time than diseases such as typhus, diphtheria and dysentery, scarlet fever, and measles. At health conferences and in reports throughout the decade when the mysterious “taiga encephalitis” emerged, Far Eastern regional health authorities discussed all of these other diseases more frequently than the newly observed illness. The public health and medical specialists who had the training and expertise to comment with some insight on the situation would have agreed that poverty and poor sanitation, poor living conditions and nutrition, underdeveloped medical care and especially poor health care for infants and children consistently threatened more lives and sacrificed more labor in the Far East than the mysterious fever in the taiga.

At the same time, historical accounts that have focused solely or with much greater emphasis on the investigative work that occurred after Zilber’s Ministry-led team arrived have missed an opportunity to learn from the difficulties and successive attempts by physicians on the periphery of Stalin’s Soviet Union to understand and control a newly emerging disease. This paper considers some of the archival evidence and published literature that historians have tended to ignore, in order to reflect more truthfully the earliest steps in investigating this disease, and to place the discovery moment in a broader contemporary context of virology and encephalitis research.

Three episodes in the earliest prewar period of Tick-Borne Encephalitis (TBE) investigation, characterized by an exclusively regional response to the mysterious disease, hold
particular interest: the analysis of the most prominent and well-published Dalkrai-based scientist, Aleksandr G. Panov, of the phenomena he observed; a symposium called by the Dalkrai public health authorities in 1936 to organize a more effective response to the outbreaks; and the published results of the first laboratory studies of the virus presumed responsible for the taiga disease. The standard historical narrative about the discovery of TBE holds that regional doctors were simply ineffectual in managing the disease. The evidence does not contradict this view, although it is interesting to note that before 1937 some Far East physicians were identifying the basics that Narkomzdrav investigators would later quickly locate themselves and follow to a fuller and more successful analysis.

At the same time, this evidence—and a more thorough analysis of it than historians of this problem have offered to date—paints a more vivid picture of the conditions and constraints within which health practitioners and officials did their work in the peripheral regions of the Soviet Union in the 1930s. Specifically, physicians and scientists based in the Soviet Far East were hampered by two significant regional handicaps: there was a dearth of specialized medical and scientific expertise, and institutions—whether oriented toward research, clinical care, administration, or education—were too poor, and the relationships between them too weak, to support quality research and investigation.

**Early Studies of TBE**

Retrospective studies have determined that tick-borne encephalitis was most likely observed by Russian physicians in scattered and isolated cases in the late nineteenth and early twentieth centuries, but the start of a conscious biomedical response to TBE came with outbreaks
of the disease in the Soviet Far East in the early 1930s.\(^3\) Before the emergence of medical virology in the early decades of the twentieth century, people simply lacked the vocabulary and the conceptual categories (and the microbiological understanding of viruses) to label and describe TBE in the way that has been possible in the twentieth century. Physicians and authorities started to notice what appeared to be a new disease in 1932 in the Soviet Far East territory (\textit{Dal’nevostochnyi krai}, also referred to as Dalkrai, or DVK), especially in the areas surrounding Khabarovsk and Vladivostok.\(^4\)

The years between 1932 and 1936 represent a threshold period when observers in the Soviet Far East were gradually becoming aware that they faced not simply a familiar disease with strange complications, but something unfamiliar and perhaps previously unknown to local clinicians, which constituted not a few anomalous cases but a more serious problem. By the end of this earliest period, however, local physicians and public health practitioners were unable to effectively identify or treat the disease, and regional laboratory scientists likewise attempted but were unsuccessful at isolating and characterizing a causal agent. In the earliest reports and observations of the disease, there tended to be relatively little attention paid to the specific sites where the outbreaks had taken place.

Observers were reluctant to consider any route of transmission other than human contact, another indication of the dearth of epidemiological expertise of the regional health practitioners called upon to manage the disease. Later, once the epidemiology of TBE was worked out in

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\(^3\) Contemporary Vladivostok virologist G. N. Leonova notes that Panov and his colleague N. L. Dankovskii completed a detailed retrospective epidemiological study of TBE all the way back to 1920, but bibliographic citations in the literature have made it impossible so far to determine when and where it was published. See G. N. Leonova, \textit{Kleshchevoy entsfali v Primorskom krai: Virusologicheskie i ekologo-epidemiologicheskie aspekty} (Vladivostok, 1997), 6; A. G. Panov, \textit{Sezonnye letnie entsfali} (Vladivostok, 1940), 75, 159-174.

\(^4\) In 1938 DVK was divided in two, resulting in Primorski krai in the south, with the port city of Vladivostok as its administrative center, and Khabarovsk krai in the north, which retained Khabarovsk as its capital. Investigation of TBE involved both territories, since the disease had been observed in parts of each.
more detail, the sites of infection (ochag) where outbreaks of the disease emerged—as well as the ecological relationships between tick vector, reservoirs in other small animals, and forest habitat—became central to the research and public health management of the disease.

Figure 1. A contemporary map of the Far East’s Priamur and Primorye regions, territories that encompass Khabarovskii and Primorskii krais, where TBE was first observed.5

In the mid-1930s, the most important outbreak sites were concentrated in the north, at the Obor forestry complex (Oborskiy lespromkhоз) about forty miles southeast of Khabarovsk; and in the south, at an Ussuriisk forest preserve (Suputinskiy zapovednik), an on-site research facility in the preserve (the Alpine-Taiga Research Station of the Far Eastern branch of the Soviet Academy of Sciences), and in the hilly, forested areas to the west of the Primorskii krai town of Suchan (today called Partizansk).6

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6 See the map in Figure 2. Obor, in the north, is located about halfway between Khabarovsk and Vyazemsky, slightly to the east of the north-south railroad artery. Both of the southern sites are about 20 miles north of the southern shores of Primorskii krai, where the Sea of Japan approaches the land in a series of smaller bays. Both of these sites were
The analysis offered by Vladivostok-based navy neurologist A. G. Panov between 1934 and 1936 illustrates the narrow specialization of many physicians in Dalkrai. Panov had only recently arrived in the Far East after completing his training in Moscow. As a young physician with little experience, fully based in Dalkrai and prominent in the regional public health structure, he exemplifies the combination of personnel and resources deployed by Dalkrai in response to the taiga disease. Panov was just beginning his assignment as acting director of the neurological department at the Soviet Pacific Fleet’s Naval Military Hospital (voenno-morskoi gospital’) in Vladivostok when he was called upon to observe the new infection.7

Panov analyzed what he was seeing fairly exclusively through the framework of the clinician, with his best work concentrated on the description of individual patients’ symptoms. Panov apparently had little or no expertise in epidemiology, demonstrating only very rudimentary ways of sleuthing out the nature of the outbreak at large. Nor did he appear to have any knowledge of the incipient field of microbiology. Although he later became something of an expert on “seasonal encephalitides,”8 early in the TBE investigations Panov was a proponent of the theory – quickly disproven by the Narkomzdrav team–that the disease was transmitted via the respiratory route rather than by the bite of an insect.9 Panov’s report of the spring 1936 investigation of an outbreak in the Soviet Army’s 24th Railroad Construction Regiment, stationed on a forested ridge north of Arseniev, illustrates further his concentration on the clinical picture. Yet Panov’s single epidemiological observation was prescient: he noted the

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8 See A. G. Panov, Sezonnye letnie entsefalitides (Vladivostok, 1940), and A. G. Panov, Kleshchevoy entsfelit (Leningrad, 1956).
disease’s stark seasonal patterns, a clue that Narkomzdrav investigators grasped early and that formed the basis of their broader and more successful analysis in 1937.10

Panov and his colleagues observed a disease that struck healthy workers laboring in or near the dense taiga—it was noticed in particular among workers cutting timber for the forestry industry. One such patient was P. K., whose infection was established retrospectively after he survived one of the very first modern outbreaks of Far East tick-borne encephalitis and was subsequently examined by Panov in 1938. Forty-four-year-old P. K. fell ill in July 1924, with sharp, strong headaches, a fever, difficulty hearing, and general lethargy on the first day. On the second, paralysis appeared in his upper limbs and neck. His fever lasted for ten days, and he was left with long-lasting muscle atrophy and flaccid paralysis in his neck and lower shoulders, unable to fully support his head, and with very limited movement in his upper limbs, all of which persisted when he saw Dr. Panov nearly 15 years after he first was infected.11

Those living in villages that had been built recently on newly reclaimed taiga land also fell ill, much like a woman identified as M. G., a 39-year-old resident of the village N.-T., who also became ill in the early 1920s, in late August, probably at her place of work. Initially she suffered a very painful headache, nausea, fever and chills, and a general heightened sensitivity. On the second day of her illness, she lost consciousness and persisted in a feverish unconscious state for a full week. When she regained consciousness, her headaches and nausea continued, together with neck pain, sluggishness, and bouts of insomnia. She had no paralysis or palsy.

About two days after her temperature returned to normal, she was able to walk again. By her 1938 consultation with Dr. Panov, M. G. retained serious memory loss and complained of frequent headaches, and the physician observed a pronounced loss of muscle reflexes in her

11 A. G. Panov, Sezonnye letnie entsefalitity, 75-76.
upper body and torso. Panov called M. G.’s case typical of TBE presenting itself in the form of aseptic meningitis.

In 1936, after a special investigation of an outbreak among a mobile railroad construction regiment of the Soviet Army, Dr. Panov described the clinical picture he saw, after first noting the unique timeframe within which cases of the disease appeared. The taiga fever was remarkably seasonal, with all seventeen cases within the 24th Railroad Construction Regiment occurring between late May and late June. As Panov noted in his book, “spring-summer seasonality represents the most stark epidemiological pattern of this disease.”12

But what struck Panov about the clinical picture were the symptoms of meningitis he was seeing. He divided the cases into three groups, according to the seriousness of the illness, finding one group with a light form of the disease, whose symptoms disappeared with recovery and whose disease passed rather quickly. A second group exhibited more a clearly marked “meningitis syndrome,” with more significant hearing loss and confusion, and experienced a much longer recovery period. Panov also noted a third group whose disease was the most serious. These patients exhibited signs of serious injury to the brain, and a high proportion of these cases ended in death.13

The variation in terminology applied to this newly emerging disease also serves as an illustration of how regional health practitioners were receiving and processing what they observed, as well as some of the difficulties they had in analyzing this data. During the earliest modern outbreaks of TBE, in the late 1920s and early 1930s, physicians tended to identify the disease as “toxic influenza” (*toksicheskii gripp*), or more rarely as “atypical poliomyelitis.” Retrospective studies determined that, prior to regional physicians’ notice of disease outbreaks in

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the 1930s, cases of TBE most likely were labeled in a variety of ways, including “cerebrospinal sclerosis” (rasseiannyi skleroz), “neuritis of the brachial plexus” (nevrit plechevogo spleteniiia), adult poliomyelitis, and epidemic or lethargic encephalitis.\(^{14}\)

Many observers in the mid-twentieth century traced the definitive diagnosis as encephalitis, in whatever form, to observations by Dr. Panov in 1935. In this period of just a couple of years, Panov wavered between using the undifferentiated term encephalitis, as in the report on the 24\(^{th}\) regiment, and attributing the disease more precisely to a seasonal form that had been observed and studied in Japan since the early 1920s, and was known in Russian and other languages as “summer encephalitis” (letnii entsefalit).\(^{15}\) From this time forward, the widespread diagnosis as “toxic influenza” at last began to fall into disuse, and by 1936 observers in the Soviet Far East almost universally were calling the disease “summer encephalitis” or “epidemic encephalitis.”

In Russian medical science in the 1930s, “summer encephalitis” was roughly synonymous with what is today recognized as Japanese encephalitis, which observers had determined as early as the 1920s also followed a seasonal pattern with more cases occurring in summer. Because the seasonal epidemiological character of the mysterious disease in the Soviet Far East was slightly different from what had been observed in Japan, some were distinguishing the Soviet form by calling it “spring-summer encephalitis,” but the term “summer encephalitis” also was used.

The fuzziness of the labeling indicates it had not been fully determined whether the disease being observed in the Soviet Far East was the same disease being observed in Japan, or

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\(^{14}\) B. G. Andriukov, et al., “Vklad voennykh vrachei TOF” (2007), 29, probably referring to the retrospective study by Panov and Dankovskii in the late 1930s (see note 1).

\(^{15}\) Panov, Sezonnye letnie entsefalit, 76; L. A. Zil’ber, “Vesennii (vesenne-letnii) endemicheskii kleshchevoi entsefalit,” Arkhiv biologicheskikh nauk 56 (1939) no. 2, 9.
something similar but independent. Some, perhaps most, local Far East observers thought that they were seeing cases of the very recently discovered Japanese encephalitis that were simply presenting in slightly different form. And probably this was the most prudent way to approach the disease, from a clinician’s point of view: after all, why ought a physician to view every new therapeutic challenge as a new and hitherto unknown nosological category of disease?

In 1936 the Dalkrai public health department (zdravotdel), in apparent recognition that they faced a real problem demanding different resources than had been deployed to date, organized investigations and a small conference in Khabarovsk to discuss the disease. The discussion at the conference indicates that there was a distinct lack of consensus about the basic epidemiological characteristics of the disease. It is likely that few of these physicians had very much experience in such epidemiological investigations, or at least with a disease of this specific nature. Apparently none was in a position to play such a leadership role. The conference lacked the authority and leadership that would facilitate an effective investigation and the approach seems especially hindered by a dearth of investigatory and technical expertise. The physicians at the conference relied almost exclusively on clinical observation and had very little if any competent microbiologists or virologists to contribute to any investigations. Even the most prominent voices in the print discussion of TBE in these years were primarily clinicians (Dr. Panov, for instance, and to some degree Dr. Ivan B. Galant in Khabarovsk, who had a similar background, with training in psychology and pathology).


17 G. N. Leonova remarks on the lack of technical knowledge among medical personnel in the Soviet Far East (“otsustvie opredelnnykh znanii v oblasti virusologii”) as a way to explain in particular the unfortunate lack of definitive findings from the first laboratory studies: see Leonova, Kleshchevoi entsfaliha v Primorskom krae, 6.
DVIEM’s director, B. Naishtat, opened the 1936 Khabarovsk investigatory conference with reference to the current tentative consensus, which hypothesized that the disease was spread via “viral respiratory route (вирусосонительный воздушно-капельный путь”).” Contrary to some subsequent recollections that Naishtat adhered to the view that the disease was caused by heat exhaustion or other environmental factors or aspects of working conditions and worker behavior, at the 1936 conference he apparently made no mention of these theories.18

Dr. Tsukerman, the director of the Soviet Army’s main laboratory in the Far East, responding to Naishtat’s introductory words, noted the seasonality of the disease and reminded the group that they ought not to rule out the possibility that an insect was involved in transmission. Indeed, the starkly seasonal nature of the disease was perhaps the feature of the disease that most observers did hit upon first in their analysis of what they saw before them.

Dr. Panov came forward with his belief that the nasopharynx represented the site of viral infection and argued that the disease was transmitted via the respiratory route. At least one other participant in the conference echoed his theory that the disease passed via human contact. A Dr. Korostylev was the one conference participant who did in fact place more emphasis on environmental factors and heat exhaustion as a likely factor. He noted that poor working conditions, subjection to overheating and chilling, poor nutrition and other factors could have contributed to a predisposition toward infection with the disease.19 Korostylev cited the

19 GAKhK, f. R-683, op. 1, d. 176, l. 12. It is worth noting that this line of thinking was very common among Russian physicians in the late nineteenth and early twentieth centuries, in part because of the close relationship between the Russian and German medical professions, and respect for ideas about the potential socio-political role to be played by medicine put forth by reformers such as Rudolf Virchow, and in part due to the very firm association of Russian physicians with a stance of political opposition and social conscience. Basically, Russian doctors, who in the main opposed the political and social status quo in the Tsarist era, were quite receptive to the idea that disease was a social phenomenon, and that, as such, disease constituted an indictment of the existing political system. Improvement of
prevailing notion that the disease was infectious in origin and caused by a “filterable virus.” He also noted that, while no good evidence had yet been found that an insect vector was at work, it still made sense to include entomologists in the future investigations of the disease, since that viral origin had been fairly well established.

**An Appeal for Help**

A handful of the participants in the conference insisted on the need to deploy more systematically the microbiological skills available in the region and to focus future investigations in the primary sites where infections were being registered. Others maintained that they needed to press for more organized study of the clinical and pathological evidence accumulated so far in the outbreaks. A Dr. Koroedov suggested that a special issue of the *Far Eastern Medical Journal* be devoted to the disease, and recommended that DVIEM and the Army laboratory be designated the centers of encephalitis research in the region.

Koroedov and others envisioned a “multidisciplinary team including epidemiologists, neuropathologists, and bacteriological laboratory technicians” that would investigate again the Obor forestry complex in particular, as well as the railroad construction team in the Primorye hills that Panov would investigate directly after the conference. Many of the participants stressed the need to formulate a set of instructions for physicians and other medical personnel who were encountering the infections, indicating that among these specialists there was a sense of urgency to distribute more information as it was gathered and that confusion on the ground was a primary obstacle to treatment and prevention of the infection.

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*health, on the other hand, according to this view, goes hand in hand with political reform and even, ultimately, revolution. See Lisa K. Walker, *Public Health, Hygiene, and the Rise of Preventive Medicine in Late Imperial Russia, 1874-1912* (Ph.D. dissertation, UC Berkeley, 2003), e.g. 14-16, and Chapter 4.*
The regional public health director, Shraiver, summed up the conference in an attempt to formulate a concrete and pragmatic approach to the problem. A team of five physicians was to gather the existing evidence and formulate a report. Dr. Panov was to develop a program for clinical observation of suspected cases and a set of instructions for diagnosis, treatment, and care. The DVIEM was to compile instructions about the registration of patients and the follow-up treatment of those who recovered, and about preventive measures that could help control the disease, with special attention to insects. Drs. Naishtat and Korostylev were to draw up an epidemiological map of the disease, and Naishtat and Panov were to give orders as to how pathological and histological materials from patients were to be handled and gathered. DVIEM, the Vladivostok city hospital, and the Army and Naval labs were to collaborate on a plan and budgetary materials for future research on the disease.

Traditionally, a request was to be forwarded to the Soviet Ministry of Health in Moscow specifically for two investigators to be sent from the Central Institute of Experimental Medicine and the Mechnikov Institute’s “viral” lab to study the outbreaks for a period of one to two months. Finally, three teams were to be formed, each with a neuropathologist, an epidemiologist, and a medical bacteriologist, for investigation at Obor forestry complex (for which DVIEM was to head the team), with the roving army railway construction team (with the OKDVA at the helm), and in vicinity of Suchan (with the Primorye district public health authorities leading the way).20

Although Dalkrai specialists apparently sensed that they faced serious hurdles in facing the challenge of this new disease, they nonetheless made their best efforts to organize their resources in an organized ongoing investigation of the emerging infection. They understood that

20 GAKhK, f. R-683, op. 1, d. 176, l. 13.
a combination of types of expertise was what was needed to understand the disease, and although they knew enough to request help from the Ministry of Health’s researchers in Moscow, they were prepared to begin such investigations using the personnel available to them immediately. They were unfortunately slow and impaired in their response to the disease, because of the important elements they lacked, but they were beginning to formulate on their own the basic outlines of the strategy that was in fact undertaken with much more resolve and confidence in the following year by the Ministry of Health scientists.

In the same year that the krai zdravotdel held its conference, laboratory scientists in Khabarovsk were making attempts to isolate and observe an infective agent for this challenging new disease at the Far East Institute of Epidemiology and Microbiology (DVIEM). The results of their work were published the following year, in 1937, in the premier Soviet journal for virologists, *The Journal of Microbiology, Epidemiology, and Immunology*.21 This publication and the results it communicates have been routinely dismissed by those surveying the history of the study of TBE, and in postwar surveys there is little evidence that commentators even read the report.

However, the article presents a surprising mix of elements. The methodology, techniques, and even the process of trial and error in isolating a viral agent reported by K. A. Grigorovich and her colleague T. Tkacheva22 in fact closely mirror the process described by the first scientists at virtually the same time in their attempts to isolate the virus causing a similar

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22 The historical record has obscured these scientists’ full names and even their initials have been reported variously in subsequent historical accounts. The 1937 article is attributed to “K. A. Grigorovich i Tkacheva […] iz Dal’nevostochnogo instituta epidemiologii i mikrobiologii [of the Far East Institute of Epidemiology and Microbiology].” Tkacheva gains the single initial “T.” in the attribution for the English abstract at the article’s end. Subsequent observers including G. N. Leonova report that both scientists were women.
emerging viral disease in the Western United States. But in their survey of existing knowledge about viral encephalitis, the DVIEM microbiologists showed little awareness of recent, rapid shifts in the field, perhaps due to the difficulty of access to current literature at their far-flung institute.

More than simply a report of the rather inconclusive results of these first attempts to isolate and characterize a causal agent of TBE, the article presents a discussion of how what is being observed in the Far East fits with a range of other types of encephalitis emerging at the time in various sites around the world. Grigorovich and Tkacheva discussed the broad phenomenon of viral encephalitis in much the same terms as researchers in other countries at the time. They used the term “epidemic encephalitis” to describe an overarching category of disease, and they located the starting point for this recent emergence of a series of such infections in the encephalitis lethargica reported first by von Economo and subsequently in epidemic form in many places in Europe and North America. They echoed an idea that von Economo’s encephalitis was a “type A” within this group, but that the more recently observed summer encephalitis in Japan and the very similar encephalitis that had struck in Illinois and St. Louis in the United States were both included in a “type B.” Like Panov, Grigorovich and Tkacheva hypothesized that they were dealing with cases of summer encephalitis that were more closely related to the Japanese form of the disease than anything else.

Lev Kiselev, in his 2004 Zilber biography, refers to the discussions of encephalitis during this time period as “full of legends and fantasies, born of the characteristics of the first [of such

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diseases] described: encephalitis lethargica (von Economo’s encephalitis).”\textsuperscript{25} It is true that, in hindsight, some of the observations and connections made in the early decades of the twentieth century look at best mistaken to us today, but the key to utilizing the historical material represented by such discussions is of course looking at the historical context into which they fit. Kiselev’s commentary here indicates that he made little effort to place discussions, whether in Grigorovich and Tkacheva’s article or elsewhere in the record of early speculations about the nature of TBE, into historical context.

A look at the Anglo-American literature of the time shows that the terms and an overall understanding of the landscape of viral encephalitis used by the two DVIEM laboratory researchers were echoed by their peers elsewhere. In the same year that Grigorovich and Tkacheva’s piece appeared, the \textit{Proceedings of the Royal Society of Medicine} contained a survey of encephalitis lethargica that discussed how emerging infections in Japan, Australia, and the Midwest United States related to it. And just after Germany invaded the Soviet Union in July 1941, an article by Ralph S. Muckenfuss, surveying “epidemic encephalitis,” appeared in the \textit{Bulletin of the New York Academy of Medicine}. In both articles it is evident that the consensus about how these infections were transmitted, how they related to one another clinically, and how the viruses that caused them related to one another, all were in the process of being worked out, and the discussion helps to put the difficulties that Far East specialists were having conceptualizing their outbreaks into context.

\textsuperscript{25} L. L. Kiselev, E. S. Levina, \textit{L. A. Zil’ber (1894-1966)} (2004), 186.
Conclusion

How did a historical narrative that is in fact quite divorced from the documentary evidence come to predominate in this set of events? This occurred first of all because it was penned primarily by Moscow scientists, or by specialists from the center, (as well as by scientific practitioners, instead of professional historians), rather than a broader range of people with a mix of perspectives on these events.26

In fact, it is not simply incidental that the difference in perspective here stems from the differences between the center and the periphery. I would argue that there were important gaps in perspective—shaped by both cultural differences and geographical distance—that naturally caused historian-virologists in Moscow to view this history differently and to write a story that diverges significantly from either what is reflected in the evidence or what might have been told by Far Eastern specialists involved in TBE investigations before 1937. This historical narrative has been shaped in part by a disparity between the periphery and the center, and also by a cultural gap and diversity of priorities that separated academic biomedical researchers from public health practitioners and local clinicians in the Soviet Union in an especially pronounced way in the prewar period.

When medical virologists from Moscow and Leningrad took up investigation of TBE in 1937, it represented a fascinating case that coincided very well with some of the central questions being asked in their young field. They fervently puzzled through the matrix of problems surrounding the disease, from its epidemiology to its immunology, and research careers

26 It is important to note that this narrative was shaped in this manner really only after the Thaw, and we must recognize that it was formulated in part as a response to the Soviet regime under which Zilber was arrested and for years repressed—as a rewriting of the original story, which left Zilber and a handful of other arrested colleagues out. Also underlying this story is most likely a desire to tell a counter-narrative to that of damage and political pressure that the biological sciences were subjected to under Stalin’s rule and the influence of Trofim Lysenko’s pseudo-scientific claims. I would argue that, although these goals are noble, the history should in fact bear a closer critical relationship to the documentary evidence.
were built around its study. As in any academic field, institutional expansion and the training and confirming of new specialists depended on having new subject matter in which to delve and make original contributions to the literature and to the existing body of scientific knowledge. TBE in this case met a demand for Soviet virologists in the 1940s and 50s, as a small group of specialists on TBE and related syndromes emerged with dissertations and published articles on the disease.

Yet, initially TBE did not preoccupy public health officials and practitioners in the Soviet Far East to the same degree. Their priorities lay elsewhere, as they fought more classic infectious and parasitic diseases that continued to plague a medically underserved population. There was very little open discussion of the emerging tick-borne encephalitis problem reflected in the documentary evidence of civilian public health authorities before 1937. No special response to these new isolated cases of paralysis and death came in the form of a program or office established to deal with the disease. No special meetings or commissions were organized by the public health department to manage the problem, save for the 1936 meeting alluded to above, where 27 officials and practitioners from the Dalkrai health department gathered to have what amounted to a wide-ranging, confused, and ultimately inconclusive discussion.27

The department did form special health commissions to investigate or perform control work on other infectious diseases such as cholera, but as a rule neither tick-borne encephalitis nor any disease resembling it was the subject of such special focus. For Dalkrai public health authorities, other infectious diseases represented a higher immediate priority than the encephalitis problem.28

27 GAKhK, f. R-683, op. 1, d. 176, ll. 11-13, “Protokol epidemicheskago Soveshchaniia pri Kraizdrave (11 iulia 1936).”
28 See, inter alia, GAPK, f. R-268, op. 2, d. 1, “Dokumenty o rabote chrezvychaynoi protivoepidemicheskoi komissii za 1938-1942gg.,” for example, which concentrated exclusively on cholera, spotted typhus, and smallpox.
The blank spaces in the documentary record, together with the analysis of the mention and discussion that tick-borne encephalitis did garner combine to give us a fuller picture of the conditions under which science and public health were conducted in the Soviet Union in the 1930s. The analysis here has shown just what it meant to do medicine and public health, and to try to manage public health problems from an official point of view, in an under-served, far-flung region of the Soviet Union. Especially in the 1930s, when we are told that priorities driving public health were changing in Moscow at the Union-level Ministry headquarters, with more pointed attention to worker health and a retreat from NEP-era dedication to universal health improvement, this picture of public health on the periphery is very valuable.

Here in the case of Dalkrai, we can see that on the ground, when it came to doing real public health work, priorities were driven by a different yardstick than that held by either Ministry-level officials or Moscow- or Leningrad-based laboratory specialists. Classic infectious diseases continued to threaten lives and drain energy from the general population, and specialized expertise continued to be a rare commodity. Institutions were also poor in resources, staff ability, and in the connections to other institutions that could foster an able, rapid response to an emerging infection. Public health in the periphery was a long way from the ideals and realities that shaped the thinking of officials and scientists in the Soviet center.