

“...this Brazilian venture...” A brief biography of Theodosius Dobzhansky before he arrived in Brazil

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Abstract: This paper describes life and career of Theodosius Dobzhansky (1900-1975) until he arrived in Brazil in 1943. During his years in Russia, Dobzhansky began his entomology studies and undertook research expeditions to Central Asia to study livestock, which focused on speciation biology. Once he arrived in the United States Dobzhansky began working with *Drosophila melanogaster* with Thomas Hunt Morgan (1866-1945) at Columbia University. Once Morgan relocated to the California Institute of Technology (Caltech), Dobzhansky started collaborating with his colleague, Alfred Henry Sturtevant (1891-1970), on studies of a wild cousin of *Drosophila melanogaster*, *Drosophila pseudoobscura*. Dobzhansky and Sturtevant’s friendship and collaboration suffered due to several factors, including most importantly, their differing approaches to *Drosophila pseudoobscura* as influenced by their different conceptions of the purpose of their work. While Sturtevant studied the flies using the same techniques as his studies of the domestic *Drosophila melanogaster*, Dobzhansky studied *Drosophila pseudoobscura* in the field considering his broader dictum that “Nothing in biology makes sense except in the light of evolution.”

Key-words: Theodosius Dobzhansky; Alfred Henry Sturtevant; *Drosophila pseudoobscura*; Genetics; Evolution.

“... este empreendimento brasileiro...” Uma breve biografia de Theodosius Dobzhansky antes de sua chegada ao Brasil

Resumo: Este artigo descreve a vida e carreira de Theodosius Dobzhansky (1900-1975) antes de sua chegada ao Brasil em 1943. Durante os anos em que vivia na Rússia Dobzhansky iniciou seus estudos de entomologia e realizou expedições científicas a Ásia Central para estudar animais domésticos com

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foco na especiação. Ao chegar nos Estados Unidos, ele começou a trabalhar com *Drosophila melanogaster* com Thomas Hunt Morgan (1866-1945) na Universidade de Colúmbia. Quando Morgan se mudou para o *California Institute of Technology* (Caltech), Dobzhansky começou a colaborar com seu colega Alfred Henry Sturtevant (1891-1970) estudando o primo selvagem de *Drosophila melanogaster*, *Drosophila pseudoobscura*. A amizade e colaboração entre Dobzhansky e Sturtevant sofreu devido a vários fatores, sendo talvez o mais importante os diferentes enfoques adotados em suas investigações sobre *Drosophila pseudoobscura* guiados por suas diferentes concepções. Enquanto Sturtevant estudava as moscas utilizando as mesmas técnicas empregadas nas investigações da mosca doméstica *Drosophila melanogaster*, Dobzhansky estudou *Drosophila pseudoobscura* no campo considerando o *dictum*: “Nada em biologia faz sentido exceto à luz da evolução”.

Palavras-chave: Theodosius Dobzhansky; Alfred Henry Sturtevant; *Drosophila pseudoobscura*; Genética; Evolução.

1 INTRODUCTION

This article describes the life and work of Theodosius Grigorovich Dobzhansky (1900-1975) before arriving for the first time in 1943 in Brazil. Dobzhansky was drawn to Brazil through his studies of the wild fruit fly, *Drosophila pseudoobscura*, which he was using to build upon previous research with its domestic counterpart, *Drosophila melanogaster*, to learn how species evolve in nature. The story begins in Russia, where he formulated his basic idea of the relationship between geography, reproduction, and evolution. He also discovered genetics, the science of heredity he would play a role in creating after moving to the United States in 1927 to work with Thomas Hunt Morgan (1866-1945) and his students in the “fly room” in New York City. Dobzhansky’s most important contribution would be to bring what had been till then a laboratory science outside into the field. He would then revise Charles Darwin’s theory of species as the state of equilibrium reached by a population of organisms isolated from others by geography. Though Dobzhansky’s distinction would later be regarded as essentially reiterating what Darwin had said in other words, his formulation accurately reflects the state of what would be described as the “modern” or “evolutionary” synthesis as it came into being. It was a view he would summarize in his most important work, *Genetics and the origin of species*, a book

which, as hinted by its title, would be regarded as among the most important contributions to Darwin's original idea.

2 RUSSIA

The summer Russia tipped over into revolution a sixteen-year-old Dobzhansky went on a secret butterfly and moth collecting trip to the Caucasus with his best friend, Vadim Aleksandrovsky. Dobzhansky's family were penniless Polish *Szlachata* lost in the *Krey* of the Russian Empire. Grigorovych Dobzhansky earned his income solely in respect of fitting a gentleman. He served his government as an assistant at the local state high school or leasing property by renting rooms to boarding students. Aleksandrovsky's father, on the other hand, was a tutor to the children of wealthy families in the region, and it was in his library the boys first discovered a copy of Darwin's *On the Origin of Species*.

Dobzhansky and Aleksandrovsky had been inspired to their mission for the same reasons Darwin had set off on the *Beagle* less than a century before, curiosity. And indeed, the revelation of Darwinism had more than made up for their natural history teacher at school who, knowing nothing about either nature or history, had bribed them to stop asking so many questions about both by giving them the keys to the science cabinet. This tiny room contained a microscope and a dissection set, which the other students – like the teacher – showed so little interest in that Dobzhansky and Aleksandrovsky were allowed to take it home for the summer.

Supplied with these tools, they took apart frogs and stared at creatures contained in drops of water from samples they had scooped out from the pond and let grow stagnant. "Little beasties" they called them. Then with nets in their hands, they were off – exploring the countryside around Kiev until these bounds seemed too limiting. And so they had set out, managing to escape alone from Kiev by telling their parents alternate versions of the same lie: My friend's mother will be accompanying us. The Dobzhanskys and Aleksandrovskys were of such remote social circles that the likelihood of discovering the ruse seemed distant. It had been a brilliant time until now, when they emerged fresh from a swim in the Black Sea following a day of butterfly collecting, to find Aleksandrovsky's mother waiting for them at the inn where they'd rented a room. Their adventure seemed to have come to an abrupt end.

Dobzhansky waited with dread outside as Vadim went in alone to speak with his mom. Much to his surprise when they emerged, he learned she had given them a little money and decided to let them go on their way. They had grown up.

Dobzhansky and Aleksandrovsky continued for a few more weeks, and it was only when they finally turned around for a home that they realized what was about to happen next. The trains were packed with worried voices and running off their timetables. History was about to begin again.¹(Reminiscences of Theodosius Dobzhansky, 1962, pp. 24-30).

3 GENETICS

In the spring of 1917, the Dnieper River's waters overflowed, washing a tide of debris to its shores. Among twigs and leaves were beetles – including one Dobzhansky had never seen before. A new genus of the *Coccinella* species was Dobzhansky's first scientific publication. (Dobzhansky, 1918). With beetles what separates one variety from another is very specific – either the sex parts of their bodies fit together or don't.² (Krementsov, 1994). As simple as attaching two pieces in a puzzle, this solution inspired him to find other methods applied across species. So it is when Dobzhansky discovered genetics.

He first learned of it through the publications of his future mentor and friend, Iurii Filipchenko (1882-1930). He invited Dobzhansky to join him at the Bureau of Eugenics and Genetics at the University of St. Petersburg to work with *Drosophila melanogaster* – fruit flies – the first strains of which had been brought a few years before by the H. J. as mentioned above Muller.³ Thanks to Filipchenko Dobzhansky would

¹ For importance of time in Soviet history see Susan Buck-Morss, 2000.

² There are, as Dobzhansky outlined in his landmark article “A critique of the species concept in biology,” exceptions to this. See below.

³ Filipchenko's institute was founded in 1919 as the first Russian department of genetics at St. Petersburg State University. In 1921 it was renamed Bureau of Eugenics at the Russian Academy of Sciences in St. Petersburg, and renamed the Bureau of Genetics and Eugenics shortly after Dobzhansky arrived to work with Filipchenko the night of January 21, 1924. This date is recorded due to the fact that Dobzhansky's personal acquaintance with Filipchenko coincided with Lenin's death. The Bureau

soon be funded by the Rockefeller Foundation to work there on what was to be a one-year fellowship.

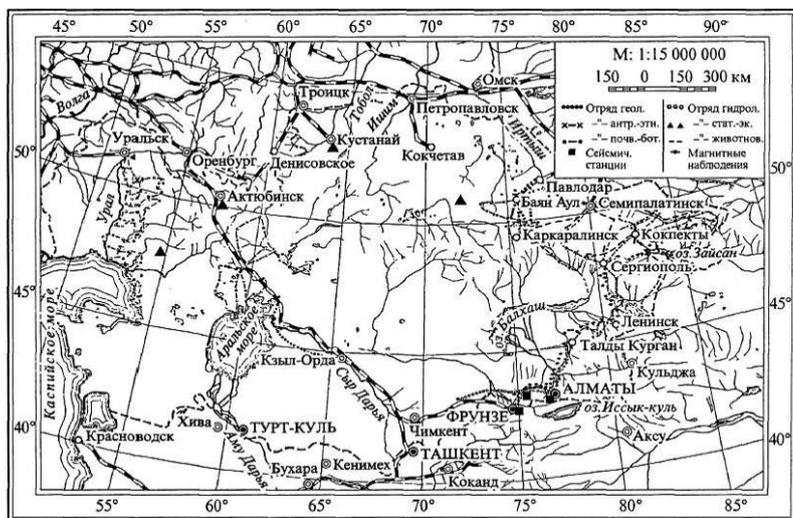
Before then, Dobzhansky undertook expeditions to Central Asia, which satisfied Filipchenko's interests in animal breeding differential effects and counted as military service in a scientific researcher's role. It also meant travelling on horseback across mountains and valleys measuring the impact of natural barriers upon the fat content of milk produced by the livestock domesticated by migrating tribes in the region.⁴

Among the details of these expeditions that would prove fateful was Dobzhansky's suspicions regarding Nikolai Medvedev's colleagues. Dobzhansky was frustrated by Medvedev's laziness and distraction from their work. However, Dobzhansky soon realized that Medvedev, and another member of his team, were secretly collecting samples of their own aside from those for Filipchenko's Institute. Dobzhansky was frustrated they were wasting time by duplicating research. However, the more disturbing realization was that Medvedev was not working there with himself and for Filipchenko but under the auspices of the State Institute for Experimental Agronomy (GIOO). Dobzhansky was suspicious, and this sense of mistrust would be reversed many years later as he sought to avoid returning to the USSR, and Medvedev was now the one to wonder what this man who seemed to be forever prolonging his stay in the United States was up to.

Dobzhansky first learned of Morgan's laboratory wonders during his stays in the Houses of Scientists in Moscow and St. Petersburg, founded by the doyen of Russian genetics Nikolai Vavilov. Here Vavilov created libraries from the literature he brought back from his journeys to the west, and it became clear that Morgan's Fly Room progressed significantly. However, Dobzhansky's instinct to travel outside of Russia to broaden his knowledge of heredity and evolution did not initially lead him to the United States.

would be named the Laboratory of Genetics in 1930, and ultimately taken out from under Filipchenko's direction to become the Institute of Genetics at USSR Academy of Sciences.

⁴ See М.Б. Конапев, 2014. See also, for example, "Horses of the nomadic population of Semiretshje", 1927 (in Russian) of Theodosius Dobzhansky.



Карта 2. Маршруты отрядов Казахстанской экспедиции 1927 г.

Fig. 1. Kazakhstan, 1927.

Source: KONASHEV, Mikhail (ed). МАКСИМУМ ВОЗМОЖНОГО: Переписка Ф. Г. Добržанского с отечественными биологами: 1920–1970 гг., Часть 1, Переписка Ф. Г. Добržанского с отечественными биологами: 1920–1930-е гг⁵. Санкт-Петербург: история Нестора, 2014.

One of his mentors, Sergei Chetverikov (1880-1859), considered sending him to join Nikolai Timofeeff-Ressovsky, a mutual colleague. He was at that time working at the *Kaiser Wilhelm Institute for Brain Research* in Berlin. Chetverikov would soon share Dobzhansky's interest in both the mutagenesis of radiation and how genes spread outside. However, for whatever reason, Chetverikov seemed to have forgotten about it and never raised the subject again. Though disappointed initially, Dobzhansky would later reflect on how much different his fate

⁵ Source: KONASHEV, Mikhail (ed). MAXIMUM POSSIBLE: Correspondence of F. G. Dobzhansky with his homeland biologists: 1920 - 1970, Part 1. Correspondence of F. G. Dobzhansky with his homeland biologists: 1920 - 1930. St. Petersburg: Nestor History, 2014.

may have been had Chetverikov followed up on the offer.⁶ (Reminiscences of Theodosius Dobzhansky, 1962, p. 227).

The following year Dobzhansky, with Filipchenko's support, applied to the *Rockefeller Foundation* to work with Morgan in New York. In light of the circumstances under which Dobzhansky would later find himself in Brazil this moment is instructive. Rockefeller courted André Dreyfus (1897-1952. to go to the United States and he accepted if they send someone to Brazil first. Dobzhansky was eager to set off in search of new experience. Though everything that happened next in Russia – the onset of Stalinism compelling Dobzhansky to do whatever he could to remain in the United States. – would make it seem like Dobzhansky sought to escape his homeland, the opposite was true. It was Russia – in the guise of what it had become, the USSR⁷ – that had become an inhospitable environment for his research. Given that the personae and the questions he pursued answers to are, as I mentioned above, indistinguishable, this point is essential. There is also no doubt that the inability to return to his homeland became part of Dobzhansky's makeup to Brazil.

Because there were no formal diplomatic relations between the United States and USSR when Dobzhansky left, he had to travel first to Riga, Latvia, to obtain his visa. When asked what type of visa he would prefer– student or professor– he did not think it would make any difference. So he said, “Give me any kind of visa you want.” It was a decision he would regret.

Dobzhansky and his wife Natalia arrived on Christmas day. The *Rockefeller Foundation* assumed Morgan's laboratory would be closed till after New Year and wrote Morgan to tell him the news. “If your rooms should be open,” however, Wallace Lund added, “I am sure that Doctor Dobzhansky will wish to get started as soon as possible.” (Letter from Wallace Lund to T. H. Morgan, December 20, 1927. RF 1.2 Projects 200 United States. 200 D Columbia University – Genetics. Folder 1650. Dobzhansky, Theodosius, 1946-1963. Rockefeller Archive Center). Morgan replied: “As a matter of fact, our laboratory is never

⁶ Timofeeff-Ressovsky would remain in Berlin during World War II and end up spending time in a prison camp before being liberated to work on the Soviet bomb project. He would never be allowed to leave the Union of the Soviet Socialist Republic.

⁷ Union of the Soviet Socialist Republic.

closed,” “not even on Christmas – but it is inaccessible on Christmas day except to the initiated.” (Letter from T. H. Morgan to Wallace Lund, December 22, 1927.RF 1.2 Projects 200 United States. 200 D Columbia University – Genetics. Folder 1650. Dobzhansky, Theodosius, 1946-1963. Rockefeller Archive Center).

Thus Dobzhansky and Natalia waited until the day after to pay their first visit. As they climbed the stairs of Schermerhorn Hall, they met a slim, bespectacled man they asked the way to Morgan’s lab. It was Alfred Henry Sturtevant (1891-1970). Having long idolized Morgan and his acolytes from afar Dobzhansky was shocked to discover a filthy attic containing one room – where Sturtevant and Morgan’s other protegee, Calvin Blackman Bridges (1889-1938), worked – along with an open antechamber from where Morgan presided. Sturtevant and Bridges’ desks were cluttered with stacks of paper, and the flies were housed in cabinets Bridges had made out of old bookshelves. The place reeked of yeast and bananas, recycled bottles were used for test tubes, and the microscopes were less sophisticated than he’d left behind in Leningrad. He tried to speak to Morgan in German, and Morgan responded in English. Dobzhansky asked him what type of research he would like to do, unaware that this was a question Morgan hated to be asked. He responded by opening the nearest drawer and pulling out the stack of whatever paper was inside. It was a recently completed dissertation on biostatistics, something Dobzhansky knew nothing about. He returned home from his first day dejected. (*Reminiscences of Theodosius Dobzhansky*, 1962, pp. 231; 239-245).

The member who would have the most significant influence upon Dobzhansky was a fellow outsider, Hermann Joseph Muller (1890-1967) who had not been there since five years earlier when it became clear he was no longer welcome. Muller’s work using radiation to mutate genes would align with Dobzhansky’s interest in the broader question of what mutation implies for the outcome of evolution. Muller was pessimistic, arguing they also must manage heredity. Dobzhansky’s opposing view– that the natural state of evolution is change– was also among the things that led him to Brazil.

In the meanwhile, Dobzhansky quickly became an invaluable member of Morgan’s lab. Dobzhansky’s search for the genetics of the origin

of species is now oriented around the chromosome map—a visual representation, derived by Sturtevant. He located five mutant genes on the chromosome relative to one another and the gene – labelled O – responsible for the red eyes *Drosophila* flies. (fig. 2). Sturtevant and Bridges had since expanded the one-dimensional chromosome map into the three-dimensional “totem pole,” mapping genes’ location on all four *Drosophila melanogaster* chromosomes. (fig. 3).

Once Morgan invited Dobzhansky to join him out west to help found the biology division at the *California Institute of Technology* Dobzhansky would expand the chromosome map even further by following a wild cousin *Drosophilas melanogaster*, *Drosophila pseudoobscura*, in its native habitat outdoors.

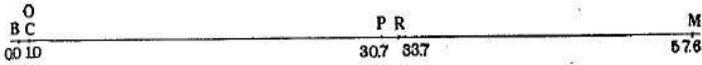


Fig. 2. The first chromosome map showing six genes on the X-chromosome of *Drosophila melanogaster*.

Source: STURTEVANT, Alfred Henry. The linear arrangement of six sex-linked factors in *Drosophila*, as shown by their mode of association. *Journal of Experimental Zoology*, **14**: 39-45, 1913.

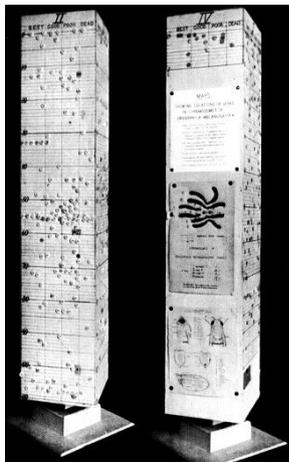


Fig. 3. Totem Pole.

Source: MORGAN, Thomas Hunt. Personal recollections of Calvin B. Bridges. *Journal of Heredity*, **30** (9): 354-358, 1939.

Before then, he would have the chance to visit Muller. By now Dobzhansky had picked up on the bad relations between Muller and his former colleagues. When Muller first published his finding that X-ray radiation mutates genes, the other Fly Room members had been skeptical. They only finally came around once others had begun adopting the technique. However, among the reasons for their skepticism was that Muller had been deliberately vague on the details of the experiment due to his worry that others unfairly stole his ideas and took credit for discoveries he had made first. This “priority complex” would plague Muller throughout his life. It is characteristic of Dobzhansky’s role as the perpetual outsider; the one was never “from there”— wherever he was — that he would have the insight to diagnose the reason from what plagued him.

Dobzhansky passed through Muller’s lab at the University of Texas in 1929 and spent the day with him. He also met his two colleagues John Thomas Patterson (1878-1960) and Theophilus Schickel Painter (1889-1969) — who Muller would soon seek to avoid by working only at night so they could not steal his ideas. In a letter to his mentor, Yuri Filipchenko (1880-1930), Dobzhansky described Muller as an “endlessly flowing fountain you could drink from forever and still feel thirsty.” (МАКСИМУМ ВОЗМОЖНОГО, 2014, pp. 185-188). By this Dobzhansky meant that Muller thought aloud as he worked — repeatedly iterated each new idea as it entered his head — the thing he could do next with what he was working on right now and then even what he might also try with that after he was done with the notion preceding it. From what Dobzhansky wrote, it was clear that his co-workers were not stealing: They were just listening, and then taking inspiration from what Muller said — ideas there would never be enough hours in the day for Muller to finish.

By this time, Dobzhansky used Muller’s X-ray method that consisted of “translocate” genes from one part of the chromosome to another. Despite the abundance of space, Morgan expected him to share a lab with Sturtevant. An essential part of Fly Room life had been the constant interaction between its members. This situation would change at Caltech.

Dobzhansky extended his stay in the United States by over a year, and Filipchenko was concerned. He was also skeptical of his *protégé*’s

enthusiasm about Muller and radiation, saying it reminded him of his younger years when he had enthusiastically chased after new ideas only to realize he was wasting his time. It was hard for Filipchenko to gauge the merit of Muller's work not only because he was not a *Drosophila* geneticist, but also because the Soviet Union was becoming cut off. The Great Break broke an increasing number of lives, careers, and relationships within the USSR along with any communication to the world outside.

Dobzhansky realized he would never have the opportunity to continue his research on translocations in fruit flies if he returned, and Morgan was eager to keep him. Meanwhile, Filipchenko became increasingly bleak, even more so once he lost control of the research institute he had founded. On May 21, 1930 – about two-and-a-half years after he had arrived – Dobzhansky received a telegram from the Nikolai Vavilov telling him Filipchenko had died – at age 48 – of meningitis, and begged Dobzhansky come home. (Letter from Nikolai Vavilov to Theodosius Dobzhansky, May 21, 1930, reproduced in МАКСИМУМ ВОЗМОЖНОГО, 2014, p. 401).

Not long after, Dobzhansky received a letter from Medvedev describing the terrible details of Filipchenko's death. This Medvedev was the same Medvedev from the expeditions in Central Asia, and now he was the one suspicious. Medvedev said Dobzhansky's prolonged stay in the United States was not only raising scepticism he had any intention of returning to Russia but by doing so, he was also breaking the law. Medvedev made it clear that though it had been once presumed Dobzhansky would assume the position of director of Filipchenko's Institute, he would now be lucky to be given any job at all. (Letter from N. N. Medvedev to Theodosius Dobzhansky, May 20, 1930, reproduced in МАКСИМУМ ВОЗМОЖНОГО, 2014, pp. 669-670; Letter from N. N. Medvedev to Theodosius Dobzhansky, July 8, 1930, reproduced in МАКСИМУМ ВОЗМОЖНОГО, 2014, pp. 673-674).

By the time Vavilov arrived in the United States the following on yet another of his expeditions seeking the genetic origins of plant species, he knew Dobzhansky would need much persuading to go back to the Union of Soviet Socialist Republics. For better or worse, Vavilov's efforts at convincing revealed his doubts about what was transpiring at

home. He told Dobzhansky “I have a kind of spectacles which permit me to see some things and not to see other things.” “We have to ignore; we have to leave out of consideration, the political matters with which we do not agree. We should, however, do our best for the advancement of science, particularly for the advancement of science in our country.” (Reminiscences of Theodosius Dobzhansky, 1962, pp. 166-167).

Dobzhansky was now even more convinced he did not want to go back— and the letters Vavilov sent afterwards were terrifying. He would need to brush up on his Lenin and the basics of dialectical materialism. Vavilov even suggested one of his recent publications interpreting the Linnaean system of identifying plants species as Marxist as preparation for the kinds of justifications he should be prepared to make for his work. Dobzhansky should also be ready to find other ways to earn money aside from research. (Letter from Nikolai Vavilov to Theodosius Dobzhansky, June 9, 1931, reproduced *in*: МАКСИМУМ ВОЗМОЖНОГО, 2014, pp. 416-419).

But Dobzhansky could not. He just could not face such a fate after the luxury of freedom in the United States – the excitement of New York City, the frontier climate of southern California – his Ford automobile, the Sears and Roebuck catalogue, the orange and lemon trees beneath the palms reminding him of his secret escape to the tropics with Aleksandrovsky when he was a young boy. (Letter from Theodosius and Natalia Dobzhansky to Iurii Filipchenko, November 16, 1929, reproduced *in* МАКСИМУМ ВОЗМОЖНОГО, 2014, pp. 228-231). Dobzhansky wrote Vavilov that no, he would not be coming back, and with that, he truly now had to figure out how to stay.

Morgan had also given Dobzhansky’s wife Natalia a job as stock keeper for the flies. With this, in addition to the paperwork testifying to his position as professor, they crossed the border to Canada where they would apply for permanent visas to remain in the United States. A terrifying set of circumstances nearly derailed them: Years ago, when Dobzhansky had obtained authorization to enter the United States, he had accepted a student visa. He could have come as a professor, but it did not seem to make any difference at the time. It did now – because by working, he and Natalia had both broken the law. In one week they would be deported to the Union of Soviet Socialist Republic, which

meant a death sentence. They spent the last of their money on telegrams. Finally, they got in touch with Sturtevant who alerted Morgan. Morgan informed Caltech, Robert Millikan – who it just so happened was vacationing aboard the United States President Herbert Hoover’s yacht. Hoover phoned his labor secretary who overruled the decision of the Canadian Consulate. At last, they were free. (Reminiscences, of Theodosius Dobzhansky, 1962, pp. 316-317).

4 *DROSOPHILA PSEUDOOBSCURA*

The following summer Dobzhansky attended the *VI International Congress of Genetics* in Ithaca, N.Y. By now he was turning out papers twice as fast as Sturtevant and Bridges. Bridges often claimed, “there is no rush,” to which Dobzhansky responded, “the only thing that is becoming better with age is wine,” and “a month gone by without a paper sent to press is a wasted month.” (Kohler, 1994, p. 265).

On his way, east from California Dobzhansky stopped in to visit one of his students, Bob Boche. Boche was collecting samples of wild *Drosophila pseudoobscura* flies over the summer break. *Drosophila pseudoobscura* had only recently been recognized as a separate wild type species of *Drosophila* fly from the European *Drosophila pseudoobscura* a few years before by Morgan’s students, Donald Lancefield. In the process of doing so, Lancefield also determined there were two hybrid strains– A and B–, and thus the question began with what made them unable to mate, i.e., “hybrid sterility.” Sturtevant and Dobzhansky quickly took an interest in *Drosophila pseudoobscura*. The only laboratory stocks were A, leaving it up to Dobzhansky to obtain the wild type B. For this, he had enlisted Boche, who had mentioned he lived in Seattle – a known breeding ground of *Drosophila pseudoobscura* flies – before leaving for summer break. Dobzhansky suggested he collect some and the results when stopped through were encouraging. Boche had discovered seven additional strains. (Lewontin *et al.*, 2003, pp. 28-29).

Two events that occurred in Ithaca gave Dobzhansky further clarity on where he heads next and assurance that his decision to stay had been correct. The first was the paper delivered by Sewall Wright (1889-1988). Dobzhansky would later describe himself as having ‘fallen in love with’ as he listened to his article on the selective mathematics of mutation, inbreeding, and cross-breeding populations. It was clear

Wright's mathematics provided a means to acquire the data needed to reflect the notions of population genetics first put in his head by Russian mentors like Chetverikov. (Lewontin *et al.*, 2003, pp. 56-57). The second was Dobzhansky's conversation with Vavilov in a crowded cafeteria where none of their other table mates spoke Russian. Vavilov had been the lone Russian geneticist allowed to attend, and he suspected that it was to be his last trip outside of the Soviet Union. Vavilov told Dobzhansky to "do what you want." "Stay here."

Given that, as Vavilov knew, Dobzhansky had already decided to do that. It seems reasonable to wonder if he was not trying to convince himself because, indeed, after Ithaca, he and Dobzhansky would never meet again. (Reminiscences, of Theodosius Dobzhansky, 1962, pp. 174-175).

Once the Congress ended Sturtevant left to spend a sabbatical year in England while Dobzhansky and Boche returned to California to continue work on *Drosophila pseudoobscura*. Sturtevant's "Anglophilia" – as Dobzhansky saw it – is among the few things that began irk him about Sturtevant. This decision coincided with a lack of interest in the Russians' work like Filipchenko and Chetverikov who had been so influential upon Dobzhansky. Other reasons for resentment would now begin to accumulate.

While Sturtevant was away a few critical developments in *Drosophila* genetics took place. First, Theophilus Painter (1889-1969), Muller's colleague from Texas, discovered that the salivary glands of fruit flies contained bands of chromosomes wound tightly together whose sequences could beread their genetic history– "fossils" of the biological world. The study of salivary glands now enabled geneticists to read into their flies developmental past.⁸

Two, Dobzhansky drove his Model A Ford along the Redwood Highway and California coast north of San Francisco to, for the first time, collect wild *Drosophila pseudoobscura* flies on his own. His equipment was yeast, bananas, bottles and string. He looped the latter round the former and hung them from trees, the fermenting banana mash inside a lure sure to attract local members of the species. (Dobzhansky, 1936, pp. 28-29). Among his discoveries was that in the wild fruit flies

⁸ For an explanation of the importance of the *Drosophila* salivary glands see Stephanie Mohr, 2018, pp. 28-29.

are most active at sunrise and sunset. Only on cloudy days are they active in-between, a finding unobservable in the lab.

This where he sent the flies once collected. This step was treacherous because not only did some states have laws against the importation of live insects, but they needed to get there quickly lest they perish from any extreme of cold or heat. The female flies arrived pregnant and were immediately put inside incubators and given its bananas and yeast bottle. Once the eggs hatched, the larvae had to be dissected before they had grown too old. The number of flies submitted to this process was monumental – the product of six trips to eleven mountain ranges – thousands of bottles spread out across work tables back in the lab.

Dobzhansky found such work – working out the cytology, mapping the chromosomes etc. – profoundly tedious, and would rely upon Sturtevant, who had the patience and talent for it, to do most of the work. (Kohler, 1994, p., 261). Though labor division would be mutually satisfying at first, it would also lead to yet another fault line between them. Dobzhansky thought of nothing of cutting corners by rushing forward as quickly as possible to get back to what he enjoyed – collecting flies in the field. As a result, his mapping would be scarce – lacking in important details – and years later, others would discover errors in his cytological analyses. (Kohler, 1994, p. 285). Though there is no direct evidence that Sturtevant was upset by Dobzhansky's carelessness in this aspect of the work, it could certainly be among the reasons they ultimately stopped collaborating.

And then finally three – Morgan was awarded the Nobel Prize for the work he and his students had done developing the chromosomal theory of heredity. It was understood that Morgan was awarded the prize alone because the maximum number of awardees was three, and the Fly Room members were four. Though Muller had initially believed that he and Morgan would be awarded the Prize jointly, Morgan would secretly split the prize money between himself, Bridges and Sturtevant. (Letter from Hermann Joseph Muller to Jessie Marie Jacobs, March 21, 1933. Muller MSS. Cold Spring Harbor, New York.). Though this would not be known for a while, in the interim it led to a disturbing episode once Sturtevant returned from the United Kingdom, Dobzhansky recounted the scene which resulted in his oral history:

And so, one day we were sitting as always in our laboratory room with Sturtevant, and I made some critical remark about Morgan – I do not remember exactly what I said. Still, I am absolutely certain it was a remark which was much less critical and certainly nowhere near as disparaging as hundreds or thousands of remarks which he used to make almost every day. And here came a reaction which startled me to an extent which probably left me with my mouth open for some time; namely, Sturtevant raised his voice and said: “I ask you never again to make in my presence any disparaging remarks about Morgan.” [...] And that was the end of our friendship.” (Reminiscences of Theodosius Dobzhansky, 1962, pp. 273-274)

It was not, but it is not surprising that decades after the fact Dobzhansky would have remembered it that way. Dobzhansky and Sturtevant would continue to collaborate on good terms for a few more years. However, it was the beginning of the end of their relationship, a process which would continue throughout the following year, 1934. In terms of this article’s topic, this is when the University of São Paulo was founded, providing the stage that would be set about a decade later for Dobzhansky’s arrival in Brazil. The content of the lectures he would deliver to the group of students who would remember themselves as the *Turma da Glete*– “Class of Glete Avenue”– was formulated during the years in between.

Dobzhansky stopped working with *Drosophila melanogaster* flies altogether and took to the field where he and Sturtevant established a routine. Dobzhansky did the fieldwork and sent flies back to the lab for Sturtevant to analyze. He went on collecting trips to Alaska, British Columbia, and the Idaho Snake River Basin – where Sturtevant assured him *Drosophila pseudoobscura* flies would not be found. Though this was not true of Idaho, the Alaska trip did prove a disappointing adventure. Due to a dock strike, Dobzhansky was forced to travel steerage, where he met several adventurers on their way to Alaska to dig for gold. The rumours of the wealth to be found in Alaska were so impressive that they assumed Dobzhansky’s claim he was headed there searching for fruit flies to be a clever ruse hiding his real purpose. Despite the rigorous voyage, Dobzhansky felt rewarded by the dramatic vista of mountains and glaciers. However, he had neglected to bring either a hand lens or a microscope and spent weeks collecting what he thought– with his naked eye– were *Drosophila pseudoobscura* flies. Only once he finally

got the chance to examine them later, he discovered they were a related species, *Drosophila athabasca*. Weeks of fly hunting wasted. (Reminiscences, of Theodosius Dobzhansky, 1962, pp. 369-376).

Fortunately, the flies collected in British Columbia and Idaho proved more fruitful, providing material for study just in time for C. C. Tan's arrival. Tan received his training from two other Chinese geneticists, Ju Chi Li and Tze-Ying Chen, who had worked in Morgan's laboratory in the 1920s. Lin had sent Morgan his Master Degree thesis, a quantitative description of variability in ladybird beetles, and since Dobzhansky was an expert, Morgan showed the paper to him. Sturtevant and Dobzhansky immediately set Lin to work using the salivary glands of *Drosophila pseudoobscura* to make genetic and cytological maps to see if he could find any systematic differences between Race A and B.

Indeed, as Tan discovered, A and B differed consistently in terms of six inversions. Moreover, as he could tell from the record provided by the salivary glands, these differences had emerged throughout the process of becoming separate races. That a decade later A and B would finally be recognized as not merely different races, but also as different species indicates the import of the direction this work was heading: the use of genes as taxonomic markers of a kind entirely unlike how species were distinguished from one another before.⁹

In 1935 Dobzhansky extended his geographic studies to Colorado, Arizona, New Mexico and Mexico City. The latter proved so productive he wished he's spent more time there. He would, and would fit in a trip to Guatemala before heading further south to Brazil. Meanwhile, Dobzhansky continued to work with what were understood to be the A and B races of *Drosophila pseudoobscura*, and also discovered a new species, which he initially planned to name *Drosophila sturtevantiana*, before deciding instead upon *Drosophila miranda* – Latin for wondrous. It is ironic – given the direction his relationship with Sturtevant was headed – that some assumed he chose *miranda* in honor of some romantic interest. (Reminiscences of Theodosius Dobzhansky, 1962, pp. 361)

⁹ *Drosophila pseudoobscura* was "Race A" while "Race B" was renamed *Drosophila persimilis* – "similis" being Latin for "similar."

Of less anecdotal import is Dobzhansky's spending several hours with Ernst Mayr (1904-2005) at the American Museum of Natural History. They discussed the effect of geography upon variation and speciation. (Lewontin *et al.*, 2003, p. 58). These ideas would be best expressed in Dobzhansky's article that year, "A critique of the species concept." Dobzhansky's critique was based on three emphases: 1. An interest in the differences within species v. those around them; 2. An emphasis on the collective v. the individual; 3. A notion of speciation based upon difference rather than similarity. Above all, he stressed the importance of geography – isolation as a mechanism for species preservation.

In a few years, Dobzhansky would coin the term "gene pool" – an English translation of a concept first described by one of his Russian mentors, Alexander Sergeevich Serebrovsky (1892-1948). Sociobiologists would adopt the term to argue their assumption that all behaviour could be explained evolutionarily in terms of species survival. Though this "biological species concept" – as it became known – would prove influential, it would, in turn, be later rejected by biologists as an unnecessary critique of what Darwin had already said a century before.¹⁰

Sturtevant was among the first critics of Dobzhansky's view. However, other reasons for the estrangement between them now accumulated to rupture their work relationship. The primary reasons seem to revolve around three events: One, Leslie C. Dunn (1893-1974) invited Dobzhansky to deliver the Jessup lectures at Columbia University (Letter from Leslie C. Dunn to Theodosius Dobzhansky, April 17, 1936. *In* Leslie C. Dunn Papers, The American Philosophical Society Archives); two, Dobzhansky received an offer to leave Caltech and join the faculty of the University of Texas; three, Milislav Demerec (1895-1966) who, with Calvin Blackman Bridges (1889-1938), had recently co-founded what quickly became the primary publication in fruit fly genetics – *The Drosophila Information Service* – asked if Dobzhansky would be interested in joining him at Cold Spring Harbor.

All three offers were honour, and Sturtevant would have been pleased to have just one. Dunn's proposal, which arrived first in April – would require spending the fall, 1936 semester at Columbia delivering

¹⁰ See Dobzhansky, 1935; Mallet, 2010a and Mallet, 2010b.

a series of lectures that Dobzhansky would turn into a book. This would give him effective authorship over the work he and Sturtevant had been conducting on *Drosophila pseudoobscura*. Dobzhansky wrote Dunn he would be delighted. “After living for eight years in the ‘wild west’, I decidedly feel a need for fresh air.”

He would prefer his only caveat if possible, to come the spring semester because Sewall Wright would be at Caltech in the fall. That said Dobzhansky sounded as though he had a sense things around him were changing, and he might be better off just letting matters happen as they may. He assured Dunn that coming in the spring instead of the fall was “merely a suggestion.” “Quite possibly after thinking more about this matter I shall find myself that your original plan is better.” (Letter from Dobzhansky to Dunn, April 21, 1936. *In* Leslie C. Dunn Papers, The American Philosophical Society Archives).

The job offers followed in May. Dobzhansky preference was Cold Spring Harbor. The problem was Demerec did not know for sure yet while the offer at Texas was definite. Sturtevant encouraged Dobzhansky to take the job at Texas, and simply move along to Cold Spring Harbor afterwards if it came through. When Dobzhansky told Morgan he immediately countered by promoting Dobzhansky to full professor – which now made him Sturtevant’s equivalent in rank, Dobzhansky sent Texas a rejection. Sturtevant was crestfallen – openly so when Dobzhansky told him the news – and it hurt. Dobzhansky realized Sturtevant wanted him gone, and from the present perspective, the reasons are not hard to discern.

When Morgan had arrived at Caltech in 1928, he had told President Millikan that in five years he would step aside and turn the lab over to someone younger – who everyone assumed would be Sturtevant. (Kohler, 1994, p. 274, ft. 47.) 1933 had come and gone with the Nobel Prize, and now Morgan was turning 70 and showing no sign of going anywhere. The notion that Morgan would choose Sturtevant no longer seemed as obvious, and now Dobzhansky had gone from being Sturtevant’s mentee, then lab partner, to being his rival.

At this point, Dobzhansky changed his mind and wrote to Texas he would like to take the job after all. He also wrote to Dunn and said that coming to Columbia as soon as possible would be best. He also said: “Dr Morgan suggested a possible title ‘Genetics and the origin of

species'. The title sounds good, but I am afraid it is somewhat too ambitious. What do you think about it?"

Texas never responded, and it would only be years later, Dobzhansky would learn that his hesitation had cost him the job. Regardless, Sturtevant now moved out of the lab they had shared for over half a decade and begun his separate research project comparing the genetics of all known *Drosophila* populations worldwide. Little would come from this work, and after a few years, Sturtevant would lose interest in it. (Kohler, 1994, pp. 286-287). Though he would continue to assist Dobzhansky in working up the genetics and cytology of the species he brought back to the lab after his adventures in the field, Sturtevant and Dobzhansky would never collaborate in the way they had ever again.¹¹

As for the flies, Dobzhansky now followed them into Death Valley and the Mojave Desert, work which would prove so productive he would return the following summer as well. Here he was able to study populations isolated in pockets at higher elevations, separated by the hot, dry climate and terrain below. He spent two summers on this work, first examining the flies he collected separately based upon the mountain range where he'd found them then, the summer following, localized to separate canyons and valleys, homing in ever more closely upon the genetic variations between populations. (Kohler, 1994, p. 273.)

In the year between he delivered the Jessup Lectures then returned to Pasadena to write them up. In December *The New York Times* reported the first news of what would become one of the most notorious episodes in the history of science— the emergence of Trofim Denisovich Lysenko (1898-1976) as an opponent of the genetic theory of heredity. The controversy was centered upon the fact that the next International Congress of Genetics was being planned for Moscow. Due to Lysenko's growing power and authority, those plans were uncertain.

¹¹ Both Kohler (1994) and Lewontin *et. al.* (2003) indicate that after 1936 Dobzhansky went their separate ways in research, however in a letter Dobzhansky sent to Dunn on April 2, 1938, after he returned from a research trip to Guatemala, he mentioned, "An indefinite number of new species of *Drosophila* (from 12 up) for Sturtevant, and about 150 new strains of *pseudoobscura* and *Azteca* for myself came safely to the lab." This indicates that their relationship continued at least to the extent that Dobzhansky was willing to collect flies that Sturtevant would find useful. (Leslie C. Dunn Papers, The American Philosophical Society Archives).

Dunn had been enchanted by Soviet biology when he visited in 1927—precisely around Dobzhansky’s time for the United States. Now he and Dobzhansky exchanged many letters on the topic, and their friendship deepened over a mutual concern for the fate of Soviet science.

Dobzhansky began writing the book, but then on February 23, he went horseback riding, as he generally did once a week. Since the last time, he had ridden the trail; someone had placed a cement post in the middle of the entrance path off the highway to keep cars from mistakenly exiting onto it. The horse unwittingly smashed Dobzhansky’s knee against the post, shattering it to pieces. After the operation, they rented a hospital bed so his leg could remain elevated as he wrote from home. As he wrote Dunn about a month later, the situation managed to prove productive: “I am still in bed, having what my little daughter describes as the ‘white box’ on my leg. The status of a cripple will continue for some time [...]. It seems writing is about the only thing that is possible [...].” (Letter from Theodosius Dobzhansky to Leslie C. Dunn, March 10, 1937. The American Philosophical Society). He made progress, and a month later wrote: “To have a broken leg is advisable when one tries to write a book.” (Letter from Theodosius Dobzhansky to L. C. Dunn, March 28, 1937. The American Philosophical Society Archives).

The *Genetics and the Origin of Species* went to press as the relationship between Dobzhansky and Sturtevant continued to devolve. As Dobzhansky wrote to Mayr—Sturtevant became “nearly furious when he saw on my desk cultures made for the extraction of lethal, etc. The studies on natural populations (then of the Death Valley region) he treated with undisguised contempt.” (Dobzhansky, *apud*, Lewontin *et al.*, 2003, p. 31).

It also seems telling that Dobzhansky now became scornful of *Drosophila melanogaster*, as though a domesticated laboratory animal somehow struck him as inferior to the diversity of flies found outside. He also began aggressively enlisting graduate students to aid in his work, acknowledge the amount there was to do and how much additional labour he needed to take on now that he and Sturtevant were no longer partners. (Kohler, 1994, p., 289).

Among the many reasons Sturtevant was growing ever more skeptical of the direction of Dobzhansky’s research was his attempt to address the issue of concealed variability in nature. The question asked

was: Why is there so much apparent diversity in laboratory populations of *Drosophila*, yet so little evident in the wild? Dobzhansky's Russian colleagues, Chetverikov and Nikolai Dubinin (1907-1998), proposed that these differences were both abundant and hidden among members of the population outside as hidden lethal or deleterious mutations. So long as they remained recessive heterozygotes, they never appear but rather are continually carried as what Muller would term the "genetic load" of mutations. Muller used this as a justification for eugenics, reasoning that this negative load of mutations would eventually lead to the extinction of the genetic line. Meanwhile, Dobzhansky would insist that the end result was salutary, simple a feature of how selection naturally functioned for the improvement of species. (Powell, 1997, p. 30).

In the meanwhile Dobzhansky's interest in this question became centered around an idea derived by Wright, "genetic drift." According to this doctrine in small, isolated populations— such as those Dobzhansky discovered on the mountain tops between the hot, dry valleys in Death Valley— natural selection broke down. In this case, features which were not beneficial, and perhaps even harmful, could become more prevalent in a population only by random occurrence. (Powell, 1997, p. 73). These shifts occurred seasonally – fewer flies in summer and more abundant flies in winter – which led Dobzhansky to ask what would happen a climate where the season remained stable, i.e., the tropics?

As Dobzhansky would learn – tropical climates are seasonal, just not in the same way as temperate climates – and the variability he was observing did not have to do with genetic drift, but rather simple seasonal fluctuations of different types. Nevertheless, the desire to research the tropics motivated him to, in February 1938, travel to Guatemala before returning to Mexico for more collecting.

An analogue to Dobzhansky's interest in how population dynamics among species is related to habit and geography is evident in what he experienced in the border crossing between Guatemala and Mexico. When a fascist dictator, Jorge Ubico (1878-1946) ruled Guatemala – while Mexico was ruled by Lázaro Cárdenas del Río (1895-1970) – believed by Guatemalans to be a communist. For this reason, the train line which would have connected the two countries ended at the Suchiate River, a footbridge over which functioned as the border crossing.

After disembarking from the train on the Guatemalan side, Dobzhansky's baggage was inspected and then carried by a porter halfway across the footbridge. It was left for a Mexican porter to ferry it the rest of the way to the other side. (Reminiscences of Theodosius Dobzhansky, 1962, p. 426).

Both trips proved productive in terms of fly collecting. However, the results Dobzhansky produced did not receive the reception he would expect. Sturtevant had recently published a paper insisting that “studies on the frequency of lethal could make possible the estimation or calculation of the parameters of populations, such as the effective population size”—an exciting proposition for a population geneticist.¹² However, when Dobzhansky sent his data to Wright, claiming he could use it to estimate population size, the latter informed him he was wrong. Like all of his contemporaries aside from Ronald Fisher (1890-1962) and John Burdon Sanderson Haldane (1892-1964), Dobzhansky could not follow what Wright was saying. The mathematics were well beyond his capability. As Dobzhansky said, “My way of reading Sewall Wright's papers, which I still think is perfectly defensible, is to examine the biological assumptions the man is making and to read the conclusions he arrives at, and hope to goodness that what comes in between is correct.” (Reminiscences of Theodosius Dobzhansky, 1962, p. 399).

However, he was informing Dobzhansky that though his data on the rate that lethal mutations occurred in the wild was important, the inference about population was incorrect. Dobzhansky had a hard time accepting this—particularly given that the person who had told him the data could be used in this way was Sturtevant, who he regarded as far more cautious in his estimation of how results could be interpreted. Moreover, the idea was just so exciting. However, what Wright's response taught him was to always consult someone with greater expertise in the mathematics of population genetics before heading into the field. It was a lesson he would remember on the eve of his trip to Brazil. Before then Dobzhansky recounted his latest research adventures to Dunn:

¹² Sturtevant published the key paper, “Autosomal lethals in wild populations of *Drosophila pseudoobscura*,” in 1937 (Lewontin *et al.*, 2003, pp. 51-52).

I had the opportunity to compare a fascist (sic) country (Guatemala) with a so-called socialist one (Mexico); clearly, the traveling for a foreigner (sic) is easier in the former, but never since leaving Russia have I felt such an atmosphere of terror around me as there, and it was a real relief to come to Mexico. Incidentally, all that was written in American newspapers regarding danger for Americans (sic) in Mexico is a damned lie—no doubt this information has been invented and handed out to the newspapers by the oil companies involved. I have seen some of the so-called anti-American (sic) demonstrations myself; the banners had some anti-American (sic) slogans, but the spirit of the crowd was fiesta-like, no hostility being shown to those having light hair and “ojos azules”. I have asked a number of Americans living in Mexico (but not involved in oil) and they confirm my impression. (Letter from Theodosius Dobzhansky to Leslie C. Dunn, April 2, 1938, The American Philosophical Society Archives)

Dunn wrote back with big news which, “I want to tell you in confidence about the possibility of a job at Columbia”. (Letter from L. C. Dunn to Theodosius Dobzhansky, April 24, 1938. The American Philosophical Society). Dobzhansky wrote back that the idea of leaving where he was “has been in my thoughts for several years,” and “I realize quite clearly that it would be much better for me to move to the other side of the continent.” He felt like he needed contact with other types of biologists doing different kinds of work; he had “too many fixed notions” and had become “provincial.” Most of all though he was anxious to work with Dunn who, despite differing points of view on everything from Lysenkoism to science and social activism, would replace Sturtevant as his closest friend and collaborator. (Letter from Theodosius Dobzhansky to Leslie C. Dunn, May 2, 1938. The American Philosophical Society Archives).

By the time Dobzhansky wrote Dunn the following January plans had begun to take shape enough to specify the types of equipment he would need for *Drosophila pseudoobscura* flies. An additional reason to leave had also occurred – the death of Calvin Bridges. As one of the founding members of the “fly room” Bridges’ untimely death at age 49 “left a hole in this Laboratory which will be filled not soon, if ever,” according to Dobzhansky. Work on *D. melanogaster* had ceased, and no member of Morgan’s lab would ever undertake to study the domestic fruit fly again. (Letter from Theodosius Dobzhansky to Leslie C.

Dunn, January 14, 1939. The American Philosophical Society Archives).

Unfortunately, the following April Sturtevant visited Columbia and when the position Dunn was planning for Dobzhansky came up, he – much to everyone’s surprise – expressed interest. As Dunn wrote Dobzhansky: “Sturt was not so firmly anchored at Pasadena as we had supposed, and might be willing to leave Morgan and avoid the succession which I supposed was impending.” Even worse, when Columbia heard they could get Sturtevant, he was enthusiastic– and regarded him as the first choice over Dobzhansky. (Letter from L. C. Dunn to Theodosius Dobzhansky, May 2, 1939. The American Philosophical Society Archives).

By summer the post had been, according to Dunn, “abolished”. However, in the fall, just as the Second World War was getting underway in Europe, there was a reprieve. It seemed that a position for Dobzhansky might be available after all. It looks as for his part Sturtevant had lost interest. (Letter from Leslie C. Dunn to Theodosius Dobzhansky, summer 1939; Letter from Leslie C. Dunn to Theodosius Dobzhansky November 20, 1939. The American Philosophical Society Archives). By Winter Dunn wrote, “Dear Dobie, Your name is too long for everyday use; I hope this version is acceptable.” A more familiar term was necessary because it seemed they would soon be colleagues.¹³

Now that he felt sure he was leaving, Dobzhansky expressed his unhappiness with Sturtevant and Caltech in letters to Demerec. “Sturtevant suffers from bad dispositions and [it] gets so that it is very difficult to talk to him nowadays. It is said to watch him in this state.” “It is better simply not to care for a person than after many years to find out that he is not worth one’s care.” (Dobzhansky, *apud*, Kohler, 1994, p. 90).

Sturtevant must have gotten wind of Dobzhansky’s disgruntlement. He was spending the semester at Harvard when he heard the news of Dobzhansky’s departure and wrote him that:

I’m sorry if you are leaving Cal Tech with the feeling that I’m down on you. We’ve had some disagreements, and I’ve allowed myself to say outrageous and inexcusable things to you– more than once. I’m afraid

¹³ Friday [between 20/11/39 and 10/1/40].

I've never apologized for them; I hereby do so and beg you to accept the apology as it's meant—sincerely. The place will seem strange without you—rather dull, I'm afraid, for none of the rest of us have your energy and go. Yes, you may be sure we'll miss you.” (Letter from Sturtevant to Dobzhansky, *apud*, Lewontin *et al.*, 2003, pp. 53-56)

Morgan tried to arrange a counteroffer, but it was no use. Dobzhansky was ready to go. (Letter from Theodosius Dobzhansky to Leslie C. Dunn, January 30, 1940. The American Philosophical Society Archives). He would remain out west through the summer conducting research, where he anticipated returning every summer in the near future to continue work on *Drosophila pseudoobscura*. Now that plans were settled Dobzhansky wrote Dunn that he expected their lab at Columbia becoming “the centre” of genetics in the United States. As he wrote, “no matter what the outcome of the war will be, New York is to become the centre of the world, and hence isn't it at least in theory possible that the genetics centre is likely to arise in the world centre?” In a Post Scriptum he assured Dunn that, “The name ‘Dobie’ is acceptable, and it is rather widely used. Mayr one of these days wrote to me that he knows that I dislike it, but this is purely his imagination!” (Letter from Theodosius Dobzhansky to Leslie C. Dunn, February 26, 1940. The American Philosophical Society Archives).

By the time summer arrived, the news of war had grown depressing. Dobzhansky was in touch with Nikolay Timofeef-Ressovsky (1900-1981) in Berlin who indicated that though food had scarce, his research remained unaffected. (Letter from Theodosius Dobzhansky to Leslie C. Dunn, May 6, 1940. The American Philosophical Society Archives). This was lucky because it is this research in which Dobzhansky was most interested. As mentioned above, Timofeef-Ressovsky had the opportunity to work in Berlin before Dobzhansky had left Russia for the United States. Now that the Second World War had broken out he remained in Berlin, a seemingly sensible decision given that, for the moment, Germany and the Union of Soviet Socialist Republics were allies. As for the work, Timofeef-Ressovsky was, like Dobzhansky, interested in how populations of flies evolved into separate species in the wild. He laid out a plot in Buch, the suburb where the Kaiser Wilhelm Institute for Brain Research was located to begin studying it. Into which he realized three species of flies—*Drosophila obscura*, *Drosophila*

junebris and *Drosophila melanogaster*. His immediate conclusion was that different species of *Drosophila* do not intermingle, but rather differ fundamentally in their temporal and spatial distribution.¹⁴

Even if, as Wright had informed Dobzhansky, his data on lethal genes could not be used to estimate population size, Timofeef-Ressovsky was onto something Dobzhansky could use to determine population density. Out west Dobzhansky now began to study how quickly *Drosophila pseudoobscura* flies spread their genes in nature. His method was capture and release experiments— release flies into an experimental plot in the morning and then trace how far they had gone by evening. Since the flies were native to the region, the problem was distinguishing which flies were his. He initially tried marking them with platinum nail polish; however, the process was tedious, but many of the flies ended up crippled and unable to fly. An alternative possibility seemed to be stocks of mutant orange-eyed *Drosophila pseudoobscura* flies maintained in the Caltech lab. Sturtevant was surprised when Dobzhansky told him fruit flies in the wild are only active in morning and evening. Flies in the lab breed and feed all the time. If the orange-eyed mutants behaved this way in the wild, they wouldn't survive, however much to Dobzhansky's shock and delight once released from the lab and set free the flies returned to their natural habits.

Dobzhansky soon determined the flies flew faster and farther than he – or Timofeef-Ressovsky – predicted. (Letter from Theodosius Dobzhansky to Leslie C. Dunn, June 15, 1941, August. 9, 1941. The American Philosophical Society Archives; Reminiscences Theodosius Dobzhansky, 1962, pp. 486-487). This procedure required continually re-measuring and setting the plot larger and larger to account for his flies. The pace of fly movement during the day was also influenced by light, temperature and humidity. Dobzhansky also needed to figure in the number of marked flies who declined naturally through death as the proportion of native flies increased through birth. Other factors to be accounted for were that some flies did not travel far from the point of release, while others travelled a long way – much as some humans remain tethered to their place of birth while others seek adventure in far off places. (Lewontin *et al.*, 2003, pp. 329-30). Still, the technique

¹⁴ В. В. Бабков Е. С. Саканян, Николай Владимирович Тимофеев-Рессовский. Москва: Памятники Исторической Мысли, 2002, pp. 94-96.

proved useful enough that they had to repeat it, with better results, the following summer.

Meanwhile, the comparison between Timofeef-Ressovsky and Dobzhansky's work can also be read as a metaphor for California's free climate *versus* the tightening atmosphere in Nazi Berlin. However, once the United States entered the war the following year, Dobzhansky would similarly find his travel constrained ability. In a letter to Dunn, he mentioned that the summer research would end. Besides that, he would arrive in New York in a few days. He was concerned with the gasoline regulation: "that gasoline regulation on the Atlantic coast will not slow us down unduly— but who knows?" (Letter from Theodosius Dobzhansky to L. C. Dunn, August 17, 1941. The American Philosophical Society Archives).

Fall arrived, and Dobzhansky returned to Columbia, where he applied to the Institute of International Education to continue his Latin America research. By now, the path was obvious. *Drosophila pseudoobscura* flies only seemed to fly further faster, and the obvious direction of pursuit was South. Dunn, who had once been described among the decision-makers at the Institute as the "most promising of the younger animal geneticists in the United States; a man of outstanding scientific promise with a charming personality, and of the right sort"— was just the man to recommend him. (Letter from Whitney H. Shepardson to C. B. Hutchinson, March 3, 1927. International Education Board. 2B 05 1.1 Folder 177 L. C. Dunn, 1927-1928. Rockefeller Archive Center).

Dobzhansky set off again out west the following summer by Greyhound bus. It was 1942, and the United States had joined the war. Travel in the United States was discouraged due to the need to preserve oil and rubber— the two essential ingredients for humans' fast movement across time and space. The journey from New York to California took several days and required transferring busses several times, often in the middle of the night. At stations, signs were posted demanding "Is this Travel necessary?" (Reminiscences of Theodosius Dobzhansky, 1962, p. 491).

Dobzhansky and his assistants repeated the experiment from the previous summer— testing how far and fast flies fly— on Mount San Jacinto, near Idyllwild. Dobzhansky described this work in letters to Dunn that he labelled at the top — "Letters de la Montagne"— evoking

his sense of isolation from “the events taking place in the valley below,” that “seem[ed] to be going on a different planet.” Here he discovered that the flies’ movement fit the Brownian motion pattern—the random movement of particles in a liquid or a gas. Nevertheless, patterns were established, and the density could be determined. (Dobzhansky & Wright, 1943, pp. 304-340).

Dobzhansky also took time to descend to Pasadena where, as he wrote Dunn, he found “Sturtevant is sourer than ever.” It is notable how hurt Dobzhansky felt about what had happened to their friendship and how angry it made him: “I was sorry to see this fine man degenerate so badly. God, we all must watch out that such does not happen to us. The worst is that if it does happen, we may not notice it, as he undoubtedly does not notice.” (Letter from Theodosius Dobzhansky to Leslie C. Dunn, June 7, 1942. The American Philosophical Society Archives).

A month later Dobzhansky wrote Dunn:

The work is eminently satisfactory. Last year nothing worked out as we wanted; this time almost everything, so far, works out nicely.” (Letter from Theodosius Dobzhansky to Leslie C. Dunn, July 8, 1942. The American Philosophical Society Archives)

They had moved on to testing how far the flies travel in different environments and the population densities in each location. Dobzhansky concluded that observations in nature showed that *Drosophila* disclosed a complex set of habits and they had merely scratched the surface in their work. He added:

I certainly did not mean to say that *Drosophila* is not interested in other *Drosophila*, or is not attracted to restaurants, dance halls, and comfortable apartments. What I do mean is this. In a *Drosophila* city the attractions are distributed more or less at random, to such an extent that in a plot of the size we build the flies need not congregate either in a financial district, or in the Times Square area, or in Central Park. The central point is, then as follows. We release something between 3000 and 4000 flies at one point – the center of our plot. ... we raise the density of the population at that point and in its immediate surroundings enormously. The flies could either (1) escape from overpopulated regions as fast as they can, and travel more slowly in sparsely populated parts, or (2) they may not mind company and travel at uniform rates. The possibility (1) would mean that variance would grow

fast at first and slowly thereafter; the possibility (2) would mean that the increase of the variance would be proportional to time. The possibility (2) is what is actually observed. (Letter from Theodosius Dobzhansky to Leslie C. Dunn, July 8, 1942. The American Philosophical Society Archives)

What did it mean? For now, he was unsure – however, the more exciting news was that his research had attracted the attention of the United States Federal Bureau of Investigation – the FBI. On the one hand, this should not have been surprising. A group of individuals going about at dawn setting up cups on wire stands and measuring distances with string, then returning to the same site in the evening was bound to – in a country at war – seem suspicious. To Dobzhansky, it revealed four conclusions:

(1) collecting flies resembles fifth column work, (2) the citizens of Idyllwild are war conscious, (3) the FBI is efficient, (4) think what poor fellows are the FBI workers if many of the reports they are investigating are of the same nature as this one, and it is almost certain that they are.” (Letter from Theodosius Dobzhansky to L. C. Dunn, July 28, 1942. The American Philosophical Society Archives)

5 EPILOGUE

Thus ended Dobzhansky’s last research trip out west before beginning work in Brazil. He was determined not to repeat the mistake he had made before collecting data without first consulting Wright about what he should be looking for. He wrote to him that, “I have been yearning to discuss the plans of this Brazilian venture with you for God knows how long a time. I still yearn to do so, but it so damnably difficult to do so in letters.” Dobzhansky invited Wright to come to New York, but he was too busy, so instead, Dobzhansky took the train to Chicago for the sole purpose of spending 24 hours with Wright discussing his plans for Brazil. (Lewontin *et al.*, 2003, p. 72).

And with that Dobzhansky set off to discover that tropical climates do have seasons and– more importantly– the fluctuations in gene arrangements he’d been observing in his flies were not the result genetic drift. Still, the seasonal fluctuations: Environment, not population size, was the factor he observed in evolution.

A few other points regarding the above should be taken into account to understand Dobzhansky's future adventures in Brazil better. One, his scientific collaboration with Sturtevant ended badly, and though he would collaborate with Dunn, their work together was primarily about opposing eugenicists and Lysenkoists than about producing scientific knowledge. Two, though Dobzhansky had long, warm, fruitful, collaborative relationships with students who would go on to become important geneticists in their own right— he had, as mentioned above, developed a reputation for being demanding about what his students researched in his last years at Caltech.

Dobzhansky's relationship with his Brazilian colleagues seems to have ended up somewhat similar. Though initially, they were grateful for the knowledge and resources he brought with him by Dobzhansky's later visits to Brazil— after 1948— things had soured. This is primarily because his principal collaborators – Crodowaldo Pavan (1919-2009) and Antonio Brito da Cunha (1925-2019) had developed interests of their own which did not involve *Drosophila* flies and the study of evolution. These events are a subject requiring further research about which there is no doubt much to be written. Hopefully, the above history will help shed some light on this story as it develops.

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