

Dobzhansky and the Brazilians from his letters and reminiscences

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Abstract: Theodosius Dobzhansky (1900-1975) had an important role in the origin and development of evolutionary genetics in Brazil. Not only in 1943, when he had been here for the first time, about six months, but also in other moments, for more extended periods, he was responsible for the formation of the first generation of *Drosophila* geneticists with an emphasis on evolution, who later originated groups of research in evolutionary genetics in their respective states/countries. His relationship with three members of the 1943 group deserves special attention. Dobzhansky's relationship with André Dreyfus (1897-1952), at the time the head of the Department of General Biology at the University of São Paulo, was always respectful and friendly; with Crodowaldo Pavan (1919-2009), Dobzhansky was highly affectionate, although he was worried about the future of his most beloved disciple; Dobzhansky's relationship with Antonio Brito da Cunha (1925-2019) was one of admiration for his general culture and sophistication, however, it was less intense than with Pavan. Moreover, several members of his research team, from 1943 to 1956 deserved comments in his reminiscences (not always positive) for the Oral History of Columbia University, for more than 40 pages.

Keywords: Dobzhansky. Correspondence. The history of evolutionary genetics in Brazil.

Dobzhansky e os brasileiros, a partir das suas reminiscências e cartas

Resumo: Theodosius Dobzhansky (1900-1975) teve um papel muito importante na origem e desenvolvimento da genética evolutiva no Brasil. Não apenas em 1943, quando ele aqui esteve pela primeira vez, por cerca de 6 meses, mas em outras ocasiões, por mais tempo, ele formou a primeira geração de

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geneticistas de *Drosophila*, com ênfase em evolução, cujos participantes posteriormente, criaram grupos de pesquisa em genética evolutiva em seus respectivos estados/países. Seu relacionamento com três participantes do grupo de 1943, merece atenção especial. Com André Dreyfus (1897-1952), na época o chefe do Departamento de Biologia Geral, foi sempre respeitoso e cordial; com Crodowaldo Pavan (1919-2009), foi extremamente afetuoso e preocupado com o futuro do seu mais diletto discípulo, enquanto com Antonio Brito da Cunha (1925-2019) foi de admiração por sua cultura geral, mas menos intenso do que com Pavan. Além desses, vários integrantes do seu grupo de pesquisa, de 1943 até 1956, mereceram comentários (nem sempre positivos), em suas “reminiscências” para a História Oral da Universidade de Columbia, em mais de 40 páginas.

Palavras-chave: Dobzhansky. Correspondência. História da genética evolutiva no Brasil.

1 INTRODUCTION

Theodosius Dobzhansky (1900-1975) was a central name in the establishment of the modern synthesis. His book *Genetics and the Origin of Species* (1937, first edition) became a standard in evolutionary studies for decades (1941, 1951, second and third editions). The series of 43 papers entitled *Genetics of Natural Populations*, published from 1938 to 1976, became a classic in studies on the evolution and genetics of populations in the wild. His influence was particularly significant in Latin America (Brazil and Mexico; for the latter, see Barahona & Ayala 2005a,b). He visited Brazil many times after his first visit in 1943, spending one year in 1948-1949 and another in 1955-1956. As for Europe (including Israel and Egypt), his influence was more restricted (Krimbas, 1995, p. 24).

Dobzhansky's visits to Brazil are now well documented and discussed (Pavan & Brito da Cunha, 2003; Araújo, 2004; 2022; Glick, 2008; Magalhães & Villela, 2014; Monte-Sião & Martins, 2020), so this will not be dealt with in this paper. However, some comments on the lectures he gave to faculty members and other professionals at the Department of General Biology, University of São Paulo (USP), in 1943 deserve to be made. These lectures were originally programmed to be for members of the staff as well as for the research group, then changed to a course on Principles of Evolution, destined for a wider audience (Pavan & Brito da Cunha, 2003, p. 387). Why such a change? Some

speculations can be made: 1. to expand the notion that biology has achieved, finally, a unification through the evolutionary synthesis; 2. the attendants would know the importance of experimental methods to solve problems in evolution; 3. independent of their background, students of different fields such as palaeontology, zoology, botany, and others, were informed that they could speak the same language as far as the mechanisms of evolution were concerned (the language of genetics !); 4. the notion of mechanisms (processes) implicitly would bring the idea that evolutionary biology has eliminated metaphysical explanations and that by the notion that there were “forces” in evolution that could be treated with mathematical models, biology would approximate physics.

2 THE CLOSEST THREE BRAZILIANS

In his Reminiscences, housed in the Oral History Collection at Columbia University, a series of interviews with Barbara Land in 1962, Dobzhansky highlights three members of the Department: André Dreyfus (1897-1952), Crodowaldo Pavan (1919-2009) and Antonio Brito da Cunha (1925-2019). As for Dreyfus, he said:

Perhaps we may just as well repeat: he was an interesting man, a complicated man, personally an unhappy man. It was just during my first stay in São Paulo that he became the Dean of the Faculty of Philosophy, Sciences and Letters. A deanship in Brazil means a lot more than a deanship in Columbia or any other university in this country. [...] That was really the beginning of his unhappiness – no, not the beginning of his unhappiness; it added to his unhappiness. In 1943 Dreyfus was however extremely active. He took me under his wing and decided to use my visit as a means of building up his department of biology, starting genetic research. (Dobzhansky, T. Oral History Memoir. Columbia University, 1962 p. 547)

André Dreyfus was known by his excellent classes in different courses he offered to undergraduate and graduate students. He was born in the city of Pelotas, State of Rio Grande do Sul, the southernmost state in Brazil. He got his degree on Medicine in 1919 from the Universidade do Rio de Janeiro, moved to São Paulo in 1927 to Faculdade de Medicina, later going to the Departamento de Biologia Geral, Universidade de São Paulo (USP). Two personal biographical

accounts of him are from Zeferino Vaz (1966) and Antonio Brito da Cunha (1994).

Pavan was, undoubtedly, the favourite of Dobzhansky's Brazilian disciples. This can be easily seen by the following reminiscences on him:

The person with whom I went chiefly was at that time a graduate student, Crodowaldo Pavan, at present Professor Pavan, cathedratico¹ of general biology, successor of Dreyfus, and a very important man, I am afraid too important to do work. [...] With Pavan we have established, I believe, a lifelong friendship. Pavan was Italian in origin, born however in Brazil. [...] His father was born in Brazil, his mother not. [...] Pavan, who was born and grew up while the family was still not rich or only getting rich, is also a man with great personal kindness – so much so that when engaged in a good hot scientific discussion, he, at least in his student days, was getting so excited that he was literally jumping up and down. (Dobzhansky, T. Oral History Memoir. Columbia University, 1962 p. 549-550)

Some details on Pavan's private life did not escape Dobzhansky scrutiny:

Tremendously successful with women. At that time, he was still unmarried. He was having love affairs right and left, several at the same time. At that time, in spite of all that, he got quite excited about biology, genetics, and he was an exception [...] he not merely could go into the field, he hugely enjoyed going into the field. For me his companionship was most pleasant. (Dobzhansky, T. Oral History Memoir. Columbia University, 1962 p. 551)

Dobzhansky and Pavan exchanged many letters; one of them is of particular interest since it manifests Dobzhansky's feelings about the scientific career of his disciple, mainly after he discovered a new model organism for cytological studies, the fly *Rhynchosciara angelae*, whose extraordinary giant salivary chromosomes, easy to examine than those from *Drosophila*. In a letter from May 26, 1956, Dobzhansky expressed his regrets:

¹ Dobzhansky used the Portuguese word “catedrático” to mean a person responsible for all the activities of a given scientific subject in a science Department. It was at the time, the highest position for a teacher to achieve (it is no longer used).

Meu caro amigo Pavan (My dear friend Pavan):

In few days I shall be out of Brazil, and so my fifth visit is coming to a close. [...] I like you tremendously I admire you as a person. You are really a wonderful guy, with your warmth, honesty, generosity, and unselfishness. [...] I feel that you are one of the best human beings whom I had the luck to meet in my lifetime. Your life, like mine, is devoted primarily to science. Of course, during this year I have thought much about your scientific future. You have many years before you, and if you do the right things you should do honor to yourself and to Brazilian science, and should secure your share of satisfaction which comes from a creative effort.

But I am worried about the possibilities of mistakes which could frustrate your scientific fulfillment. The most dangerous of such mistakes comes from the lure of the spectacular and the difficulty of the sheer hard work on more fundamental but less easily popularized problems. Even a Harry Miller² is still able to admire a good slide of chromosomes. It takes more comprehension than most people have to appreciate the importance of the synthetic lethals. You would not be human if you were not tempted by this more facile though less profound direction of studies. The danger is especially great since you quite systematically overestimate the amount of research that you and your assistants can do at the same time; hence, you may decide to pursue both lines to the detriment of both.

In his *Reminiscences*, Dobzhansky said a few words about Antonio Brito da Cunha, the youngest of his disciples in the leading group at USP:

Another man who, at that time, was merely a beginner is Antonio Brito da Cunha. He is several years younger than Pavan. [...] Now, Antonio da Cunha is about as different a person from Pavan as can be. He is a real intellectual, which Pavan is not. He is widely educated, widely read, not only in genetics or biology, but in general. He's extremely introvert. I understand that more recently he became a real recluse. He had these tendencies already when I first met him. [...] In spite of his very wide

² Harry Milton Miller Jr. (1895-1980), a representative of the Rockefeller Foundation, is known of all geneticists of Pavan's and later generations. The Brazilians much appreciated him due to his friendship and readiness to cooperate with equipment and financial research support.

knowledge and great education, he has not proved to be successful, either as a teacher or as a professor. As a research worker, he was successful... (Dobzhansky, T. *Oral History Memoir*. Columbia University, 1962 p. 553)

Despite these differences, both Pavan and Brito da Cunha were close friends and were hard workers, as is easily seen in this testimony by the later: “Our work schedules were: Pavan’s, from 9:30 a.m. to midnight, and mine, from 7:30 a.m. to 10 p.m.” (Brito da Cunha, 1990 p. 691).

3 DOBZHANSKY’S RECOLLECTIONS ON THE VISITS TO BRAZIL

Two of Dobzhansky’s visits to Brazil more intensely occupy his memories about the country, in 1948-1949 and 1955-1956, as can be seen in the following quotations from his oral history:

One day I really added up the dates and it came up to three years within a couple of days plus or minus, something like that. After three years, one has a lot of feelings about the place; it is said that one understands a place after two weeks, loses one’s understanding after two months, possibly after two years somehow begins to reacquire it. My last trip to Brazil was six years ago, 1956³ ((Dobzhansky, *Oral History Memoir*. Columbia University, 1962 p. 518)

Scientifically that year [1948-1949] was the most productive of all the Brazilian trips. Between 1943 and 1948 – in these intervening years – my colleague Spassky and myself were able to work out two species of *Drosophila*, namely *Drosophila willistoni* and *Drosophila prosaltans*⁴, prepare them for more detailed, more intense work, which was to take place later – it took place in 1948-49. That was dull work, technical work. I owe it to Spassky, who did it extremely well. That is published in two papers in *Heredity*. (Dobzhansky, *Oral History Memoir*. Columbia University, 1962 p. 559)

³He would return in July 1966 for the International Symposium of Genetics, held at Piracicaba, São Paulo – he was then chosen as the Honorary President of the meeting.

⁴Usually, the name of a species is written in italics. However, in the typescript of the Oral History, they were not.

Not only Dobzhansky and Spassky worked hard, but the Brazilian fellows too. An analysis of the number of papers published without Dobzhansky co-authorship during the period 1943 and 1948 by the Brazilian team shows a total of 24, with an average of 4 per year (data obtained in Magalhães & Vilela 2014, suppl. 1).

There is another side of Dobzhansky's personality, perhaps unknown to most people, which appeared in a letter to Sewall Wright on December 28, 1948:

Dear Wright:

It's more than a month since we arrived in Brazil, and we feel quite settled now. The external conditions of our life are simple enough. A 'pensão'⁵ in which we have a two-room apartment and all the meals, about 15 walking minutes distance from the laboratory in which Natasha and myself are working, some 30 minutes direct bus connection to Sophie's American school. [...] It was less funny when the Professora⁶ Rosina de Barros, the first assistant of Dreyfus (and she never misses the opportunity to point out that she is the first assistant, while Pavan is merely the second, while Antonio da Cunha is no less than the third) decide to use DDT as a precaution against mites.⁷ There are compensations. About every week we go for two day long collecting expedition, using the station wagon presented by uncle Rockefeller to this laboratory. This mean that we spend some good long hours in the jungle. And this fact compensates not only for a lot of chocolate-colored gentlemen but even for several first assistants.

If this extract about Rosina de Barros (1909-1994) surprised the reader, the last part, about one of the "compensations", is a shock⁸. In

⁵ "Pensão" is a kind of simple cottage available at low prices

⁶ Professora" in Brazil means a professorship owned by a woman. I think Dobzhansky is being ironic here since Rosina de Barros was not a "Professor" at the time. It is worth mentioning that "Professor – Professora" has a more general meaning in Brazil, the same as "Teacher".

⁷ Rosina de Barros was an outstanding researcher who was not a member of Dobzhansky's group, although she worked with *Drosophila* and other organisms. In this letter, it is explicit how Dobzhansky disliked her. See additional comments on her in Araújo (2022, pp. 511- 512).

⁸ Positively, it is a shock, mainly coming from a person who wrote nice books such as

his reminiscences for the Oral History of Columbia University, where he spoke freely, there are other very interesting parts:

Now, I probably sound as saying Portugal and Brazil are 100 percent wonderful, the rest 100 percent bad. I don't mean to say that at all. Of course, this dedication to pleasure has clearly disadvantages. The dedication to pleasure makes very frequently sad results if you have to do any work. Everybody is tremendously enthusiastic to start with, but when it comes to work, it just isn't done. In particular, nothing is every prepared in advance, even when a preparation is necessary. My very good friend, Professor Pavan, - he's of pure Italian descent but culturally 100 percent Brazilian - cannot do anything for tomorrow. Tomorrow will take care of itself. It quite often doesn't take care of itself. That has some disadvantages, but there exists this difference (Dobzhansky, T. *Oral History Memoir*. Columbia University, 1962 p. 524)

Two others of his collaborators, however, deserved eulogies:

...there are two people in Brazil who are exceptions. One is Chana Malogolowkin [later, Malogolowkin-Cohen], at present in Columbia University a second year, who, as her name very clearly suggests, is really not Brazilian. She's of Russian Jewish origin. She certainly works, which is not surprising, because she is not a cathedratica! We are very good friends, so I many times asked her, "Chana, tell me, when you will be a cathedratica, will you work or will you drop work?" (Dobzhansky, T. *Oral History Memoir*. Columbia University, 1962 p. 524)

Chana Malogolowkin-Cohen (1925-2022), one of the founders of the Sociedade Brasileira de Genética (SBG - Brazilian Society of Genetics) in 1955, published important papers during two consecutive years she spent with Dobzhansky at Columbia University: one-page paper from 1957 (with D.F. Poulson as coauthor), they described a case of maternal inheritance of abnormal sex-ratio in *Drosophila willistoni* (Malogolowkin & Poulson, 1957). They concluded: "It is demonstrated

The Biological Basis of Human Freedom (1956) and *Genetic Diversity and Human Equality* (1973).

⁹ Despite these well-tempered comments, Dobzhansky did not appreciate Chana Malogolowkin's marriage. Professor Fábio de Melo Sene (1942-2023), who was her colleague at USP, sent an e-mail to the author of this paper where he says: "I remember that one of Chana's regrets was that Dobzhansky broke with her since her wedding followed by her move to Israel. He didn't even answer her letters." (Fábio de Mello Sene. Personal communication, March 22, 2022).

that the ‘sex-ratio’ condition in *Drosophila willistoni* can be transferred to normal females, and that it is essentially infectious in nature”. (p. 32). In the following year she published as a single author another paper, showing that not only *D. willistoni* had a ‘sex-ratio’ inheritance but also *D. paulistorum* (Malogolowkin, 1958). The third paper, with *D. equinoxialis*, showed that the infectious agent could be exterminated by temperature treatment (Malogolowkin, 1959).

In the sequence, Dobzhansky says in his Reminiscences:

The other is Warwick Kerr¹⁰, who is also not cathedratico yet, although his position is quite safe, but he is in a provincial university, place called Rio Claro in the interior of the state of São Paulo. He was also here a month or six weeks ago, on his way to Europe as representative of Brazilian National Research Council or something of that sort. I just wonder if that’s the beginning of the end. He became also an influential man. (p. 532)

For a similar reason, he wrote a letter to a graduate student, in 1966, who had just received a doctoral degree from Faculdade de Filosofia, Ciências e Letras, in São José do Rio Preto, State of São Paulo, and who was planning to stay a year with him, studying the genetics of *Drosophila*:

Let me congratulate you with your doctorate, and wish you that this achievement be the beginning, not the end, of a long, productive, and successful scientific career. It so often happens to be the end; this happens everywhere, but as you know and I know it happens to be the end most often in Brazil, because of the characteristics of the ‘ambiente’¹¹ (‘environment’). (letter from December 7, 1966)

Dobzhansky’s last prolonged stay in Brazil was in the year 1955-1956, the most turbulent of all; his memories of this period are worth

¹⁰ Warwick Estevan Kerr (1922-2018) was twice the President of the Brazilian Society of Genetics, in 1964-1966 in the beginning of the hard years of the military regime in Brazil. Indeed, in 1964 when he was arrested for four months, he had received for a period of one year the British biologist William Hamilton (who, in consequence stayed with him only 8 months). The other period as President of SBG was in 1994-1996.

¹¹ “Ambiente” is the Portuguese word to “environment”, although here Dobzhansky referred to “surroundings”.

comparing with that of one group member, who was indeed accused of having caused the problems. In his reminiscences, he frankly says:

1955-56, I regret to say, was the least pleasant trip of the whole bunch. Here the Rockefeller Foundation did an even more generous thing. It gave grants for a series of people to come in to work, among them my very good friend Charles Birch, the Australian, an Italian by name Bruno Battaglia, a complete worthless Dane¹² who did absolutely nothing – I even forgot his name. Just as well to forget him. And a group of Brazilians¹³. Now, this was an ill-starred expedition. The project this time was to work on the little islands in the bay of Angra dos Reis. This is a bay, rather deep bay, of the coast between Rio de Janeiro and São Paulo. It has a large number, probably something like a hundred of islands of assorted sizes. The work which we planned was to release on these islands *Drosophila willistoni* with certain mutant genes or certain chromosomes. Of course, every one of these islands had its own native population of *Drosophila willistoni*. The project then was to see what happens when you introduce flies, hybridized with natives, what will be the effect of natural selection, working on this mixture? (Dobzhansky, T. *Oral History Memoir*. Columbia University, 1962 p. 576)

In the following pages of his Reminiscences, he concludes that:

...the end was tragic, as far as the scientific work may be tragic. Just exactly what happened, we never will find out. I strongly suspect that one of the graduate students who was given a sort of a subsidiary problem, which was to release flies marked with a certain – flies of a different species, *Drosophila paulistorum*, marked with certain dyes, to come with a number of flies in the island (I hope by mistake), released *Drosophila willistoni*¹⁴. In short, the whole thing caved in, and a year's work, not by myself but by others, was almost dead loss. The only thing which came out of that were purely secondary matters¹⁵. I stayed there

¹² The Danish member of the group was Ove Frydenberg (1928-1975).

¹³ A complete list of the people who was working in the project of Angra dos Reis is in Araújo (2022, p. 515).

¹⁴ In this quotation, from the Reminiscences, it seems that Dobzhansky made a mistake. After all, it was planned to release *Drosophila willistoni* on the islands as can be seen by the previous quotation.

¹⁵ Concerning publications, it is interesting to note that in the period after Dobzhansky

to do a little more traveling in Brazil, a little more collecting in various places, to study the chromosomal variation in *Drosophila willistoni*. The last paper on that was published, I think – of course, later, I think in 1959 or '60. That is almost the sum total of what we were able to do (Dobzhansky, T. *Oral History Memoir*. Columbia University, 1962 p. 578-579)

The one supposedly responsible for the mistake was Luiz Edmundo de Magalhães (1927-2012), who at the time was getting data for his doctoral dissertation at Pirassununga, State of São Paulo, under Crodowaldo Pavan's guidance. The main subject of his thesis was to estimate the dispersal rates and population size of *Drosophila willistoni*. Of course, with the help of the “complete worthless Dane”, in Dobzhansky's own words, Ove Frydenberg. Luiz E. Magalhães became a successful teacher and researcher at the University of São Paulo for a while, then transferred to the University of São Carlos (state of São Paulo), where he became Dean; he was also the president of the Brazilian Society of Genetics from 1982-1984. He rarely talked about this episode in public; his version is expressed in a paper published posthumously, with the help of a colleague, Carlos Ribeiro Vilela (Magalhães & Vilela, 2014). It is from this publication that some parts are transcribed below:

The group was very excited and full of hope regarding the great research project. [...] Pavan and Dobzhansky held a meeting with the most representative members of the group, to expose the project and discuss the execution. That was where the problems started. Dobzhansky exposed the project that he has conceived himself, but, to his great surprise and displeasure, the fresh PhD Ove Frydenberg, the youngest in the group, presented severe criticisms, contesting its viability. The project did not tested the intended hypotheses. It was statistically inconsistent, so there was no justification whatsoever to perform an experiment known, from the beginning, to be impossible to work out. (Magalhães & Vilela 2014, p. 140-141)

left, between the years 1957 and 1959, the total number of papers published without-Dobzhansky's co-authorship, dealing with different aspects of the genetics of *Drosophila* species was 24, which gives an average per year of 8 (see the complete list in Magalhães & Vilela 2014, suppl. 1).

Magalhães and Vilela's description continues:

...Dobzhansky did not give in to his arguments. He answered that, in his opinion, there were only two kinds of science: the innovative and the non-innovative. [...] Frydenberg was excluded from the team, but kept his fellowship from the University and developed several other projects on his own. [...] The episode regarding the disagreement between Dobzhansky and Frydenberg receded somewhat, but Dobzhansky was still very upset by Ove's presence, whom he did not even greet. (Magalhães & Vilela, 2014, pp. 141-142)

The sequence of this text is particularly important:

One morning, Pavan approached Luiz Edmundo and went straight to the point, saying: - "Look, Dobzhansky wants you to stop working in Pirassununga and go prepare your doctoral work in Angra dos Reis. Check if there is any migration of flies among the islands and determine the size of the local populations. He said that, if you don't agree with this, he will abandon the program and go back to the US and write to all his friends saying that it is not possible to do scientific work in Brazil". [...] What interest could Dobzhansky have in the young Luiz Edmundo and his work? [...] Actually, the real reasons of the "old man's" move would become clear to us only later on. [...] Suddenly, in certain samples, markers started to appear, i.e., certain inversions which did not exist in these populations nor had they been introduced, were found at high frequencies. Dobzhansky immediately drew his conclusions: - "Frydenberg and Magalhães are sabotaging the experiments so they won't succeed!". (Magalhães & Vilela, 2014, p. 142)

The fact is that Antonio Brito da Cunha decided to go on with the work by himself and found later, in one of the drawers of the worktable that Dobzhansky used to observe flies in the photomicroscope, a notebook where he registered the presence of a genetic marker, an inversion in chromosome II of *Drosophila willistoni*, that were common in the samples from Belém do Pará. Magalhães and Vilela conclude their narrative with this comment:

Dobzhansky had disparaged this finding when he started the experiment, considering this marker to be absent in the island populations. This is the actual explanation: the marker already existed in the population of one of the islands, so its presence in the samples needed no explanation, for it was already there. It is regrettable that a group of so

highly qualified researchers, of great scientific relevance, had to go down such a suspicious path, supported by vanities and prides. (Magalhães & Vilela 2014, p. 142)

The above facts represented the end of Dobzhansky's collaboration with Brazilians; he returned to this country to participate in examination committees and to participate in the International Symposium on Genetics in Piracicaba, state of São Paulo, in 1966.

4 CONCLUSIONS

Dobzhansky has been described as charismatic, friendly, and with an extraordinary capacity for writing and a solid scientific and cultural background. He deserved indeed all these qualifications. However, like any other human being, he had his downside. His authoritarian behaviour became apparent with the episode of Angra dos Reis (1955) and his animosity toward Luiz Edmundo de Magalhães and Ove Frydenberg. Earlier, in 1948, it was against Newton Freire-Maia (1918-2003) who decided to marry, contrary to the expectations of the “boss”; the price he paid was the exclusion from the group after returning from the honeymoon. His admiration towards Chana Malogolowkin did not prevent him from breaking off his epistolary communication with her after her wedding. Some questions remained unanswered: why his antagonism towards Rosina de Barros? Why was she not a member of the research group? After all, she later (post-1943) worked with *Drosophila*. Two related questions are: why did she fail André Dreyfus as the head of the Departamento de Biologia Geral after his death? Why Natasha, his wife, who worked as others in the team, when they were in Brazil, coauthored only a single paper? (Pavan *et al.*, 1951)¹⁶ She was a biologist like him.

Another interesting aspect of Dobzhansky's behaviour appears when he comments on the work the Brazilians have done. Although he recognized the Brazilian collaborators' intellectual capacity, he usually depreciated the works done without his participation as “subsidi-

¹⁶ Pavan, C; Cordeiro, A.R.; Dobzhansky, N.; Dobzhansky, T.; Malogolowkin, C.; Spassky, B. and Wedel, M. 1951 – Concealed genic variability in Brazilian populations of *Drosophila willistoni*. *Genetics* 36 (1): 13-30.

ary” or “minor” works. Maybe this expresses his competitive personality. His letter to Pavan on May 26, 1956, reproduced in part here, can be viewed as an attempt to keep Pavan under his control. Of course, this is speculation, but not devoid of interest.

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