Interview with Theodosius Dobzhansky (October, 1966, Rockefeller University, New York)

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Introduction

In October, 1966, I had the good fortune to be able to arrange an interview with the late Theodosius Dobzhansky, who was then still at Rockefeller Institute (now Rockefeller University) in New York. I spent several hours with him on a Saturday afternoon, asking him about his early interests in genetics and evolution, and particularly about his work with Thomas Hunt Morgan (1866—1945) at both Columbia University and at the California Institute of Technology (Caltech, as it is generally referred to). I was at the time preparing to write a biography of Morgan, having just completed my Ph. D. dissertation on his genetic, evolutionary and embryological work. Dobzhansky was very lively and clearly enthusiastic about Morgan and his own experiences in the Morgan laboratory. Although I used many ideas and quotations from this interview in my biography of Morgan[1], the full text of the interview was never transcribed until 1992, and was never published. It is offered here as part of the record of Dobzhansky's life and work that is now being collected in Russia.

DOBZ.: You told me, or Ernst Mayer told me, that you had already interviewed one of Morgan's daughters. Which one?

G. A.: Isabel. She is the youngest and she and her husband invited me to Mt. Kisco, which was very informative.

DOBZ.: And also [he said] you interviewed Muller and Sturtevant? Did you get compatible or incompatible accounts? [*much laughter*].

G. A.: Sometimes compatible and sometimes incompatible.

DOBZ.: Mostly incompatible.

G. A.: They had very different interpretations, but both were trying to be fair, and present their side of the story.

DOBZ.: Yes — you know, there will be two histories of genetics published soon, one by Sturtevant, and one by my good friend L.C. Dunn. I think reading them will be an amusing experience.

G. A.: I visited Sturtevant in Woods Hole and he showed me galley proofs of his book, but I didn't get to read very much. I met with Dunn just two weeks ago as he was coming through Cambridge, and he told me something of what his book is about.

DOBZ.: I have read several chapters of Dunn's book. You know, a chapter of Sturtevant's appeared in <u>American Scientist.</u>

G. A.: Have you read any other parts of Sturtevant's book?

DOBZ.: No, just that. You know that I, along with a Japanese student, was the last foreign person to come to the "fly room".

G. A.: And what year was that?

DOBZ.: 1927, so I had just a year (before Morgan left for Caltech] in the fly room, but I have seen it ... [*laughter*].

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[1] *Allen G.E.* Thomas Hunt Morgan: The Man and His Science. Princeton: Princeton University Press, 1978. 447 p.

G. A.: Did you come specifically to work with Morgan?

DOBZ.: That's right. You see, I came from the University of Leningrad as a Fellow of the Rockefeller Foundation, at that time it was called the International Education Board. I came for a year, hoping perhaps to stay two years, and stayed the rest of my life [*laughter*].

G. A.: A long two years! I am interested in what kind of biological education, in genetics and evolution, you had before you came to this country.

DOBZ.: Well, I don't know how that relates to Morgan, but I can easily tell you about [my biological education]. You see, in Russia, evolution was always considered to be a matter of utmost importance, both philosophically and even sociologically. You may or may not know but to a number of Russian naturalists and thinkers in the 19th century Darwin was not just a scientific theory but the basis for a whole philosophy.

G. A.: They made it more of a philosophy than happened in Europe?

DOBZ: Oh yes, if you read any of their writings you will see that at once. And that sort of temptation continued and as you well know they had some unfortunate consequences in the form of Mr. Lysenko.

G. A.: Yes.

DOBZ.: And that [Darwinism] was, you see, the sort of background that every one, at least in biology, had.

G. A.: What was the connection made between evolution and genetics in Russia?

DOBZ.: Now, you see, that is a more difficult question. My teacher was a man by the name of Sergey Kurshakevitch (1878–1920) at the University of Kiev. He was not a geneticist in the modern technical sense, but I am sure if he had lived longer than he did, he would have gone into genetics. He was a student of Richard Hertwig in Germany. He called himself [Dobzhansky's teacher, that is] an experimental zoologist — you know Morgan always called himself an experimental zoologist, not a geneticist. One of his early books, one of his best books, is called *Experimental Zoology*, and it was pre-genetical. So Kursekevitch was an experimental zoologist, he worked on sex determination in frogs. Then he became a cytologist, working on spermatogenesis in some snails; he was very clearly going in that direction [toward genetics] but he died in 1920. Now, during the first World War and the Revolution there was very little literature — foreign literature — in Russia, and it [Mendelian genetics] appeared in two places in 1921, in Vavilov's Institute in Petrograd … and in Moscow in Kolt'zov's Institute.

G. A.: Appeared in what form?

DOBZ.: Well, you see he [Vavilov] went to Western Europe and this country [United States] and brought back with him I don't know how many pounds of biological, agricultural, literature, files of journals and everything else but it was available only in these two places, in Vavilov's Institute in Petrograd, and in Moscow in Kolt'zov's Institute so that young fellows had to go to one of these two places if they wanted to read [this literature]. Later my chief, by the name of Filipchenko, was Professor in Petrograd, later Leningrad, made a sort of review article in a journal called [*Nature*] which presented the work of the Morgan school. I have not seen this article for many years, probably it would be fun to see it now, to see how well it was presented. That was a sort of revelation.

G. A.: That was about what period?

DOBZ.: I would say it might be 21st or 22nd [1921 or 1922]; that, as I said, to the older biologists was a revelation. Now in 1924 I went to Leningrad to work with Filipchenko. I was his, what is it known in Europe as his "assistant", probably would correspond to something between instructor and assistant professor here. I came — the date can be established very precisely — I arrived on the night of Lenin's death, so I was for probably three and a half years with Filipchenko there. And of course, by that time, genetics was *the thing* and Morgan was a hero or a saint [*laughter*].

G. A.: Did Filipchenko pick this up from Vavilov?

DOBZ.: Yes, you see, the scientific literature was by that time completely available.

G. A.: Yes, I am just interested in the lines of transmission of scientific information at that time.

DOBZ.: I probably heard for the first time about the work of Morgan from this article by Filipchenko. But by 1927 — and that may sound at present most fantastic — but at that time the genetical literature was still something manageable. By 1927 I think I could have made a statement that I had at least seen every paper published on genetics up to that date. I don't think anybody can possibly achieve that now.

G. A.: That's true.

DOBZ.: Well, at that time the Rockefeller Foundation and the International Education Board, was active. A number of people went to foreign countries from Europe and Russia. In Morgan's laboratory, you know, there was Curt Stern, from Berlin. He spent two years in Morgan's laboratory just before my arrival. I met him a little later, but he was there before I was. There was no question that if you got a fellowship that was where you had to go.

G. A.: The Morgan lab was then a center of genetic research at the time?

DOBZ.: Oh yes, there was no question about it.

G. A.: One question that has come up in my mind is, for what reason do you think that genetics flourished not only so much in Morgan's laboratory, but after 1910 it seemed to center itself in America,while there had been important genetic work going on elsewhere in England, France… but it never carried through.

DOBZ.: Now, look, let's not be so completely nationalistic [*laughter*]. There was some genetics in England; after all [William] Bateson was an Englishman, and Bateson did a lot. There was considerable genetics in Scandinavia.

G. A.: That's something I don't know very much about.

DOBZ.: Yes, there was a very important genetics school. As a matter of fact in plant genetics that was the foremost school…

G. A.: You mean [Wilhelm] Johannsen?

DOBZ.: Johannsen was a Dane, and that was in Copenhagen. But in Svalof and Lund there was a group of — a number of geneticists — including [Hermann] Nilsson-Ehle working on genetics of wheats and other cereals. There was genetics in Uppsala, Oslo, incidentally the foremost Norwegian geneticist was also a student of Morgan, Otto Mohr. So, you see, Scandinavia was an important center. In Germany you had a considerable number of geneticists.

G. A.: Where were these centered in Germany?

DOBZ.: In Germany, you see, it was not centered in any particular place. You know in Germany you always had several universities in various places. Now you had people like [Theodor] Boveri, a cytologist and certainly one of the founders of genetics, there is no doubt about that. Again, Hertwig, also a cytologist/geneticist — that was at München [Munich]. Then there was the Max Planck Institute, but at that time it was called the Kaiser Wilhelm Institute für Biologie, in Berlin-Dahlem; there was [also] the Erwin Baur Institute, historically, of considerable importance; there was curiously enough the Institute for the Study of the Brain. That was headed by a man named [Oskar] Vogt, he was interested in genetics as a psychiatrist, and also in evolution. He, among others, invited one of the best Russian geneticists [Nikolai] Timofeef-Ressovsky — he [Timofeef-Ressovsky] was still living in Russia at the time. We left Russia about the same time, he went to Berlin and I to New York. The country which had very little genetics at that time was, curiously enough, France. Why that was so, I cannot give you an explanation. So, if you take genetics from let's say 1910, if I may be so bold to say to a Harvard man [laughter], United States was not necessarily in the first place. It was put in the first place by the Morgan school.

G. A.: Yes, I see, my question was not really meant to be along nationalistic grounds. What I was really trying to get at was something that Muller had first mentioned, and that was the Mendelian school had flourished in the United States, and I should have asked my question about that. Because certainly the study of heredity had originally flourished much more in Europe than here.

The real point of the question was why did Mendelian genetics seem to take up so much more fully in the United States after around1910 than elsewhere?

DOBZ.: Yes, after 1910, then it was surely the Morgan school. Before 1910 there certainly were people [doing genetics in the United States] such as [William E.] Castle, [Edward M.] East, and others, but before 1910 I would not say that Mendelian genetics was any stronger in the United States than anywhere else.

G. A.: Was it pretty much Morgan's way of doing things that was responsible for this?

DOBZ.: Most people saw the significance of Morgan's work, and that was most exciting. There was, of course, some opposition. There were some old fogeys and some not-so-old fogeys, who violently opposed Morgan.

G. A.: Was there any strong opposition within Columbia to the Mendelian-chromosome interpretation?

DOBZ.: Certainly not by the time I came.

G. A.: Was there any that you heard about prior to that?

DOBZ.: Well no, not really. Morgan and E. B. Wilson were the foremost authorities [at the time], but there was opposition — violent opposition — as you probably know from Henry Fairfield Osborn, who at that time held forth as president of the American Museum [of Natural History]. Now, Osborn referred to genetics as DeVriesianism.

G. A.: Oh he did? He outwardly opposed genetics?

DOBZ.: You can read in the *American Naturalist*, I think, as late as 1930 Osborn published an article where he said that all this *Drosophila* business is nonsense, and has nothing to do with nature, it's just pathological changes. You have instead DeVriesianism, and aristogenesis [his own invention] and of course in good old Harvard University there was a gentleman by the name of [Edward Charles] Jeffrey. Well now Professor Jeffrey, who lived to be quite old — when he died I'm not quite sure [it was 1952] — but he was violently opposed to Morgan and genetics. He believed everything was due to hybridization. He published an article in 1924 or '25 in the *American Naturalist*, an article in which he claimed that *Drosophila* was a hybrid — a between what and what was not quite clear — but also the chromosomes are completely unreliable, their number is variable; and while that made some impression some places, most people just laughed at him. He was still active in at least 1930.

G. A.: What was his training do you know?

DOBZ: He was an eminent botanist, I say, professor of Harvard University [from 1907–1933], with all the authority attending to that position.

G. A.: Why do you think many of the evolutionists, like Osborn, in the early part of the century were not amenable to accepting Mendelian genetics?

DOBZ.: I'm not sure how much I need to talk about it, you probably have enough information on that, you have the thing going back into the 19th century.

G. A.: (Interrupting) Yes.

DOBZ.: At that time, Lamarckism was a respectable hypothesis. I would say probably, of the people writing about evolution especially in continental Europe, Lamarckism was probably the predominant view. And that was of course genetics again that caused the change. Historically, you surely know the importance of Hardy-Weinberg; there was this divorce between genetics on one side and evolution on the other; they were largely independent; most evolutionists were comparative anatomists, systematists, paleontologists, with proper academic lag [for new ideas to catch on]. They knew little about genetics; of course many geneticists were not evolutionists, a situation that I think you will find persists until now. You may find that your neighbors at Harvard are not interested in evolution. It took approximately thirty years to put the two together — Hardy-Weinberg, 1908, and next after that, you may or may not know, a man by the name of [Sergei] Chetverikov in 1926 published an article — it had very little if any influence outside of Russia —

it has now been translated and published in the *Proceedings of the American Philosophical Society*. Do you know that piece?

G. A.: Oh yes.

DOBZ.: And now it can be read in English translation. In 1926 he [Chetverikov] really had the essentials of what we call now the biological theory of evolution or what George Simpson called the synthetic theory of evolution. I prefer the name biological because to many people the word synthetic means something not quite real and exact (laughter). The culmination of that is of course the triplet of [J.B.S.] Haldane, [R.A.] Fisher, [Sewall] Wright — put them in whatever order you want — who independently of each other, actually, not knowing anything about Chetverikov, around 1930, outlined the theory of evolution as we have it now — outlined, as you know, for the first time in the history of biology by pure deduction from a single fundamental postulate of Mendelian segregation.

G. A.: Yes, I just finished reading Fisher's book a couple of weeks ago.

DOBZ.: Now, there are some patriotic Britishers who want to make Fisher *the one*, as a matter of fact Gavin DeBeer published a book, "Darwin, Mendel and Fisher". Now Fisher was quite clearly one of the founders, but he is one of the *three* founders — to make it Darwin, Mendel and Fisher is probably not quite right either. So, you see, the opposition — well, it would not be quite right to say it has vanished, it has not vanished and probably never will — but it is in 1931–1932 that the theory of evolution was put on a modern basis. Now, as you know, Morgan's books on evolution, although they are quite important in their own time, really do not belong into this [stream, or tradition]. Morgan, either implicitly or explicitly thought … if you have mutations, and a few lucky mutations put together make a new species — [Hugo] DeVries, you know, thought a mutation creates a new species all at once. Now, of course, what he was talking about is what at present is sometimes called a genotype or biotype or fundamental species not the Linnaean, biological species at all. Now Morgan, I think it is fair to say, was not particularly interested in species. To Morgan, species and evolution in that sense was a bit secondary. What he was really interested in, and that is certainly what he did, is [to study] the origin of hereditary variations — the origin of what we now call the raw materials of evolution.

G. A.: Right.

DOBZ.: Now that is what the Morgan school has produced. Of course, evolution as we see it has three levels: first level, the origin of hereditary materials, hereditary variation; second, is the way they are combined together into adaptive systems by natural selection and genetic drift; the third is reproductive isolation, packaging. My favorite analogy is the manufacturing process. If you want to build a building you get a supply of bricks, cement and so on, and then you build the house, and then finally you paint it. If it is the manufacturing process, then you package it, you put in cellophane [*laughter*]. Now you have these three levels in the evolutionary process. Morgan's attention — and you can see it very clearly in his work — was almost entirely on the first level, which to be sure was a key.

G. A.: Sure, but it was not the same thing.

DOBZ.: Exactly, yes.

G. A.: Well, Morgan was terribly anti-evolution up until about 1915, or 1914.

DOBZ.: That is certainly not true. He was never anti-evolution.

G. A.: Well, anti-natural selection.

DOBZ.: That, you see, is a different matter [*chuckling*].

G. A.: Excuse my slip. I was wondering if you had any idea what factors may have caused him to change his mind?

DOBZ.: Let me start from a different angle. You know Bateson, whose interest in genetics came from his interest in evolution. Bateson was interested in evolution before he became a geneticist. In 1920 [actually December 1921 — January 1922] he came to the AAAS meeting which I think was in Toronto. At that meeting he gave the famous address with the title, "Evolutionary Faith and Modern Doubts" [published 1922]. Have you read it?

G. A.: No, I haven't.

DOBZ.: I highly recommend it. That address, by the way, played a very unfortunate role that was exploited by Lysenko and before him by Timiariez, who said, "Now here you are, this guy is against genetics [*chuckle*]." What he [Bateson] really said if you read it, was that what we know about genetics does not exactly add up to an explanation of the evolutionary process, and indeed it didn't. You had to have Chetverikov, Fisher, Wright and Haldane to make the bridge.

G. A.: Right.

DOBZ.: Now, to say that Morgan was ever against evolution is, to say the least, unfair. But what he … again if you read *Experimental Zoology*, or *A Critique of the Theory of Evolution* of 1915 or 1916 [1916, Princeton University Press], you find the man pointing out that there is a gap between what we know about what we at present would call the origin of evolutionary raw materials and the evolutionary process as such — and indeed there was such a gap! With the benefit of hindsight you can say it was an unfortunate phraseology. It should have been expressed in just those terms. We have processes which intervene between the production of mutations and the formation of species or macroevolution. These processes have to be studied.

G. A.: Have you read a book Morgan wrote very early in his career, in 1903, called *Evolution and Adaptation*?

DOBZ.: Yes. In fact, I think I have it. Again, what Morgan …here, again, it would perhaps not be irrelevant to say that this [book] is partly a matter of personality. Morgan liked to shock. I think the best way to describe him is that he was sort of inclined to impish behavior. Now, everybody believed in evolution, but what evolution meant then was phylogeny, the biogenetic law. If you study the development, you read off the phylogeny. [indistinguishable word] it seems like that is the basis of comparative morphological evidence. That he [Morgan] wanted to say, and said, and perhaps said it in a way that now sounds strange, is that these things do not necessarily hold water. And they didn't!

G. A.: He brought up in that book, and in several articles published within the period 1903 to 1908 or '09 — after which for several years he stopped writing about any topic relating to evolution — that natural selection was not the main means of the origin of species, that there were other processes, other mechanisms involved. He was a strong De Vriesian at that time.

DOBZ.: At that time natural selection was conceived to be … well, sometimes an analogy used even at present, the sieve analogy. What happens is you have a sieve and you have some favorable things that are retained, and the rest are lost. Now, on that basis, you see, if you have some changes that are considered to be sudden or drastic changes, adaptation becomes something of a mystery, because then you have to ask why the change can be adaptive in the first place. That sort of approach — the latest one you can find — is in the books of [Richard] Goldschmidt. Of course, Goldschmidt was saying that Morgan, and the Morgan school, were violently opposing him. But if you wish, philosophically, at least, that was the same point. You have adaptive changes; now, why the changes should be adaptive… well, the solution to this problem is not absolutely clear to many modern biologists. So, the famous analogy — you doubtless know the analogy that if a million monkeys pounded a million typewriters one of them might produce Hamlet — Darwin himself — you know this famous sentence of Darwin's that the human eye to this day gives me fright or something like that; what you have to realize [is] that evolution in modern terms, and a very fashionable term, is a cybernetic process. Now, I think it would be hard to blame Morgan and Bateson for not having figured that out in 1900. I think you can blame, or reproach them for having expressed it in language which sounded anti-evolutionary. Well, we say rash expressions. But I do not think it was ever true that Morgan was anti-evolution.

G. A.: Well no, that was a slip of my tongue. But he did use the arguments that were common in the later 19th century against Darwin: (1) blending inheritance would dilute the effects of any new variation, and (2) a complex structure like the eye or the process of regeneration could not come about by natural selection, because the initial stages would not have any adaptive value.

DOBZ.: And at that time, again…

G. A.: I'm not blaming him, but I'm wondering if what caused him between about 1910 and 1915 to have so changed his mind that he could present a much clearer picture of evolution and adaptation than he was able to do earlier…

DOBZ.: Obviously the discovery of mutations in *Drosophila*, the realization that mutations do not create new species, mutations are all sizes so to speak, big and small. Mutations can be regarded as the source of raw materials on which selection acts. Mind you he didn't even say that explicitly in 1915. But, I thought you would ask something else. You know, of course, before 1910 Morgan opposed Mendelism.

G. A.: Yes, I'm getting to that.

DOBZ.: He didn't … I say it isn't true that he opposed evolution or natural selection. He pointed out that natural selection — the story of natural selection — could not be made self consistent. But he was *contra* Mendelism. But then I may say, I have tremendous respect for Morgan. Incidentally, somebody should write Morgan's biography. Indeed, Morgan is one of the people that you can write full tomes about, holding back nothing, and leave him a great man.

G. A.: No question about that.

DOBZ.: All the obituaries, probably a couple dozen of them, were written by Sturtevant, and obviously quite inadequate that way. That is maybe not your job, but I think somebody would probably do a real, honest-to-goodness biography of the man. He was a complex person full of contradictions, not biological alone, but much beyond that. I say, and as you probably also know, [Herman Joseph] Muller, and also Sturtevant and to a lesser extend [Calvin B.] Bridges consider that Morgan was, so to speak, given credit for their discoveries. Muller wrote about it. [Alfred H.] Sturtevant didn't but before Morgan got his Nobel Prize he [Sturtevant] was numerous times making this…

G. A.: Muller was making this claim?

DOBZ.: No, Sturtevant was making this claim. I was sitting in the same room with Sturtevant, but I don't know whether you do or do not know that when Morgan got his Nobel Prize he divided it into three equal parts, which went respectively, to his own grandchildren, the children of Bridges and the children of Sturtevant. That was a very characteristic act on Morgan's part.

G. A.: He was a very generous person, I understand.

DOBZ.: Well, there again — Morgan was a highly complex and contradictory person. He was more tight-fisted than you can imagine with laboratory money, but very generous with his own. I know that very well, because in the l930s, depression time, there were a lot of students who did not have dinner every day. Since I was the youngest member of the group I had more contact with graduate students and others. Time and again Morgan would say, "Is it true that X is in a tight situation?" He would invent some work for him to do around the laboratory. The fellow would believe he got a job for which he was being paid by the Institute. It came from Morgan's own pocket. I am sure that most persons concerned do not know to this day that their salary came from Morgan. At the same time, to get two dollars from Morgan to buy something for the laboratory was almost impossible. In that respect, it was just plain ridiculous [*laughter*]. There you have this contradiction: personal generosity and institutional penny-pinching.

G. A.: Do you know why Morgan was anti-Mendel, anti-Mendelism in this period 1903–1910?

DOBZ.: I can only guess. And there I may say one small thing about Morgan: in the library of the biology department [at Columbia] there were bound copies of Morgan's reprints. I don't know, you possibly know the full bibliography of Morgan, probably hundreds of papers work after work, that collection did *not* have the paper, which you possibly know, which is about 1908 or something like that…

G. A.: Well there were a whole bunch of them, but possible the 1909 paper in the *American Breeder's Association Report*…

DOBZ.: … was not in that collection! That was the only paper not in that collection [*laughter*].

G. A.: I see. Do you think he purposely took it out? Many of the other papers in that period were also in the same vein…

DOBZ.: I don't see any other way. That you see was so long before my day that I can only make inferences. That came from a sort of a scientific puritanism. It seemed to him that [Mendelism] was a bit too easy. If you get results which cannot be accounted for by one gene, alright, that's two genes; if you can't get results with two genes you make another assumption and make it three. He did not realize until he himself had begun to work on that, that in contrast to inferential conclusions in some other fields that a Mendelian hypothesis is susceptible to proof because whatever hypothesis you make can devise an experimental test. Now at that time [between 1903–1910] he did not realize this. Without realizing that, you would say, indeed, it is a strange procedure — people inventing… [*laughter*].

G. A.: What was the relationship between Morgan Jacques Loeb?

DOBZ.: That again was before my time.

G. A.: They weren't…

DOBZ.: I have never met Loeb. Loeb died the year I came [actually Loeb died in 1924], he was probably still alive when I came but I never saw him. So that is largely hearsay. There, you may say, was a kind of inferiority complex. Morgan positively worshipped what at present is called molecular biology.

G. A.: Yes, I know.

DOBZ.: Again he worshipped it in an almost superstitious way [*laughter*], and it gave him a kind of, I think this is the proper way, an inferiority complex. Here was this fellow [Loeb] working with chemical reactions — he [Morgan] was not quite familiar with it — but Morgan was charmed by a chemist. The way to his heart was to talk chemistry, and indeed some people utilized that.

G. A.: In what way — you mean they talked chemistry when they really didn't need to?

DOBZ.: There was a chemist — in this case it would probably be best for me not to name the person — it was in Woods Hole, there was a fellow who talked to Morgan about the chemistry of sex determination or something. Morgan got him a fellowship — it was again in the early '30s, in the depths of the depression — he came to Pasadena and spent a year, and did absolutely nothing. Obviously he was a fake, so to speak. Now the very characteristic thing of Morgan, recognizing his mistake, he compensated the Institute for the sum of money paid to the individual. I dare say very few people would do that.

G. A.: I would say so, that's quite unusual.

DOBZ.: Here is a fellow who got it [the fellowship] because he talked as we say at present molecularly [*laughter*].

G. A.: What was Morgan's method of operation as a research director, in the fly room or at Caltech?

DOBZ.: I would say complete freedom. In that sense Morgan was — you see there was a combination of unquestioned authority, he was known to everybody as "the boss" — at the same time Morgan not only never ordered but I don't think ever suggested or encouraged anybody to do this rather than that.

G. A.: Did he expect people to have their own ideas for projects or did he give them one?

DOBZ.: I should say definitely the former. In that sense I can give you a personal reminiscence that shows that [point]. Coming from Europe, coming to Morgan himself I certainly thought I must get to the newest, most active [muffled] work and asked what he would like me to do. So, the first day or second day I asked him to do it, and he was sort of indefinite. So in my very poor English I repeated that several times. After several repetitions he pushed a box toward me and he said "This." [*much laughter*] So, I went home and my so-to-speak hair [*much laughter*] stood on end. It was perfectly respectable work but it had nothing to do with genetics. It didn't interest me the least. My wife and myself still remember the most uncomfortable night which we passed thinking well what do we do now? Well, of course, very quickly I saw that that was not meant at all, he didn't in the least intend to urge me to do just that. In fact, he clearly gave the thing to me to get rid of it [fade out]; later on I think I can claim, in spite of great difference in age, background and everything else, I became a very good friend of his [Morgan's] and Mrs. Morgan. Mrs. Morgan — the first impression was of a very stand-offish, very cool, unapproachable person — she had really a wonderful warm heart under that shell, once you got through it. She was to both of us and particularly my wife, so to speak *in loco parentis* [*laughter*]. We got to know them quite well. One of the things was that Mrs. Morgan was much interested in music. Nobody out there had much interest in music or anything else, living in Pasadena; symphony concerts were in Los Angeles, as you know about fifteen miles, so Morgan didn't drive much, Mrs. Morgan drove more, but he didn't drive much, so we used to go together to these concerts in Los Angeles. Otherwise we were alone. Well the old man had been amusing himself — he liked to shock and was engaged in various philosophical discussions. He would make statements just for the hell of it [*laughter*]. But that was extremely profitable, educational — that to be sure was not biological, but more extra-biological or philosophical — all the time disagreed and disputed, but the old man enjoyed that, he actually enjoyed that.

G. A.: People arguing with him?

DOBZ.: He enjoyed it and far from the attitude which a few scientists would have taken, that this fellow doesn't agree with me so the devil with him, so through these attacks we argued a lot, I got to know his general extra-biological views as well as, or possibly better, than his biological views.

G. A.: You went with Morgan [from New York] to Pasadena?

DOBZ.: You see we came officially for a year hoping to have the fellowship extended for a second. So, we stayed for the winter in Columbia and then went to Woods Hole, and then decided to stay for a second year. Of course, staying for 40 years at that time seemed like something stupendous [*chuckle*] and during that winter and the next summer, we decided to stay for good. There was still this year and a half…

G. A.: … during which time you weren't sure whether you were going back or not?

DOBZ.: Yes.

G. A.: When you came to study with Morgan were you interested in studying just genetics?

DOBZ.: Oh yes it was *Drosophila*. I stared to work with *Drosophila* in Russia and published two papers before I came.

G. A.: But the evolution side, you were not particularly interested in that?

DOBZ.: No, on the contrary I was … my interest in genetics came from my interest in evolution. My interest in evolution I may say was philosophical, it came first, interest in genetics came from it. So, I had no doubt from the start that this is what I wanted to get into. But to go back to your question of what he did with his people, he certainly, never once did he say stop doing that and better do something else.

G. A.: So he would always give you pretty much free reign, never check up…

DOBZ.: Not pretty much, I would say completely. I don't think he ever cared unless you went, actually went, and wanted to talk to him.

G. A.: He just assumed you were doing your own line of work?

DOBZ.: The man was not [*laughter*] …but even then, he probably found that out (muffled word)…

G. A.: Why did Morgan go to Caltech?

DOBZ.: Well, now, I think the best way I can answer that [is to tell you that] some people ask me why did you leave Columbia, and came [sic] here. To people who knew Columbia it was simply saying well come now and see. That doubtless was true with Morgan, you see to some extent the famous fly room was to be sure a big room but you see this room, that was the arrangement [pointing to a hand-drawn layout of the fly room]. That was the arrangement when we came [pointing to the hand-drawn layout]. Here was Bridges — Bridges was a very interesting person; this was Morgan's desk, full of all sorts of …

G. A.: Was he separated by any kind of partition or just piles of stuff?

DOBZ.: No partition at all, just tables. This [pointing to a particular desk] was where they put me. Here was Sturtevant; here was the desk where the Japanese came; here was the desk occupied by [Alexander] Weinstein, later of Cambridge [Massachusetts]. And here was the desk that was sort of used for preparing fly food and here was where they sat an incubator. Here was a door and that was called Morgan's "office." There sat a lady by the name of Miss [Edith] Wallace. She was an old maid from New England. She was Morgan's secretary and artist, very versatile. All pictures of Morgan's work were painted by her…

G. A.: Is that right?

DOBZ.: All. [continuing to point out layout positions in the fly room] and here is bottle washing, and here was a sort of a storeroom. The whole place was absolutely filthy [laughter], which to me was a real shock. I was coming from Russia, and American laboratories were supposed to be palatial, and of course Morgan's laboratory the palace of palaces. That place was more [indistinguishable] and dirtier than anything I had ever seen. This place, you see, where *Drosophila* food was prepared, had cockroaches in absolutely fantastic numbers; and the door was covered with agar, you would open this door and it would be just one moving mass of cockroaches [laughter].

G. A.: But a lot of work was done there.

DOBZ.: Now the desks were an incredible mess, including Morgan's own desk. You know the little tile plates on which flies are counted? Morgan had the curious habit when he was through with flies, instead of dumping them in oil, which is called the morgue, he would squash them, so this plate was covered, again, with squashed flies. I did at first the kind of work that requires staying for the night — hatching — so I stayed for the night and my wife came and watched them during the day. Now she came one evening, and we were looking at this thing [Morgan's counting plate] and she said "Look at this filthy thing." So she took it and washed it. Next morning Morgan inquired who interfered with my desk?

G. A.: He didn't want people to fool around with it? One last question, and this is something we talked about earlier, and I have no way of knowing whether you would know the answer to this question, but I am wondering what you know about the introduction of Mendel or Mendelism into Germany in the early decade of the century?

DOBZ.: The introduction of Mendelism into Germany…

G. A.: That is, how many German biologists had accepted Mendelism?

DOBZ.: Well, quite a lot, but I really don't know anything much about it. I had been in Germany only occasionally, but of course [Carl] Correns was a German, and he was still alive [when I was starting out] but people still alive would know about that; Curt Stern would be a man to talk to.

G. A.: Where is he?

DOBZ.: Berkeley. Now as I said, he was one of the early visitors, probably in terms of present time he came just before me. He was originally German, and originally a student of Goldschmidt. Now, he is interested in history and about the history of genetics in Germany, he would be a far better man to ask.

G. A.: I am curious because one gets the impression that many of the big German geneticists were not particularly responsive to Mendelism…

DOBZ.: But look that was true everywhere, including the United States. I think it would be better than misrepresenting the case if you would say that it was embraced in the United States but scoffed at…

G. A.: No, no, no that's not what…

DOBZ.: A lot of people in the United States were opposed to Mendel.

G. A.: Including Morgan…

DOBZ.: Harvard is apparently still full of stories about Jeffrey, who never accepted Mendelism or Morganism. He was a very influential man.

G. A.: Some day I would like to make a comparative study, country by country, on the reception of Mendelism — who accepted it and who did not in each country.

DOBZ.: Well, as usual, as is true in science in general, there was opposition on the part of the elder statesmen, and more ready acceptance on the part of the younger group. That's what would be expected, and that is presumably what always happens.

G. A.: I wonder if there would be any differences in fields, that is, fields like embryology or cytology would be more inclined to accept it than people in descriptive fields.

DOBZ: I suppose so, but…

G. A.: I really don't have any idea whether that's true.

DOBZ.: As a guess. I would say yes but a more important difference might be age, which is something useful to remember [laughter].

G. A.: Were there any biologists around, people older than Morgan or among his contemporaries, whom he admired very much, or thought very highly of?

DOBZ.: Well, I don't think Morgan was very much given to hero worship [*chuckling*]. Now you mentioned Loeb. Certainly, he had a healthy respect for Loeb. Now, I remember a most amusing visit, it probably happened a few years, even a month before he died, with William Morton Wheeler. They were exchanging pin-pricks. Two old men, of course, and they knew extremely well how to administer this exchange to each other, I think that was done in a good-natured way. [Edwin Grant] Conklin was less popular, that was because, as you know, Conklin was religious, a student of theology. And as you know, or if you don't, this is a very basic thing about Morgan, Morgan was extremely anti-religion.

G. A.: I know that he had no religious beliefs himself, but I didn't know to what extent he was anti-religion, that's interesting.

DOBZ.: I would tell you then more about it. Morgan's idea about what science was for was to dispel mystery. Mystery is something which feeds religion. The point of science is to deprive religion of this source of support. That is what all science is for. And that is, if you please, what *evolution* and *genetics* is for.

G. A.: Is to get rid of mystery and…

DOBZ.: Evolution and genetics dispels the mystery of the origin of the world and the origin of man. Now, by 1931 or 1935…

G. A.: His book, *The Scientific Basis of Evolution*?

DOBZ.: [thumbing through a copy of Morgan's book on evolution] No, it's not an autographed copy, at the time that this book was published Morgan felt that in a sense evolution and genetics had done their job. Heredity and evolution are no longer mysteries. Consequently, to him, the most important branch of biology was not genetics, not evolution but, if you please, neurophysiology, the process of thinking; that is still mysterious, that's where God can still find a place to stand on. And, accordingly, when he got his Nobel Prize the Rockefeller Foundation offered him what was at that time a tremendous amount of money — I forget if it was \$100,000 or \$200,000 (it would be more than \$10,000,000 now) — that was invested entirely in developing in Caltech neurophysiology. He [Morgan] went to Holland, and brought from there three Dutchmen: their names were [Cornelius A.G.] Wiersma, who is still in Caltech working on neurophysiology; and as you know laboratory equipment for working on this sort of thing is the most expensive of all. The laboratories are now full of electronic devices, but at that time those devices did not exist. But this costs enormous amounts of money; now this is where Morgan thought the light is to come, and for that reason [that is, to dispel mystery]. So, you see, later on he was elected a member of the Academy of Sciences of Rome, and for some time the question which was discussed in the laboratory was: "Will the old man accept?" He did, which some people considered to be very bad. Obviously he accepted not out of any sympathy for the Pope or the Roman Catholic Church, but because not accepting would be a scandal [*much laughter*], which he was not willing to make.

G. A.: Was his opposition to religion to organized religion or just to religion…

DOBZ.: Any religion, any mysticism, any metaphysics. Now mysticism and metaphysics, they are very inclusive. Now these, you see, are things [that] belong to this realm which I say we were discussing many times together, where he amused himself talking, frequently making purposefully shocking statements.

G. A.: So he purposely tried to come out with…

DOBZ.: However, mind you, having said that, I must go on to say that Morgan was a real gentlemen. There is nothing "of course" about it, not everybody is or was [a gentlemen] so … again the students … there were cases when a graduate student who went to church on Sunday was often discriminated against by other people, but never by Morgan. Never. He did not share those views but he was a gentlemen — that's private business.

G. A.: From what I know about him and of his life activities, I would never think he would hold that sort of thing against anyone.

DOBZ.: There were people that did, very emphatically, but not Morgan. More philosophically, I say, metaphysics — metaphysics was a swear word [to Morgan]…

G. A.: It certainly was in his writings.

DOBZ.: Something being metaphysical is just no good [*chuckles*].

G. A.: How often did he like to get into these kinds of conversations?

DOBZ.: Now this may sound almost conceited, but with me, quite often. Not with many others.

G. A.: This is a side of his intellectual life that doesn't come out very much in his writings.

DOBZ.: Yes it does! This business of something being metaphysical…

G. A.: Oh yes, metaphysics, he did seem to have a strong interest in questions of metaphysics. . ….

DOBZ.: Oh! I would say that I do not agree with you: Almost every book in either the conclusion or the preface of the books you have this periodic of debunking of metaphysics.

G. A.: Well, maybe I'm not making myself clear. He was not the sort of person that sat down and expressed any philosophical ideas. He was not the sort of person, as far as I know, like, he did not like to speculate…

DOBZ.: That's because philosophical ideas smack of metaphysics, and metaphysics is a slippery thing, which may lead you to even such a horrible thing as religion.

G. A.: To him being led to religion was equally as bad as metaphyics or was it…

DOBZ.: All of that's an ultimate … *the* ultimate degeneration [*chuckles*].

G. A.: You would say, then, that Morgan was very definitely a mechanist.

DOBZ.: Oh yes. Well, you see, this is what was really an anecdote, in the 1930s Lysenko has accused Morgan of being an idealist and metaphysician. I must say that in this thing I was an imp, because whenever this [sort of thing] came, I brought it to Morgan and translated it [laughter] and the old man was, I may say, greatly amused.

G. A.: I'm sure he would have been.

DOBZ.: He was greatly amused.

Epilogue

I had the occasion to meet with Dobzhansky several years later (1969) after I was an Assistant Professor at Washington University in St. Louis, where my colleague Hampton Carson had invited Dobby to give a department seminar. By that time I was working on a biography of

Morgan, and was pleased to remind Dobzhansky that he had suggested it in our interview a few years earlier. He was very supportive, and my only regret is that I was not able to finish before his death. Much of the material in the interview found its way into the biography, but probably the most important element was the general picture of Morgan's "style", both personal and scientific that he provided. Dobzhansky painted a lively picture of his own interaction with Morgan that I did not get from any of the other students or associates that I interviewed. I have always been grateful to Dobby for his down-to-earth insights and reminiscences.

Интервью с Феодосием Добржанским (Октябрь, 1966 г., Рокфеллеровский университет, Нью-Йорк)

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В октябре 1966 г. мне удалось договориться об интервью с уже пожилым Феодосием Добржанским, который тогда всё ещё был профессором в Институте Рокфеллера (теперь Рокфеллеровский университет) в Нью-Йорке. Я провёл с ним несколько часов в субботу в послеполуденное время, расспрашивая о его ранних интересах в генетике и эволюции, и особенно о его работе с Томасом Хантом Морганом (1866–1945) в Колумбийском университете и Калифорнийском технологическом институте (в Калтехе, как его обычно называют). Я в то время готовился писать биографию Моргана, только что закончив мою диссертацию доктора философии по его генетической, эволюционной и эмбриологической работе. Добжанский был очень живым и восторженно отзывался о Моргане и собственных опытах в его лаборатории. Хотя я использовал много идей и оценок из этого интервью в моей биографии Моргана[2], интервью не было записано полностью в виде текста до 1992 г. и никогда не публиковалось.

[2] *Allen*. Thomas Hunt Morgan, 1978.