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Loren Eiseley

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Loren Eiseley
“The Unknown Darwin”
April 17, 1959
Portland State College

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HOST: Dr. Charles Brandt. Dr. Brandt will introduce our speaker this evening. Dr. Brandt.

CHARLES BRANDT: As I'm sure you all know, 1959 is a year of at least two centenaries; of course the centenary of Oregon's statehood, but also, and very importantly, the centenary of the publication of Charles Darwin's *Origin of Species*. I think it is appropriate that in celebration of this latter centennial that I mentioned, that we have with us a speaker who is a distinguished authority on Darwin and on the evolution of man. A native of Lincoln, Nebraska, our speaker was born into a family which homesteaded in that region when it was still a territory. His first contact with nature lay in the salt flats and the ponds around Lincoln, and in the mammoth bones hoarded in the old red brick museum on the campus of the University of Nebraska. Receiving his A. B. degree there, he did graduate work in anthropology at the University of Pennsylvania. Returning to the Midwest for his first academic position, he taught at the University of Kansas. Later, he became the head of the Department of Sociology and Anthropology at Oberlin College in Ohio, then returned to the University of Pennsylvania in 1947 to become the chairman of the Department of Anthropology. He also is the curator of “Early Man” in the university museum there.

Dr. Eiseley has lectured at a number of universities including Harvard, Columbia, and the University of California. He is the past president of the American Institute of Human Paleontology, and a contributor to many leading scientific journals as well as such well-known

popular periodicals as *Harper's*, *The American Scholar*, *The Reader's Digest* and *The Saturday Evening Post*. For a number of years he was active in the search for early post-glacial man in the western United States, and has worked extensively in the high plains, mountains, and deserts bordering the Rocky Mountains from Canada down into Mexico. Both of Dr. Eiseley's recent books, *The Immense Journey* and *Darwin's Century*, have received wide acclaim in scientific and literary circles in the United States and abroad. For his book *Darwin's Century*, he was awarded only two days ago the Athenaeum award for the best book of non-fiction produced by any Philadelphian.

It is a pleasure to present Dr. Eiseley, who will speak to us on the subject "The Unknown Darwin." Dr. Eiseley. [applause]

LOREN EISELEY: Dr. Brandt, ladies and gentleman. It is a difficult thing to talk about Charles Darwin at the time of the centennial of *The Origin of Species*. The reason that I say this is because it introduces various emotional elements into a situation which has many facets and many aspects to be considered. We are emotional about Charles Darwin in a certain way, and I don't mean this at all critically; it is part of the human attitude.

We like to think of distinguished contributions in the field of science as somehow emerging out of a vacuum, as standing alone, as a tremendous contribution of the human mind. And certainly, that tremendous contributions have been made, and have been made by this man, is very obvious, and yet there are aspects of this story which are intriguing, curious, and about which little has been said, I think partly because of some aspects of Darwin's career, which I want to look at a little more closely this evening.

There is, in other words, a myth about Darwin. A myth that this man, sailing down the coast of South America, more or less conceived in a vacuum the theory of natural selection. What I'm interested in this evening is to try and present to you, among other things, the story of this achievement, and to look at it from a different angle. Because—and this is why I said that to talk about Darwin or, for that matter, any distinguished figure in the centennial year is difficult—is because at this time of all times, the field ordinarily left to the quiet cultivation of the student of the history of ideas is filled with rustlings. And many people who ordinarily are not particularly interested in the history of ideas or the history of science become emotional on this subject, and as a consequence, you hear a great many eulogies.

But the examination of ideas from a historical standpoint is quite another thing. And even people who are distinguished in the scientific field, when asked about Darwin, I think, of all people, have a little tendency to grow emotional in one way or another, partly because he

looms so large in the scientific thinking of the nineteenth century, and the religious and other controversies which were so striking then and which continue, I find, to go on into our own time, even 100 years later. And so, as a consequence of all this, it becomes difficult to see the man for the cloud of verbiage which arises around him, just as it is difficult to see Darwin's precursors, because his own figure, through public acclaim, has come to bulk so large in our thinking that the small invisible steps of one kind or another that led up to his achievement become neglected and ignored. This is simply part of the way that we look at things of this kind.

From the standpoint of the biographer, of course, the voyage of the *Beagle*—this long, five-year voyage that Darwin made around the earth, and during which it tends to be generally assumed that he arrived at most of his ideas. This voyage, I think, more than anything else perhaps, has obscured our conceptions of what Darwin contributed to human thinking. It is dramatic: here is a boy who had failed at medicine at Edinburgh; who had been termed by his father a rat-catcher and a no-good of one sort or another. Who had failed also at theology, and then, by good fortune, gets this chance to escape in a sense, and to go around the world and then come back and confound his professors. Now this appeals to the boy in every one of us; it appeals to some escape impulse that is current even to professors.

And so, we tend to read this story as a story of spectacular accomplishment in the field: the story that has to do with turtles and birds beaks in the Galápagos, and of the meeting with obscure Indian groups at the tip of South America. All of which is true enough, all of which is interesting; but to be direct, has very little pertinence, actually, in some ways, to the story of where Charles Darwin really got his ideas. I realize that in making this comment, I'm being perhaps a bit heretical, but as we go on with our discussion, I hope to be able to demonstrate to you that at the time Darwin was making his voyage around the world, this long erratic passage about which we know so much from his letters, his notebooks and all of this, that nevertheless, the secret which he discovered was lying back home in England in unread books.

Maybe there's a moral in this for college students; I hope so. But, so deep is this legend, so much have we heard about it in terms of our childhood and our typical conceptions of Charles Darwin, that it's very difficult to get out of this pattern of thinking. Just a few months ago, for example, one of the most distinguished biologists in the United States, a man for whose work I have tremendous admiration, made the comment that the work of Darwin's precursors had as little to do with his achievements as science fiction writers today had to do with the development of the atomic bomb. Now, this remark interested me for the simple reason that it runs so counter to what the historian of science is able to tell you about this particular epoch, and at the same time, coming from the source that it did, it revealed again this tremendous emotional involvement which many of us have with Charles Darwin, who has come to stand in

our eyes as, in a sense, representative of all science. Representative of human achievement on the level of genius. And so we are very loath to go back and to look at the way this particular idea was developed, and the precursive steps which led onward by infinitesimal degrees, even when there appears to be spectacular dissension, which led onward to Darwin's final achievement. And it is this that I want to examine this evening.

Now, if you will recall, when Darwin came ashore in 1836, he brought with him notebooks of the voyage. As a matter of fact, there is a sizeable quantity of literature here which is still not published and still under examination by scholars. But in the writings of that period, around 1836 or thereabouts, there are some surprisingly interesting statements. Now, again, let me repeat that all of you are probably acquainted here with the story which has been hallowed by tradition and reaffirmed by Darwin's descendants: namely, that in October of 1838, Charles Darwin, chancing to read Thomas Malthus on population, suddenly conceived for the first time the idea of natural selection. Before we go any further, let me caution you to look at that term "natural selection," because one of the reasons that we have difficulty in reading the history of this particular period, is because the word "natural selection," due to Darwin's use of it, has become synonymous in many minds with evolution itself, so that we are unable to conceive of it in the other terms.

Also, we have come to feel that the words "natural selection" so clearly represent Darwin's presentation of his point of view that we are unable or unwilling to examine the earlier literature of the nineteenth century to see if, by any chance, what we now call natural selection was known under other names. Now, returning to Charles Darwin and his notebooks of this early period, we come upon a very interesting statement, which he made during this time. "If species change," he said, "if species change, it must be *per saltum*"—that is, through quick jumps; mackerel mutations, you might call them today—"If species change, it must be *per saltum*, or species may perish." Now, this is just one of these little notes jotted away in the notebook of 1836. But the interesting thing in all this is that this is a point of view which is quite distinct from what Darwin was later to express in *The Origin of Species*. Where, if you will remember, he presents quite cautiously the idea of slow biological change through what we would call very small mutating steps, by gradual degrees.

So, that although it becomes evident in these journals of the young Darwin that he was thinking upon such subjects and contemplating such subjects, there is every evidence that he came back to England after that long five-year voyage without any clear realization of how evolution had come about. In other words, the mechanism, the mechanism which he sought—the mechanism which many of us tend to feel was somehow or other correlated with this voyage around the world—is something that he found, something that he first saw clearly into in this period after

his return from the voyage. Now, let me repeat again, there is nothing new about the assumption that, confirmed by Darwin, that he had read Thomas Malthus in 1838 and that this is where he got the idea. But it is curious that here in 1836, after his return, that he was expressing a point of view so curiously divergent from the one that was later to occupy his whole lifelong attention. Where did that idea come from? Why was it necessary for him to make this statement? Why did it seem to him more likely at that time that species changed, if they changed at all, by jumps? Well, in that query and in his later attempts to answer that problem is the real story of where Darwin's discovery of natural selection emerges. And it is a curious one, far more currently than we imagine. The idea of natural selection did exist in the writing of the early nineteenth century.

Now, if we were going to go into this in more detail than there will be time for, we could point out that the idea, that is, of the struggle for existence having some effect on organic life, runs back even into the eighteenth century. But the thing that I want to call your attention to here particularly is the presence of natural selection under another name. And then we will try and see what relationship it may have to Darwin. Now, if you will examine the writings of Sir Charles Lyell, who was one of those people whom Darwin greatly admired and who had a tremendous influence upon Darwin—indeed, Darwin remarks somewhere that that half of these books seem to have emerged from the brain of Sir Charles Lyell—one can observe that in the writings of this man, there is an expression about a biological condition which he terms the “principle of pre-occupancy.”

Another writer of the time refers to something that he calls a “localizing principle,” which is similarly a treatment, if you begin to examine it carefully, a treatment of natural selection under another name than that which Darwin was later to apply to it. Now, what was meant by these curious phrases? Well, first of all, we may say that this early part of the century which was rather theological in outlook, conceived, of course, still, of biological species as being created in the place where they were presently to be found. In other words, it was recognized that species occupied particular ecological zones, as we would say now; places. And even that there was a certain degree of variation manifested among these species. But it was assumed that these creatures could not vary beyond a certain point, nor move out of their present localities too far, for the simple reason that if they did so, they would be eliminated through natural selection. Let me repeat, the word “natural selection” does not occur in this literature, but that is what they are talking about.

Remember that natural selection—and here I come back to the point I was trying to make earlier—that natural selection is actually capable of two interpretations. And that one of these interpretations, one, was current in the early part of this century. It was a situation in which—

and curiously enough we recognize this today—it was a situation in which the selective power of selection, or natural selection as we term it now, was exerted in a conservative fashion. Now, today, in the light of modern biology, most of our attention has been to a very considerable degree concentrated upon how animals diverge. What there is about this force, which throughout the ages of past time, makes animals change? Changes the faces of you and I, and all of us, on into the unknown future.

But at this time, it was something else; it was, in a sense, an explanation of how providence kept things in a specific environment. The zoologists observed that an animal which was cryptically colored, that is, colored in such a fashion that it was not easily observed by its enemies; as you know some insects, birds, and other small mammals are. The zoologists observed that any creature which varied from the proper coloring, like an albino for example, became visible to its enemies and was immediately eliminated. And so, as a result of this, there grew up the conception of this power—and here's what Lyell really talking about when he talked about pre-occupancy—the notion that this selective power held everything in a kind of dynamic balance. It was a world, in other words, which could move, but would not.

Now, today we realize that this is still a genetically acceptable idea, but it does not tell the whole story about natural selection. In other words, the geneticist will recognize that there is a short-time conservative effect, under certain zoological or botanical situations, in which the selective forces operate in precisely this matter. But coming back to this problem for a moment, let us remember that the world of the geological past was still very inadequately known. These men, and particularly one of them, who I'll mention in a moment, were looking at the world essentially from the standpoint of what today we would call a neo-zoologist. They were interested in life as it existed at this moment, on the face of the planet. Now, Sir Charles Lyell, of course, who ironically enough was concerned with this principle of pre-occupancy, was also one of the great introducers of historical geology in the modern sense, and had begun to realize that the story of the past was a story of contraction and expansion of animal forms.

Nevertheless, and although he recognized this power which I have been speaking of, this sort of negative selection—and by the way, in a lot of the literature of this time, you get phrases like “pruning,” “policing,” “natural government”—all phrases which, besides suggesting a certain degree of providential interference in the machinery to keep any animal from expanding beyond its normal limits, all of these terms indicate quite well that the struggle for existence, of course, was recognized, and that some kind of natural selection was eliminating the variant individual.

Now, it is for this specific reason that young Darwin, when he came back from the voyage of the *Beagle*, was puzzled and confounded with the idea of how you were going to get organic change. Because if this providential arrangement were true, if organisms were so perfectly selected for the environment which they were inhabiting, then how was it possible for one organism to supplant another? And here, Sir Charles Lyell, for example, pointed out that it was inconceivable that you could get evolutionary change, because before any opening could be occupied, it would be swarmed in upon by organisms already adapted to existing in that particular ecological zone.

So that Darwin, who was beginning to believe in evolution, was trying to seek some way around this particular barrier, and at that point all he could think of was: “If species change, it must be *per saltum*, or species perish.” Although one of the interesting and unnoted aspects of this story lies in the fact that in 1835, and again in 1837, in a prominent English journal of natural history, a man who was afterwards to be known for his contributions in other fields of biology, namely one Edward Blyth—who was later to turn aside into primarily taxonomical problems—wrote a couple of essays. And if you will examine these essays, you will find, curiously enough, that the principle of natural selection (without the term) is expressed in those works.

I see here again, over and over again: you hear it repeated, “Darwin was the first man to realize the relationship between artificial selection, the artificial breeding of plants and animals, and to take that principle and apply it to the organic world of nature.” But this was done by Edward Blyth in 1835. Who also—this is not the first time that some of these things were mentioned—but he mentions sexual selection; again, something which has become widely associated with Darwin's name, though it was also known to Darwin's grandfather. What the interesting thing here, in connection with the Blyth papers, to which I would like to call your attention, is the fact that their dating falls so closely upon the time of Darwin's returned to England, and at a time when he was seeking an evolutionary mechanism.

Now, a great deal has been said about Darwin's precursors; it is been commented upon here and there that this or that man... [audio skips a few seconds] ...the distinguished gentleman whom I mentioned a moment ago was so vociferous and vigorous in his expression of the fact that none of these fellows had anything to do with Charles Darwin's work *On the Origin of Species*.

Now, you ask me, what evidence is there that Charles Darwin knew about any of these writers? And I will answer that the evidence is just this: it has always seemed curious that Charles Darwin, who was so tremendously able as a field naturalist—everyone is willing to acknowledge this—who remembered so much about his experiences in South America, in the Galápagos and

elsewhere—and I'm not trying to denigrate that experience, but merely to place it in proper perspective. It is often suggested that here was the primary source of his ideas; and you get the impression, not directly spoken, but somehow intimated, that after he came home to London, in spite of obviously reading a certain amount on artificial selection and things of that kind and consulting breeders, that he knew nothing about this background.

This has always seemed to me to be a curious oversimplification of events, for the simple reason that Charles Darwin, according to his sons, according to his own statements, was a tremendous, omnivorous reader who went through, at the time he became interested in this subject, in whole files of journals; abstracted them, dealt with them in great detail, used them in the compilations which later entered into his books. And so it has always seemed to me a little puzzling that the minute he arrived home, to all intents and purposes, Darwin ceased reading or having any consciousness of what was going on in the zoological world around him.

Well now, this may be all very well from the theoretical point of view, but you will again ask: what evidence do we have that Charles Darwin had any acquaintance with this literature? Now, as far as Sir Charles Lyell is concerned this is no trouble at all, because we know that Darwin was tremendously struck with the biological ideas which had been expressed by Sir Charles Lyell. I think there's been a little tendency to minimize this in modern literature, for the reason that Lyell was essentially a geologist; and in the years since the early part of the nineteenth century, biology and geology have tended to a degree to go their own ways, so that the geologist has remembered Sir Charles Lyell as a distinguished contributor to geological thought, but has tended to neglect or to forget his biological contributions. Just as the biologist fails to read them because he says, "Well, Sir Charles Lyell was a geologist"; so that in-between the two, there's been a tendency for this whole structure of thought not to be examined very satisfactorily, and also to be ignored, in spite of the fact, and I repeat once more, that Sir Charles... er, that Darwin himself, in a burst of honesty, remarked that, as I quoted, that half his books he felt at times had been written out of Sir Charles's brain. Now, there is one point, however, where Lyell's principle of pre-occupancy does not achieve the relationship which I'm about to speak of. That is, the genetic aspects of the struggle for existence as a creative force were not expressed quite as satisfactorily in Lyell's rather scattered discussion of his principle of pre-occupancy that they reach in the short writings of Edward Blyth.

Now, Edward Blyth was a young man, one year younger than Darwin when Darwin came home from the voyage of the *Beagle*. He did not have the wealth or position or influence that Darwin's family did. He was a young man of bad fortune, in the sense that he was so tremendously devoted to the subject of natural history that he had let a small inheritance more or less go by the board, which had been invested in a small business. He fell into ill health. He

had to go out to India in the 1840s. And so, as a consequence of this, and perhaps partly because of a shift of interest and other influences, he did not pursue nor carry out to its logical conclusion what Darwin was later to do. And yet, as one examines these two papers to which I have referred, one finds some very curious things about them in terms of materials which were later to appear in Darwin's own work.

Now, Lyell... or rather Blyth, among other things, had really for the first time stated this principle, though he was still stating it in this conservative sense that I have mentioned. Stated it far more explicitly than Sir Charles Lyell and in briefer form, and, in spite of the fact that he went on to say this is a conservative principle which holds things in its place. Somewhere in these papers he makes two quite interesting remarks. He says, "If these ideas were carried out farther and more research work done upon them, they might lead to some highly interesting results." In addition to this, he stated in another place that in spite of the fact that he himself believed in this conservative approach to the subject, that is, that there were species limitations and that things were held in place in this fashion, he does go on to comment that, if one looks at it—I can't quote specifically here—but if one looks at this problem, one might entertain the idea that if this point of view were looked at in a certain way, it might suggest that the diversity of the living world might have emerged from a few forms.

A rather strange and prophetic little statement, which might very well catch the eye; and, a little like Buffon at an earlier time, after having said this, he realizes he's made a kind of heretical remark and he draws back and says, "But no, no, of course we know that if this were really true, if this were really true you would see such a blending of all living forms, which we do not see." That this would make a difference; in other words, you see once more here the lack of a clear glimpse into the past. What people are really looking at is the present and growing puzzled over the evolutionary problem, because the idea is everything should merge into everything else on our particular time level. Now, as long as you look at the problem from this standpoint you will make very little progress; something else is needed here. Something involving historical depth.

But, coming back to Darwin and the question of whether he had knowledge of this work or not, if we look once more into those notebooks of the voyage, we find that in one port in Peru somewhere along about 1835, Darwin jots a few things into his notebook, which he was often in the habit of doing, whether it involved the price of melons in a particular area or something about zoology, or just anything. He has a brief, cryptic little note in there referring to certain experiments on the subject of smell in some of the carrion birds, and right after it, there is the single statement: "Magazine of Natural History." Now this happens to be the biological journal with which I am now concerned. And it is interesting, because on the basis of that reference,

we can catch the fact the Darwin, even as a young man still voyaging around the world, was regarding this magazine importantly enough that he was having it sent from home. Now, if we go forward—and remember also, this is one of the things which is so intriguing about all this—this was a magazine, let me remind you, that Darwin's friends contributed to. Sir Charles Lyell, his friend Jennens, who was the first man approached to go around the world on the *Beagle*, but who refused the job; men of this caliber.

Now, let us go forward in time for a moment; and you will recall that *The Origin of Species* is un-footnoted. It was un-footnoted because Darwin, after being stimulated into more rapid production by Wallace, dashed off what he thought at this time was to be an abstract of his much larger *Origin of Species*, which was never published in this form. And, as a consequence, he wrote one time to Wallace, for example, “You must not expect footnotes in this edition, because I don't have time for them.” So there is no use looking directly in *The Origin of Species* for any references to this particular problem. There is, however, if you want to pursue the question of whether Darwin had ever held in his hands the particular volumes containing these papers: there is one other thing you can do, and you can go home tonight to your own library and check it for yourself. You can look in Darwin's book... two of his books, *The Descent of Man* and *The Variation of Animals and Plants under Domestication*. And among the many footnotes, particularly in *The Variation of Animals and Plants under Domestication*, you will find many, many, references to Edward Blyth.

They are not to the specific papers which I am referring to now. But besides documenting Blyth on specific formal items at great length—and I mention this here to show you that Darwin was acquainted with the man and admired his work and indeed speaks in a laudatory fashion of it—other places, and again, I'm speaking of his general taxonomical work now. In addition to this, however, in the documentation of these books—and let me remind you that portions of those works are taken out of the big *Origin of Species* which was never published, but which was, afterwards, parts of it reworked into things like *The Descent of Man* and *The Variation of Animals and Plants under Domestication*—there are many, many references to the *Magazine of Natural History* throughout practically the entire series of volumes. And that among the volumes listed there for other purposes, and indeed, representing small items showing that the volumes had not been glanced at just in terms of some particular large paper, but at great length, and minute items, notes, in other words, examined with care, there are a number of references to the specific volumes containing the papers to which I refer.

Now, you ask yourself, “What led Darwin to approach this problem in this particular way?” Well, of course, to pass back and to try and estimate everything in the mind of a young man 122 years ago becomes very difficult indeed. But one can observe, for what it is worth, that

Darwin seems to have made use of these papers. Now, you would not be able to establish this very satisfactorily except for one or two perhaps highly suspicious passages in the *Origin of Species*. But there is another aspect to this story, and that is the fact that some time after Darwin's death at Downe, his children turned up, in an old closet under the stairs, the original essays of 1842 and 1844 which were more or less trial essays on the road to the *Origin*. In other words, they were essays which Darwin wrote not for publication, but when he was working on this problem in the 1840s. It was the publication of these papers, in 1909 by his son Francis, which give us some clue to this whole situation, because certain items, certain phrases, certain uses of material which have vanished in *The Origin of Species*, and which seem to bear a relationship to Blyth's work, are present in these essays of 1842 and '4.

Blyth, for example, speaks at one point of a series of rather queer mutations. He talks about Ancon sheep, also sometimes referred to as otter sheep, a breed that I believe is largely vanished now, but which was developed at one time from a single current macro-mutation in connection with the... they're short-legged, like dachshunds. And hence, they were of use at one time, because they couldn't jump over fences with the facility of a regular sheep. He talks about Ancon sheep, rumpless fowls, and tailless cats, an odd little combination of forms, along with one or two other references to the fact that certain dogs and fowls have five toes. Now, in these essays of Darwin, surprisingly enough, there occurs once more "Ancon sheep, rumpless fowls and tailless cats," along with certain other materials which I won't try to document in detail here, but which strongly suggest once more the fact that these papers had been used at some time by Darwin. And indeed, may well in terms of the suggestion thrown off by young Blyth and rushed by, might very well have been at least an added stimulus to the utilization of the whole principle of natural selection.

Now, once more, you ask, "Why did Darwin do this?" In terms of modern footnoting and modern approaches to these subjects, it is generally observed that when you get at least partial ideas from other people, you have a tendency to refer to them in some fashion. Now, that Darwin did refer to Blyth in every other possible manner is quite evident. If you will examine *The Variation of Animals and Plants under Domestication*, the edition's very little, in terms of the way they're printed, but I took my own, and was interested to find that Blyth had about four and a quarter inches of reference to other specific things in that volume, more than practically anyone else in it. Furthermore, Darwin himself used every occasion to speak of Blyth in a favorable fashion, but never at any time did he refer specifically to those two papers among all the things of Blyth's that he referred to.

Now, this early part of the century here is a curious one, in the sense that British biology was extremely provincial in certain ways; remember that this follows the period of the French

Revolution and the Napoleonic Wars. British attitude to French philosophical ideas—I'm thinking now of Lamarck, for example, the earlier evolutionist—British ideas toward these people were just about the equivalent of what the average American, I suppose, would entertain about Russian theories at the present time. In other words, there was a withdrawal, a constant reiteration in the statements of the biologists of the time, that such thinking—and they are referring to Lamarck, frequently, and other French evolutionists—that such thinking is atheistic, godless; and it becomes obvious that these men have become linked in the British mind with social upheaval. And you find a curious situation that even British biologists who might refer in other ways to evolution, find a way at the same time of disclaiming what they termed the exploding ideas of Lamarck. As did young Blyth himself, somewhere in these papers.

So that, in looking at this whole story, we have to see that Darwin, in a sense, was presented with an unusual problem. Unusual in the sense that practically everybody whom you might say he was intent upon using in the development of his theory had other ideas. Lyell, who had written so ably upon geology and the long range of past time had expressed himself, during this period, as opposed to evolution. Edward Blyth, the young gentleman we've just been talking about, after this rather nervous and tentative play of thought around the subject, and glimpsing perhaps dimly its potentialities, ended up by disclaiming it and returning to the hedge-constricted, stable English landscape.

Now, what Charles Darwin apparently did at this point was—and this is where so often we find that contributions in the history of thought are derived from information which some men may have been able to pull out of more than one science—Darwin looked at this conception of natural selection as conservative, holding everything in its place. And he realized, as all the voyager naturalists were beginning to realize in this period, that this really was not... this principle of pre-occupancy didn't really work. That it was not true that things were so precisely fixed for a given ecological environment. That they could not suffer intrusion and that everything was stable. Darwin, on these voyages—and he was not the first—had seen that in the islands of the Pacific and elsewhere, plants, animals introduced by man into regions where they had never been before, were competing with, exterminating, and eliminating, and forcing back creatures that had been supposedly, you see, on the older theory, divinely created for that particular spot. And again, you find evidence of this kind of thinking in Darwin's journals and in the first edition of *The Naturalist's Voyage around the World*. And it becomes evident that he is beginning to see that this tightly constricted, supposedly balanced, supposedly unchanging world that had been visualized by the naturalists of the early part of the century was erroneous, that if one added to this kind of competition and struggle the long course of geological time as visualized by Lyell; if one added that and realized that no summer is ever exactly like another summer, that no winter is exactly like another winter, that continents by degrees move up and

down and fauna shift and move and change. And if we begin to realize this—and since we have previously accepted the fact that variation exists; this was already current in the literature—then it becomes evident that what was seen as a thoroughly stabilized system is in reality an illusion. An illusion created by the short span of human life and by the fact that we, in other words, do not live long enough to see what really goes on under these circumstances, nor how that divergence, which seemed so easily controlled and conserved, could lead on to absolutely endless organic divergence.

And this is what Charles Darwin made of this system. Now, let me repeat, without going into things which we are not in a position to go into at length here, that Darwin was placed in a genuinely peculiar position by the events which I have narrated. It may well be that at some point, perhaps, if he had ever re-done *The Origin of Species* in terms of footnoting, which he never did in spite of the fact that he added, in later editions a historical note to it, it might well be that perhaps he might have had a change of thought on this particular subject. But I think that the evidence now is such as to indicate that—with all due respect to the fact that Malthus had a wide influence and in one way or another was present at least secondarily in many books which we know that Darwin read, over and beyond the book itself, that is, Malthus' book—that it would seem unlikely really, that Malthus, although he had the convenience of being a man outside in the political realm essentially, so that one could refer to him perhaps more readily without bringing him into one's own problems directly; it would appear that even though Malthus may undoubtedly may have had some additional stimulation upon Darwin, that in actuality, there is every indication that between this combination of Sir Charles Lyell himself and Blyth, with his even more specific and clear statement of the principle, that Darwin was equated with this particular problem in the biological... [audio skips a few words and resumes]

...statement of the principle that Darwin was equated with this particular problem in the biological world. And that what he did, rather than to make the kind of fantastic in-a-vacuum act of sheer brain achievement, which we are so fond of attributing to our great men, that what he did was to take this conservative principle, which, as you trace it up through Lyell and certain other writers on to Blyth, and then to Darwin. What he did was to take this current principle, and just by a hair's breadth, one might say, reoriented it into something that changed the thinking of the world. Now as I said, this is not the standard interpretation of Darwin, but the documentation is there; you don't have to rely on me for it. And I think that we have confused ourselves by the assumption that you did not have natural selection until the words were used. And as a consequence, we have in a sense failed to see what some of these biologists of the early part of the century were thinking, and how that particular world view which we could go on and explore in terms of its relation to the whole concept of Newtonian world order, which you get in the eighteenth century, how that world view, for a brief period of a few

decades there, held biological thought in a certain position which had to be broken out of, not so much by the accumulation of new discoveries as by looking at things in a different way.

And one of the curious and interesting things about all that is that Darwin, sometime long after the publication of *The Origin of Species*, commented in a letter to Hooker that he did not think scientific men read enough. "If there were more reading," he said, "I think there would be more scientific discoveries on the part of science." And for those of you who are students and who perhaps have dreamed of these achievements as being beyond your grasp, not being a genius, I just call your attention to that statement, which could be documented also from other sources, in which Darwin has commented upon the importance of even third- and fourth-rate men in the history of science. He had a tremendous ability to take things which in other minds remained within the accepted world view; take them and make something which was quite different and his own out of them. And so I leave that thought for you; just remember: scientists do not do enough reading. Thank you.

[applause; program ends]