

# Foucault Studies

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## TRANSLATION

### **Cuvier's Situation in the History of Biology**

Michel Foucault<sup>1</sup>

#### **Introduction by Lynne Huffer**

In May 1969, the Institut d'Histoire des Sciences et des Techniques at the University of Paris held a colloquium to mark the bicentennial of the birth of Georges Cuvier (1769-1832), anatomist, zoologist, and founder of the discipline of vertebrate paleontology. Foucault opened the colloquium with a brief presentation, followed by a lively discussion with colloquium participants Bernard Balan, Georges Canguilhem, Yvette Conry, Francis Courtès, François Dagognet, and Camille Limoges. The springboard for the discussion was a thesis Foucault had put forward in his 1966 book, *The Order of Things*: that contrary to standard conceptions of Cuvier's fixism as counter to evolutionary thinking, Cuvier made Darwin possible.

In his opening presentation, Foucault elaborates on his earlier claim in *The Order of Things*: that Cuvier's function-based system made possible anatomical disarticulation and thereby created the conditions of possibility for modern biology. Darwin's work, Foucault insists, could not have occurred without the transformation of knowledge brought about by Cuvier. Foucault renders Cuvier as a transitional figure rather than as a static classifier stuck in the classical age; he becomes, as Foucault puts it, the "passage" between the "unity of type" of the classical age and the "conditions of existence" of evolutionary biology.

In focusing on Cuvier as a thinker who made possible the biology that followed, we might recall Foucault's "What Is an Author?," where Foucault describes Cuvier as an "initiator of discursivity."<sup>2</sup> As an initiating practice, Cuvier's discourse "created a possibility for something other

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<sup>1</sup> Originally published as "La situation de Cuvier dans l'histoire de la biologie," *Revue d'histoire des sciences et de leurs applications* 23, #1 (janvier-mars 1970, pp. 63-92.) (Journées Cuvier, Institute for the History of Science, Paris, May 30-31, 1969). Republished in Michel Foucault, *Dits et écrits I, 1954-1975* (Paris: Quarto Gallimard, 2001), 898-934.

<sup>2</sup> Michel Foucault, "What Is an Author?," translated Josué Harari, in *Essential Works of Foucault, 1954-1984*, Volume 2: *Aesthetics, Method, and Epistemology*, ed. James D. Faubion (New York: New Press, 1998), 217, translation modified.

than [his] discourse.”<sup>3</sup> From this perspective, Cuvier made possible “a theory of evolution diametrically opposed to his own fixism.”<sup>4</sup> Unlike the normative founding of a science, the discourse called “Cuvier” initiates a discursive practice but “does not participate in its later transformations.”<sup>5</sup> Rather than determining the laws and structures that have come to define evolutionary biology, Cuvier’s discourse engages the history of biology recursively: as a discursive practice, “Cuvier” necessitates a “return to [an] origin” whose forgetting is itself constitutive of the field and “part of the discursivity in question.”<sup>6</sup> This means, Foucault insists in “What Is an Author?,” that we must return to the “text itself,” both for “its nudity” and for the textual “lacuna” such a return exposes.<sup>7</sup>

Today we might read the translated colloquium in that recursive mode: as a return to the origin of a discursive field, and as an approach to a text whose lacunary nudity Foucault had anticipated, three months earlier, in a more famous 1969 colloquium, “What Is an Author?” Toward the end of the Cuvier colloquium, Foucault refers indirectly to “What Is an Author?,” lamenting his own use of the proper name, Cuvier. “Only with difficulty can the analysis of these transformations be referred to a precise individual,” he says. He admits to feeling uneasy about the proper names he used in *The Order of Things*: Cuvier, Bopp, Ricardo. “My concern,” he says, “is to track transformation. In other words, the author does not exist.”

Originally published in 1970 in *Revue d'histoire des sciences et de leurs applications*, the 1969 Cuvier colloquium appears here in English in its entirety for the first time.<sup>8</sup> “Cuvier’s Situation in the History of Biology” not only offers new insights into Foucault’s thinking, but also gives a glimpse of a range of scientific reactions to his work. The colloquium as a whole represents a valuable resource not only for Foucault specialists, but also for those interested in the contemporary return to biology in theory and philosophy, the rise of new materialisms and animal studies, and the Anthropocene discourses that draw, implicitly, on Cuvier’s paleontological perspective on extinction.<sup>9</sup> The Cuvier colloquium covers a range of topics: anatomical function and situation; the epistemological and ontological thresholds that divide class, order, genus, and species; the

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<sup>3</sup> Ibid., 218.

<sup>4</sup> Ibid.

<sup>5</sup> Ibid., 219.

<sup>6</sup> Michel Foucault, “Qu’est-ce qu’un auteur?” *Dits et écrits I, 1954-1975* (Paris: Quarto Gallimard), 836, translation mine. This section of Foucault’s original lecture, presented in French at the Société Française de philosophie in February 1969, was omitted from the published English translation.

<sup>7</sup> Ibid., 836.

<sup>8</sup> For an earlier English translation of Foucault’s presentation without the discussion that follows see “Cuvier’s Position in the History of Biology,” trans. Felicity Edholm, *Critique of Anthropology* 4 (1979): 125-130.

<sup>9</sup> As Martin Rudwick explains, Cuvier’s “fossil bones seemed ‘to prove the existence of a world anterior to ours, destroyed by some kind of catastrophe.’” See Martin J. S. Rudwick, *The Meaning of Fossils: Episodes in the History of Palaeontology* (Chicago: University of Chicago Press, 1972, 109). Also see Martin J. S. Rudwick, *Georges Cuvier, Fossil Bones, and Geological Catastrophes: New Translations and Interpretations of the Primary Texts* (Chicago: University of Chicago Press, 1997).

limits of extension; the importance of milieu; perspectives on the classical scale of beings; Buffon, Linnaeus, and Geoffroy Saint-Hilaire; spider monkeys and cephalopods; the relations that bring together human hands, cats' claws, and bats' wings; the connections between dogs' paws and seals' flippers.

Through all of this detail, Foucault makes the case for a "Cuvier transformation" that prepares the way for the modern life sciences. Accompanying the eruption of life in the 19<sup>th</sup> century are three specifically modern philosophical preoccupations: death, sexuality, and history. Foucault ends the colloquium by insisting on the humanism subtending this eruption: "humanist philosophy: every philosophy that views death as the last and ultimate meaning of life;" "humanist philosophy: every philosophy that thinks sexuality is for loving and proliferating;" "humanist philosophy: every philosophy that believes history is tied to the continuity of consciousness." Foucault's reminder that these humanist themes, which provide the "philosophical sanction" of a transformation that began with Cuvier, might give us all—humanists, posthumanists, and anti-humanists alike—some much needed food for thought.

### **Presentation by Michel Foucault:**

I would like to specify what I mean by the term epistemological transformation, and illustrate this with two examples.

The first concerns biology: the position of the individual and of individual variation in biological knowledge.

It could be said that if there were anyone who believed in species—anyone who, unconcerned with what existed below the level of species and who, running up against the limit of species, could never get below the level of species to apply biological knowledge to the individual—it would be Cuvier. Cuvier held that everything was arranged from, for, and toward species. On the other hand, we all know what Darwin says about species. For Darwin, species is never, substantively or analytically, the primary and ultimate reality that it is for Cuvier. For Darwin, it is difficult to distinguish species from variety. And he offers numerous examples about which we could not say, as good botanists or zoologists: "this is a species," or "this is a variety." Further, Darwin acknowledged the progressive reinforcement of individual variation. According to him, even within a species we find small variations; those variations become increasingly more pronounced, eventually breaking through the frame which, *a posteriori*, was prescribed for that species; and finally, from variation to variation, individuals are linked with one another on top of and beyond the definition given for their species. In general, Darwin acknowledges that all the taxonomic frames proposed for classifying animals and plants are, to a certain point, abstract categories. For Darwin, then, there is one reality that is the individual and a second reality that defines the "varietivity" [*variativité*] of the individual: its capacity to vary. Everything else (be it species, genus, order) is a kind of construction built from this reality's starting point: the individual. In that sense, we can say that Darwin is absolutely opposed to Cuvier. And curiously, he seems to return to a tendency we find in the classical taxonomy of the 17<sup>th</sup> and 18<sup>th</sup> centuries: the methodists, for example, and Lamarck in particular, asked about the reality of species and as-

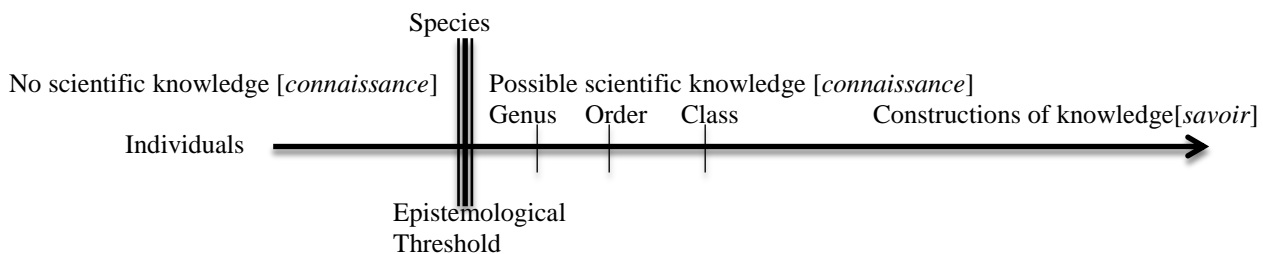
sumed a continuity of nature so tightly woven, without interruption, that even species would be seen as an abstract category. There is a return, in Darwin, to the themes found not only in Lamarck, but also in the methodists of the Lamarckian period. One might ask if, in the history of the biology of the individual, there was not a leap from Jussieu or Lamarck to Darwin that bypassed Cuvier. Such a leap would mean enucleating Cuvier from this history altogether. I think such an analysis would be neither adequate nor completely justified. As is often the case with these phenomena of return, repetition, and reactivation, beneath them lies another complex phenomenon – a highly charged process of transformation.

I would like to show how the individual, or more precisely the critique of species in Lamarck and his contemporaries, is neither superimposable nor isomorphic with the critique of species we find in Darwin. I want to show that the Darwinian critique of species could not have occurred without the transformation, reorganization, and redistribution of biological knowledge brought about by the work of Cuvier. What was that transformation?

Classical taxonomy was essentially *the science of species*: the definition of differences that separate one species from another; the classification of those differences; the establishment of general categories for those differences; the organization of those differences into hierarchies that relate them to one another. In other words, the entire edifice of classical taxonomy begins with specific difference and tries to define high-level differences on a scale calibrated to specific difference.

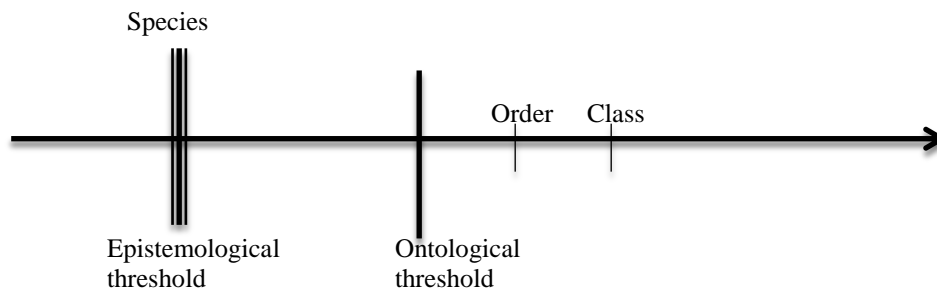
I think we have evidence for the fact that biological classification chooses specific difference as its minimal element, or that it cannot break through to the level below it; for example, Linnaeus says that knowledge [*connaissance*] of individuals and variety is a florist’s knowledge and not a botanist’s difference. He also says that knowledge of variety is important for the economy, medicine, and cooking, but goes no further than this. He adds: knowledge of variety is a practical knowledge. By contrast, theory and science begin beyond the level of species. The existence of this threshold between the individual and species brings with it a series of consequences.

First, between specific difference and individual difference there is a gap, a leap, a threshold. This threshold is the one out of which scientific knowledge can begin. Individual difference is not relevant for science. One can say that between individuals and species there is an epistemological threshold.



On the other hand, if it is true that what is given as the primary object of science is species and specific differences, then everything that will be built, starting with specific difference—the dif-

ference of differences, or the resemblance of differences, differences more general than specific differences, and thus categories more general than species—will be constructions. Those constructions of knowledge [*savoir*] which, unlike the definition of species, will not be grounded in a given offered to experience; hypotheses that will be more or less verified; hypotheses that will be more or less grounded [*bien fondées*]; hypotheses that will perhaps coincide with facts. And everything above the level of species will not belong to the same ontological category as that which belongs to species or that which belongs to the individual, below the level of species. Between species and genus we will have a new threshold that will no longer be epistemological but, this time, ontological.



This means that it is above the level of the individual that knowledge [*savoir*] will be organized. Starting with species, we enter into the order of knowledge, a knowledge not given but constructed. And below the level of species, we have a set of realities that are effectively given in experience.

From there we encounter the problem of classical taxonomy: how to build genera that are real, or at least—since genera are never real—well grounded [*bien fondé*] genera? Here is the antinomy and opposition between the systematists and the methodists. The systematists say that, in any event, beyond the level of species reality cannot be directly accessed. One must choose a classificatory technique which, although arbitrary, will be efficient and convenient. By contrast, the methodists say that, to some extent, classes and classificatory constructions have to be adjusted to the general resemblances given in experience. A salad and a fir tree cannot be slotted into the same category. But whether based in a natural method or an arbitrary system, it will always be beyond the ontological threshold.

The problem is to know how this configuration of classical taxonomy will be transformed. How one will be able to find, in individuals who from then on will be known across species and genus, the same single thread of reality (for Darwin, this thread will be *genealogy*.) How Darwin will, on the one hand, eradicate the epistemological threshold and show that, in fact, we have to begin by knowing the individual with its individual variations; on the other, he will show how, beginning with the individual, what will be established as its species, its order, or its class will be the reality of its genealogy: a succession of individuals. We have, then, a uniform table, without a system, with a double threshold. This transformation was achieved through the work of Cuvier.

Comparative anatomy, as Cuvier practiced it, made its first impact by introducing itself as an instrument for the taxonomic classification and organization of species. It also had the effect of according the same ontological weight to species, genus, order, and class. The first effect of comparative anatomy was therefore to eradicate this ontological threshold. What comparative anatomy showed was that all the categories super-ordinate to species, above the level of species, are not simply, as in classical taxonomy, kinds of fields of resemblance or analogical groupings to be established, either arbitrarily through a system of signs or through a general patterning of plants and animals, but are instead types of organization. From then on, to belong to a genus, an order, or a class is not to share with other species those characters that are less common than specific characters; it is not to have a generic character or a character of class; it will be to have a precise organization: to have a double lung and heart or a digestive apparatus situated above or below the nervous system. In short, to belong to a genus, a class, or an order—to belong to all that is above the level of species—will be to possess within oneself—in one's anatomy, functionality, physiology, and mode of existence—a certain, completely analyzable structure: a structure which, as a result, has its own positivity.

We therefore have positive systems of correlation. To that extent, we cannot say that genus exists less than species, or that class exists less than species. From species to the most general category, there will be a single identical reality, biological reality, the reality of anatomophysiological function.

The species-genus ontological threshold is thereby eradicated. From then on, ontological homogeneity will proceed—from the individual to the level of species, and to genus, order, and class—within an uninterrupted continuity. By contrast, the embedding of categories in classical taxonomy had been guided by the embedded structure proper to a classificatory table. But in Cuvier we see an anatomo-physiological embedding of all these categories with their internal support. We have this in the individual itself: it is the individual, in its actual functioning, that will carry within itself, in the thickness of its mechanism, all the superimpositions, all the determinations, commands, regulations, and correlations that could exist among the different instances of the table. For Cuvier, the individual will be constituted through the embedding of anatomical functional structures that will constitute its branching, its class, its order, and its genus. Taken together, these structures that are actually present in the individual, that patiently organize themselves and physiologically order themselves within it, will therefore partly define its conditions of existence. By conditions of existence Cuvier means the confrontation of two totalities: on the one hand, the totality of correlations that are physiologically compatible with one another and, on the other, the milieu in which it lives: the nature of molecules that it has to assimilate into itself either through respiration or through feeding. Thus one finds, at the beginning of *Revolutions of the Globe*,<sup>10</sup> a passage where Cuvier shows how the conditions of existence function. The individual in

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<sup>10</sup> Georges Cuvier, *A Discourse on the Revolutions of the Surface of the Globe, and of the Changes Thereby Produced in the Animal Kingdom*, Paris, 1825.

its real existence, in its life, is nothing other than the totality of both taxonomic and anatomo-physiological structures, and this totality is also present in some way in the individual, within a given milieu. As a result, we have two series: one where the individual falls below the level of knowledge [*savoir*], where species, genus, and order have been ontologically linked to one another; and another, with the real life of the individual, and the milieu within its specific generic characters are found, functioning. Two types of knowledge [*connaissance*] can therefore be established: a comparative anatomy which allows for the selection, at the level of individual, of the most general characters and the most global structures, in order to track the class, order, genus, and species to which it belongs; paleontology will begin with the individual as it can eventually be observed at the sub-individual scale when dealing with an organ, then, considering this organ, it will be able to recover the species by paying attention to the milieu in which it lives or by relying simultaneously on anatomical considerations and considerations of milieu. Thus we have two epistemological lines: one for comparative anatomy and one for paleontology, which are two systems of knowledge [*savoir*] that are different from the system of classical taxonomy. The ontological and epistemological thresholds are thus eliminated. We also see how this made Darwin possible. To make Darwin possible does not mean that after Cuvier there were no more transformations or that Darwin did not add a certain number of other transformations. In particular, what is unique to Cuvier, and the limit of the Cuvier transformation, is that in order to accommodate the two lines to each other, Cuvier was obliged to admit to a finality, which means that, with creation, class, order, genus, and species were calculated in a manner that would allow the individual to live; we have a kind of predetermination of the real conditions of the life of the individual through this system of finality. On the other hand, according to Cuvier the individual carries within itself the characters of species, of genus that are, for it, inescapable determinations. From there, fixism. Fixism and finality are the supplementary theoretical conditions that Cuvier was obliged to include in order to make his system work—this system that conditioned the whole of his knowledge [*savoir*]. This analysis of comparative anatomy with the thread of finality that guides it defines what Cuvier calls the unity of type. By contrast, the movement by which Cuvier conducts his analysis, starting with a given individual, species, or genus, within the conditions of the milieu in which it functions, is the analysis of conditions of existence. One can say that Cuvier was only able to make his system hold together by submitting the conditions of existence to the unity of type. This is what Darwin did, as he says in fact in *The Origin of Species*:<sup>11</sup> it is to free the conditions of existence in relation to the unity of type. The unity of type is fundamentally no more than the result of work on the level of the individual. Darwin had to modify the very meaning of conditions of existence while, for Cuvier, conditions of existence depended on the confrontation between, on the one hand, the anatomo-physiological baggage that characterizes the individual

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<sup>11</sup> Charles Darwin, *On the Origin of Species by Means of Natural Selection or the Preservation of Favoured Races in the Struggle for Life*, 1859.

and encloses within it the taxonomy to which it belongs and, on the other, the milieu in which it lives.

Starting with Darwin, conditions freed from the unity of type will become the conditions of existence given to a living individual by its milieu.

We can thus describe the transformation out of the species-individual problem of the classical age into the species-individual problem in Darwin. It seems to me that the passage from the one to the other could not have happened without an entire consolidation of the epistemological field of biology we see operating in Cuvier's work. And whatever the errors committed by Cuvier, we can say there was a "Cuvier transformation."

### **Discussion:**

*J. Piveteau:* The paleontologists and anatomists who have closely followed Cuvier's work—who consult it in the laboratory, who use it—obviously have never reached such an extended epistemological analysis. But I can tell you they would be very satisfied by it. It's very illuminating to see such a presentation.

*F. Dagognet:* Over the course of your remarks, a somewhat negative attitude about "taxonomy" comes through, I think. Aren't you turning it into an abstract knowledge cut off from nature?

And yet, nothing seems more extraordinary to me. For example, Jussieu, better than anyone, rediscovers reality. With his system and his reference points, he only needs a few indications in order to know everything, to derive or deduce everything.

*M. Foucault:* I began by saying that there was a return in Darwin back toward Lamarck and Jussieu. I should perhaps offer a correction. It is indeed true that, since the middle of the 18<sup>th</sup> century, there was always an attempt to:

1. lower the epistemological threshold a bit below the level of specific difference;
2. raise the ontological threshold a bit above the level of species. This was the case for the methodists; they criticized the Linnaean system for being arbitrary and for grouping together beings that perhaps had the same characters, on condition that certain elements (sexual organs, for example) were not taken as differentiating characters. But if one takes more general, more visible, and more immediate criteria (general plant or animal morphology), and if one establishes groups, genera, orders, and classes—groups that acknowledge the totality of resemblances—one will end up with a grounded [*fondé*] classification. When I say "grounded," I do not mean to say that the classification is cut off as an actual discontinuity. In other words, I do not think that Jussieu or Lamarck imagine that genera exist in a clean, neat way, or are inscribed in the individual organism itself.

*F. Dagognet:* Unfortunately yes. For Jussieu, there is a character....

*M. Foucault:* But it is necessary to make a distinction between grounded [*fondé*] and real [*réel*]: a taxonomic category is grounded [*fondée*]:

1. if, in fact, in the continuum of differences, the category groups individuals that are close to one another within the continuum; and



2. if one can, between the last element that belongs to this category and the first of the elements of the subsequent category, find a determination that is visible, certain, easy to establish, and recognizable to everyone. Those are two criteria for a grounded category.

To the grounded [*fondée*] category, the methodists oppose the non-grounded [*non-fondée*] category of the Linnaean type. We can criticize Linnaeus for establishing categories deduced from different groups of individuals, for grouping together beings that belong to completely separate fields of resemblance and, under the pretext that their sexual organs conform to each other in the same way, for establishing a category that leapt, in a way, beyond immediately given resemblances; in that sense they constituted abstract categories, or non-grounded [*non fondées*] categories. What Jussieu, Lamarck, and the methodists all want to create is a grounded [*fondé*] genus.

*J.-F. Leroy*: I don't understand. You said that species is the given for classical taxonomy. Genus is the constructed. I claim that the first entity that became apparent to the naturalist was genus, especially among botanists. At the end of the 17<sup>th</sup> century, Tournefort defined genus. Genera appeared to him, not species. Species was not recognized until Tournefort. When we observe nature, it is genera and even families that appear. We are far from species. To such an extent that the notion of family was discovered very early. There were families of plants, umbellifers, composites. These were all more striking than species. They were totalities. Also, you speak about Cuvier's introduction of positivity with regard to genus. You don't think that at the end of the 17<sup>th</sup> century there was already positivity. Obviously you will tell me that this was natural history. I don't see why you make it stop with Cuvier. As for biology, whose existence for you begins with Cuvier, I see it being constituted long before the 18<sup>th</sup> century and even in the 17<sup>th</sup> century. It defined itself progressively. Naturalists were engaged with natural history. They were not conscious of engaging with biology but, little by little, they came closer to biology which, at a certain moment, couldn't help but become conscious of itself and constitute itself as an autonomous discipline.

*M. Foucault*: I subscribe to what you've just said. Even before Linnaeus, great families like umbellifers were established. When did I say the opposite of this? I tried to define the way in which, from Tournefort to Lamarck, taxonomic tables were established. That doesn't mean that in the history of botany all species were successively recognized, and subsequently ordered and grouped. I'm looking for what was created as the *law of construction* for constituting something like taxonomy. As for natural history and biology, I don't know what you mean by biology. For my part, in perhaps an arbitrary way, what I meant by natural history was the totality of methods through which living beings were defined as objects for possible classification, and what relations of order were established among them. From the end of the 17<sup>th</sup> century to the beginning of the 19<sup>th</sup> century, in order to define an object to be classified, in order to establish methods of classification, in order to create descriptions that allowed for classification, a certain number of rules were put into place that characterize what I've called natural history, taking up the word frequently used at the time. It is understood that during this period many experiments were conducted with the aid of a microscope, and numerous studies were done on human and animal physiology. But having systematically bracketed that, I've said fairly clearly: my problem was to know how, indeed, for a certain number of years, living beings were classified. Consequently, I subscribe to

what you say when you affirm that biology began before Cuvier. It's somewhat the same problem with grammar. When I studied grammar, philologists told me that historical studies had already been done on Latin. That was not my concern. It was to study what was once general grammar: how language [*la langue*] in general was given as an object of possible analysis.

J.-F. Leroy: When I say that natural history continues, I mean that a certain way of classifying, more or less, continues.

M. Foucault: Yes, classifying continues and indeed one begins again to classify by using a certain number of methods not without analogy to those utilized in the 17<sup>th</sup> century. The way Cuvier classified species was different. This was where I thought I recognized a characteristic transformation. I've never held that the classification of living beings ceased with Cuvier. I called natural history, perhaps conventionally, a mode of classification but also a certain mode of definition of objects, concepts, and methods.

J. Piveteau: Contemporary classification is completely different. We try to find an order of genesis, whereas in the time of Cuvier it is a logical order. It is easy to transpose Cuvierian classification into this order of genesis. That was what Daudin showed before and what we do every day.

F. Dagognet: The grounded order [*l'ordre fondé*] is not the preexisting order. A plant has been definitively classified, it has a unique character that makes it belong to a totality and...

M. Foucault: That's the grounded order [*ordre fondé*].

F. Dagognet: Why would it not be real?

M. Foucault: To the extent that a natural continuum was accepted at that moment....

F. Dagognet: It was not accepted.

M. Foucault: .... the cut between genera can only be a cut that comes from our knowledge [*connaissance*] and not a cut that comes from nature itself. It is neither absolute nor invariable, Adanson says.

F. Dagognet: Jussieu says in fact that this cut is in nature and he found its key.

M. Foucault: The fact that he found the key that allows him, in this natural continuum, to draw on a set of homogenous criteria which, from one end to the other, allow him to create fixed groups, shows that his method is grounded [*fondé*]. But he compares natural continuity either to a chain or to a geographical map.

F. Dagognet: Genus and individual are clearly separated. The individual is the living [*le vivant*] when it has developed. But the seed is the summary of individual and genus. We can read genus as we read the individual.

M. Foucault: "Grounded" [*fondé*] means that genus is not arbitrary, as opposed to arbitrary genus in Linnaeus. Grounded genus [*genre fondé*] will be natural. And the word *natural* returns repeatedly when we are dealing with method. I don't think you have the right to use the word 'real' when naturalists used the word grounded [*fondé*] or *natural*. Adanson speaks in fact about "real" divisions, but only to say they are not real except in relation to us and not in relation to nature. The real cuts will be those cuts attributable to catastrophes in Buffon; for Lamarck they are attributable to conditions of existence.

The great discussion which, around 1830, pitted Cuvier against Geoffrey Saint-Hilaire, influenced, in part, how to go about identifying an element, an organ, a biological segment across an ensemble of species or genera: to what extent and in the name of what can one identify a man's hand, a cat's claw, a bat's wing?

In classical taxonomy, there was no debate about an identity among elements. It was immediately given, because it was precisely that which was relied upon for the classification of beings. Where an organ presented, from one individual to another, from one variety to another, identical elements—and very visibly identical, by size, volume, and configuration—then one was dealing with a character: the problem was to know if it was limited to species, if it counted for the entire genus, or even beyond. The point was to establish the limits of extension of an immediately recognizable identity. Even more, Linnaean taxonomy established the variables that alone were supposed to be relevant for defining a difference and consequently a limit within identity: only variations of form, height, disposition, and number were retained (color, by contrast, did not affect the identity of an element and did not become part of the scientific character). In sum, we can say that in this natural history identity was immediately visible and that its limits were systematically constructed.

For Geoffroy Saint-Hilaire, identity can be hidden. It is not immediately apparent that the pieces of membrane one finds over the gills of fish can be put in correspondence with the ossicles of the inner ear in higher vertebrates. As for the limits established by the systematists, we need to challenge them one by one. A difference in number should not impede the tracking of an identical element (the hyoid bone is composed of five little bones in man, nine in cats); a difference in size is not necessarily relevant: one needs to recognize a thumb in the rudimentary fingers found in certain spider monkeys; form, also, can vary against a background of identity (one has to learn to move from a dog's paw to a seal's flipper); finally, disposition can change without the disappearance of identity (the cephalopod can be considered as a vertebrate bent forward to bring the pelvis and the legs closer to the head). Geoffroy Saint-Hilaire therefore does not retain any of the criteria of identification usually accepted in the 18<sup>th</sup> century.

Further, he directly challenges the functional criterion of identity: the same function can be assumed by different elements (will one say that a crutch is a leg?); the same ensemble of elements can have very different functions in a child or in an adult (a child's feet are of no use for walking, and yet they are feet).

At the same time, Geoffroy Saint-Hilaire acknowledges the identity of a biological element across so much diversity, if the situation or the transformation in the species that would make it recognizable can be established. Therefore, he says, I give the name foot to the set of anatomical elements that will follow from the third segment of the lowest limb of an animal.

The foot is a certain anatomical *situation*, or again, I can recognize a human hyoid bone in that of the cat because I can define the elements that grew together, those that have disappeared, those that persist as ligaments, those that have changed their profile. Identity is not a visible given: it is the result of the establishment of a relation (of an "analogy," says Geoffroy Saint-Hilaire) and the tracking of a transformation.

How did this passage occur between the “taxonomic” identity of natural history and this analogical identity? Once again we must refer back to Cuvier. Cuvier, like Geoffroy Saint-Hilaire, accepts a general principle of analogy: “The bodies of all animals are formed by the same elements and are composed of analogous organs.” Further, for Cuvier as for Geoffroy Saint-Hilaire, the correspondence between two organs is not established by an identity of form (from worms to higher vertebrates, they grow in complexity), nor by proportion (depending on the animal the level of respiration and movement can vary), nor by position (in the animal kingdom, there is a spatial inversion of the nervous and digestive systems.) Neither Cuvier nor Geoffroy Saint-Hilaire accept any of the criteria of identification used by the classical thinkers. The disappearance of these criteria is a transformation common to Cuvier and Geoffroy Saint-Hilaire.

So it seems to me that this transformation was implied by comparative anatomy as we find it in Cuvier; and that it, in turn, made possible two different systematizations—that of Cuvier and that of Geoffroy Saint-Hilaire.

1. Comparative anatomy made possible a confrontation of species that did not happen gradually, but moved from one extreme to the other. It made it possible to retain everything living beings held in common, whatever their complexity or their level of organization. It made it possible to seize each set of elements in their maximal transformation. And, as a result, the criteria for identification (form, size, disposition, number) that counted for establishing close differences had to be excluded. The space of differentiation changed in scale.

2. Cuvier and Geoffroy Saint-Hilaire therefore had to resolve the same problem: to track an organic identity by following a constant that was not immediately given to perception. Cuvier extracts this constant from function, which remains the same across the diversity of instruments that make it operate: respiration, movement, sensation, digestion, circulation. Geoffroy Saint-Hilaire refuses it for the reasons I just explained; and he replaces it with the principle of position and transformation in space.

We have two solutions: the functionalist solution and the topological solution for resolving the same problem born out of the same transformation: the eradication of visible criteria for the identification of biological segments. Two solutions that produced two different outcomes in the history of science. On the one hand, Cuvier, in looking to function for factors that would allow for the individualization of organs, made possible the doubling of anatomy by a physiology that became increasingly independent. Through this operation, Cuvier in a way made it possible for physiology to emerge out of anatomy. On the other hand, Geoffroy Saint-Hilaire introduced a certain analysis of interior space into the individual through his discovery of topological criteria. Geoffroy Saint-Hilaire therefore enriched anatomy while Cuvier liberated physiology.

In this way the two solutions, applied to the same problem and emerging from the same transformation, find their place in the history of biology: one for the development of physiology, the other for the insertion of topology into anatomy.

It is obvious that the freeing of physiology was more immediately fruitful: from Magendie to Claude Bernard, physiology at that time had achieved an epistemological level that made possible its direct use in biology. By contrast, by inserting a topological analysis into the anatomical

relation, Geoffroy Saint-Hilaire took part in a more risky operation which, at the time, might have seemed fanciful. In fact, Cuvier did not understand this move by Geoffroy Saint-Hilaire. He saw in it the reappearance of the theme of naturalism [*Naturphilosophie*]. In reality, it was also something else. Topology as an applied science did not become useful until long after 1830. It made sense for Cuvier to be effective and productive at that very moment. Geoffroy Saint-Hilaire, by contrast, remaining in a sort of limbo in the history of science, was not able to effectively return to being productive until after the moment when normal anatomy, like teratology, was able to recognize the problem of topology.

*J. Piveteau:* In tracing in our own research the outcome of the two great principles in Cuvier and Geoffroy Saint-Hilaire, the principle of correlations and the principle of connections, we have become increasingly aware that, for us, these principles are complementary principles. The principle of correlations gives unity to the animal while the principle of connections gives unity to the animal series. We need both principles. The principle of correlation makes possible reconstructions with Cuvier. The principle of connections makes it possible to track all along the way a genetic series of transformations. Right now we do not see the need to oppose them to each other. Following this research, we can think of each of these as two complementary principles that do not work on the same level.

*Y. Conry:* 1. Is there not in Cuvier's work a condition of impossibility for a thought of evolution, specified as a Darwinian theory of evolution? This condition of impossibility could be articulated thus: in Cuvier's "discursive field," isn't the representation of the organism, within the modality of strict correlations, an obstacle, even the major obstacle, to a theory of evolution?—in other words, I refer here to the context of this morning's lecture by M. Limoges.

2. M. Foucault said that the transformations we find in Darwin came about through the texts of Cuvier.

a. If we accept this assertion, I would like to know how it is that the foundation of Darwinian thought is found *elsewhere* than in Cuvier's discursive field. I'm alluding to the ecological and bio-geographical context that is the determining place for Darwin's thought. It seems to me that if we accept that Darwin was formed elsewhere than in Cuvier's field, then Cuvier cannot even be a relay in relation to a theory of subsequent evolution.

b. How to explain the resistance to Darwinism in the name of the Cuvier school (for example, in Flourens, one of Cuvier's disciples)?

c. M. Foucault's proposal outlining the disappearance of thresholds ended up convincing me that Cuvier does in fact constitute a moment of rupture in relation to the 18<sup>th</sup> century. But doesn't this pattern remain indifferent with regard to the problem of evolution?

B. Balan: 1. The first question concerns the nature of the internal link.<sup>12</sup> You said in *The Order of Things* (p. 263): “The internal link by which structures are dependent upon one another is no longer situated solely at the level of frequency; it becomes the very foundation of all correlation.”

I’m convinced that the passage from the problems of the classifiers of the 18<sup>th</sup> century to the problem of species in Darwin involves a new conception of the internal link, an analysis of the internal structure of the organism. On this point, the principle of correlation in Cuvier plays a crucial role, and could have an importance that goes beyond Cuvier. But this leads me to ask about the nature of the internal link which, taking into account the development of the “Cuvier” paragraph in *The Order of Things*, leads to Cuvier’s adoption of a finalist, vitalist, and fixist view.

From there the problem: does the idea of the internal link require a principle of correlations in the way Cuvier proposed? I take up again the argumentation I developed with regard to the elaboration of chemical theory. In his letters to Pfaff,<sup>13</sup> Cuvier early on made clear his interest in chemistry: the chemistry of Lavoisier. In these letters, he recommends reading Lavoisier and the *Annals of Chemistry*. I remember a letter where he discusses the first six volumes of the *Annals of Chemistry*, which he urges Pfaff to read; in the seventh volume, he recommends the analyses made by Lavoisier about problems of chemistry relating to Pfaff. Cuvier distances himself from Aristotle who, for good reason, had no knowledge of the laws of physics and chemistry. This gives me a sense of the great importance of the role played by chemistry in the *Lessons of Comparative Anatomy* (the 1<sup>st</sup> lesson and the 24<sup>th</sup>, t. IV) and the letter to Lacépède. In Cuvier we find the

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<sup>12</sup> A mimeographed text distributed before the session articulated the question as the following: “Is it really Cuvier rather than Lavoisier who constitutes the moment of rupture between natural history and biology (if there was a rupture)?

*The Order of Things* (p. 263): “The internal link by which structures are dependent upon one another is no longer situated solely at the level of frequency; it becomes the very foundation of all correlation.”

This internal link seems to refer to the “calculation” [“calcul”] of the nature of beings. Cf. *The History of the Progress of the Natural Sciences*, 1826, t. I, p. 249. This calculation itself seems grounded in *respiration*. Cf. *Lessons in Comparative Anatomy*, 1805, t. IV, 24<sup>th</sup> lesson, p. 168: importance of circulation grounded in blood, vehicle of oxygen. The perspective on *oxygenation* makes possible the introduction of a *quantitative* point of view. Cf. op. cit., p. 172 and following.

This problem seems homologous to the one in *Fourcroy*. Cf. *System of Chemical Knowledge*, Brumaire year IX, section VIII, order IV, art. 2, 7 (t. X, p. 373 sq.), art 11 and 12 (pp. 405-413).

Finally, the theory of combustion is understood by Cuvier to be the most important in the revolutions approved by the natural sciences since the 18<sup>th</sup> century. Cf. *The History of the Progress of the Natural Sciences*, 1826, t. I, p. 62 sq.

*Consequences*: can these ruptures not be understood as the result of a series of displacements producing new articulations within a preliminary notional system?

Does the existence of such a game of displacements not make the consideration of a domain or a plurality of domains impossible without taking into account, at the same time, the articulation of a chosen object or objects of study in relation to the general network of available scientific concepts at a given moment?

<sup>13</sup> Letter dated December 31, 1790.

possibility of interpreting physiology out of chemistry, the chemistry of Lavoisier. And that refers us back to the text by Fourcroy.<sup>14</sup>

There are texts present in *The History of the Progress of the Natural Sciences* where we see that the principle of the correlation of forms was preceded by the problem of the correlation of functions. This problem concerns the relation between circulation and respiration. Finally, respiration took the first step because of a theory of oxygenation that introduced a quantitative point of view; but this point of view was subsequently eliminated. As for modern scientific physiology, Cuvier was able to contribute to its founding because of the importance he attributed to chemistry in his system. But I had the impression that after having spoken about physiology in his work, he then dodged it. Starting from the moment when the correlation of functions is transformed into the correlation of forms, at that precise moment I think we can no longer say that Cuvier directly introduces physiology. It seems that experimental physiology will move to the sidelines. With Cuvier, one can conceive, with much nuance—with Dareste and Milne-Edwards it will be different—that there is a comparative physiology that is established. But that physiology tends to orient itself toward metaphorical pseudo-explanations that have little to do with experimentation in the status it achieved with Claude Bernard. But this is about a physiological experimentation whose principles go back much further.

Even if one considers the principle of correlations as necessary for moving from a pre-Cuvierian theory of species to a post-Cuvierian theory, one can ask oneself if the principle itself justifies finalism and fixism. In fact, I found a text of the reports by Geoffroy Saint-Hilaire and Latreille on the *Mémoire of Laurencet and Meyran*; in this report, Geoffroy Saint-Hilaire and Latreille take up again the principle of correlation against Cuvier himself (in *Minutes of the Academy of Sciences*, February 15, 1830, t. IX, 1828-1831). The problem involves knowing if, with vertebrates and invertebrates, one is dealing with a totality that is differently interwoven or differently combined. "In order to prove this proposition, we must find there the subject of a text that supports the most surprising of anomalies. There would be more to do there than to support the opposite thesis, because we would have to admit that these organs could only exist if they were generated by one another and, because of the reciprocal suitability of the circulatory actions of the nervous system, would give up the possibility of belonging to each other, to be together in agreement. But such a hypothesis is not admissible because as soon as there is no longer harmony among organs, life ceases. Thus, more animal, no more animal . . . But if, on the contrary, life persists, it is because all the organs remain in their habitual and inevitable relations, and play amongst themselves as they ordinarily would, from moment to moment; they are linked together by the same order of formation, subjected to the same rule and, like all animal compositions, will not be able to escape the effects of the universal law of Nature: the unity of composition."

Further, the unity of composition is a transformational model that makes it possible to introduce the problem of specifically experimental teratology. Consequently, the correlation be-

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<sup>14</sup> *The System of Chemical Knowledges*, year IX, t. X, p. 363 sq.

tween organs is functionally justified by the necessity of survival: it can also function independently of fixist theories, within the context of rules of development.

Is the problem of the correlation of functions necessary? Couldn't the principle of connections we find in Geoffroy Saint-Hilaire fulfill the same function?

I will justify this point of view, taking into account the students of Cuvier, Richard Owen, and Milne-Edwards. From the point of view of specialists in morphology, comparative anatomy, or embryology, it appears they were not able to preserve the principle of correlations as Cuvier had developed it, except in paleontology. I'm thinking specifically of Richard Owen. There we see the abandonment of the principle of functional correlations in favor of the systematic utilization of principles of connection considered as heuristic principles in comparative anatomy. This, in fact, is how I read Richard Owen's *Lessons in Comparative Anatomy*, and his work on the archetype and homologies in the vertebrate skeleton. There is a transformation in the problems posed following taxonomy of the 18<sup>th</sup> century. What are the conditions of this transformation and what is the philosophy implied by these conditions? I think that the reworking of principles from the beginning of the century made more things possible than a theory of life defined through interiority. Because we cannot speak of interiority when we read texts like those by Virchow or Haeckel and his school.

2. The second question concerns the scale of beings.<sup>15</sup>

I remain unconvinced concerning the problem of the scale of beings, for it is necessary to distinguish between what a certain number of authors affirm on a theoretical level, on the one hand, and on the other, the elements that they refuse to take into consideration, about which they refuse to speak, but which seem to play a deep role, and that force them to introduce gaps and complications into the pattern. These complications in the pattern could be considered, in some

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<sup>15</sup> A mimeographed text distributed before the session articulated the question as the following: "Is Cuvier's fractioning of the scale of beings a major fact? *The Order of Things*, pp. 271-272. Cf. *Mémoire Concerning the Hyale Animal, a New Genre of Mollusk but Intermediary between the Hyale and the Clio, and the Establishment of a New Order in the Class of Mollusks*, 1817, where the idea that apparent voids are often there only because we do not know all beings. Cf. p. 10: the Pneumoderma: neither cephalopods nor gasteropods nor acephalous organisms: tendency of nature to utilize all possible combinations. Cf. Colman, *G. Cuvier, Zoologist*, pp. 172-173.

There is a substitution of beam for scale in a context of plenitude. This plenitude appears as a constant in Cuvier's thought. Cf. Daudin, *Zoological Classes and the Idea of Animal Series*, 1926, t. II, p. 249 sq.

*Consequences*: is the introduction of difference not in fact due to an antagonism between, on the one hand, the scale of beings and, on the other, the renewal of the living combination thanks to perspectives opened up by chemistry?

Also, if difference introduced in this way constitutes a possibility for thinking life, does that new possibility not belong to a system that confronts it with the theme of the scale of beings and the conception of the plan that morphologists created starting with Goethe?

In other words, do we have a founding space of new oppositions or a new opposition that appears inside a preliminary system which, on the one hand, has become inadequate but which, on the other, always constitutes an efficient system of reference points?



way, as anticipatory elements of later breaks. In fact, I think that in the field of neurology, for example, you've had a period characterized by the principle of localizations that developed in a completely coherent and systematic way. But it seems that the augmentation of the clinical material led to complications in the theory, and it's starting from the moment when theory became so complicated that in fact a break occurred and a certain number of authors agreed to figure out how to approach this problem in a completely different way.

During the 18<sup>th</sup> century, various kinds of devices for observation had accumulated. These devices produced a point of rupture and Cuvier was the first beneficiary of this rupture. All the givens brought forward by specialists in zoology and paleontology made it no longer possible to think the living world within the frame of the scale of beings. It had to be reworked. We therefore have this question: what will this system of concepts be that will end with the most important and most effective reorganization on the epistemological level? There is a problem of the opening of a new epistemological field. There is the problem of how the opening of this epistemological field will happen. With Cuvier, there are ideological elements. What is the role of these elements? To what extent have these elements subtended scientific research? Have they served as obstacles to this scientific research?

*M. Foucault:* It seemed to me that there are three technical questions that we could discuss.

1. The first concerns the possibility or impossibility, beginning with Cuvier and his biology, of thinking the history of living beings.

2. The problem of the continuity of beings and the way in which Cuvier conceived, exorcized, banished, reutilized, fragmented, whatever you like, the scale of beings.

3. The relation between Cuvier's biology and certain closely related sciences and, in particular, chemistry.

There are also two series of general questions, methodological questions.

1. The problem of method concerning the very functioning of the history of science. And first the notion of obstacle. What do we mean when we say Cuvier was the obstacle to.... or that the chain of beings was an obstacle to....?

2. The problem of the individual or individuality. We have spent our time saying: "Cuvier," "Geoffroy Saint-Hilaire," or "That happens through Cuvier" or "We find that in Cuvier's work." What is this curious individualization? How do we manipulate the concepts of author, work, individual when we create the history of science?

#### **A. Let's first look at the problem of chemistry.**

Cuvier says himself that Lavoisier was an important moment in the natural sciences.

M. Balan, from there, presents the problems of calculation and quantification: he asks if there was not in Cuvier, at a given moment, the temptation, the open possibility of using chemistry inside biology, of introducing experimental methods and quantitative analyses.

I will make a few remarks. In the text cited, Cuvier talks about Lavoisier and his importance; he speaks of Lavoisier's importance in the "natural sciences." This is something completely different than "natural history." The natural sciences constitute a higher category than

natural history, which includes physics, chemistry, geography. Lamarck himself makes this distinction. The natural sciences include everything that is not mathematics. Cuvier therefore thinks about Lavoisier and chemistry not in their relation to natural history, but in their relation to the experimental sciences. I'll compare this text to another where Cuvier speaks about someone who was as important as Lavoisier in chemistry: Jussieu in natural history. In general, Cuvier situates Lavoisier and Jussieu in the pantheon of the natural sciences: Lavoisier for chemistry and Jussieu for natural history. But M. Balan's analysis will not be more accepted when it concerns the calculation of quantification in Cuvier.

There is, here, an important problem. In fact, the term calculation [*calcul*] is often used by Cuvier. But what does he mean by it? In the texts from the period 1789-1808, he says that taxonomy calculates the nature of each species, according to the number of organs, their extension, their shape, their connections, their direction. Calculation in Cuvier is not the calculation of quantity, but in some way the logical calculation of variable structural elements. It is a structural calculation and not a quantitative one. Further, when Cuvier uses the vocabulary of quantity, he speaks about it in a different context than that of calculation. He speaks about it with regard to physiological or chemical processes of respiration. But to say what? That the force of the movements of vertebrates depends on the quantity of their respiration; that the quantity of respiration depends on the quantity of blood reaching the organs; and that this quantity of blood reaching the organs depends on the disposition of respiratory organs and organs of circulation. These organs of circulation can be double. The quantity of blood is therefore important. The organs can be simple, and the quantity of blood lesser. In this way quantity becomes purely a matter of interpretation. It is a matter of degree. There is more or less movement, more or less blood. Cuvier never utilized measurement to calculate quantity. Consequently, these three notions: calculation, quantity, measurement, all of which, for us, are associated with one another, for Cuvier are curiously distinct. We have:

1. a calculation that is the structural calculation of organic variables;
2. the consideration of quantity which in some way is an interpreted quantity;
3. absence of measurement.

It cannot be said without extreme confusion that Cuvier's consideration of chemistry ever opened up for him the possibility of a certain quantitative and measurable biology.

On this subject I would like to introduce a remark. In the history of science we must make a clear distinction between two different processes.

We assert sometimes the actual introduction of an epistemological field constructed elsewhere into another epistemological field. This happened, for example, when the relatively closed and autonomous epistemological field, about which I tried to define the principles of closure and autonomy and that one could call taxonomy, toward the end of the 18<sup>th</sup> century was traversed and penetrated by another epistemological field constituted elsewhere, that of anatomy. The intertwining of these two different epistemological threads determines a new discourse that can be characterized as biology. I do not want to say that this is the only interference that was produced. That the physiological field, to the extent that it existed then, was introduced is another fact. From

there we must distinguish the possibility (given by the constitution, organization, and distribution of the epistemological field) of utilizing— either generally or regionally, either at the level of method or at the level of concepts—epistemological elements that function elsewhere. It seems to me, then, that Cuvier's biology as it was constituted and to the extent that it introduced the problem of respiration, introduced a problem that, at a given moment, could not fail to call on chemical theory. And to that extent Cuvier's biology made possible, in its time, the constitution of biochemistry. But it did not carry it out.

**B. Let's examine now the problem of the scale of beings.**

In certain theoretical texts, Cuvier says that the reign of the chain of beings is over. Do these reflexive propositions translate well into Cuvier's actual practice? Are they not, in fact, a kind of ideal claim? Didn't Cuvier's scientific practice, in one way or another, continue to use the chain of beings as its guiding thread?

Cuvier critiques the chain of beings, and not continuity. At any rate, no one ever accepted, even within the frame of classical taxonomy, an actual continuity of beings. In one way or another, either by means of catastrophes, or by means of a blurring due to milieu, one always admitted to a kind of discontinuity. What Cuvier critiques is the affirmation that every being, whatever it might be—as long as it is not the first, or the simplest, or the most complex, or man— is a passage; the affirmation, in other words, that one can find for it in one way or another two adjacent areas at once immediate and symmetrical. Cuvier also refuses the idea of a progressive gradation—the idea that there is a constant difference between beings succeeding one another and that every degree of the scale is occupied, has been occupied, or will be occupied. Finally, in the last place, Cuvier refuses the idea of a single series on which all beings, whatever the criteria of classification one uses, could be uniformly arranged.

There is, then, in Cuvier, a critique of three themes: the theme of passage, the theme of gradation, and the theme of the unity of a series.

However, the concept Cuvier constantly uses is that of *hiatus*. What does he mean by that? He does not mean (and he says so expressly) either the catastrophic disappearance of certain species that would have assured the continuity of a unitary biological chain, or the “chance dissemination” of differences. By *hiatus*, Cuvier means:

1. The first effect of the principle of correlations; if a certain organ is present (or absent), certain others should necessarily be present (or absent); this does not then give us a gradation of species presenting the entire table of all possible presences and absences, but indissociable “packets” of presences and absences. From there the *hiatus* of biological reality in relation to the abstract calculation of possibilities.

2. The effect of the unity of the plan: each great category obeys a certain anatomical and functional plan. Another category will follow another plan. From one to the other, there is an entire reorganization, an entire redistribution. These diverse plans do not constitute a linear series of isolated transformations. Cephalopods, Cuvier says, are not the passage from nothing to nothing.

One cannot say that they are more or less perfect than this or that. They do not result from the development of other animals and they will not develop into perfected animals.

3. The effect of the principle of heterogeneous gradations: if it is true that a unique and global scale cannot be established, one can nevertheless establish diverse gradations: across the species one can follow, for example, an increase in circulation and the quantity of oxygen absorbed; or again, the growing complexity of the digestive system. One can thus arrive at several series, some of them parallel and others intersecting one another. In any case, it is not possible to place them all on a single line and from there to form an uninterrupted series. One cannot run through all of them without discontinuity. What we have is not a scale but a network.

There would be no sense in saying: everything is continuous before Cuvier, everything is discontinuous after him. Classical taxonomy accepted certain forms of discontinuity; and Cuvier, certain forms of continuity. But what is important and should be determined is the very new and particular way in which Cuvier put into play the continuous with the discontinuous.

Here is a precise example: the way in which Cuvier came to define two genera next to the genus *Clio* (*Hyale* and *Pneumoderma*). M. Balan sees in this discovery the application of the old principle of the "chain of beings." The genus *Clio* being isolated and vaguely situated between cephalopods and gasteropods, Cuvier would have sought out indispensable intermediaries: he would have tried to reconstitute the degrees that would allow the lacuna to be filled.

But this is to completely misrecognize Cuvier's work. What did Cuvier actually do?

First, a declaration of principle. "It seems that nature has been too fertile to not have created any principle form without successively dressing it in all the accessory details to which it is predisposed." This text, despite its appearance, does not refer to a continuous chain of beings. Cuvier does not affirm that there should necessarily be an intermediary between the gasteropod and the cephalopod. What he says is that there exists a form, that of the *Clio*, and that this form is single, isolated. Then, following the principle of the richness of nature, one can affirm that when nature has a form it takes advantage of it by varying it and giving it a certain number of sub-models of this general model. This is not about the continuity of the chain, of a passage from one extremity to the other, of a bridge thrown from one shore of nature to the other. It is simply about a principle of nature filling in the form it has been given. It is the saturation of an order in genera. We find an animal like the *Clio*, whose genus is neither among the cephalopods nor the gasteropods. As a function of the principle that nature is miserly and generous (miserly in the number of forms, generous in the way it fills in each of these forms), there should well be other genera that must fill in this species with the form that we see appearing in the *Clio*. This is Cuvier's heuristic principle. He will not seek out other genera next to the *Clio* in order to fill in this family that is still empty, or occupied by a genus. He looks for the *Clio*'s own character and, doing this, he finds two other animals, the *Hyale* and the *Pneumoderma*, who obey the same form. They can constitute a family characterized in this way: free swimming body; distinct head with no other limbs but fins. Thus the progress of research toward constituting the new family was not meant to fill in a lacuna on the scale of beings; it was intended to show how nature fills in form from the moment form is given to it. There cannot be a unique genus in an order; this is the postulation and not:

there should be an intermediary between two different genera. One must saturate the order, finally constituting a multiplicity of genera that in fact say what the full reality of the order consists of.

*G. Canguilhem*: I would like to add a word about the scale of beings by recalling the existence of the article, "Nature," in the *Dictionary of the Natural Sciences*,<sup>16</sup> where Cuvier uses the three scholastic concepts of leap, hiatus, and void; that is, these three concepts that appear in the axioms that Kant comments on in the *Transcendental Methodology*. It is there that he says: there is not a leap, there are hiatuses—despite those who say, referring to the scale of beings, that when they discover a gap an intermediary should be found. However, in multiplying by a hundred the number of known species, these voids remain. And this is what is strange, that one could have reproached Cuvier, in his supposed Aristotelianism, for thinking scholastically, when in fact his refutation and his critiques apply to the three fundamental concepts that scholastic philosophy used when it came to demonstrating the continuity of forms.

*J. Piveteau*: I thank M. Foucault and all who took part in these debates.

*G. Canguilhem*: We thank M. Piveteau, in the name of this institute's teachers and researchers, for accepting to preside over these debates.

*S. Delorme*: I would like to ask Mlle Conry to present her objections.

*Y. Conry*: 1. To say that the critique of Darwinism could only have happened through Cuvier's texts, that Darwinism's conditions of possibility were Cuvier's discourse, leaves two facts unexplained, even unintelligible:

- a. the fact that Darwin's discursive field was foreign to that of Cuvier, that is, it was instigated and developed starting from an ecological and bio-geographical problem;
- b. the fact that a part of the resistance to Darwinism was developed within the context of the Cuvier school itself, however broadly one understands this school.

2. Isn't the pattern of the disarticulation of epistemological and ontological thresholds, if it is really a moment and place of rupture in classical thought, indifferent to a theory of evolution? In other words, does the study of epistemological transformations authorize us to think of Cuvier as Darwin's relay?

*M. Foucault*: Your second question: "How to explain the resistance to Darwinism, by Cuvier's disciples, like Flourens, for example, if it is true that Cuvier was Darwinism's condition of possibility?" touches on the problem of method. I do not think that, in the historical field, we can give the same status or function to resistance on the conceptual level and "archeological" resistance situated at the level of discursive formations.

1. A concept like that of the fixity of species is opposed, term for term, to that of the evolution of species and, as a consequence, can create an obstacle to it.

2. A theory like that of a nature in historical evolution is opposed to that of a nature created once and for all by an all-powerful hand and, from this fact, they resist one another. Already, these two orders of resistance are not the same and do not function in the same way. At a third

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<sup>16</sup> T. XXXIV, 1825, p. 261.

level, that of discursive formations, we can also speak about phenomena of resistance. But they are of a completely different order; they unfold according to very different processes (thus the resistance of a natural history, based in the analysis of characters, to a biology based in the analysis of physiological functions and anatomical structures). On the one hand, this last form of resistance may well be more important and more massive, but it does not necessarily bring with it the longest and noisiest polemics; and, on the other hand, the first two forms of resistance can easily be produced inside the same single discursive formation. I tried to show with regard to Cuvier and Geoffroy Saint-Hilaire how their opposition regarding the criteria for identifying organic segments had the same condition of possibility in the biology they cofounded.

We can now look at the Darwin-Cuvier opposition and the function of relay found in the constitution of Darwinism through Cuvier's biology.

The concept of condition of existence is, without a doubt, one of the fundamental concepts of biology starting at the beginning of the 19<sup>th</sup> century. It does not seem to me to be either superimposable or isomorphic with the concepts of influence or milieu as we find them in the natural history of the 18<sup>th</sup> century. Those notions, in fact, were meant to account for an excess of variety; they were concerned with factors of additional diversification; they served to account for the fact that a type could become another. By contrast, the notion of condition of existence is concerned with the eventual impossibility of an organism continuing to live if it were not as it is and exactly where it is: it refers to that which constitutes the limit between life and death. In a general way, the object of natural history in the classical age is an ensemble of differences to be observed; in the 19<sup>th</sup> century, the object of biology is that which is capable of living and subject to dying. This idea that the living is linked to the possibility of dying refers to two possible systems of conditions of existence:

--conditions of existence understood as an internal system, that is, correlations. If you take away its claws, or if you don't give it teeth that crush, it will die. This is the internal condition of existence, and that implies a biology that articulates itself directly onto anatomo-physiology;

--conditions of existence understood as a threat coming from the milieu or a threat to the individual—of no longer being able to live—if the milieu changes. Biology is articulated through the analysis of the relations between the milieu and the living, that is, through ecology.

The double articulation of biology through physiology, on the one hand, and through ecology, on the other, is contained in the conditions of possibility from the moment one defines the living through its conditions of existence and its possibilities of death.

From then on we see that ecology, as a science integrable with biology, has the same conditions of possibility as physiology as a science integrable with taxonomy. The integration of anatomo-physiology into taxonomy was achieved by Cuvier. The integration of ecology into biology was achieved by Darwin. All this starting from the same epistemological conditions.

C. *Limoges*: There is nothing that contradicts what Mlle Conry and I think. I'm very satisfied with this second response.

S. *Delorme*: The second question, posed by M. Saint-Sernin, involves the difference emphasized by M. Foucault between *grounded* [*fondé*], *natural*, and *real*.

*M. Foucault:* From the moment one accepts a continuum of variations from one individual to another, genera cannot be settled, nor can they exist with perfectly designated thresholds. Nature does not isolate genera; in establishing regions of resemblance, it simply allows for the reestablishment of genera that will be well grounded [*bien fondés*] provided they follow the layer of resemblances among individuals with different morphologies. When Linnaeus takes a simple, constant criterion for all plants, he categorizes all plants within his own system. But, to the extent that he took as a variable only a small sector of vegetal being, because they have similar sexual organs, he categorizes into the same category living beings that have very different general appearances. As a consequence, he will have chosen a localized criterion of resemblance without taking into consideration the natural series of global resemblances. In this sense the Linnaean categories are arbitrary and abstract. The problem for those who succeeded Linnaeus—the methodists and Jussieu, for example—was to come up with classifications such as those found in the same genus, in the same class as plants that actually resemble each other in every aspect. This is the grounded genus [*genre fondé*] as opposed to the abstract genus of Linnaeus.

*B. Saint-Sernin:* I thought I understood “grounded” [*“fondé”*] to mean that which allowed a suitable, natural distribution to take place, a distribution that would align with observation and experience.

*M. Foucault:* ... with the total observation of the species. The real as given to intuition can be tracked as such by a certain number of procedures that can be either methodological or systematic.

*M.-D. Grmek:* The pattern proposed by M. Foucault, and his demarcation of the two major thresholds, is a logical construction. This begs the question: what is its historical content? And, in the context of this debate, does Cuvier's work really represent a fundamental break in the process of the historical explanation of this pattern?

It is certain that the two proposed thresholds—the passage from species to genus and from individual to species—have a historical reality: for a long time they have posed a problem to be resolved. Parenthetically, I'm surprised that you called the first threshold “ontological” and the second “epistemological;” I would have expected the inverse, because the first threshold introduces the problem of classification and the second that of existence, of being. In order to surpass these two thresholds, a series of solutions was proposed in the history of biology. Early on, all the logical possibilities were imagined and I do not see, from an epistemological perspective, how Cuvier's work really brings anything new.

To be sure, it brings something new from the point of view of concrete classification or taxonomic details, but the thresholds you spoke about are not actually surpassed. For contemporary science, the two thresholds are crossed: for the first threshold, the solution is in phylogenetic affiliations, that is, in the theory of evolution and, for the second, in modern genetics. In order to find the historical break, we would have to look for the origin of these two solutions and, with the problem before us here, see if Cuvier's work is part of this process of radical change.

*M. Foucault:* In the history of science I do not think we can speak of change in absolute terms. How we classify discourses, the level at which we approach them, or the analytic grid we

impose on them, will determine what appears to us: continuities, discontinuities, constancies, or modifications. If you follow the history of the concept of species, or that of the theory of evolution, obviously Cuvier does not constitute a change. But the level on which I situate myself is not that of conceptions or theories: rather, I engage the operations within a scientific discourse that allow objects to appear, concepts to be put to work, and theories to be constructed. At this level, one can identify cuts: but, on the one hand, they do not necessarily coincide with those that can be identified elsewhere (for example at the level of the concepts themselves or at the level of theory); and, on the other hand, they do not offer themselves in a visible way on the surface of discourse. They have to be detected, starting with a certain number of signs.

We can find the first indication of a cut in a sudden change affecting all the objects, concepts, and theories that appear at a given moment (thus, we can say generally that medical objects, concepts, and theories from Hippocrates to the end of the 18<sup>th</sup> century show relatively weak indications of modification. By contrast, if you take Boissier de Sauvages and Bichat, you see that in forty years, in twenty-five years, everything changed, and much more than over the course of the preceding several centuries). We can find another indication of a cut in the exact inverse phenomenon: return and repetition; suddenly, a state of knowledge [*savoir*] mimes in a certain way a prior state. All these signs indicating cuts can function as initial identifiers. But for me, the final goal of the analysis is not to say that there is a cut; rather, starting with these strange phenomena—sudden changes, overlaps—it is to ask at what level the transformations that made the cuts possible are situated. In the end, an analysis should not indicate a cut and then respect it indefinitely; it should describe a transformation.

It seems to me there exists a Cuvier transformation, and that this transformation was necessary in order to go from the state of knowledge characteristic of the classical period (I have tried to define this abstractly through the pattern of thresholds) to this other state of knowledge that we find in Darwin. Indeed, this passage implies a homogenization of all the supra-individual categories, from variety to order, class, and family (we find this homogenization actualized in Cuvier with the exception of variety); this passage also implied that the individual contains, at the level of its anatomo-physiological structures and its internal conditions of existence, that which makes it belong to the ensemble of its species, its genus, and its family (this is in fact how Cuvier conceived species, genus). In order to move from the state of Linnaeus to the state of Darwin in biological knowledge, the Cuvier transformation was necessary.

*M.-D. Grmek*: Those who create a “historiographic” history of science need to bring it closer to “epistemological” history. A link should exist between these two modes of historical presentation. You have left aside the question that most affects the historiographer: if a change in the solution to a problem comes about, we need to specify what this change is, when and by whom it is produced. In the case we’re analyzing here, is Cuvier the point of departure for this change? For me he is not.

*M. Foucault*: The pattern I’ve proposed is not meant to lock up all the concepts or theories that could be formed in a given period within an internal and impassable condition of existence; for example, between Linnaeus and Jussieu there is a difference of method, concepts, and perhaps



theories at least as great as the difference between Jussieu and Cuvier. Further, Cuvier said repeatedly that Jussieu was the one who discovered everything. At the level of conceptual or theoretical distance, Jussieu is closer to Cuvier than Linnaeus. The history of theories or concepts could establish links and distances and show that Jussieu is close to Cuvier.

But that is not what concerns me. My problem is to see how they were formed, starting with what and according to which rules of constitution. Here we are confronted with paradoxical things: we can have analogous concepts and isomorphic theories that nonetheless obey different systems and rules of formation. It seems to me that Jussieu's taxonomy is formed according to the same pattern as that of Linnaeus, even though he tries to surpass him. By contrast, Cuvier's biology seems to me to obey different rules of formation. A conceptual continuity or a theoretical isomorphism can perfectly well overlap with an archeological cut at the level of the rules of formation for objects, concepts, and theories.

*M.-D. Grmek:* In the history of biology, Cuvier thus represents a transformation, not a revolution.

*M. Foucault:* With this topic I've always avoided the word revolution. I've preferred the word transformation.

Further, we encounter here an important methodological question: the problem of attribution.

This problem does not arise in the same way at every level. Suppose we call the study of opinions of one or many individuals doxology: the individual is then taken as an invariant; the question becomes determining if we can legitimately attribute to him this thought, this formation, this text. A problem of authenticity. The major mistake is therefore to attribute to him what doesn't belong to him, or on the contrary to leave in shadow a part of what he said, believed, or affirmed. We do not (at least in the first instance) ask questions about what an individual is, but rather about what can be attributed to him.

If we make an epistemological analysis of a concept or a theory, there is a good chance we are dealing with a meta-individual phenomenon; at the same time, it's a phenomenon that traverses and demarcates the domain we can attribute to an individual. In the work of an author we are led to leave aside texts that are not relevant (juvenilia, personal writings, opinions put forth for an instant and then quickly abandoned). From then on, what does author mean? What use do we make, exactly, of the proper name? What do we designate when, under these conditions, we say Darwin or Cuvier?

When it comes to studying discursive layers, or epistemological fields that include a plurality of concepts and theories (simultaneous or successive plurality), it is obvious that individual attribution becomes almost impossible. Similarly, only with difficulty can the analysis of these transformations be referred to a precise individual. Transformation in general happens across the works of different individuals; it is not something like a discovery, a proposition, or a clearly formulated thought explicitly given within a work. Rather, the one who is looking for it notices transformation as something at work across different texts. The description I'm offering should be

able to dispense with any reference to individuality; or rather, it should take up again, from top to bottom, the problem of the author.

I must admit that I was uneasy (and with an unease I've had a hard time overcoming) when I emphasized names in *The Order of Things*. I said "Cuvier," "Bopp," "Ricardo;" in fact, in doing this I tried to use the name not to designate the totality of a work [*oeuvre*] that would respond to a certain delimitation, but to designate a certain transformation that takes place in a given period and that we can see at work at a certain moment and, in particular, in the texts in question. The use I made of the proper name in *The Order of Things* should be invalidated, and Ricardo or Bopp should be understood not as names that allow us to classify a certain number of works or a certain set of opinions, but as the acronyms of a transformation. We should say "Ricardo transformation" the way we say "Ramsay effect." This "Ricardo transformation" you find in Ricardo—even though you find it elsewhere, before or after—is not important. My concern is to track transformation. In other words, the author does not exist.

*J.-F. Leroy*: From a historical point of view, names are somewhat annoying.

*M. Foucault*: I readily agree. And I think that just as logicians and linguists confront the problem of the proper name, so too with the history of science and epistemology we should try to reflect on the usage of proper names. What do we mean to say when we say Cuvier or Newton? In fact it's not clear. Even in literary history, we need a theory of the proper name.

*F. Dagognet*: The term "conditions of possibility" to which you refer has a "theoretical" meaning. But couldn't it take on a more material meaning or content?

Why did we suddenly begin to list or classify animals? It certainly wasn't so we could comprehend them in their diversity or better depict them. It was not for the purpose of order or amusement or theorization.

Hidden politico-economic pressures impose themselves. Every workshop in the 18<sup>th</sup> century and, through them, the life of the nation, depends on plants and animals. There is an attempt to escape certain subjections. It quickly becomes possible to replace "fellow creatures" with others, those who eventually will be close to us and subject to the same "jobs" or customs. An attractive replacement. This replacement fills people with enthusiasm for further examination of similarities and the creation of families. Indeed, a promising axiom was discovered according to which, in the words of Linnaeus or Jussieu, an "individual" cannot enter a category (for which he possessed, moreover, the characteristic sign that would authorize immediate identification) without possessing all the fundamental qualities of that category. And if one does not discover those qualities, it is because one did not look for them hard enough or one did not know how to express them. Time to search again.

Thus, such and such a plant is a legume: in these conditions, one should learn how to use it. It should, in one way or another, provide nourishment. One should therefore stimulate its production.

From then on, one will know how to avoid extravagant imports and costly modes of influence. In short, the conditions of possibility, the agents of transformation refer back to national and industrial exigencies, actual situations rather than theoretical concerns or documentary or scrip-

tural examinations. Modifications in writing or rankings [*rangement*] respond to necessities that are often technological or agronomical: to the contingency and the necessity of things.

M. Foucault: If you are speaking about material, social, and economic conditions, or techniques of possibility, then I don't think I've ignored them. In two instances—with regard to psychiatry and clinical medicine—I sought the conditions of constitution and transformation of these orders of knowledge. It is careless to say that I busied myself with words at the expense of things.

C. Salomon: With regard to Cuvier, is it legitimate to use the term biology, to the extent that biology is interested in something that a cork plant, an elephant, and a man have in common?

M. Foucault: The formulation is pleasing. Overcome, perhaps, by my pleasure in hearing it, I'm having trouble understanding the point of the question.

C. Salomon: ... You speak of Cuvier's "biology." Whoever says "biology" assumes the completion of the passage, or break, between a classical taxonomy (the classification of the living) and a physiology that concerns itself with life, an element that a cork plant, an elephant, and a man have in common, the object of a cellular physiology or a microbiology.

For Cuvier it is a matter of similarities of relations, not of common objects. Correlations are the object of taxonomy, not a vital unity. This means there are only living beings [*des vivants*] in Cuvier: there is not yet life [*la vie*] and therefore, strictly speaking, there is no "Cuvierian biology."

M. Foucault: That brings us to conditions of existence.

B. Balan: The philosophical implications of the principle of correlations. Isn't this principle, implying finality, one from which the concept of finality will break away?

M. Foucault: Of course. The decisions, the links I try to establish between theories, concepts, etc. and their systems of formation do not exclude the opposite possibility that a concept or a theory can break away from such a system. Take the concept of organization that was formed within classical taxonomy. Because it was essentially developed around Daubenton and Jussieu, it was reutilized by biology.

It seems to me that, after all the analyses and research projects that have a bearing on classes, kinship, and resemblances among living beings, in the end it is growth that characterizes the living [*le vivant*]. That which lives is that which grows and can grow in different ways.

1. To grow in size. The living is that which is subject to increase in size. This theme was important enough in natural history that, for a long time, it was thought that minerals grew and were therefore living.

2. To grow according to the variable of number. This growth through the variable of number is reproduction. It is interesting to note that for a long time it was believed that reproduction, through cuttings or sexuality, was a phenomenon of growth. Sexuality was not granted real independence in its physiological functioning. To reproduce was to grow larger, but no longer within the individual context and through simple growth in size. To reproduce was to grow larger beyond one's own size, through the procreation of new individuals. "Grow and multiply."

3. Growth in the order of the living being can be identified in a third dimension which is neither that of the individual nor that of generation, but that of all species. Growth occurred through growth in complexity. Growth in form became more and more complex.

In other words, the living is what grows according to the variables of size or number and the variability of form: these three variables serve to classify individuals, characterize species, and identify genera.

We can even recognize among naturalists of the classical age a fourth variable of growth: position in space. To the extent that individuals multiply and revolutions of the globe occur, the interweaving of species increases; individuals previously separated and belonging to very different groups mix together, and we see the development of those hybridizations that Linnaeus found so important at the end of his life; in this way, types are born which, in turn, respond to each other, etc.

So we can see that these four variables through which individuals and species grow according to natural history are also the four variables through which individuals and species can be characterized and classified. All this makes natural history into a solid and coherent edifice. This means:

- a. life is not defined through its relation to death, but through its possibility of extension. Life is that which continues and continues itself;
- b. this continuity is not simply spatial but temporal;
- c. sexuality is not recognized in its specificity but as a phenomenon of growth;
- d. natural history encounters, as a major epistemological problem, the problem of continuity-increase, also one of the problems of physics and mechanics.

Starting in the 19<sup>th</sup> century, biology is characterized by a certain number of essential modifications.

1. The individual is no longer defined so much through a possibility of growth inside a given form, but as a form that can only manifest itself under rigorous conditions and whose erasure is not only disappearance but death (according to a process that is itself biological).

2. Sexuality appears as an autonomous biological function. Until then, sexuality was considered to be a kind of supplementary apparatus through which, having reached a certain stage, an individual shifted toward another mode of growth: no longer increase in size, but multiplication. Sexuality was a kind of growth alternator. Starting in the 19<sup>th</sup> century, one begins to look for what is specific about the relation between sexuality and growth. This research leads, on the one hand, to the discovery of the fusion of gametes and chromosomal reduction (in a sense, the opposite of growth), and, on the other hand, to the idea—developed by Nussbaum and Weissmann—that the individual is itself no more than an excrescence on the continuity of the germinal strain. Sexuality, instead of appearing at the point of the individual as the moment when growth becomes proliferation, becomes an underlying function in relation to this episode which is the individual.

3. Also appearing is the theme of a history no longer tied to continuity: starting from the moment, in time, of the confrontation between a life that does not want to die and a death that threatens life, there will be discontinuity. Discontinuity occurs in the conditions of this struggle and in its outcomes and phases. This is the principle of anatomico-physiological conditions: the theme of transformations and mutations.

The fact that, in 19<sup>th</sup> century thought, the themes of death, sexuality, and history appear seems to me to be the philosophical sanction of the transformation that happened in the field of life sciences. These three notions—Death, Sexuality, and History—which were weak, derived, secondary notions in the 17<sup>th</sup> and 18<sup>th</sup> centuries, suddenly erupt into the field of thought in the 19<sup>th</sup> century as major and autonomous notions, and provoke in the domain of philosophy a certain number of “reactions” in the strong sense of the term, that is, in its Nietzschean sense. And the problem for an entire philosophy of the 19<sup>th</sup> and 20<sup>th</sup> centuries was to catch hold of these notions that had just appeared. Against the emergence of the notion of death, philosophy responded with the idea that it is normal, after all, for death and life to confront one another: because death is the fulfillment of life; because, in death, life finds its meaning; because death transforms life into fate. Against the theme of sexuality as an autonomous function with regard to the individual or individual growth, philosophy responded with the idea that sexuality is not in reality independent of the individual since, through sexuality, the individual can in some sense develop itself, overflow itself, and enter into communication with others through love, within time, and through lineage. As for history and the discontinuity to which it is linked, it is useless to say how the use of a certain form of dialectic responded there to give it the unity of a meaning and the fundamental unity of a free consciousness with its project.

I call every philosophy that claims death as the last and ultimate meaning of life *humanist philosophy*.

*Humanist philosophy*: every philosophy that thinks sexuality is for loving and proliferating.

*Humanist philosophy*: every philosophy that believes history is tied to the continuity of consciousness.

*M.-D. Grmek*: I admire the philosophico-historical tableau of the great theme of life you have just painted, but I'm troubled by the fact that, from Aristotle to the 19<sup>th</sup> century, the definitions of life, formulated by the most influential scholars, take into account neither growth nor sexuality, but call on other characteristics considered to be the *quid proprium* of the vital phenomenon.

*M. Foucault*: I do not place myself on the level of theories and concepts, but on the level of how scientific discourse is practiced. See in fact how the living is distinguished from that which is not living. See what is analyzed in the living, what is deduced from the living in order to make it into a problem of natural history: it is still a matter of growth.

*J.F. Leroy*: During the 17<sup>th</sup> and 18<sup>th</sup> centuries, it is growth that is fundamental and that makes possible the advent of this notion of biology: growth as increase in size, multiplication, and differentiation.

This will last for a long time, for it appears again in Darwin's pangenesis theory. We already find it in Buffon and, over the course of the entire 18<sup>th</sup> century, one tries to explain evolution through diet and growth in size. One compares the evolution of species to the evolution of individuals. There is no doubt that this is what I call the historical point of view on biology before the 18<sup>th</sup> century.

M. Foucault's presentation was illuminating. Before, I did not understand why he approached biology beginning with Cuvier. Now I understand that he gives a certain meaning to the term biology that we biologists expand. For us, biology is something broader. This first part of biology where we find this shift is also part of biology. This is how, in a sense, biology defines itself in the 18<sup>th</sup> century and, in botany for example, it is starting at the end of the 17<sup>th</sup> century that the question of sexuality becomes essential.

*S. Delorme*: I thank the Institute for the History of Science for allowing us to meet in order to better understand Cuvier's philosophy... but also, and especially, the philosophy of M. Foucault.

*G. Canguilhem*: The researchers and teachers at the Institute for the History of Science thank all the members of the audience, the natives, and the illustrious visitors who responded to our invitation with, for our part, the regret that we missed other illustrious visitors such as MM. Jacob and Vuillemin of the Collège de France, whom we had hoped to see here, and who sent their apologies. I want to thank the speakers. And so that my thanks will not seem like the speech for an award ceremony, I will thank them in the order of the increasing distances they had to travel in order to join us: M. Michel Foucault, from Vincennes; M. François Dagognet, from Lyons; M. Francis Courtès, from Montpellier; M. Camille Limoges, from Montreal.

Finally, allow me a final thought for the one in whose name we have gathered together to listen to MM. Foucault, Dagognet, Courtès, Limoges: the man who, on August 23, 1769, received forever as "sign" the name of his father, Cuvier.

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