prehistorian's educated eye), are "seen" as succeeding each other in a "rigorously determined order," no form ever reaching full maturity without some preceding form acting as a preliminary "sketch" for it; instead of a chaotic welter, the informed eye detects just that "ordered, organized, and ineluctable distribution of living beings through time and space" which the mind spontaneously takes as representing a "natural distribution." More complicated now than formerly, the scientific reconstruction of this evolutionary process remains, for the paleontologist, no less compelling than in its earlier, simpler, form. And the reason for that conviction remains, for Teilhard, as Duhem had once explained it (VP 84–89/119–26).[6]

The essence of the transformist hypothesis, Teilhard concludes, must not be confused with one or other theory—Darwinian or Lamarckian—adduced to explain its mechanism; *that* evolution took place is beyond serious dispute, but *how* it took place is still matter for continued discussion. But uncertainties about the "how" do not undermine the certainty of the "that." And yet, he is compelled in the end to avow, there may be more to Vialleton's objections than that: identify evolutionary theory with its mechanistic explanation of *how* evolution works (as Vialleton seems to have identified them) and there may be something inexplicable about evolution after all. This sets Teilhard off, as early as the year 1924, on a series of considerations which come to fruition only some years later. It will be best, at the moment, to postpone them until a later chapter in our study of his method and its development.

[6] The change of expression, natural "distribution" for natural "classification," is consistent with the evolutionary view already insinuated by Duhem's own illustration drawn from "natural history" and with his insistence that the logical relationships commending a scientific classification as "natural" must reflect "ontological" relationships among the specimens so classified. See above, chap. 1.

# 3

# Seeing "Wholes"

ONE PARTICULAR TACTIC Vialleton employed in his anti-evolutionary polemic was to invite his readers to focus, at close range, on this or that segment of the fossil record. Seen at close range, each segment took on an air of relative rigidity and fixity; at the same time the gaps between any two such segments seemed to argue for their relative independence of and discontinuity from each other. Teilhard's counterthrust consisted in asking those same readers to take the sort of view which, he is implicitly convinced, the evolutionist prehistorian, in fact, habitually takes, even if he is not always reflectively conscious of doing so. The concluding sections of his reply to Vialleton bring this requirement of scientific "seeing" to more explicit articulation.

The more ordinary way of understanding the activities of science, he admits, would tend more to stress the analytic and regressive character of their procedures. Science, generally, endeavors to explain the actions of larger wholes in terms of their more elementary, molecular, then atomic, then subatomic parts; so too, the tendency to explain the actions of collective entities as the sum of the actions of the individuals composing them (VP 98/137). The advantages of this mode of procedure are undeniable, Teilhard admits, but one may wonder whether it does not stand in need of a complementary way of viewing reality. To make the scientific picture of evolution intelligibly comprehensible, we may have to complement that atomistic method by taking into consideration those properties of collectivities *as such*, properties "which neither the analysis nor the sum of elementary forces could ever account for." We may be compelled to stand back from our examination of the elemental parts to take a view of all terrestrial life "considered as forming a specific whole" and rooted in its activity in the terrestrial world, itself considered as a "whole" (VP 98–100/137–40).

Teilhard's proposal in this instance has a somewhat surprising ring: for instead of reminding his fellow-evolutionists that they do, in fact, take something very like this long-range view of "wholes"—and that the compelling nature of that view is what sustains their theory against such objections as Vialleton's—he makes it sound as though he were proposing something quite novel in the way of "doing science." That implication might be considered all the more surprising in that the Teilhardian penchant for seeing the "whole" dates from his earliest extant writings, the essays written during the First World War. But there, it took a markedly mystical tinge, and Teilhard may be betraying his consciousness of that. But an essay published in *Etudes*[1] in 1921 on (of all things) geology shows that same inclination already working in high gear, and on scientific materials. He means to get his readers quite literally to gaze upon "The Face of the Earth," as the Austrian geologist Suess had termed it. The words of his title, Teilhard assures his readers, express, not some romantic fallacy, but the actual "results reached by geological science" during the past fifty years. It may, and must, be said that the earth has a "physiognomy, a countenance, a face" (VP 26/41) whose features we are "just beginning to decipher." The geological syntheses of Suess and others invite us to see our earth not as just an assemblage of disparate details unrelated to each other—an eye here, a nose there—but as organically related "features" which assume "meaning for our eyes" precisely when we adjust our vision to behold them as such. Again, his intent is to inform, to

[1] "The Face of the Earth," reprinted in VP (26–46/41–70).



"educate" his readers' non-scientific eyes to regard the earth as a geologist like Suess would.

He proceeds to show that to understand the formation of mountains one may have to take into account the contractions of our earthly globe *as a whole*, consider the several great "Alpine" mountain ranges as a single "system," and entertain the possibility that there may be some general law of "crystallization" which works for planets precisely as such. Only in that way can we succeed in making the form which our planet has taken clear and intelligible to both eyes and "minds" (VP 30, 33, 43/48, 51, 64).

This, he claims, is not futuristic projection; the concerted efforts of geological scientists have already succeeded in fitting together the formerly "disjointed features" of the earth's topography until they take on a unitary aspect. Those features, one is tempted to say, have composed themselves, as it were, into a settled facial expression, to which each distinct feature may contribute; but the expression itself must be "read" as the expression of the whole "face." This way of beholding the earth implies that our beholding minds attain to this result only at the price of "lending" to the earth some of our own human sense of "unity," by consenting to allow our intelligence to "infuse" a kind of life into this "material mass." The specters of subjectivism and anthropomorphism have been deliberately raised and candidly acknowledged. What makes the resulting picture so convincing, though, is the allure of "natural classification" which shines out from it; the "network of relationships" geologists have uncovered by viewing the earth this way reveals itself as both "solid" and "true." The ultimate justification of this quasi-anthropomorphic technique of "seeing" may not yet be in Teilhard's grasp, but he will later spell it out more boldly: only when the human observer consciously exploits the evolutionary "kinship" we enjoy, not only with the biological realm, but with the earth itself which gave us birth, can the earth be "seen" as it truly is.[2]

But for such knowledge to emerge, the eye must be accustomed to seeing things as "wholes." It is significant, again, that a principle of his later hyperphysical method, one which could so easily deteriorate into the pathetic fallacies of romanticism, was brought to the surface of Teilhard's more scientific writings precisely because he needed it to defend the evolutionary constructions of science itself. Unlike so many other evolutionists of his day, Teilhard could not afford to brush off the skeptics with a lordly *odi profanum vulgus*; he strained every nerve to meet their skepticism, to illumine them, to train their eye to *see* as the informed scientist had come to see. And Vialleton in particular compelled him to bring to his defense of science a refinement of analysis of which few of his fellow-scientists would have been capable.

The scientist does, Teilhard is convinced, see each life form in its connections with the whole; he simply has not reflected on, and drawn out all the implications of, that methodological fact. Of it, Teilhard might well have said what he said of the connected "postulate" underlying "all modern scientific research": namely, that to be drawn into the web of scientific thinking, "everything must extend its empirical roots indefinitely backward and in all directions."

[2] He begins to orchestrate this insight in the essays on "Hominization" and "The Phenomenon of Man" which we shall examine shortly. Note that although Teilhard does not explicitly use the term "natural classification," his mind is clearly working with this scientific aspiration in view. See, for example, his question about purely "accidental features" which might conceivably be "individual" to our earth and, hence, never become truly "intelligible to our minds" (VP 43/64); his hope of attaining to that "ideal of every science," the "deduction of the earth's physiognomy, starting from a few simple givens" (VP 44/65); and, finally, the hope that the scientific mind may eventually move from the apparently "disjointed features" and "incoherent altitudes" which first greet the eye to a "solid network of true relationships" which harmonizes that incoherent disjointedness into a satisfying "unity" of aspect (VP 45/66–67). The Duhemian flavor is unmistakable.



The "majority of scientists," he admits, "do not even think of proclaiming this postulate"; but the reason for that, he suggests, is simply that it has become so "evident" and "habitual" to them. Teilhard's pro-scientific bias may have persuaded him to give more credit to his fellow-scientists than later events will justify: he may have been more accurate in admitting, to the readers of *Etudes*, that there might be sympathizers with the evolutionary theory who could not always "explain," even to themselves, the "reasons" for their sympathies (VP 7/18). Not everyone is equally aware of what is implied by their "habitual" way of doing, or seeing, things. But bring those habitual implicits to the surface, Teilhard is convinced, and it will become clear as day that in order to understand why any particular atom or molecule occurs at this rather than some other "place" in the evolving universe, the explanation must bear in mind no less than the "immensity of a whole astral evolution" (VP 101/141). Even when attending to the most elementary particle of scientific analysis, therefore, the scientist precisely as scientist must keep peripherally in mind the cosmic "whole," with whose every other item it interrelates.

Two closing comments are in order concerning Teilhard's notion of seeing with an "educated eye": (*a*) that he seems clearly to have been influenced by the landmark essay on "The Eyes of Faith," written by his friend Father Pierre Rousselot; and (*b*) that his confidence in the "educated eye" is very much the confidence of a geologist-paleontologist.

Pierre Rousselot was a fellow-Jesuit, one of Teilhard's companions during the first of his four years of theological studies at Hastings, and a friend he was obviously happy to rejoin in Paris. A powerful theological mind, Rousselot in 1910 published his famous essay on how the believer's "eyes" could see signs of God's working which escaped the eyes of others.[3] It soon caused a furor, for it was written at a time when "modernism" was likely to be charged against any adventurous theological proposal. More than likely Teilhard read it while at Hastings, and, as was his habit, discussed aspects of Rousselot's theory with the brilliant confrères who were to become his lifelong confidants on matters theological, Pierre Charles and Auguste Valensin. In any event, an explicit allusion to "the eyes of faith" in a letter written in January 1920 makes it clear that he was both familiar with and sympathetic to Rousselot's way of thinking.[4]

Capital to his thinking was the distinction between the believer's and the non-believer's *way* of "seeing." To introduce that distinction, Rousselot suggests that we consider how an experienced scientific researcher, or a seasoned detective "sees" what he sees. He brings to the process of looking for the solution latent in a tangle of evidence all the "experience" of dealing with similar problems in the past. But his knowledge from that past experience, while helping him enormously to "see" what someone less experienced could easily miss, does not function now in his work of detection as a body of knowledge which he consults, as it were, focusing on it to search for points of similarity with the problem currently confronting him; it has become part of his "habitual" *way* of looking at the evidence and for the solution: it is "perceptive, not perceived."[5] His very "eye" has been educated, by his accumulated experience,

[3] "Les Yeux de la foi," *Recherches des Sciences Religieuses*, 1 (1910), 241–59.

[4] *See Lettres intimes à Auguste Valensin, Bruno de Solages, Henri de Lubac, André Ravier, 1919–1925*, ed. Henri de Lubac, S.J. (Paris: Aubier-Montaigne, 1974), particularly the letter of January 10, 1920, p. 48, along with pp. 51–52*n*7.8. See also pp. 38, 68–69, 92. Directed to four theologians of mark, these letters go far to dispel any surviving notions one might entertain about Teilhard, the theological *naïf*.

[5] "Les Yeux de la foi," 251. Acting as *perceptive, non comme percue*, the researcher's knowledge is, in another of Rousselot's expressions, not an *avoir*, but a *habitus*, in the Thomist sense of a developed "power" of seeing in a specified way.



to *work* differently from the eye of one possessing less, or different sorts of, experience.

This, I submit, is evidently how Teilhard envisions the work of the scientifically "educated eye." To cite an analogous proposal from a more recent thinker: Michael Polanyi has stressed how much skillful activity, scientific or otherwise, depends on the functioning of the "tacit" dimension which enables us to see connections, for example, or to make instantaneous identifications, without focusing on the atoms of evidence and skeins of reasons which work behind the scenes, and always (necessarily) behind the scenes.[6] Obviously, too, there is a striking analogy here with Newman's "illative sense": both Rousselot and Teilhard are known to have been deeply interested in Newman's writings.[7]

But the "educated eye" to which Teilhard refers (and this he may have done so "tacitly" as to be unaware of the fact) was *primarily* that of the geologist-paleontologist. That was, after all, the kind of eye with which he was most familiar; hence, it was scarcely an accident that some of his richest early allusions to it are prompted by Suess's work on geology. Teilhard's specialty P. B. Medawar, in the classic rant which passed for his review of *The Phenomenon of Man*, has patronizingly termed a "comparatively . . . unexacting kind of science,"[8] but others will answer that charge better than I; at all events, to understand what kind of "eye" Teilhard is asking us to adopt, it is crucial to dwell for a moment on how the geologist-paleontologist "sees" the realities he explores.

Nothing could be luckier, to illustrate the workings of that kind of eye, than the publication of Carl Pantin's

[6] See his compact work *The Tacit Dimension* (Garden City, N.Y.: Doubleday, 1966), devoted to this facet of the earlier *Personal Knowledge: Towards a Post-Critical Philosophy* (Chicago: The University of Chicago Press, 1958; corrected edition, 1962).

[7] *Lettres intimes*, pp. 407–408.

[8] See pp. 101 and 105 of his review, cited in Introduction, note 2.

Tarner Lectures given at Trinity College, Cambridge University, in 1959. An eminent zoologist, but thoroughly innocent of any influence from either Teilhard or Rousselot, Pantin addressed himself to *The Relations Between the Sciences.*[9] The somewhat more usual but hackneyed divisions into descriptive and exact, observational and experimental, sciences, Pantin argues, do less than justice to the reality: he devises another distinction, between "restricted" sciences (like physics and chemistry) and "unrestricted" sciences (like biology and geology). The former, he notes, restrict themselves to a narrower range of phenomena, and need not consult their sister sciences in pursuing their explorations; the latter, on the contrary, range over a vaster field of phenomena, and must always be alert to the moment when physical or chemical analyses may become relevant to their investigations: they must "traverse" all the other scientific domains in pursuit of their own kind of findings. This, Pantin avers (*pace* Medawar), is why the unrestricted sciences are more, not less, demanding than physics and chemistry.

There is another difference between them: unrestricted sciences regularly deal with phenomena of larger size and/or duration than their restricted sisters;[10] Pantin gives the (geological) example of trying to understand the formation of an entire river-system.[11] This correlates, in part at least, with the consequence that the "experiments" of geology and biology occur far more frequently in "nature" than in the laboratory,[12] and with the further fact that a sort of total aesthetic recognition is more crucial in the unrestricted than in the restricted sciences: analyses of macro-phenomena down to their more and more minute component elements, along with exact mathematical measurements, although sometimes appropriate or even neces-

9 (Cambridge: Cambridge University Press, 1968).
10 Ibid., p. 20.
11 Ibid., pp. 9–13; see also pp. 153–54, 173–77.
12 Ibid., pp. 16–17.



sary, tend to take second place. The unrestricted scientist may well return from the scene of his natural-site observation, sure of his findings there, but still feel obliged to set down a list of analytic criteria, including mathematical measurements when appropriate, then check them off, one by one, in order to present to his confrères the "evidence" for his conclusions. This is no purely formalistic exercise, for "aesthetic" recognitions *can*, after all, involve such an element of subjectivity that the scientist *may* end up seeing what he wants to see; integrity and honesty, here, are all-important. But granting integrity and honesty as part of the scientist's equipment, he is always keenly aware that his analytic procedures in the laboratory or museum seldom if ever take in all he has observed; a considerable "residue" from his initial observation leaks out, as it were, and never gets conveyed in his analytic report. This compels him, more often than not, to strive to communicate that "residue" by the use of striking similes or metaphors,[13] which he knows his fellow-scientists, who have found themselves in like situations, will interpret sympathetically as his effort to pass on the total impression which in fact grounds his conviction.[14]

Pantin concretizes all this by narrating the experience of a particular geological "experiment." The problem involved explaining the historical formation of some Welsh plateaux in the area of the river Towy. The first step in solving it implied surveying the site, gaining that familiarity with its contours which only "field recognition" can provide. Two hypotheses then presented themselves, each of them, significantly, in conformity with the past experi-

13 Ibid., pp. 84, 112–13. Pantin makes no apologies for this use of metaphor; it is standard form of communication in unrestricted science, and particularly in geology. Again, the "languages" of science can be as legitimately diverse as the various sciences require. This point should be borne in mind when considering Teilhard's resort to metaphor; it need not imply, without further examination, that he is being poetical *rather than* scientific.
14 Ibid., pp. 112–13; see also, on the scientific value of analogy, pp. 93–95.

ence of the American and English geologists, respectively, examining the site. Finally, O. T. Jones proposed that a closer study of the Towy, from its headwaters to the sea, might decide between the two competing hypotheses: this involved restudy of the site, and making exact measurements of the downward gradient that river followed on its journey toward the sea, a study which finally decided in favor of the theory Jones had proposed. Field recognition, hypothetical explanation of the features presented to that kind of "observation," but then, the discrimination between hypotheses by more "experimental" procedures involving exact measurements—this was the complete cycle of this piece of geological exploration.

But Pantin himself had only heard from other authorities of the theory which Jones had proposed; he found it, however, conformable with his own limited experience in geology. Nonetheless, he found that after learning of Jones's work he "immediately" began to view similar geological features in other parts of the world "with a different eye. It is not," he insists, "that I have the same perceptions as before and then apply Professor Jones's reasoning to them; it is as though my actual mode of perception has been changed by the historical experience of this argument."[15]

That education of the eye, of the very "machinery of perception"[16] itself, has several other implications as well. First of all, it suggests that terms like "observation" and "description" are deceptive even when applied to the geologist's very first steps toward "field recognition"; for the same tacit dimension which eventually works in the "recognition" stage is also at work before it. Secondly, it prompts the unrestricted scientist in choosing both time and place appropriate for crucial observation, as well as in actively

[15] Ibid., p. 13; see also note 11, above, for other loci where Pantin alludes to this same "experiment."

[16] Ibid., p. 17; the echo of Rousselot's *perceptive, non perçue* is striking.



selecting out those of the innumerable phenomena before him which his past experience and theoretical "templates" cue him into knowing as relevant to the problem he is working with.[17] Of particular concern to the geologist, of course, is his choice of appropriate observation points, of what Teilhard calls privileged "points of vantage" (PM 32–33/26–27) from which the landscape "lights up," as it were, and the geologist "sees" how the work of ages went into producing this mountain range, this moraine, these intersecting valleys.

But this habit of educated vision is of special importance to the geologist, more so perhaps than to any other scientist; hence, one may guess, Teilhard's outspoken predilection for it as the metaphor for all scientific knowing. George Barbour and Helmut de Terra, two fellow-scientists who traveled with him on lengthy field expeditions, were especially struck by Teilhard's powers of vision: from the acuteness of sheer eyesight (but educated eyesight) which could spot from yards away a primitive tool lying in what his professional peers saw as only a pile of pebbles, up through the ease with which he saw the Himalayas through his experience of Egypt and the Alps, to the "inner eye" which unhesitatingly glimpsed the evolutionary meaning encased in a fossil tooth or a mountain upthrust.[18] Literally, as de Terra puts it, his "eyes were everywhere." But this heightened power to "see" may ironically have worked to his ultimate disadvantage: like an accomplished golfer who cannot empathetically understand why the game is so difficult for others, he may have been led to underestimate the task of educating eyes, even scientific eyes, whose "tacit dimensions" were far less or otherwise equipped than his own.

[17] Ibid.

[18] See Barbour's *In the Field with Teilhard de Chardin* (New York: Herder & Herder, 1965), p. 35; and de Terra's *Memories of Teilhard de Chardin*, trans. J. Maxwell Brownjohn (New York: Harper & Row, 1964), pp. 36–39, 62, 67, 72.

For Teilhard's scientific "eyes" were, again, very much those of a geologist-paleontologist. That must be kept in mind when we encounter two further particularities of his thinking: his use of the term "experimental," and the ease with which he deals with that scientific bogeyman, "anthropomorphism."

The French term *expériment* has as its corresponding adjective the term *expérimental.* The terms have a wider extension than their cognates in English, which permits of a distinction between properties which can be dealt with by "experimental" method in the strict sense and those which can be "experienced" in the more everyday sense of that term. The situation is complicated by the fact that "experiment," for the English scientific reader, more often suggests the style and standards of the "laboratory" experiment practiced by physicists and chemists.

But the French term *expérimental* stretches wider than that; it includes any and all phenomenal properties which can be "experienced," even if they are not of the sort which can be verified or tested by scientific "experiment" on the physical model. Teilhard's use of the term is influenced not only by the breadth of denotation it enjoys in French, but also by the kinds of "experimental" observations the geologist-paleontologist is accustomed to making "in the field" rather than in the museum or laboratory. So he will later feel entitled to say that "psychic" properties like thought, groping, invention, and the rest are just as "experimental," just as accessible to verification by observation, as physical properties like weight, mass, velocity, or radiation. The scientist whose model is physics will be tempted to quarrel with that contention, but Teilhard had exorcised early on any lurking adoration he may once have had of physics as the very model and norm for the way scientific activity was supposed to be conducted.

But talk of thought, groping, and invention as scientifically "experimental" properties would tend to alienate the



physicist and chemist for an additional reason. Are these properties observable—experienced, even if not "experimentally" verifiable—on the human level? Some "experimental" scientists would concede it, but others, even psychologists like B. F. Skinner, would flatly deny it.[19] In any event, the question need not interest them greatly as *practicing scientists,* for the reason that physics and chemistry have long since come tacitly to agree that no such "anthropomorphic" properties may be invoked as operative on the physical and chemical levels of reality. The history of this decision on the part of post-Renaissance science to eschew all "anthropomorphism" is an interesting one; particularly fascinating is the way it has been transformed, in the minds of many scientists, from a methodological *abstraction* eschewing appeal to any such properties to a dogmatic *negation* that any such properties either exist or can be known to exist.

But the paleontologist is something of an oddity in this respect: not only must he constantly appeal to anthropomorphic forms of thought; one might even say they are his stock in trade, essential to the precise business he is about. For one of the most frequent questions he is called upon to answer is whether this or that fossil find is a "human" find, and to answer such questions he must avail himself of what he knows about human beings: that they make tools, use fire, invent not only material instruments but rites, languages, cultures. Thought, groping, invention: he moves at ease with the evidence he finds for the presence and operation of such "psychic" properties; indeed, he could not do otherwise. And in doing so, he never for a moment doubts the "scientific" character of the evidence he deals with, or of the methods he brings to dealing with

[19] Skinner's *Beyond Freedom and Dignity* (New York: Knopf, 1971) is a perfect illustration of this. See, for starters, pp. 1–23, where Skinner lays out his overall program—one based, be it noted, on the hopes he shares (with Duhem, and with Teilhard) for a comprehensive and unitary science.

it. "Experimental" evidence and method, one might query? The answer comes, especially when the question is asked in French, in an unhesitating "yes."

But even while Teilhard was penning the lines we have just examined, there was another aspect of scientific procedure which was rising to the level of his reflective awareness. He is beginning to grasp more focally the implications of the fact that the evolutionary cosmic "whole" constitutes the stuff of "history." Already implicit in everything he has written thus far, that historical dimension, once focused on, will prompt a series of proposals which his fellow-scientists will find, as time goes on, profoundly unsettling.

# 4

# Science as History and Product of History

ONE OF THE MOST PIVOTAL ESSAYS Teilhard wrote, in gradually developing the method he eventually brought to writing *The Phenomenon of Man*, appeared in the review *Scientia* in 1925. It bears the title: "The Natural History of the World" (VP 103–13/143–57).

He starts from the attempts of Linnaeus and others to furnish a "systematics" aimed at presenting the "natural distribution" of living beings into classes and subclasses. Working, as they had to, with no idea that the assemblage of living forms was the result of evolutionary history, they thought of their divisions and subdivisions as corresponding, presumably, to the divine creative ideas presiding over the creation of a fixist universe. This obliged them, Teilhard explains, to apply a certain number of "extrinsic" criteria born of the play of their ordering intelligence; the result was that their divisions in many instances remained merely "logical" and to that extent artificial. Only when these life forms could finally be placed in *historical* sequence, the ordering mind placing each form and the entire assemblage in a "physical system" involving both "temporal antecedents" and "spatial links," could this logical and partially artificial system of classification yield to what Duhem had years ago proclaimed to be the ideal of scientific activity, a "natural classification" (VP 103–104/145–46).[1]

[1] Note the express correspondence between natural "distribution" and natural "classification," and compare chap. 2, note 6, above.