Charles Lyell and the Uniformity Principle *

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ABSTRACT: The theoretical system Lyell presented in 1830 was composed of three requirements or principles: 1) the Uniformity Principle which states that past geological events must be explained by the same causes now in operation. 2) the Uniformity of Rate Principle which states that geological laws operate with the same force as at present 3) the Steady-state Principle which states that the earth does not undergo any directional change. The three principles form a single thesis called "uniformitarianism" which has been repeatedly questioned and which has been reputed to be unable to face the competing "directional synthesis" based on the theory of the earth's cooling down. As a result, the significance of Lyell's system has been reduced to a simple "actualism" which admits the validity of the only Uniformity Principle. I believe that the only way to understand Lyell's role in the history of science is to maintain the unity of his synthesis. To show the newtonian roots of this synthesis I will compare Lyell's principles and Newton's *Rules of Reasoning*. I will conclude with an analysis of the methodological function of principles in Lyell's scientific endeavour.

KEY WORDS: catastrophism, central heat theory, laws of nature, Lyell, Newton, principles, semantic conception of theories, uniformitarianism.

Charles Lyell is celebrated as the scientist who formulated the uniformitarian synthesis, opposed deists and catastrophists and sought to establish geology as parallel to Newton's celestial mechanics. Part of his theory became a fundamental component of Darwin's theory of evolution and another part, under the form of a Uniformity Principle, was even assumed by John Stuart Mill as a major pillar of his *System of Logic*. On the other hand, it is undeniable that Lyell was outclassed by the scientific achievement of his most outstanding disciple, Charles Darwin. Even though Darwin always sincerely considered him as his "honoured guide and master", some years after the theory of Natural Selection stepped into the scientific arena, Lyell was led to a partial revision of the seemingly weakest, but still fundamental, tenet of his system, the "anti-progressionism" which he had for a long time firmly defended. After a painful process, which his *Scientific Journals* trace back to the mid fifties, Lyell accepted the doctrine of "progressive development of the organic life". [1] Unfortunately, this withdrawal, in the face of the more powerful Darwinian paradigm, has convinced some historians of science that Lyell's program had to be deeply reformed, being a simple assortment of heterogeneous parts liable to be detached from one another or a recessive synthesis to be recast according to the latest brand new reconstruction of the history of science. They have taken great advantage of the privilege of severing the objects of their research, going so far as to detect and to find unfair Lyell's psychological motivations and complaining that he had manipulated or even falsified the history of geology in order to highlight his own role.

To be sure, this is the normal course of the history of science. A research program is not only a logical unity, it is also an historical entity (Hull 1975, p. 183), competing with other programs (Hull 1988) in order to improve available scientific knowledge. Therefore, it has to fit the purposes of scientists and critics, who can modify it, detach its parts and join them to another program to produce a better theoretical device. Thus, as a matter of principle, a philosophical reconstruction which keeps a theory or a program unaltered and supports its integrity cannot claim any advantage of credibility. It has to prove itself more advantageous than others, on the grounds of a cost-benefit calculus which, in its turn, depends on a tentative and speculative interpretation of the issues and tendencies currently existing in the scientific community.

I will argue that a reconstruction which keeps together all the three requirements which constitute the main structure of the *Principles of Geology*, is more beneficial than an analysis which dissociates them from one another. These requirements are to be interpreted as methodological principles. Actually, Lyell does not explicitly call them "principles" but, as this paper progresses, it should be clear that they cannot but have this logical status. The very principles will ensure the coherence and the unity of uniformitarian synthesis in spite of the fact that Lyell's most radical claim, that it is possible to make geology into a Newtonian science endowed with a body of "laws of nature", will not have great success. Thus, the principles will turn out to be the theoretical tool by which Lyell played that role of a founder of modern geology which all historians grant him.

Hence, an important part of this paper will be devoted to an analysis of the methodological function of principles. To this purpose, I will firstly consider the conflicts in which Lyell was involved, especially the uniformitarianism-catastrophism debate. This historical review should provide the background for analyzing the logic of uniformitarianism, its Newtonian roots, its connections with the Victorian philosophy of science and with Scottish common sense philosophy.

Uniformitarianism, Catastrophism, Directionalism

Let us see the three requirements of the Lyellian scientific program. 1) *Uniformity Principle*. This is the first and central one, also expressed in the subtitle of the *Principles of Geology*. It states that geological events of the past must be explained by means of the same causes now in operation. 2) *Uniformity of Rate Principle*. This is the second principle, and it states that geological laws must be supposed to have been operating with the same force or intensity with which they operate at present (Lyell 1830, pp. 64, 88; 1881, p. 234). 3) *Steady-state Principle*. This states that – leaving aside periods of local disturbance – "the energy of subterranean movements [is] always uniform as regards the *whole earth*". The planet is therefore a steady-state system (Rudwick 1976. pp. 187 ff.) and does not undergo any directional change (Lyell 1830, pp. 64, 87).

Consistent with these premises, Lyell displays a complete theory of earth, encompassing both inanimate and animate kingdoms. The *prima causa* is represented by a theory of elevation and subsidence, which, on the basis of subterranean phenomena which were not well known at the time (1830, p. 142), determines the reciprocal distribution of land and sea (1830, p. 112), which, in its turn, influences the climate and then vegetal and animal adaptations (1830, VII chap.). The causal cohesiveness of this system both demands and ensures its steady-state equilibrium in the face of which unavoidable convulsive episodes appear secondary and derivative. Thus, Lyell maintains that the progress of geology has been delayed by "fancyful conjectures" about "imaginary pictures of catastrophes" and "extraordinary agencies". The main target of these charges was his former Oxford professor, William Buckland. His speculations are based on the prejudice of a complete "absence of analogy" between the ancient and the present state of the earth and he includes divine intervention within the range of possible causes: as a consequence "the thread of induction [is] broken" (1830, pp. 71-77). To frame this dispute, William Whewell (1832, p. 126) coined the terms "uniformitarianism" and "catastrophism" and, therefore, ended up sanctioning the approach of Lyell, despite his frequent disagreements with him. Consequently, uniformitarianism became a part of the traditional history of geology (see, for exemple, Gillispie 1951).

The above reconstruction, inspired by the historical sketch which opens the *Principles of Geology*, tends to privilege the unity of Lyell's program, and hence it is expected to be followed by a critical but confirmative investigation of the logical links between the first methodological principle and the other two claims (which is exactly what I intend to do in the next section of this paper). That would also entail a preference for catastrophism as the "natural" opponent of the uniformitarian program, because the former, contesting the program as a whole, implicitly admits the unity of the latter.

Neverthless, we cannot be satisfied with this account. To portray the discredited catastrophism as the only adversary of uniformistic geology would be an oversimplification not less awful than it was to contrast Darwin only with Creationists. As I mentioned above, there are different historical reports which list other opponents to Lyell's synthesis. Since these recontructions call into question one principle or another of uniformitarian geology, they finally give up the unity of lyellian program. Thus, it will be appropriate before starting the inquiry into the meaning of uniformitarianism – either for methodological fairness or, rather, to provide further strenght to the positions we are going to support – to review these historical reconstructions and to make clear the reasons why we will reject them.

The first doubts about the validity of the terms "uniformitarianism" and "catastrophism" were raised by Reyer Hooykaas (1963, pp. 4-14). He leaves aside the steady-state equilibrium and reserves a more restricted designation, that of "uniformitarianism in the proper sense", to the first two tenets of Lyell's theory. Then he inserts a sort of conceptual wedge between them, picking up a somewhat moderate and more flexible kind of uniformitarianism, which retains only the first Lyellian principle – the continuity of present laws – and is therefore termed "*Actualism*". Hooykaas remarks that many uniformitarians and some catastrophists fall under the heading of actualism (1963, pp. 32 ff.). Accordingly, the contrast that Lyell had considered so clear cut, now should look rather fuzzy and insuffcient to frame the real positions existing in geology around 1830.

Hooykaas, among later historians, was responsible for a tendency to play down Lyell's uniformitarianism by first splitting its meaning into a hard core (actualism) and a flabby coating (uniformitarianism) and then, by taking away or hiding its counterpart, catastrophism (see also Rudwick 1971, p. 212 and Gould 1980, pp. 150-151). Following this path, some scholars have gone so far as to view the historical introduction placed at the beginning of the *Principles of Geology* as one of the keystones of a "rhetorical strategy" (Rudwick 1970) or even simple "propaganda" (Cannon 1976), to accredit its author as the true reformer of geological science. In this framework the catastrophists would be assigned the role of strawmen, to conceal the more redoubtable and powerful opponent of Lyellian theory, "*another* geological synthesis, of equally great scope, sophistication and explanatory power; a synthesis that shared the essentials of Lyell's method but differed significantly in its conclusions". Rudwick (1971, p. 213) calls it "the directionalist synthesis".

This synthesis is based on the view that the earth was originally an incandescent mass which loses its heat progressively. A cooling earth can conveniently explain the geological cataclysms of the past, but within the actualistic frame of present geophysical laws. The thesis of a progressive cooling was confirmed by the most updated scientifc theories then available, such as Cordier's analysis of the geothermal gradient, Fourier's theory of "residual heat" and also the Nebular Hypothesis of Laplace and William Herschel (Lawrence 1977 and 1978). The connection between the theory of Central Heat and the dynamics of the earth's crust was made by the French geologist L. Élie De Beaumont. Reasoning from the difference between the strata in mountainous regions, the most recent ones placed horizontally, the most ancient ones inclined at considerable angles or even vertically, and from the sharp demarcation separating them, De Beaumont came to the conclusion that the latter had been elevated in a sudden manner. He also observed "a general uniformity in the direction of all beds upheaved at the same epoch", and examining the map of the European continent determined twelve systems of mountains. Such an overall arrangment could not be due to local volcanic actions but must be traced back to a more general cause, the contractions of the earth's crust as a consequence of the residual heat (De Beaumont 1831).

According to Rudwick, De Beaumont's theory is no less complex than the Lyellian theory of Elevation and Subsidence and displays, in addition, a comparable explanatory power. A further reason to take it seriously is its methodological relevance. It is actualistic rather than uniformitarian because it is based on a still active cause (the cooling of the earth). Rudwick (1971, p. 220) suggests that the history of the planet can be wholly explained only by means of this basic actualism, “without any suspension of the ordinary ‘laws of nature’ ”. Hence, this condition would be enough to ensure the scientific quality of geology, even though the directionalist synthesis was apparently not compatible with both the Uniformity of Rate Principle and the Steady-state Principle. Indeed, if the amount of igneous energy available on the whole earth was considerably greater in the remote past, then geological laws had to operate at a much greater rate of intensity than at present, and outside a steady-state context. (see Rudwick 1971, pp. 214-215 and Hooykaas 1970, p. 7) In Rudwick's opinion (1971, p. 224) the second and the third principle on which Lyell insisted are "only an extreme form of methodological policy" . He seems to propose actualism as a *moderate version* of the uniformistic program, a shift which, by excluding the further methodological standards of the latter, would *reduce* it to a sort of simple scientific common-sense. Or rather to a failure, because if one allows causes whose "intensity" differs significantly from the present and observable ones, a wide range of causes, even *ad hoc* or conjectural ones, can be introduced into the explanatory mechanism of geology without an adequate (inductive) test of empirical existence (Lyell 1830-33, III, p. 6). The ambiguous category of "Actualistic Catastrophist" (Hooykaas 1970) would be admitted in scientific geology.

I think that the simple criterion of conformity to primary physico-chemical laws is not sufficient to define all the empiricist obligations which geology had to meet in order to be considered a genuine science. Rudwick realizes this, in so far as he remarks that the geological processes and agencies – like marine erosion or vulcanism – are "evidently causes of a greater complexity" requiring a more complex philosophical discussion (1971, p. 211). But subsequently, blaming the Lyellian "confusion" about geological causes, confines himself to consider only the physico-chemical causality as a standard of empirical significance.

A good case that exemplifies this point is the elevation theory of De Beaumont, based – as has been said earlier – on the supposed cause of terrestrial cooling. Commenting upon it, Lyell notices that "The speculations of M. De Beaumont concerning the 'secular refrigeration' of the internal nucleus of the globe, considered as a cause of the instantaneous rise of mountain chains, appears to us mysterious in the extreme, and not founded upon any *inductions from facts*." (1830-33, III, p. 339; emphasis added) Actually, in spite of the fact that Sedgwick, Conybeare and Scrope supported it in the Thirties in contrast to Lyell's position (Lawrence 1977, Rudwick 1970), its fortune did not last for long. It is remarkable that Darwin mentioned in his *Autobiography* that "the present total oblivion of Élie de Beaumont's wild hypotheses (...) may be largely attributed to Lyell" (Darwin 1897, I, p. 60). Indeed, at the turn of the century, the causal basis of elevation theory, that is the doctrine of progressive cooling and of residual heat, was destined to be superseded by the notion of radioactive heat, which is inexhaustible and does not call, as such, for any directional explanation – as far as geology is concerned. [2] Given this, an account which depicts catastrophism in terms of "false problem" and progressionism in terms of "real problem", does not appear really credible. A more traditional outline (Cannon 1960), which depicts progressionism as a sort of sequel to catastrophism, works much better. It moves ahead, at the decade beginning in 1850, the definitive emergence of progressionism as the problem of organic evolution.

Thus it seems to me that analytic strategies which clip the wings – so to speak – of the Lyellian research program ("strict" uniformitarianism and steady-state equilibrium) and concentrates on actualism, have not proved to be all that helpful: they have not opened a new and credible line of research in the history of geological science, and they fail to grasp the methodological perspective which was at issue in geological disputes and which was one of Lyell's major interests.

Other studies, though valuable, have persisted in decoupling the concept of non-progression from the Principle of Uniformity. For example, anti-progressionism has been framed as an "anti-evolutionary stratagem that works against the grain of [Lyell's] book" (Bartholomew 1973, pp. 264, 271). Or it has been suggested that it does not follow from the logic of Uniformity Principle but from the exigence of vindicating Lyell's "cyclical" theory of climate, [3] or of preserving the exceptional creation of man (Ospovat 1977, p. 318; Bartholomew 1973, p. 263). These studies appear to be moved by the hidden conviction that non-progressionism is an obstacle in the transition leading from uniformitarianism to evolutionism. This entails also the premise that Lyell's achievement has to be settled under a perspective subservient to the triumphant Darwinism and tailored accordingly. On that subject, I would first remark that the Darwinian concept of evolution, far from being an exclusively progressive concept, was, rather, shaped by its author to be coherent with Lyellian uniformitarianism (Richards, 1991: 80-90). Darwin's evolutionism was not scientifically committed to any progressionist law of development. Progress was less a theoretical device apt for enriching the analysis of evolutive processes than an ill-defined "cultural notion" suitable for gaining a following for the *Origin* (Ruse 1997, chap. 4). And also in modern evolutionary biology it counts mostly as a "legacy of popular evolutionism", antithetic to professional science. (ibidem, p. 448) Even though we replace the "value-laden" concept of progress with the seemingly more empirical concept of "direction", the latter is however "theory-laden". (Hull 1988).

Hence, I do not see any special reason for raging against the purported backwardness of Lyell's anti-progrssionism. Such arguments would rather appear to be the expression of a negative attitude towards Lyell's work, resulting in its marginalization in a corner of the history of science. Quite the opposite, I am convinced that all the three principles of uniformitarianism have an autonomous role in the progressive definition of the idea of a scientific method. In order to check this hypothesis, I intend to explore the possibility which has been outlined at the beginning of this paper, namely, the possibility of assuming the Lyellian program in its full integrity and of holding the unity of its three reqirements. In this direction, Walter Cannon has defended the originality of Lyell's approach, pointing out that non-progression "seemed not to be a minor feature of Lyell's system nor to represent a lapse of reasoning power", and has used the term " 'system of indifference' to characterize [...] the interaction of forces in the organic and inorganic worlds", [4] or, in other words, his concept of dymanic equilibrium (Cannon 1976, p. 110). He has stressed the methodological aspect of the *Principles*, "postulating" that

> it contains the boldest and indeed most breath-taking presentation of a large-scale system of hypotheses, deductions and comparisons with data that I am acquainted with in the period in Britain. Charles Darwin's theory is much more limited, by contrast. Consider this: that Lyell postulates the nature and intensities of *all possible* geological causes. I do not remember Isaac Newton being that extreme. (ibidem: 113)

The Logic of Uniformity: Methodological vs. Substantive Principles

The methodological analysis of the Uniformity Principle performed in this section will consist essentially of making clear the connections existing between the elements of Lyell's program. The evaluation of these connections is not a strictly logical issue, because there is no deductive or necessary link between the three principles. It is rather a methodological issue because it depends on the methodological ideal or option which is adopted.

I will consider two options.

a) The first one is inspired by an ideal of complexity which privileges the investigation of particular or special causes and it has been firmly advocated by William Whewell, so I call it *Whewell's option*. In this view, any uniformitarian restriction to the admission of new causes is perceived as "an injurious limitation of the field of induction. For it forbids us to look for a cause, except among the causes with which we are already familiar." (Whewell 1847, II, p. 280). Given that geological causes are secondary with respect to physico-chemical causes (the primary causes), [5] geological laws have to be able to draw legitimate connections out of the greater complexity of geological processes. So they have to encompass or to take into account many "mapping relationships" (Suppe 1989) which are necessary to follow how a physical law operates in the specific context of a geological phenomenon. And there is no "super-law" to rule the interactions of many or different kinds of laws (Cartwright 1983, p. 70). As a result of such inquiries, we get a sort of "proliferation" of theories which are informed by several boundary conditions.

b) The second option aims at a different ideal. I call it *Newtonian option*. Even though the geological causes are secondary, the better methodological policy would be to avoid the proliferation of local or *ad hoc* theories and to try to pick up simple fundamental laws, common to a vast domain of phenomena and based on *verae causae*. Accordingly, scientists are strongly committed to the control of the scientific quality of theories and to demarcate real causes from fanciful ones. In this case they adopt the ideal of "simplicity" and the "principle of parsimony", which are typical of Newtonian science among others.

I will not argue that one option is correct and the other is mistaken. My claim is that each option determines a methodological bias, and that Lyell's bias is obviously for the second option, that is for a Newtonian attitude. This very attitude conveys his great ambition to establish geology as a true science that dispenses with "all hypothetical causes" and possesses "fixed *principles* [...] as Newton had succeeded in doing to astronomy" (Lyell 1830, p. 61; emphasis added).

Did Lyell realize his ambition?

The rigorous search for the most general causes (*verae causae*), which is to be developed as a condition of this realization, demands the unity of the previously considered three principles, for the simple reason that it turns into setting necessary limits (in addition to those already posited by the Principle of Uniformity) to the plurality of theories as well as to the assumption of less general causes. Hence, before coming to a reasonable estimate of Lyell's achievement, one has to reject, once and for all, the opposite possibility that his program might be affected by an essential methodological "disunity", as many scholars who foster the ideal of complexity tend to maintain.

The methodological value of the Principle of Uniformity remains undisputed (Gould 1980, 1987; Hooykaas 1963, p. 161; Rachel Laudan 1987). The idea of uniformity, or the belief in the regularity of nature, is a necessary condition for even starting any scientific endeavour. But when Lyell limits himself to saying that changes on the earth's surface must be explained "by reference to causes now in operation", he does not state a law but a principle, a second-level or meta-law. Thus, the statement which proposes the uniformity is neatly constructed as a methodological principle. Its objects are not matters of fact or inferences from one observed fact to another, but the substantive laws which should introduce an order in these facts. This is the reason why Lyell was able to defend the principle from his critics so successfully: as a methodological statement, it simply evades direct empirical refutations (see Ospovat 1977, p. 319 and Ruse 1979, p. 46).

At this point the disarticulation of the Lyellian project is started by hanging on to this very "meta-" position of the principle, that is by blowing up that sort of incompleteness which, obviously enough, is implied in its status of being logically separated from experience. So far, indeed, we know only that there is some kind of uniformity, not what uniformity exactly is or what is its degree. Whewell reproached Lyell for having arbitrarily supposed the existence of a demarcation line between the so-called uniform phenomena and the catastrophic ones: "What are the limits of velocity for them to be called rigorously uniform ?" (1847, I, pp. 665). [6] That incompleteness occurs, according to the tradition of early logical empiricism, because a methodological tool can master the complexity of scientific knowledge only if endowed with a logical and linguistic structure "essentially richer" than that of the substantive laws to be ruled. [7] This is the well known problem of underdetermination of the methodological rules with respect to substantive scientific laws. On these premises, it is impossible that a single statement, like the principle of uniformity, could claim to impose any form of arrangement or discipline on substantive laws. As I noted above, the principle turns out to be close to a common-sense rule, and J. S. Mill (1973, p. 311) remarked that "it possesses rather the brevity suitable to popular, than the precision requisite in philosophical language". Stephen Jay Gould (1965, p. 227) even concludes that today it represents nothing but a "superfluous" and anachronistic claim. Now, the right answer for contrasting the alleged underdetermination of the first principle could be to follow the very suggestion of Lyell and to join it to the second and the third requirement.

But this solution does not seem to be open because the logical status of both requirements has been strongly questioned for its ambiguity: they share with the first one the methodological claim to rule substantive laws, but they also contain a principle of measurement which directly affects the empirical facts. Hence, Gould (1965, 1987) has argued that the second and the third requirement are "substantive" laws, constituting a specific doctrine which he calls "substantive uniformitarianism", and which has been repeatedly falsified. As a consequence, he dissociates them from and even opposes them to the Uniformity Principle. He charges Lyell with having confused under the common "umbrella" of uniformity these two heterogeneous kinds of claims: Lyell then "pulled a fast one – perhaps the neatest trick of rhetoric, measured by subsequent success, in the entire history of science. He labeled all these different meanings as 'uniformity', and argued that since all working scientists embrace the methological principles, the substantive claims must be true as well." (1987, pp. 118-19)

Apart from his personal rhetoric and indulgence in the usual tendency to put Lyell's psychology on trial, Gould seems to have forgotten that the second requirement has also an undeniable methodological impact, in so far as it can rule acceptance or rejection of substantive laws. Actually, in the history of science the case of principles which are formulated both as methodological rules and as law of nature is a frequent one, e.g., Bohr's Principle of Correspondence (Bohr 1961, pp. 37, 70) and Einstein's Principle of Relativity [8]. Thus I think that Gould fails to convince us that the first principle and the two other requirements are such logically different and heterogeneous claims and, as a matter of principle, he does not offer any logical reason for ruling out the possibility that methodological and substantive statements could be gathered up either to form a methodological set or system or to form the methodological premise of a work of science. Thus, I will assume, at least as an hypothesis, the unity of Lyell's principles, even though a better arrangement for them and a more appropriate meaning for uniformity intended as a "principle" have to be found.

Lyell and Newton

Once it has been ascertained that there are no serious reasons to doubt the unity of the Lyellian method, the next step will be to scrutinize Lyell's positive targets and the meaning of his conception of science. In this section I will try to set out that, although Newton was quoted only occasionally in the *Principles of Geology*, we can comprehend properly the theoretical disposition, the achievements and the shortcomings of this work in the light of Newtonian method of reasoning.

Scholars have widely recognized this. They have referred to well known passages of Lyell's collection of letters, where he declares that he will attempt to set up "the *principle of reasoning* in the science" (1881, I, p. 234), or he contends, writing to Whewell in 1837 (ibid., II, p. 3), that, "The reiteration of minor convulsions and changes is [...] a *vera causa*, a force and mode of operation we know to be true. The former intensity of the same or other terrestrial forces may be true; I never denied this possibility; but it is conjectural." In this sense, the role of John Herschel in establishing the Nineteenth Century's standards of scientific rigour upon the Newtonian concept of *vera causa* [9] has been highlighted (Ruse 1979, pp. 56-60) and the equivalence of the Lyellian concept of uniformity to Newtonian idea of natural legality, as expressed in the First and in the Second Rule of the *Principia* has been stressed (Hooykaas 1963, p. 140). Indeed, uniform causes and *verae causae* share the character of being spatio-temporally unrestricted (see especially the Second Rule). Rachel Laudan (1987, pp. 205-206) has appropriately argued that the only causes which, according to Newtonian rules, are observable ("collected by general induction", as the Fourth Rule says) and not hypothetical (first rule: "both true and sufficient to explain their apparences") are precisely the causes *now in operation*. So, briefly, Lyell's geological uniformitarianism "can be seen as a straightforward extention of the *vera causa* principle to a situation in which the cause and effect are widely separated in time". (ibid.)

Rachel Laudan also suggests that, once we accept Lyell's commitment to this principle, all the problems regarding the unity of his program vanish. I intend to develop this point, because it gives us a hopefully conclusive reason to figure out the meaning of the controversial Lyell's second and third principle. Then, let us consider Newton's Third Rule. It states that "The qualities of bodies, which admit neither intention nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever". (1968, p. 203) This is addressed to guarantee the physical stability of bodies in order to render them objects of laws of nature (McMullin, 1978, p. 7 ff.). To this purpose, Newton suggests that a group of universal and not variable qualities has to be identified, and then these qualities have to be ascribed to all bodies belonging to a certain type. [10] Likewise, Lyell aims to fix the stability of a cause through the relative invariance of its degree of operative energy, in such a way that the same stability is bestowed both to laws describing how the cause operates and to bodies which are objects of these laws. However, a body is supposed to undergo physical transformations, due to its interaction with its environment. To ensure that these transformations do not entail any substantial alteration of its structure (or of its universal qualities, in Newton's words), one has to posit that changes were not unidirectional and, according to Lyell's third principle, that they did not move progressively away from a stationary standpoint. Thus, Newton's *Rules of Reasoning* lead us to assume that *the whole set* of Lyell's requirements is both a proper and a necessary support for his ambition of raising geology "to the rank of an exact science" (Lyell 1833, III, p. 3), endowed with true laws of nature.

But as soon as the unity of Lyell's program is recognized, new problems arise. Indeed, if geology is to be considered as an exact science, one needs to distinguish geological *verae causae* from the plurality of proximate causes and of the "intermixture of effects", as Mill used to say. And inasmuch as Newton speaks of "Investigation of difficult Things by the Method of Analysis", he himself seems to be well aware that the way to Simplicity is not easy:

> By this way of Analysis we may proceed from Compound to Ingredients, and from Motions to the Forces producing them; and in general, from Effects to their Causes, and from particular Causes to more general ones, till the Argument end in the most general. This is the Method of Analysis: And the Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phænomena proceeding from them, and proving the Explanations. (Newton 1952, pp. 404-405)

When Newton says that the Synthesis assumes the most general causes as *Principles*, he puts forward the idea of a hierarchy of laws, in which ultimate causes are regulated by Laws of Nature (Principles) and derivative causes are ruled by local, or spatio-temporally restricted laws. Given that, the concept of "principle" no longer has the methodological value which had been previously attributed to it: at least, not exclusively. A principle is not only a meta-law. Can it be a sort of super-law (in the sense mentioned at page 6) and therefore be active in coordinating the laws of nature and the local laws which are typical of geology as a secondary science, but which exist (see Herschel 1830, pp. 85 ff.) also in physics? Or is there another device to get those laws connected? Neither Newton nor Lyell give an *explicit* answer to this question. But an *implicit* answer can be found in Newton's scientific practice. His method was worked out in the following terms: he was able to reformulate complex physical problems in simpler mathematical relations which were "analogous to, but not identical with", the physical situation. Then he could make more clear the physical problem by studying the mathematical properties of the analogue. By this procedure, "certain aspects of natural philosophy are reduced to mathematical principles, then developed as mathematical exercises and finally reapplied to physical problems" (Cohen 1990, pp. 32-35; see also Mamiani 1976). On this premise, cause-effect connections can gain a substantial credibility, which vindicates the inductive claims of Newtonian science. The typical unreliability (or lack of universality) of inductive generalizations is effectively counterbalanced by the cultural authority of mathematical theorems.

Newton's style reveals a special modernity. It is remarkable that the present-day semantic conception of scientific theories is based on a similar, although more sophisticated, process. It includes two stages. In the first stage a theory is correlated with the phenomena it is supposed to account for by means of "mapping relationships" which translate raw facts into a "physical system". This is an "abstract replica" or an "ideal" representation of phenomena via mathematical measurements, correction procedures, experiments, which are essentially counterfactual in nature. Then, in the second stage (computational in nature), the data of the physical system, together with data about boundary conditions, "are used in conjunction with the laws of the theory to deduce various predictions or explanations" (Suppe 1989, pp. 68-71). Using Suppe's terminology, one could state that mathematics has been the great Newtonian "mapping relationship".

Indeed, regarding the treatment of causal stratification, Lyellian policy is more than elusive. There is not much room in his work for a mathematical appraisal of this matter. Lyell seemingly declines to tackle the increasing complexity of the foundation of geology. Let us consider a passage of the *Principles* regarding the "causes of vicissitudes of climate": "[...] if instead of vague conjectures [...] we fix our thoughts steadily on the connection at present between climate and the distribution of land and sea [...] we may perhaps approximate a true theory. If doubt still remain, it should be ascribed to our ignorance of the laws of Nature, not to revolutions in her economy." (Lyell 1830, p. 105) Here Lyell introduces the possibility that the distribution of forces in the "economy of nature" (a teleological concept!) turns out to be different from the essentially simple structure of fundamental causes. And in the previously quoted letter to Whewell, he mentions that the uniformity of causes "must for ever produce an endless variety of effects" which men hardly conceive in an "aggregate" form (Lyell 1881, II, pp. 2, 6). But he seems less bothered by the philosophical implications of this discrepancy than he is worried about ruling hypothetical causes out of geology. He does not seem interested in the construction – however difficult it could be – of analytic machinery similar to that which had made great the name of Newton. He limits himself to the placement of a demarcation line between true geology and pseudo geology. Is this like to tone down the ambitions and the tasks to which he had committed himself ?

To be sure, the absence of such an analytical apparatus has significant negative consequences for the overall firmness of Lyell's theory. So far this has been successfully defended in the face of its critics' assaults, but now two crucial points turn out to be greatly weakened. First, the centrality Lyell had granted to present causes in the process of selection of *verae causae* is called into issue: if cause-effect links (or the whole process of causality) are not secured by means of lawful or, at least, lawlike functions, present causes cannot be fully allowed to count as effects of past causes, and cannot, therefore, take part in the regressive movement towards true causes. Whewell (1863, II, p. 593; see also Ruse 1991, p. 108) pitilessly draws our attention on to these flaws in Lyell's reasoning:

> In truth, we know causes only by their effects; and in order to learn the nature of the causes which modify the earth, we must study them through all ages of their action, and not select arbitrarily the period in which we live as the standard for all other epochs.

The second point is that the lack of an adequate network of functional (mathematical) correlations complicates the status of geology as a secondary science – as has been repeatedly mentioned. Ironically enough, Sedgwick (1831, p. 305) contends that, "To assume that the secondary combinations arising out of the primary laws of matter, have been the same in all periods of the earth, is, I repeat, an unwarrantable hypothesis with no *a-priori* probability." These limitations also justify Herschel's judgments about geology: it should theoretically

> ... rank, in the scale of the sciences, next to astronomy; like astronomy, too, its progress depends on the continual accumulation of observations carried on for ages. But, unlike astronomy, the observations on which it depends ... can hardly yet be said to be more than commenced. ... Of course inductions founded on such limited examination can only be regarded as provisional, except in those remarkable cases where the same great formations in the same order have been recognised in very distant quarters, and without exception. (1830, p. 288)

Herschel's realistic judgements confirm that *in Lyell's time*, geology did not have the capacity to process empirical data as rigorously as Newtonian physics had (see Porter 1977). It was not able to isolate scientific rules as effective as the laws of motion or to construct a body of knowledge as powerful as Newtonian mechanics and optics, such as to draw even elementary predictions out of causal complexity. As I said above, the best product of the Lyellian method of *vera causa*, namely the theory of climate, ended up with losing its predominant role in the last editions of the *Principles of Geology*, because it supported a simply adaptationist theory of organic evolution which proved to be at odds with the Darwinian theory of natural selection. Hence, Lyell's research program met a paradoxical destiny or perhaps a contradiction: the system of methodological principles which had been hopefully cast to guarantee a proper support to a set of reliable laws of nature, has, at last, yielded only false laws. Lyell was not the author of a scientific revolution.

Nevertheless, Newtonian science has to be taken as a frame of reference to understand Lyellian geology, not as a standard of evaluation to level criticism against it. We must be aware that *principles*, not laws, have turned out to be the most significant theoretical units of Lyell's science and, among them, the three requirements have a peculiar methodological role. They have been the tools by which he conferred legitimacy on geology. This was clearly perceived by his contemporaries. They have maintained that Lyell settled the tradition, which is different but perhaps not much less than making a revolution. "Lyell's function was mainly that of a critic [...] a philosophical writer [...] with a rare faculty of perceiving the connection of scattered facts with each other and with *the general principles of science*." (Geikie 1897, p. 404, emphasis added) A. C. Ramsay (who was president of the Geological Society in 1863) said, "We collect the data and Lyell teaches us to comprehend the meaning of them," and Charles Darwin wrote in 1844, "I always feel as if my books came half out of Lyell's brain".

To illustrate the philosophical grounds of these statements, a further reappraisal of the general disposition of Lyell's program is needed. This reappraisal should ascertain precisely whether principles, although not supplemented by laws of nature and by a mathematical apparatus, can perform a role in a scientific research program. As a scientist, Lyell does not provide any philosophical treatment of this question, so that we will have to try and gather all the helpful elements to frame the function of principles and their cognitive limits.

The Function of the Principles

I would highlight two main differences between principles and laws.

First, principles do not hold the same empirical content as laws do and, as a consequence, they cannot be directly falsified. The principle of uniformity, as I have stressed above, has no empirical content at all. It is unfalsifiable. The principle of uniformity of rate and that of steady-state might be falsified in an indirect way or fall into disuse only if a certain number of laws which connect them to empirical data is falsified as well. And this has never happened in a definite way, despite Gould's opinion.

Secondly, principles seem to claim a systematic priority over laws of nature. It can be generically admitted that a principle constitutes a source of justification for the laws of nature which, in their turn, govern the stream of experience. But this claim is indemonstrable. [11] Scottish philosophers have widely recognized it. Notoriously, Hume had put his finger on the very contradiction by which the principle of uniformity, far from being able to assure the regularity of experience, is stated beginning from the very experience it should guarantee. Thomas Reid had echoed him saying that our belief in the uniformity of nature is not rationally arguable but grounded upon an "instinctive prescience" of the operations of nature. (Reid 1983, p. 199) And according to Curt Ducasse (Blake, Ducasse, Madden 1966, pp. 226-227), even Mill, despite his efforts, was not able to settle down the problem much better than Hume had done. Ducasse quotes a passage from *A System of Logic* where Mill aknowledges that the principle of Uniformity was originally inferred from empirical uniformities not by means of a rigorous induction, but rather by means of "the loose and uncertain mode of induction *per enumerationem simplicem*." (Mill 1973, p. 567)

Unfalsifiable and indemostrable as they are, principles prove to be difficult to handle for philosophers of science. The more consolidated they are, the more their role can be paralleled to the one of the the well known "rules of thumb" used by all scientists. They could be even taken as simple common sense rules. The principle of uniformity surely can. Actually Thomas Reid (1983, p. 97), the leader of Scottish School of Common Sense, considered Newton's rules as "maxims of common sense" and Scottish philosophy exerted a considerable influence upon British science throughout the Eighteenth and Nineteenth Centuries (Olson 1975; Grave 1960). But Lyell went beyond the trivial application of a common sense philosophy and beyond the rhetorical respects which all Nineteenth Century scientists paid to the Newtonian method.

Thus, our problem has almost reached his final settlement: Lyell did not discover laws of nature; he did not provide any mathematical treatment of geological phenomena or any "semantic" model which could put some order in the stratification of primary and secondary causes; and, finally, his methodological principles are unseizable and underdetermined. How was he able to be regarded as the founder of scientific geology?

Lyell's first goal was to ensure the success of geology as a new science and to sweep away its poor reputation "for vague speculation and divisive disagreement". (Hodge 1991, p. 276) To this purpose, principles were, despite their limits, the right conceptual tool.

Some concerns could arise from the fact that the philosophical status of principles has not been sharply determined, as yet. Indeed we have not been able to set up any logical distinction between methodological and substative principles or between principles and laws. But the problem can be overcome since Quine (1961) has debunked the dogma of a fundamental cleavage between analytic statement (indipendent of matters of fact) and synthetic statements (grounded in fact). Therefore, it does seem to me that the distinction between non-empirical principles and laws having empirical content is equally blurred. Quine (ibid., pp. 17, 44) has also shown that a true empiricist philosophy has to consider the conceptual schemes (or the ontologies) used by scientists as "cultural posits" rather than a truths of reason. Thus, I think that Lyell's principles may produce the unificaton of geological knowledge when they are worked out into a unitary strategic scheme and when they are so consistent with the philosophy of science of that time as to ensure for the new geology the consensus it then needed.

To start with, if a single principle is liable to be paralleled to a mere common sense rule, many principles, when they are arranged together, can constitute a scientific research program or even a paradigm. If we assume the theoretical unity of the three principles (Uniformity, Uniformity of Rate and Steady-state) we must admit that they form a coherent scheme and even a powerful and well integrated scientific program which allowed Lyell to differentiate uniformitarian geology from other competing theories and to confront catastrophism and directionalism. As a second step, we have to revise the above statement which parallels principles to common sense rules. It would be more appropriate to say not that principles *are* common sense rules, but that they *make up a connection between* science on the one hand and, on the other hand, philosophy, culture or common sense. Aristotle attributes to Dialectic the task of discussing the first principles of science.

(*Topics* 101 *a*35-*b*4) And Dialectic indeed works to establish a mutual link between the opinions of people and those of philosophers. (*Topics*, 104 *a*9-13) Accordingly, principles can be associated to "theoretical assumptions" held by scientists and philosophers as "unproblematic" or "unchallenged at least for the time being", assumptions which are located by Karl Popper (1972, pp. 165, 181; 1976, pp. 238 ff.) within the so-called "background knowledge". On this ground, Lyell's principles can be considered widespread "cultural" values which, although underdetermined with respect to laws, have a significant impact on the structure of scientific research.

My contention is that Lyell's principles (although not explicitly analyzed) and his peculiar interpretation of some philosophical key-concepts, such as "cause" and "hypothesis", reinforced his geology locating it in the framework of the dominant British empiricism, namely in the complex "empiricist-inductivist tradition" which was started by Thomas Reid and "reached its culmination in the work of Mill." (L. Laudan 1981, p. 96) The Humean concept of "belief", the Scottish common sense philosophy, Newtonian heritage and natural theology merged in this tradition and found a synthesis in Herschelian philosophy of science. (Cannon 1961b)

In this view, Lyellian horror of "the utmost licence of conjecture in speculating" on causes (1830, I, p. 86) and the strict discipline for the selection of hypotheses, exasperated by the struggle with Catastrophists and successfully turned against De Beaumont's speculations, can be traced back more to Reid's ideas than to Newtonian statements. Newton had used the term "hypothesis", notwithstanding the motto "*Hypotheses non fingo*", with many legitimate meanings. He had employed hypothetical reasoning to introduce postulates, mathematical axioms or other principles not liable to be empirically tested. The only meaning which Newton had completely rejected was that of "philosophical romance", attributed to Descartes' physical speculations. (Cohen 1956, pp. 138-143) On the contrary, Reid stressed the literal meaning of the Newtonian statement and interpreted it as a sort of anathema indiscriminately addressed to every kind of hypotheses. (Reid 1983, pp. 234-236) Following this path, Lyell's restrictive attitude ended up backing the hyper-empiricism of John Stuart Mill, who argued, against Whewell, that an hypothetic law can be admitted only if there is no other law available which leads to the same conclusions. (Mill 1973 p. 492) Lyell did not even use theoretical terms to designate unknown causes – a procedure which had been common in Eighteenth Century biology (see Hall 1968).

By the same token, Reid's treatment of the concept of cause might be viewed as an endorsement for Lyell to disregard the cause-effect link, as I mentioned above. Reid had emphasized the theological perspective which attributes the character of efficiency – or even the name of cause – exclusively to God as first cause. Hence, he maintains that physical causes are passive and concludes that "we have no ground to ascribe efficiency to natural causes, or even *necessary connection with the effect*". (Reid 1983, p. 76, emphasis added)

Finally, there is also a third feature of Scottish culture which can bring about a strong vindication for the lack of mathematical structure in the work of Lyell. This is the traditional Scottish idea of science, which was based less upon mathematical formalizations than upon a mutual relation betweeen science, experimental knowledge and technology. (Davie 1964, ch. 8; Olson 1975, pp. 62 ff.) Nevertheless, the question is not to be put in such a radical way. The Scottish scientists had different positions, and if, on the one hand, Hamilton strongly advocated the humanistic and democratic bias of Scottish science against the "arrogance" of mathematicians and the artificiality of algebraic procedure, on the other hand, Forbes and Maxwell were convinced of the necessity for reaching a compromise and introducing mathematical physics in Scottish universities (Davie 1964, p. 198). Be this as it may, I would like to note that the experimental inclination of Scottish science furnishes us with grounds to defend the overall consistency of Lyell's scientific program by means of another framework, alternative to that latent Newtonian paradigm within which the absence of a mathematical structure is considered to be an irremediable shortcoming. Herschel's adherence to the *Principles of Geology* [12] hints at a further confirmation of that consistency. If Herschel had not supported Lyell with his authority, Catastrophist opposition could have solidified "as a mathematically-oriented Cambridge group" (Cannon 1961, p. 302).

In this paper my aim has been to show that the scientific endeavour of Lyell was inspired by an ideal of simplicity convergent with the one that had moved Newton, and I hope the framework I have outlined supports this view.

If scientists represent the values of their epoch, then Charles Lyell did. In 1830 the models of science were Newtonian physics and Herschelian astronomy. If geology had to be established as a science, it needed firm principles to be cast on the ideal of simplicity and such as to confront other reconstructions of earth's history and to keep at bay the fancy hypotheses about the extraordinariness of past events. The principle of uniformity, the principle of uniformity of rate, and the postulate of steady-state – *when bound together* – are tools designed for the purpose, methodological premises for a scientific program which, nevertheless, has been less successfull than the Newtonian program. However, I do not intend to endorse the idea that geology is logically different, as a matter of principle, from other sciences, and I think that it has been argued correctly (Watson 1969) that the purported difference amounts only to a contingent disproportion between the complexity of geological events and the present limits of our intellectual capacities. Nor do I want to deal with the broader question of the present existence of geological or biological laws. This question is still open [13] and it is beyond the scope of this paper. Since only few geological laws and theories are available, it is not possible to bridge the logical gap between methodological principles and the huge amount of empirical data which are at our disposal in the *Principles of Geology* in the form of historical observations. This is a raw fact, but a contingent one, and it does not say anything about the scientific status of geology. Indeed, the historical data can participate in the process of accumulation of scientific knowledge. As Hull (1974, p. 141) suggests, when we study partially open systems – like biological and geological ones – and when we have only weak laws "most of the explanatory weight" is carried by background knowledge and boundary conditions. The specification of such conditions is so complicated that it "takes on the character of an historical narrative".

Let me finish with the quotation of a long passage where Lyell, perfectly aware of the nature of his work, admirably mixes realism and pride:

> By the geometer were measured the regions of space, and the relative distances of heavenly bodies – by the geologist myriads of ages were reckoned, not by arithmetical computation, but by a train of physical events – a succession of phenomena in the animate and inanimate worlds – signs which convey to our minds more definite ideas than figures can do, of the immensity of time.
>
> Whether our investigation of the earth's history and structure will be productive of as great practical benefits to mankind as a knowledge of the distant heavens, must remain for the decision of posterity. [...] The practical advantages already derived from it have not been inconsiderable: but our generalizations are yet imperfect, and they who follow may be expected to reap the most valuable fruits of our labour. Meanwhile the charm of first discovery is our own, and as we explore this magnificent field of inquiry, the sentiment of a great historian of our times [Niebuhr] may continually be present to our minds, that "he who calls

what has vanished back again into being enjoys a bliss like that of creating." (1830, I, pp. 73-74)

Giovanni Camardi

ABSTRACT: The theoretical system Lyell presented in 1830 was composed of three requirements or principles: 1) the Uniformity Principle which states that past geological events must be explained by the same causes now in operation. 2) the Uniformity of Rate Principle which states that geological laws operate with the same force as at present 3) the Steady-state Principle which states that the earth does not undergo any directional change. The three principles form a single thesis called "uniformitarianism" which has been repeatedly questioned and which has been reputed to be unable to face the competing "directional synthesis" based on the theory of the earth's cooling down. As a result, the significance of Lyell's system has been reduced to a simple "actualism" which admits the validity of the only Uniformity Principle. I believe that the only way to understand Lyell's role in the history of science is to maintain the unity of his synthesis. To show the newtonian roots of this synthesis I will compare Lyell's principles and Newton's *Rules of Reasoning*. I will conclude with an analysis of the methodological function of principles in Lyell's scientific endeavour.

KEY WORDS: catastrophism, central heat theory, laws of nature, Lyell, Newton, principles, semantic conception of theories, uniformitarianism.

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NOTES

1 Lyell, 1872, I: 143 ff. Lyell made his crucial modifications in the tenth edition, 1867. I quote now from the eleventh edition, which does not show considerable changes with respect to the preceding one with regard to the matters with which I am concerned. It is worth pointing out that Lyell dismissed his

environmental determinism and accepted the doctrine of organic progression as an "indispensable hypothesis" for "guiding our speculation" (1863: 404, 475; see also 1970: 55, 223, 267, 275-82), which, however, had to be reconcilied with the concept of the uniformity of change (1872, I: 303-307). Bartholomew (1973: 302; and 1976) says that Lyell's acceptance of evolution was "grudgingly given and limited in extent". Actually Lyell never dismissed anti-progressionism *as for the "inanimate world"*. Indeed, the tenth and eleventh editions of the *Principles* maintain his tectonic and climatic theory, his definition of the theory of cooling down as "arbitrary" (1872, I: 234) and the objections to De Beaumont's theory, which are restated "in the same terms" as in the first edition (1872, I: 120-25).

[2] For a more prudent evaluation of the impact of de Beaumont's work see Rachel Laudan (1987: 221) and Wilson (1969: 430). For the substitution of Kelvin's and Fourier's theories by that of radioactive origin of the heat see Gohau (1990: 172-74), Hamilton (1965: 2) and Brown (1982).

[3] Ospovat, 1977: 329. I am not sure that the oscillations typical of a steady-state equilibrium had to be *necessarily* coincident with the repetitions typical of a cyclical process.

[4] Cannon claims to have no memory of the source of this expression. I guess that it came from the theorems of equilibrium in neoclassical economics, which are, indeed, illustrated by means of "curves of indifference".

[5] The dichotomy of primary and secondary causes can also be interpreted as referring to theological and natural causes. I will provide the relevant explanations when and if the context turns out to be ambiguous.

[6] Whewell seems to consider the concept of uniformity less an underdetermined methodological principle than a Kantian category.

[7] See Tarski, 1944. According to Tarski's theory, if the Uniformity Principle is a meta-law, it should be located in a language of superior level (meta-language) with respect to the substantive laws (second and third requirement). Thus, it can be meaningfully considered true only within this second level language (by means of a third level language). As a matter of principle, its truth cannot imply the truth of statements belonging to a lower level (such as the second and third requirements do), unless the meta-language contains the lower level language as a part, and unless "in its logical parts it is '*essentially richer*' than the object-language". But if we take into account that the Uniformity Principle is the only statement included in the meta-linguistic level, it is rather unthinkable that it could be so rich and yield conditions of truth or of control for the statements or the laws of lower level.

[8] This last case has been suggested to me by Arthur Fine, who has pointed out that the principle of Einstein definitely ruined any sharp distinction between methodological principles and laws of nature.

[9] Actually, Herschel (1830: 146) quotes Lyell's theory of climate as an example of correct application of the concept of true cause.

[10] For a more extensive study of the Third rule and of "universal" or essential qualities see McGuire (1995).

[11] Aristotle (*An. Po.*, 100 *b* 5-15) argued that principles of science are collected by induction and are grasped by "intellect" or "intuitive reason" (*nous*). Therefore – he says – they are not an object of scientific or *demonstrative* knowledge (epistêmê). I think that this is equivalent to saying that their cognitive claim is logically indemonstrable. In any event, Aristotle is not concerned of the skeptical conclusions which could possibly be derived from this statement.

[12] See the letters that Herschel sent to Lyell from South Africa in 1836 and 1837, edited by W. F. Cannon (Cannon, 1961).

[13] For the most updated outline of the issue, see Sober (1996).

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